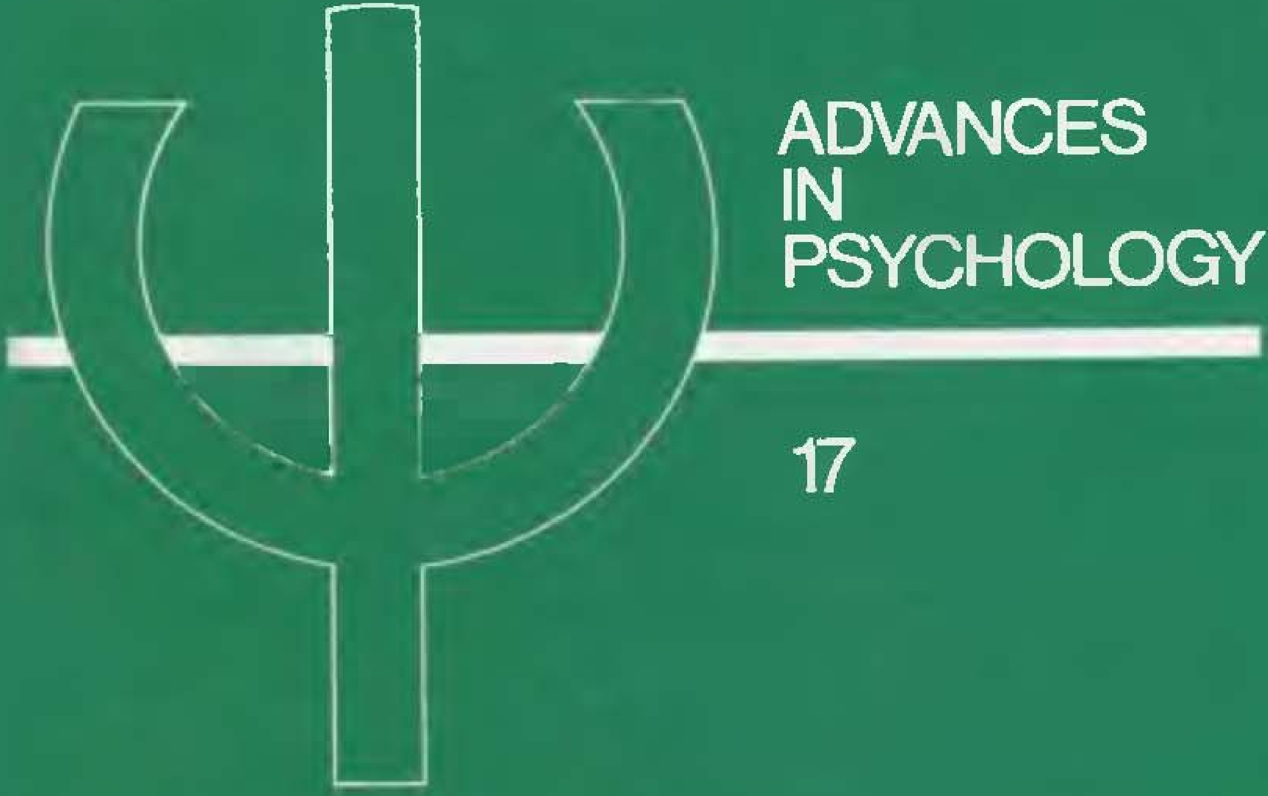

ADVANCES
IN
PSYCHOLOGY

17

Human Motor Actions
Bernstein Reassessed

Edited by
H.T.A. Whiting

North-Holland

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Editors

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NORTH-HOLLAND
AMSTERDAM • NEW YORK • OXFORD

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FOREWORD

This book comprises six sections. Each section provides two sources of information:

a) a reproduction of one of the six chapters from the original English-language text:

Bernstein, N. The Co-ordination and Regulation of Movements.
Oxford: Pergamon, 1967.

b) Two original chapters provided by scientists from an international forum.

The sequence:
Bernstein chapter (without post script - e.g. CHAPTER I);
original contribution chapter (with post script - e.g. *CHAPTER Ia*);
original contribution chapter (with post script - e.g. *CHAPTER Ib*) is maintained throughout each section.

The nature of the original contributions is elaborated upon in the Preface.

Apart from the correction of spelling faults, where they arise, the six chapters of Bernstein are reproduced verbatim from the original text. This means that throughout the book two different systems of referencing are used (some of the references from Bernstein also being incomplete). It was thought however better not to magnify any errors which may have arisen as a result of translation from the original German or Russian language. To help the reader further to obtain access to original sources, the complete list of references from the Bernstein book is reproduced at the end of the text.

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PREFACE

In 1967, Pergamon Press Ltd. (Oxford, England) published a seminal text:

The coordination and regulation of movements

which comprised a collection of the more important publications - written between 1934 and 1962 - of the eminent Russian physiologist Nicholas Bernstein. Unfortunately Bernstein died shortly before the appearance of that publication but his immense influence on what he called 'the science of human movements' lives on both through his own publications and those of his students and devotees. The extent of his influence is reflected in the fact that it would be unusual - even to this day - to find any important publication on movement control and organization which did not make reference to this, the best known English translation of his work (see for example: Arbib, 1981; Fitch & Turvey, 1978; Gallistel, 1980; Magill, 1983; Pribram, 1971; Shaw & Bransford, 1977; Stelmach, 1976; Stelmach & Requin, 1980; Wadman, 1979).

In spite of this fact, 'The coordination and regulation of movements' has, for the last five years, been out of print and there were no plans by the original publishers to reprint.

It is not intended here to debate the reasons for and against such a decision except to say that it was undoubtedly contributed to by a feeling that the literature was somewhat 'dated'. However, in the light of the frequency of its citation, it is not at all clear to what extent this label is either meaningful or justified and in how far the position reached by Bernstein needs to be challenged and/or modified. This interesting and important question together with the fact that many students and scholars find it difficult to gain access to the 1967 text, prompted the decision not only to re-publish the original articles but, in addition, to include an evaluation and elaboration by some of the scholars who have distinguished themselves in the field of human movement study.

It is interesting to note that in his 1957 paper 'Some emergent problems of the regulation of motor acts', Bernstein refers to the fact that 'during periods of scientific quarrying, glimpses of the new ideas and general forecasts may now be noted in the work of the older classical physiologists' - thus recognising his indebtedness to his predecessors. In turn, in the present text, a similar indebtedness to Bernstein is recognized for, as Reed in his reaction to Chapter II clearly indicates, Bernstein (in this chapter) not only produced glimpses of what today are often considered to be new ideas, but also produced provocative analyses of 'some of the most contested issues of contemporary movement science'. Amongst such issues would figure 'distributed brain function', 'mass-spring models', 'phase relations in action', 'functional hierarchies of the nervous system' and 'the role of sensory processes in motor control'. This is only to confirm Luria's (in his introduction to Bernstein's 1967 book) contention that Bernstein 'was a man who had the remarkable gift of penetrating into the future'.

Although generally labelled a physiologist it is clear from the breadth and scope of his ideas that such pigeon-holing is a reflection more of his formal training than of his conceptualization. In the papers that follow in the present text, there are hints of the biologist, the psychologist, the mathematician and the philosopher. It is indeed humbling to recognize so many talents in one man. This makes not only the selection of the contributors to this text that more difficult but also makes their task that much more complex. It was with these considerations in mind that a team of contributors was put together. Fortunately, to a man, they were enthusiastic about the proposal. While one or two of the original team were unable, for personal reasons, to complete their assignments there were fortunately others of similar stature able to take their places.

The assignment for the contributor was twofold:

1. To give a general evaluation of the chapter assigned to them,
2. To indicate the way in which more recent research and thinking has led to the need either for a reconsideration of the ideas presented by Bernstein or the way in which this thinking needs to be extended and/or modified.

In general,contributors were asked to restrict the extent of their contribution to a similar number of words as appeared in the chapter assigned to them. Deviations in this respect reflect the individual wishes of the contributors.

It was never the intention that the final text should comprise a critique of Bernstein but that it should rather be prospective in nature. In his chapter on the Biodynamics of Locomotion, Bernstein distinguishes between the prospective approach of his own workers and the retrospective approach of Fischer in the following way:

> Fischer's approach was essentially retrospective, that is, he was mainly motivated by a desire to order critically the basic information available on the mechanics of the act of walking which had been gathered up to his time. Our approach may be termed prospective as we were not particularly concerned with which details might be found to be reliable and which false in the work of the older authors, but attempted to provide a more reliable and comprehensive descriptive basis for a subsequent broad extension of investigation into the genesis and pathology of locomotion.

It was in this spirit, while not confining themselves to the locomotive act, that contributors were asked to work. The extent to which they have fulfilled their assignment is left to the reader. As editor I express my gratitude to all the contributors for their enthusiasm and commitment. A special word of thanks is due to Peter Greene and Curtis Boylls who had the very difficult assignment of writing the introduction based, not only on the papers of Bernstein but, also on the work of the other contributors.

Finally, I would like to express my gratitude to Pergamon Press for allowing reproduction of the original text of Bernstein, to North-Holland Publishing Co. for undertaking the publication of the present text and to George Stelmach and Peter Vroon (editors) for allowing the text to form a part of the series 'Advances in Psychology'. The mammoth task of typing this very lengthy manuscript fell to my secretary Irma Reijnhout. Her skill in this respect is attested to by the quality of the manuscript - for her contribution I am indebted.

H.T.A. Whiting
Amsterdam, The Netherlands
July, 1983

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INTRODUCTION

BERNSTEIN'S SIGNIFICANCE TODAY

C.C. Boylls, Jr., P.H. Greene

N.A. Bernstein, dead now for nearly twenty years, remains a difficult figure for many Western scientists to confront. He is often the center of an uncomfortable mythology, the predictable fate of individuals who, like Socrates, become known more through their students' accounts and accomplishments, than via their own presence. Certainly the language barrier can be blamed for some of Bernstein's remoteness. Even at this writing, the classic monograph, On the Construction of Movements (5), for which Bernstein was awarded a State price in 1947 (10), remains untranslated. However, we also have little doubt that the ill-informed prejudice of past years compromised both the development and the evaluation of Bernstein's achievements. Only one year after Bernstein received his prize, the Lysenko clique began its regency over Soviet biological science (50). Bernstein began to be "subjected to unworthy and unfounded 'criticism'..." which materially interfered with his research program (10). The early Soviet attitude toward cybernetics was also not conducive to Bernstein's thinking during much of this time (Arbib, this volume). Both in the Soviet Union and in the West, a paucity of conceptual tools for studying structure at the task level of description fostered a tendency to dismiss reports of the Soviet accomplishments, as each of us (PHG and CCB) discovered in the 1960's and early 1970's when we discussed the work of Bernstein and his colleagues (and the constructive role of the CNS in sensorimotor activities in general) with Western neuroscientists (3, 23, 24, 25, 26).

There consequently is reason, as Requin and co-workers (this volume) observe, for some to feel "guilty" about not having taken stock of Bernstein's output sooner (not to mention that of his successors). There has been room for a Bernstein "cult" to play upon such uneasiness. We feel that our task here, therefore, must in part be to demythologize Bernstein. But having done that, we believe that the result will be to leave his legacy unspent - a valuable inheritance worthy of the most responsible and diligent scientific stewardship.

Essential Bernstein: The Realization of Controllability and Observability in Motor Tasks

In his introduction to the first English publication of the present collection, Bernstein stressed that his was the study of motor tasks - activities which feature not only motor performances, but also the setting and realization of behaviorally significant goals through planning and problem-solving. Although Bernstein clearly prided himself on the ability of his experimental methods to examine such tasks in great detail (sometimes amplifying error in the process (Rozendal, this volume), he also required that such minutiae be brought back to the understanding of complete tasks using "the widest possible synthetic grasp of experimental evidence". We would point out that as part of this "synthetic grasp", Bernstein was quite prepared to exploit the full scope of longitudinal, clinical, and rehabilitative studies in a framework scarcely attempted even now.

Bernstein's emphasis upon motor tasks led him naturally to consider both the efferent initiation of motor activity and its resulting afferential or sensory consequences. In metaphors such as the "peripheral synapse" (Chapter III, this volume), he forcefully argued that afferent and efferent signals merely mediate communication between concurrent biomechanical and neurophysiological processes that unwind in time during the course of a motor act. This formulation of a neuro-biomechanical dynamics for a given motor task is implicit in the taxonomic mathematics of Chapter II (this volume). Had Bernstein left his description of task dynamics at this stage, then his views would not have differed much from the closed-system notions of Sechenov years earlier, or the advocates of homeostatic control (Arbib, this volume). However, Bernstein left his task-dynamical systems open to external "forcing functions" and parameterizations (or "tuning") supplied by outside agencies - most notably, the resources of the CNS - and in doing so, asked and answered questions that comprise (we believe) the essence of his philosophy:

The first such question has become famous as the "degrees of freedom" problem (e.g., 71). Oftentimes this is posed in the context of the many hundreds of muscles the nervous system is to control, and how nice it would be if the brain had mechanisms for handling such muscles as global units with "simple" control signals (70). Bernstein, however, originally expressed the issue as the straightforward consequence of determining the integration constants for the solution of the dynamical equations of motion (Chapter II, this volume). He implied that the CNS would have to take cognizance of such constants (most likely in the form of initial conditions on the neuro-biomechanical state) in order both to calculate motor commands and to achieve predictable outcomes from their issuance. In other words, the absence of this information would render the skeletomotor system "uncontrollable"; and sensory feedback would have to be arranged minimally to make the system state "observable". With the abundance of sensory information reaching the CNS, one might consider that problem trivial. But Bernstein pointed out (Chapter II, this volume) that the mapping between, say, muscular contractile states and skeletal dynamics is not always unique (differing contractile patterns lead to the same mechanical result, and vice versa). To the extent that motor commands constitute feedback to preserve a pre-established set point, we should expect (59) them to be correlated, not with movements, but with disturbances. But Bernstein's problem surely arises in specifying efferent commands to establish conditions close to the desired set points in the first place. In the present chapters, Bernstein returns to these issues repeatedly in a variety of guises, perhaps because in his era the relationship between CNS activity and motor outcomes was thought to be determinate (Rozendal, this volume). To us it seems rather apparent that Bernstein's concern with the degrees-of-freedom problem is very much bound up in the avoidance of the uncertain, the unpredictable, and the equivocal in synthesizing motor tasks. Other interpretations (see below) came later.

Bernstein's classic solution to the degrees-of-freedom problem was to achieve both skeletomotor controllability and observability by means of constraint. In principle, this idea followed directly from his pursuit of functional relationships describing the dynamics of motor tasks in the interaction of peripheral and central agencies. The intent of constraint is to render these dynamics tractable both to observation and control. In practice, of course, the entire study of "simplifying constraints" - either biomechanical or neural - has now developed such a vitality of its own (to be dealt with shortly) that it is difficult to separate Bernstein's original notions from what has followed. We can safely say (see Chapters II and III) that Bernstein posited agencies in the CNS whose task is to establish a known dynamical regime within which particular motor tasks can be conducted. This regime is to be established in advance of the delivery of any commands for explicit movement, and is chosen "to guarantee optimal selection of conductivity" allowing delivery to the periphery of "the right impulse at the right moment" (Chapter III). Furthermore, these dynamics are to take advantage of such biomechanical phenomena as reaction torques, and they are to minimize the number of equivocal degrees of freedom. As to the CNS faculties which are to effect the necessary constraints among skeletomuscular system states, Bernstein has little to say, except that they "must operate through quite different paths and employ quite different innervational processes" than those used by explicit descending control signals (Chapter III). We shall discuss the accuracy of these insights below.

In other portions of his work, Bernstein may have been considering the further ramifications of motor activities given known dynamics by means of active constraints. Thus, he developed at length (Requin, et al., this volume) the notion that the brain utilizes a model of the future sensory consequences of action in elaborating motor activity, an idea not altogether foreign to portions of Williams James, "ideomotor" mechanism for movement (28). He also considered the so-called "topological" representations of a motor task, whereby invariant relationships among the elements of a task are encoded. Both of these ideas are clearly related to the concept of a stored recipe or function that defines a family of dynamical "flows" of the sensorimotor state during the elaboration of a task in time. Bernstein indeed is quite explicit at times in arguing that the complete time course of a motor act is neurally represented as a manipulable whole (see Chapter II), although he remains vague on mechanism. It is noteworthy that, although present-day theorists tend to presume that hierarchical organization permits the supervisory level to operate with less complexity than lower levels, in Bernstein's hierarchy it appears that higher levels are at least as complex as lower.

Bernstein is also typically associated with hierarchical theories of motor coordination (e.g., 35). The externalized control variables and "tunings" of his task-dynamical functions, which could be supplied by higher levels of a motor hierarchy, lend themselves readily to that interpretation. Bernstein often postulates (Chapters II, IV) "a series of hierarchical levels, each of them, inevitably, having a degree of qualitative independence". However, beyond the positing of constraint mechanisms and accounting for the "automatization" of certain motor tasks (Chapter III; 35), it remained for others to exploit hierarchical motor control architectures to the fullest, as we shall presently describe.

Extensions, Modifications, and Criticisms

As we have described above, Bernstein's conception of motor task realization is founded on the achievement of a known neuro-biomechanical dynamics by means of a hierarchically organized system of constraints that ameliorate the degrees-of-freedom problem. Each element of this philosophy has undergone evaluation and development since its first prescription, certain elements much more than others. We scarcely can deal with every such offshoot here; but there certainly are particular trends and ramifications that bear watching, in our opinion, as we hope now to illustrate:

(1) The status of the 'degrees of freedom' problem

So-called "endpoint" control" has handily finessed Bernstein's original concerns about the nervous system's requirements for knowledge of the skeletomotor state prior to the issuance of motor commands. As is well known, this development can be traced to Feldman's utilization of the "controllable spring" metaphor for muscle parameterization (4), which originally assigned to the nervous system the role of positioning muscle force-length characteristics along the length dimension, thereby storing potential energy in the musculature. Release of this energy, via the relaxation of the musculature to equilibrium "endpoints" governed by the balance of all static forces, could then account for the carrying out of motor tasks independently of initial conditions. The theory has been augmented by demonstrations that descending spinal pathways could indeed effect the necessary endpoint adjustments (18) and that ratios of antagonistic muscular activity could encode movement endpoints (41). Most recently, propositions about "impedance" or "compliance" control (33, 49) have generalized the endpoint strategy to include the dynamics of endpoint achievement through parametric manipulations of joint stiffness, damping, etc. (see also 17).

Bernstein, who seemed to identify the elasticity of muscle as yet another source of dynamical uncertainty in movement programming (as in the elastically-powered ski pole described in Chapter IV of the present volume), might conceivably have viewed these notions as examples of "the older physiology of rest and equilibrium" (Chapter V, present volume) that lacked the dynamics of directed development which he favored (Chapter VI). However, it seems likely that the theory of impedance or endpoint control will soon be recast in terms of potential functions (with endpoints identifiable as the extrema of such functions to be "sought", gradient-fashion, by the state of the skeletomotor system). The realization of this formulation will then lead, we predict, to instances where potential functions are permitted to admit system states into their algebraic structure, thus preparing us for the "discovery" of the bifurcations and catastrophes that may model more closely the unidirectional, branching task-dynamics which Bernstein envisioned.

But would such developments replace Bernstein's original concerns about the problems introduced by degrees-of-freedom in the skeletomotor system? In one sense, the answer is "yes", since considerations of optimum effector configurations taken within the framework of impedance control have identified rationales for the apparently "redundant" presence of muscle that often disturbed Bernstein. Thus, for example, it can be proved that two-joint muscles are required in addition to one-joint muscles to program the directions of maximum overall stiffness or free movement of a limb (33); a four-bar linkage of near-isometric, multijoint "muscles" proves useful in minimizing the energy utilization of legged vehicles (69, 73).

In another sense, Bernstein would probably have felt more comfortable with experimental demonstrations that humans do sometimes attend to both initial and in-progress conditions in the programming and retention of motor tasks (47, 9), and that many elements of even simple task performances are inexplicable in the simple context of endpoint control (68,72). Moreover, as we now will see, one need only to survey recent work on skeletomotor constraints to witness the continued relevance of the degrees-of-freedom issue:

(2) The status of constrained skeletomotor dynamics

In the mid-1960's, as Bernstein's ideas began to be explored by a newer generation of Soviet investigators, remakable multijoint "synergisms" were found to characterize the covariation of joint trajectories in a number of basic motor tasks (e.g., postural maintenance (32); movement of wrist and elbow (37)). While subsequent investigations have uncovered many more instances of "neurally constrained" movement dynamics (e.g., 11, 12, 14, 39, 40, 51), they also have allowed a more systematic grouping of such constraints according to their likely methods of implementation:

Constraints mediated by structured efference: Among students of motor control, can there be any who have not yet heard of "muscle linkages" (7), "synergies" (21, 20), motor "oscillators" (76), "pattern" (65) and "function" (27) generators, and other members of the class of "coordinative structures" (34, 71)? All these constructs describe ways in which efferent signals correlated in space and/or time (i.e., as either an outflow vector or temporal sequence thereof) can be employed task-specifically to constrain the actions of groups of muscles. And to one extent or another, they all owe something to Bernstein's dictum that ongoing movement "responds as a whole to changes in each small part, such changes being particularly prominent in phases and details sometimes considerably distant both spatially and temporally from those initially encountered" (Chapter II, this volume). Each construct additionally effects that "chunking" of the control effort (74) ostensibly required for the hierarchical organization of task execution (described further below).

Constraints mediated by structured afference: Sherrington suspected (64) and Gellhorn proved (44, 45) that the musculature assigned to particular spinal reflexes often share in a common pool of afferent signals. In a trivial sense, therefore, the spatial structuring of this afference applies the constraint which creates reflexive muscular groupings. Recently, however, the infant "motor psychophysics" that has been applied to reveal the invariant properties of the efferent neural constraints described above has recreated interest in an older idea about afference. Following Gibson's postulates of "direct perception" (see (43) for details), one could propose that the physical structure of sensory inputs (as manifested, for example, in Euclidean visual or auditory space) can become mirrored in constraints placed on motor outputs. An operational example of this notion may be found in the (untenable!) mechanism proposed years ago by Pitts and McCulloch (58) for constraining the eyeball to aim at the physical centroid of a retinal image. We doubt that so immediate a correlation will often obtain between sensory and motor constraints. However, the translation process between the two (dealt with more fully later) may certainly rely upon an appreciation of sensory structure in the form, for example, of standard "reference orientations" of body parts (30, 67).

Constraints mediated by structured performance criteria: The idea that motor activity might be constrained by certain performance or "cost" guidelines has probably occurred to almost every scholar of the subject; and it was certainly the approach taken by Gelfand, Tsetlin, and colleagues in their initial attempts to formalize mathematically Bernstein's constrained dynamics (22). This work posited the existence not only of cost functionals of the skeletomotor state, but also of mechanisms for "searching" such functionals for their extrema. A functional could be associated with each motor task category; and searching (which required manipulation of the motor state) produce task execution almost as a byproduct, with dynamics constrained both by the search method (e.g., steepest descent) and the topology of the cost function. This seemingly roundabout formulation of dynamical constraints led to some rather strange experimental demonstrations of "searching" in both humans (1, 2, 38) and animals (77) that today look very curious indeed. However, if potential function theory and its associated gradient "searches" are used to describe neuro-biomechanical dynamics as we suggested earlier, then we may well see the resurrection of some of the earlier theoretical (and experimental?) approaches. Of course, performance criteria have also been applied in quite other ways to the understanding of motor programming, and we shall have occasion to consider certain of the results in the next section.

(3) The status of hierarchical control

Perhaps no element of Bernstein's thinking has had more impact than the hypothesis that predictable motor task dynamics result from control signals delivered to a pre-orchestrated interaction among hierarchically arranged centers of the CNS (see above). This proposition was initially applied in detail to the putative relationship between spinal and supraspinal circuitry (21, 60) and provided the rationale for numerous experimental investigations of spinal reflex "pretuning" in advance of movement (see (36) for review). It was only logical for attention then to be focussed upon the nature of the descending commands reaching these interacting spinal centers. The well-known sequence of studies on locomotion, scratching, and other spinal "automatisms" in animals resulted (54, 65). Recently, however, at least four aspects of the "traditional" Bernsteinian hierarchy have come under closer scrutiny:

Defining borders between hierarchical levels: In their "principle of least interaction", Gelfand and colleagues (21) argued that the cooperating processes of the motor task hierarchy ought to be arranged to promote maximum functional autonomy at each level, and a minimal necessity for communication among levels. This proposition accords with the release of complex behavior through a paucity of "command neurons" of the sort described for animal locomotion (29) and even complex human performances (61). Nevertheless, Bernstein himself also advanced a vague "principle of equal simplicity" (Chapter II, this volume) which appears to call for a distribution of hierarchical responsibilities based upon the partitioning of motor "task space" into regions that correspond to the (parameterized) actions of a single hierarchical level - see also the "equivalence classes" of motor function generators described by one of us (27). While such a partitioning rule also preserves the autonomy of hierarchical levels, it places no strictures on the complexity of communication between them. In that regard, then, it is interesting that recent attempts to compute optimal motor control strategies constrained either by cost functions (42) or by both cost functions and the assumption of "muscle linkages" (52) have discovered boundaries in the control space where saltatory changes in control strategy are required as a function of skeletomotor system state. Might the control regions so partitioned be construed as the domains of different hierarchical centers?

Communication among hierarchical levels: It is easy to conceive of what sorts of signals might percolate down a motor task hierarchy to the periphery; but until lately, there has been little consideration of what any returning reports might look like beyond the raw data of peripheral receptors. One worry that comes to mind is how to get different levels to understand each others' "language" while still preserving the autonomy of levels. This problem is currently being recast in terms of the "reconciliation of coordinate systems" (8). Thus, for example, the optimal spatial response axes of the semicircular canals and those of visually-driven climbing fibers do not quite coincide in the rabbit (66). But each provides information in its own "coordinates" about rotations the animal experiences (and this even ignores the differing temporal dynamics of the two systems!). How are these separate communications to be integrated into a single experience (or visuovestibular response)? One can raise similar questions about how one leg of a spinal frog is able to reach discrete areas on other legs (19), or how vestibular information is reinterpreted to account for voluntary inclinations of the head (53). Answers ranging from complex tensorial transformations (56) to the simple construction of a "transform table" in short-term memory have been directed at this problem; but we likely remain ignorant of many of the relevant phenomena. In any event, such considerations require a substantially broader view of the constraints on communication among cooperating motormanagement processes than heretofore has been the case.

The serial versus parallel activation of hierarchical levels: Hinton (this volume) proposes that we see the work of differing motor hierarchical levels primarily as the algebraic sum of their efforts. The levels work in parallel, and no level is truly out of communication with the periphery. Clearly there are many instances in which this view is quite accurate - as typified in vestibular influences on locomotion (46), optokinetic reactions (13), and eye-head coordination (6; but see 63). Still, a number of studies suggest that certain elements of human reaction-time movement are prescribed serially in time (e.g. limb selection and gross coordination versus the explicit timing of muscle activity; (48, 62, 75)), with each such stage depending upon the proper completion of its antecedent.

The validity of hierarchies: Despite the support given hierachies in the preceding discussion, it may well be possible that we sometimes invoke these artifices in lieu of more fastidious scrutiny of motor control processes (discarding, as it were, the attention to detail and experimental confirmation that Bernstein himself advocated). Thus, for example, Gurfinkel and his colleagues established experimentally that the threshold of short-latency ankle stretch reflexes was lowered during free standing relative to their excitability in seated subjects (16). The observation fits conveniently with a hierarchical view that allows such reflexes to "free higher centers" from the details of equilibrium maintenance. However, these investigators then found that not only did stretch responses not occur during the sway periods of free standing (i.e., when the hierarchy would have them act (31)), but that those reflexes became even more active when subjects stood with support (i.e., when the reflexes would apparently be "unnecessary" (15)). Gurfinkel et al. were finally drawn to the conclusion that perhaps free standing is a task that higher hierarchical centers would like to accomplish on their own, using the global information available to them, and without the meddling of low-level reflex systems operating with partial information (15). Similarly, we wonder if faculties such as spinal locomotor pattern generators exist primarily to support hierarchical control - or instead, to "bootstrap" basic movement patterns into a young, developing CNS that knows neither much about movement programming, nor about the meaning of the sensory data that results from program execution.

Conclusions

This brief survey of ours may only have compounded the history of Western neglect of Bernstein by overlooking in turn many facets of motor task control that his work has touched upon. We scarcely have dealt with the broad issue of performance feedback, where observations of the necessity for integrated trajectory measures (57) and of the correlation of efference with peripheral disturbances (59) are very much in the spirit of Bernstein's early observations. We have avoided completely any questions concerning the role of experience in, say, the acquisition of neural constraints - even though Bernstein's successors have also produced provocative results in this area (e.g., 55). We have not considered the ontogeny of posture or locomotion.

We have only hinted at how geometrical and physical considerations can minimize the task of intervention by higher levels of the nervous system. But we do hope that the positive outcome of this exercise has been to reinforce one rationale for the republication of the present reports: That Bernstein's ideas remain a renewable resource for all attempts to understand the physiology of human activity; and that, as was once said of the senior programming language ALGOL, Bernstein's contributions continue to be a significant advance over many of their successors!

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SECTION 1

Chapter I **THE TECHNIQUES OF THE STUDY OF MOVEMENT**
N. Bernstein

Chapter Ia THE UNIT OF ANALYSIS
R.B. Wilberg

Chapter Ib ON METHODOLOGY IN THE STUDY OF HUMAN MOVEMENT
H.J. Woltring

CHAPTER 1

THE TECHNIQUES OF THE STUDY OF MOVEMENTS

N. Bernstein

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1. THE CINEMATOGRAPHIC STUDY OF MOVEMENTS

Human movements have long been of great interest to investigators. Attempts have been made to observe and measure them for a considerable time. In the beginning of the 19th century the brothers Weber (76) carried out an extensive study of walking, employing the primitive observational techniques of their period - measuring lines and diopters. However, the rapid sequential patterns of human movements would have made precise observation unattainable if special opportunities had not arisen; these opportunities could be realized by the experimenter only with the development of instantaneous photography in the sixties of the 19th century. It was at just this time that energetic attempts to decipher the physiological mechanisms of human motor skills were begun; the end of the 19th century is marked in our field by such outstanding studies as those of Muybridge in the United States, Marey in France, and Braune and Fischer in Germany. The methodological innovations of the first two authors form the basis of later developments in cinematography; the last two authors laid the foundations of rigorously scientific quantitative investigation of movements with the help of photography.

It is not necessary in our own time to prove the value of cinematography as a technique for the study of movements; it provides an opportunity to record and to fix the rapidly interchanging phases of live movements in order to subsequently analyse them with any required degree of accuracy. Figure 1 presents a series of successive phases of fast running by a human being, taken by synchronizing two cameras from separate view points. These photographs were taken by Muybridge in 1887 and are evidence of the high technical level attained by this forefather of cinematography.

It can be seen in Fig. 1 that the right leg later assumes the position which was earlier occupied by the left leg in the sixth frame; there is a correspondence, for example, between frames 1 and 7, 2 and 8, and so on. In this way a single step is spread out over 6 frames. This corresponds, in the given series, to a frequency of 24 frames per sec; during this period of time the subject could accomplish 4 single steps. For comparison we may observe that in contemporary cinematography the standard frequencies are 16 frames per sec for silent films, and 24 frames per sec for films with a sound track. At the present time high-speed cameras have shutter speeds of 160, 400 and even 1500 frames per sec. A single step of Muybridge's run would be broken down by these later cameras (Zeiss system) into 375 separate frames.

It may be easily be imagined what occurs if motion-picture films exposed in high-speed processes are projected through conventional cinematographic equipment. Four hundred frames taken by a high-speed camera in 1 sec may be run through a projector which operates at 16 frames per sec over the period 400/16 = 25 sec; consequently, we may examine on the screen events photographed in the course of a single second. The time is increased, as it were, by a factor of 25; the term 'time magnification' which has been applied to high-speed cameras can be justified in this way. Events which take place quickly in nature can be made many times slower, permitting leisurely examination of all their temporal details. Bernstein and Dement'ev (17) have constructed a special motion-picture projector allowing smooth projection on a screen at a frequency of 4-5 images per sec, that is, one-third or one-fourth as fast as the normal projection rate. This technique allows an increase in the augmentational effect of time magnification and it is possible, for example, to examine the high-speed camera photographs referred to above with a time augmentation of 400/4 = 100 times.

2. CYCLOGRAPHY- THE PLANAR PROJECTION OF MOVEMENTS

Marey introduced another method which, after numerous improvements of his basic idea, has come to occupy an unconditionally predominant place in the study of movement. Separate, independent cinematographic exposures illustrate every phase of a movement excellently but they do not give a direct representation of the interrelations between successive phases. In the photographs in Fig. 1 there are positions but not movements. It cannot be seen from them in what direction or with what velocity any given joint of the body is moving at the moment when a single exposure is made. These elements of a movement, its direction and velocity, are clearly visible by projection of the film on a screen but they can still only be observed by the naked eye and not by means of measuring instruments. Cine-film reproduces the movement in its entirety, but fixes in a form convenient for scientific measurement merely positions and isolated phases. Marey's idea consisted in recording on the photograph all the dynamic changes in the phases of the movement.

In order to achieve this, Marey isolated from the rest of the subject's body narrow strips on the long axes of the limbs and made them luminous while keeping the rest of the subject's body black so as not to record on the photographic emulsion (Fig. 2). He then photographed the subject, in this clothing, many times on a single plate in the course of making a movement (Fig. 3). As a result multiple images were obtained - chronophotography - providing a perfect opportunity to measure the directions and velocities of movements of particular points of the body. Braune and Fischer improved this technique by replacing Marey's tapes with Geisler tubes which provided intermittent light at intervals of 26 per sec. A photograph of walking taken by Braune and Fischer is given in Fig. 4.

Marey's school (Bulle, Gastine) replaced the tapes by points; in order to do this they fixed miniature incandescent bulbs to the points of the body under investigation. This alteration in technique, as adopted by American (Gilbreth, Townsend) and German (Thun) investigators, was termed cyclography. The Moscow School of Biometrics developed the cyclographic technique to a high degree in comparison with the forms in which it was employed in the West; for this reason the most useful approach will be to describe the current status of cyclography in terms of the studies carried out by this school. A typical cyclogram of walking is given in Fig. 5.

Small electric bulbs (Fig. 6) are fixed at the points of the subject's body that are to be photographed. If necessary, flashlight bulbs may be used for the light (Fig. 6(c)), but tiny gas-filled bulbs (Fig. 6(b)) with a spiral filament, as prepared by F. Wolf in Berlin for Bernstein's project, are more suitable and provide ideal point representations because their filaments are only 1 mm in length. The bulbs are most frequently fixed over the centres of joints. The current (at a potential of 3-5 V) is led to the bulbs through thin flex from a belt worn by the subject; it is led to the latter through thin six-stand flex-a 'tail'-20-80m long, and thus not interfering with any movements, even running, which is connected to the experimenter's distribution board. It is also possible to supply the bulbs from dry batteries worn by the subject, but then they cannot be controlled from a distance, and it is often very important.

A cyclogram of uncomplicated movements (e.g. walking or running) is recorded on a fixed plate in a standard camera. If the subject carries out a movement in front of the camera with bulbs lit when the lens is uncovered, the displacement of each bulb will be represented on the photograph by a single continuous curve. In order to break down the image of the movement into distinct successive phases - to obtain a chronogram - the exposure is made through a rapidly rotating shutter (an interrupter, Fig. 7) which covers the lens for very brief periods at equal intervals some tens or hundred times a second. When the lens shutter is open the light from all the bulbs falls simultaneously on the plate and then is immediately blocked out when the lens is again covered. For this reason all the dots (Fig. 5) into which the rotating shutter breaks the different traces of the bulb trajectories correspond closely to each other in time. If points from adjecent trajectories which correspond to each other in time are joined by straight lines, schemata of successive positions of the movement are obtained which are exactly similar to the chronophotographs of Marey and Fischer (Fig. 8; see also Fig. 27(a)). With the help of a rotating shutter it is easy to obtain frequencies of up to 600 per sec, i.e. higher than are given by Debri's high-speed camera; it is difficult to operate faster than this frequency as the points of the trajectory then begin to merge into each other. We will not touch here on auxiliary techniques employed for more reliable identification of corresponding points of merging trajectories.

Exact measurement of the frequencies given by the rotating shutter is essential for the quantitative study of movements. A siren device employed by the Moscow school is very useful in this respect, the technique being based on determination of the frequency of a tone emitted by a siren located on the shutter and rotating with it. This device, with a series of auxiliary devices, permits accuracy within a fraction of one per cent in the determination of frequencies. Mukhin's technique, employing the characteristics of neon bulbs, is even more accurate. Bernstein's measuring device, called a strobometer, is constructed on this principle and consists of an accurately calibrated electromagnetic tuning-fork, a low frequency amplifier and a neon bulb. Oscillations of the tuning-fork are amplified and control the neon bulb which goes on and off in synchrony with the oscillations. The bulb illuminates a small circle of concentric asterisks fixed on the axis of the rotating shutter. If the rotation speed is regulated with a rheostat so that the asterisk intermittently illuminated by the neon bulb appears to be motionless, the speed wil be exactly syncronous with the oscillations of the tuning-fork. It is possible to calibrate it within some thousandths of one per cent with this apparatus. The advantage of Mukhin's method over the siren lies in the fact that several rotating shutters may be very accurately synchronized so as to work together, that is to say, the movement may be studied from different angles with several cameras. The action of the shutter may be rendered even more accurate if a frequency stabilizer with a Lacour wheel is coupled to the drive motor. The low frequency source for these stabilizers may be obtained from a sound-frequency oscillatory circuit with a subsequent stage of amplification.

Cyclography, in the form just described, is a poor device for the investigation of cyclical overlapping movements - to which category the majority of industrial processes belong: in movements of this type the images of the trajectories of the bulbs on a stationary plate appear as overlapping undifferentiable tangles. In order to overcome this inadequacy in cyclography Bernstein introduced kymocyclography (12), i.e. cyclographical exposures on slowly and evenly moving photographic film. The traces of rapid repetitive movements are resolved on the film by this technique into wave-like curves (Fig. 9) which are always easy to decipher. Taking the movement of the film into consideration (this can be done by means of very simple devices) and excluding it in calculations the observer can arrive at just as accurate and reliable data on the duration of movements from the curves obtained on the film as is available for the simplest movements from standard cyclograms.

3. STEROSCOPIC RECORDING OF MOVEMENTS

Both cyclography and kymocyclography give, in the applications described above, only the planar projection of the movement photographed. In order to examine movements in depth one must turn to complex observations. The displacement of any object in space, in other words changes in all three spatial coordinates of an object, must be recorded by observation from no fewer than two different points of view. Braune and Fischer photographed walking from 4 points simultaneously with separate independent cameras. Nowadays many investigators rely chiefly on stereoscopic cameras, i.e. cameras with two lenses with parallel optical axes. It is, however, easy to show that the accuracy of determination of the coordinate of depth is greater, the further apart are the two points of observation, in our case of both stereoscopic lenses. In standard stereoscopic cameras the lenses are 6.5 cm apart and the accuracy of measurement of the coordinate of depth is very low. It is necessary to separate the stereoscopic lenses a greater distance of the order of tens of centimetres. This separation results in gross differences in the fields of view of both cameras with parallel optical axes of the lenses. For this reason it is more convenient to photograph the movements with two cameras, and with the optical axes of the lenses convergent rather than parallel. This type of apparatus was employed by Drill (36). For synchrony this author set the shutters of two cameras placed at some distance from each other on the same long axis. The Moscow biomechanic school selected another technique which is incomparably more accurate and convenient. The mirror method developed by the author (16, 18) allows one to obtain two distinct points of view with a single camera and thus only a single rotating shutter.

For this purpose a large plane mirror is placed in the field of view of a camera at a given angle to its optical axis. The mirror is set so that the moving object to be studied is visible twice in the field of vision of the camera, (a) directly and (b) reflected in the mirror (Fig. 10). The mirror replaces the second, distant point of view. If, for example, it is placed at an angle of 45° to the main optical axis of the objective, then the accuracy obtained is equivalent to the accuracy for convergent photographs with two cameras separated by twice the distance of the camera from the mirror. Strictly speaking, the accuracy of the photograph with the mirror is still higher as here we need not worry about either the perfect matching of two lenses or the careful alignment of their two main axes which is of decisive importance for photography with two convergent cameras.

Material obtained with the help of mirror kymocyclography is in a class of its own in comparison with the accuracy which may be obtained by means of other existing techniques of recording movements. Kymocyclography allows one to obtain several hundred phases of a moment in a second, while measurement of the time intervals between successive phases may be carried out with the help of a strobometer with an accuracy of within one millionth of a second. Skilfully, taken mirror kymocyclograms also give excellent spatial accuracy, to within not less than 1 mm on any of three mirror coordinates. However, the most important advantage of the technique just described in the most general terms lies in the ease with which the material obtained in this way can be subjected to quantitative analysis and to mechanical interpretation. Photographic registration of movements is in no way the final aim of investigation but merely its raw data, and thus permits an approach to the real goal of investigation - physiological and biomechanical analysis of the processes of movement.

Methods of Analysis of the Cyclogram. To decipher a cyclogram it is necessary in the first place to measure the photograph of the movement which we have obtained. It is exceedingly difficult to measure the positions of the cyclographic points directly on the photographic plate or film; moreover, such a process would in this case be very inaccurate. Fischer measured his negatives under a special microscope which is also inconvenient and insufficiently accurate because of the small field of view of the lens. The Moscow biomechanicians employ for this purpose a process of photographic measurement suggested by Lavrentiev. Cyclographical or kymocyclographical negatives are greatly enlarged, and during this process a millimetre or even a half-millimetre grid is transferred to the paper by the same photographic process. Then it is easy to calculate the coordinates of all cyclographical points with a high degree of accuracy (Fig. 11). These coordinates are the basic raw material to obtain which all the techniques that have been described above are necessary, and from the analysis of which it is now possible to extract the maximum available amount of information about the process of the movement which has been photographed. The inadequacy of the cinematographic method lies in the facts that it is considerably more difficult to obtain these coordinates from a moving-picture film and that the degree of accuracy which can be attained is much lower.

All the cyclographical devices and procedures which have been described in this chapter have perforce been treated very briefly and superficially. The reader who wishes to acquaint himself more thoroughly with cyclogrammetrical processes may refer to Refs. 12, 16 and 18.

First of all it is necessary to establish from the coordinates obtained the successive positions in space occupied by the joints of the body from moment to moment during the time over which the cyclographical exposure was made (see Figs. 22 and 23(a)). Once a graph of these successive positions has been obtained, it is easy to measure the angles of articulation, repeating the process for all variations with their gradual changes. The coordinate data obtained from mirror kymocyclograms allow one to obtain all required projections of the successive stages of the movement under observation: to 'see' it from behind, from the side and from above. This type of observation gives information about the whole extent or range of movements, the amplitudes of movements of particular points of the body, the limits of the changes of the angles of articulation, the distribution of the trajectories of the movements in relation to surrounding objects and the forms of these trajectories. The value and the practical importance of information of this type does not require further emphasis.

Changes in one of the coordinates of movements in time may be represented in the form of a curve. Such a curve describes with particular clarity the characteristic peculiarities of movements, their differences from other similar movements, symptoms of fatigue which appear in them, and so forth. These features are expressed more clearly in curves of changes in velocity of the movements which may also be obtained by very simple methods from data on the coordinates of movements. Curves of the angular velocities of the movements of joints may also be obtained with the help of simple techniques.

Analysis of the forces which produce a given movement is of great and sometimes decisive importance. The first steps towards the description of these forces by means of chronophotograms were made by Braune and Fischer who indicated the way in which calculations of this type might be made. It is impossible to obtain direct data on these forces from chronophotographs or cyclograms. However, from these, and especially from the velocity curves which have just been mentioned, it is possible to obtain information on the acceleration of one or another point of the body. The dynamic forces are calculated by multiplication of the accelerations by the masses of the parts of the body undergoing acceleration. As will be apparent from the subsequent discussion, analysis of the forces producing movements and juxtaposition of these force data with data about the movements produced by operation of these forces gives a clear insight into the biomechanical and physiological characteristics of the processes of movement. The reader will see this for himself from the examples to be presented later.

4. MASSES AND CENTRES OF GRAVITY OF THE LIMBS OF THE HUMAN BODY

It is clear from the preceding discussion that it is possible to describe the work done by the skeletal musculature only if we have precise knowledge of the masses of the limbs of the human body and of the locations of their centres of gravity. Until recently this aspect of the problem was one of the most obscure in anatomy, and it is only now that an extensive investigation carried out by the author of this paper and his colleagues has in some degree begun to illuminate the problem of distribution of mass.

Two investigations of the problem indicated above are available in the literature; these are studies by Harless and by Braune and Fischer. These investigators employed the technique of dissection of frozen cadavers, following which the separate limbs were weighed and their centres of gravity determined by one of the methods of elementary mechanics. The number of cadavers used was extremely small; Braune and Fischer used 3 or 5 (the authors statements are contradictory), and the number used by Harless is unknown. The cadavers were those of adult males; there is no indication of their ages or physiques. The figures obtained from both studies are mean values without any indication of variation, and they differ significantly from each other (Tables 1 and 2).

It is already clear from the data presented here in brief that the material is inadequate. It is impossible to determine the most important facts of all - in what way these figures may be applied to a given individual and in what ways they may vary for persons of different sex, ag

TABLE 1. RELATIVE MASSES OF THE LIMBS (MASS OF THE ENTIRE BODY = 1)

	Fischer	Harless
Head	0.0706	0.0712
Upper arm	0.0336	0.0324
Forearm	0.0228	0.0181
Hand	0.0084	0.0084
Thigh	0.1156	0.1118
Lower leg	0.0527	0.0439
Foot	0.0179	0.0183
Trunk	0.4270	0.4630

TABLE 2. THE DISTANCES OF THE CENTRES OF GRAVITY OF THE LIMBS FROM THE PROXIMAL JOINT (LENGTH OF LIMB = 1)

	Fischer	Harless
Thigh	0.44	0.467
Lower leg	0.42	0.36
Upper arm	0.47	0.485
Forearm	0.42	0.44

and body structure. Finally, they beg the most important question of all - to what extent the relationships that hold true for cadavers are characteristic of live subjects.

The primary obstacle to experimental analysis of all these questions has until recently been the complete absence of methods which would permit the necessary measurements to be made on living subjects. It appeared to be an impossible business to weigh a living human being, as it were, piecemeal. It was only after the author of this paper together with O. Salzgeber and P. Pavlenko solved experimentally the auxiliary problems which were most important for this purpose that it was possible to proceed to the study of the weights and the centres of gravity of the limbs of living subjects by employing the ideas of Scheidt and Hebestreit. It is impossible to present in this chapter even a brief account of the complicated and delicate method employed by the author and his colleagues for measurements of his type. It can only be said that the problem is ultimately related to the planimetric measurements of the volumes and volume moments of the limbs of the body and to the weighing of the subject in numerous carefully determined controlled positions on special twin-support scales (see Fig. 12). From analyses of the figures obtained in this way and by comparison with data obtained from the most accurate microscopic examination of photographic plates of the positions assumed during the weighing, data on the locations of the centres of gravity of the limbs and on their masses could be obtained.

An analysis was undertaken of material obtained from 152 subjects of both sexes with an age range of 10-75 years. This study did not include investigation of the locations of the centres of gravity of head, hands or feet such as were determined by Braune and Fischer; rather, we investigated the locations of the centres of gravity of the upper arm, forearm, thigh and lower leg and the masses of all the major limbs of the body. The locations of the centres of gravity of the trunk and of the body as a whole were also included in the program of investigation.

I append below some of the data from the results we have obtained.

The mean values of the radii of the centres of gravity of the long limbs appeared to be much closer to those obtained by Fischer than to those by Harless (I term as the radius of the centre of gravity the distance from the centre of gravity to the centre of the proximal joint with the length of the limb taken as a unit). We may recall that Fischer's material was obtained on 3-5 subjects, while our material provides information on about 150 persons; because of this the reliability of the present data is many times greater than that of the old figures. I append a list of the means we obtained for comparison with those obtained by Fischer (Table 3).

TABLE 3. THE RADII OF THE CENTRE OF GRAVITY

	From our data		According to Fischer
	Mean value	Mean square deviation	
Thigh	0.3880	± 0.0332	0.44
Lower leg	0.4175	± 0.0224	0.42
Upper arm	0.4746	± 0.0338	0.47
Forearm	0.4145	± 0.0309	0.42

In the material as a whole, therefore, only in the case of the thigh does a significant difference from the position determined by Fischer occur, but the second column of figures in Table 3 is of much greater importance indicating that the spread of the data, in other words the variation, is considerable. If we take the mean square deviation as a measure of the variation, it appears that the overwhelming majority of cases fall between the following limits.

Thigh	0.3548 - 0.4212
Lower leg	0.3951 - 0.4399
Upper arm	0.4408 - 0.5084
Forearm	0.3836 - 0.4454

These variations are comparatively insignificant. The deviations found

TABLE 4. RADII OF THE CENTRES OF GRAVITY OF THE LIMBS (IN MEN AND WOMEN)

	Men		Women	
	Mean value	Variations due to the mean square deviation	Mean value	Variations due to the mean square deviation
Thigh	0.3857	0.3543-0.4171	0.3888	0.3534-0.4242
Lower leg	0.4140	0.3942-0.4318	0.4226	0.3983-0.4469
Upper arm	0.4657	0.4394-0.4920	0.4840	0.4484-0.5196
Forearm	0.4124	0.3850-0.4398	0.4174	0.3835-0.4513

with sex of subject, contrary to expectation, do not significantly affect the values of the radii obtained as Table 4 shows.

In the first place, it is apparent from this list that sex differences have very little effect on the radii of the centres of gravity. Generally speaking, the radii are slightly longer in women, that is, the centres of gravity lie closer to the middle of the limb and in the case of the upper arm they sometimes lie even lower which is almost never observed in men. In the second place, the indication in the first table of the great variation of the radii as encountered in practice is confirmed. Even if Fischer's figures, for example, those for the upper arm, closely coincide with our mean values (0.47 and 0.4746), it is possible to employ them in calculations, given the probability that for the overwhelming majority of subjects the values of the radius for the upper arm may vary in men between 0.44-0.49, and in women between 0.45-0.52. Figure 13 provides a picture of how the distribution of the values of radii of the forearm appear for men and women.

There are only two possible paths to choose in order to analyse this chaos of variations. Either we may resign ourselves to measuring with the complex techniques we have developed every new subject with whom we deal - or we may attempt to find such anthropometric and structural correspondencies (correlations) as will enable us to determine with sufficient accuracy the probable radii of our subjects on the basis of their general habitus and anthropometric data. It was this latter which we set as the objective of our investigations.

If we now turn to the masses of the limbs of the body, we may say that in this respect the data of Fischer and Harless were even more unreliable. The massive sample of material we examined gives an entirely different picture[*], even if we consider only the mean values obtained quite independently of any variation (Table 5).

The following interesting circumstances may be observed from Table 5. In the first place Fischer greatly overestimated figures for all the extremities of the limbs except the feet. In fact, the masses of all these extremities of the limbs are much smaller than is represented by these figures, which have been for 40 years the only data available on the question of the distribution of masses in the human body. In the second place, we here observe significant and characteristic differences between the sexes. The column giving the ratio of the mean masses illustrates these differences

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[*] The figures given here are only preliminary and after final revision may undergo small changes.

TABLE 5. RELATIVE MASSES OF THE LIMBS (MASS OF THE BODY AS A WHOLE = 1)

	Our Data				Mass according to Fischer
	Men	Women	General mean	Ratio M/W	
Thigh	0.12213	0.12815	0.12485	0.948	0.1158
Lower leg	0.04655	0.04845	0.04731	0.961	0.0527
Foot	0.01458	0.01295	0.01313	1.126	0.0179
Upper arm	0.02655	0.02600	0.02632	1.021	0.0336
Forearm	0.01818	0.01820	0.01819	1.000	0.0228
Hand	0.00703	0.00550	0.00642	1.279	0.0084

most clearly. Male thighs are significantly lighter than female thighs and lower legs and upper arms are almost the same for men and women (it should not be forgotten that in all these cases we are discussing relative masses, that is to say masses estimated with the total body weight taken as the unit); but distal portions of the limbs in men are significantly heavier than those of women. Both for the legs and for the arms the ratio M/W shows an increase from the proximal to the distal end of the limbs becoming particularly significant for the feet (13 per cent) and for the hands (28 per cent). It is also necessary to determine variation in the relative masses of human limbs. So as not to enter into complications we give only a general table which illustrates this considerable variation (Table 6).

TABLE 6. THE VARIANCE OF THE RELATIVE MASSES OF THE LIMBS (THE VALUES ARE GIVEN IN HUNDRED-THOUSANDTHS OF THE WEIGHT OF THE WHOLE BODY AND IN PERCENTAGES OF THE MEAN MASS OF THE LIMB)

	Men		Women		General figure	
Foot	± 1620	± 13.3%	± 1190	± 9.2%	± 1480	± 11.8%
Lower leg	507	10.9%	389	8.0%	469	9.9%
Thigh	126	8.6%	105	8.1%	142	10.2%
Upper arm	312	11.8%	344	13.2%	322	12.2%
Forearm	184	10.1%	169	9.3%	177	9.7%
Hand	84	11.9%	98	17.8%	117	18.2%

The variation both in the radii and in the relative masses is least for the lower legs, feet and forearms; the masses of the thighs, upper arms and in particular of the hands display greatest variance.

The Centre of Gravity of the Entire Body and of the System as a Whole. If the masses and the positions of the centres of gravity of all the separate limbs of the body are known, the problem of discovering the centre of gravity of the whole body or of any particular system (for example, that of the whole arm or of the whole leg) presents no difficulty whatever. This possibility is of inestimable importance for the physiology of movements because it opens the way to the dynamic analysis of the movements of the whole body and of its sub-systems and also allows us to study the statics of the body and, as has been explained above, the loads on any given group of muscles.

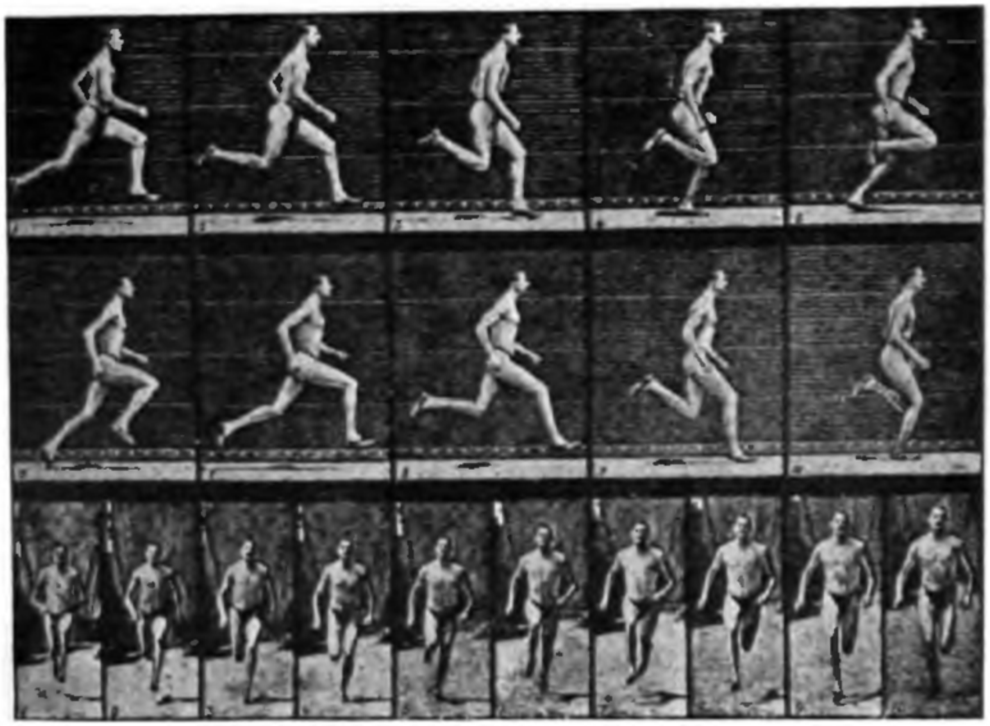

Fig. 1. Instantaneous photographs of running taken by Muybridge in the 1880's at the dawn of instantaneous photography. The time interval between successive frames is $^1/_{24}$ sec.

Fig. 2. One of Marey's subjects in a black costume with white tape.

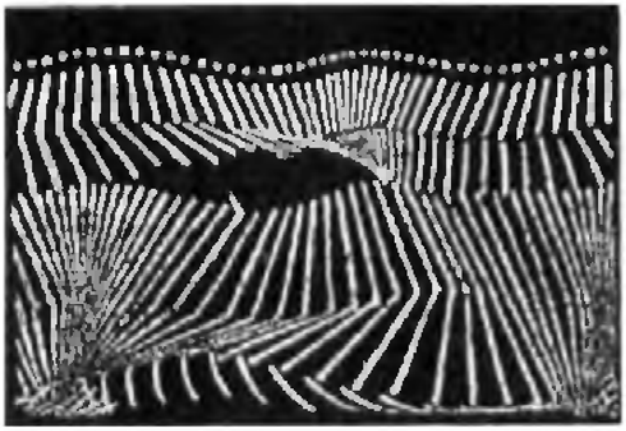

Fig. 3. Chronophotograph of walking taken by Marey. Movement is from left to right. The frequency is about 20 exposures per sec.

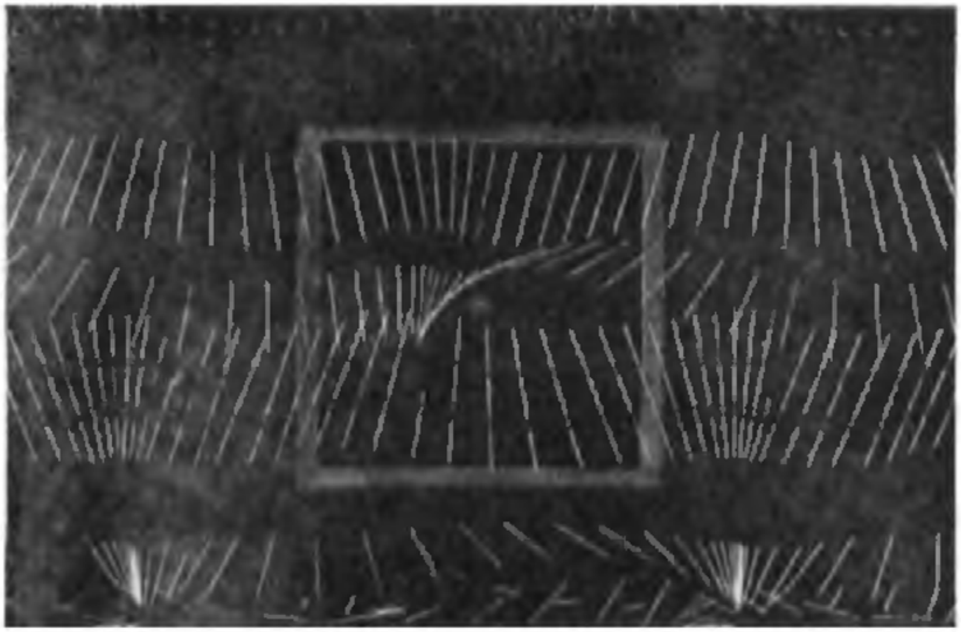

Fig. 4. Chronophotograph of walking taken by Braune and Fischer. Right side of the body; movement is from left to right. The square in the centre of the picture is a superimposed scale. Frequency - 26 exposures per sec.

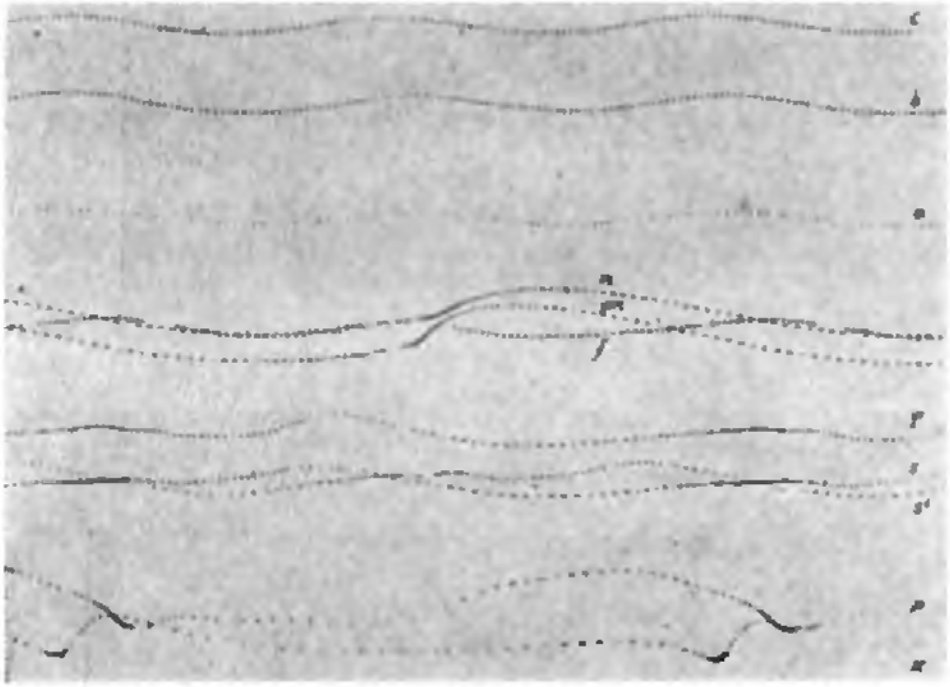

Fig. 5. Cyclogram of walking (Bernstein and Popova). Left side of the body; movement is from right to left. Trajectories from top to bottom: c, centre of gravity of the head; b, shoulder joint of the left arm; a, elbow joint of the left arm; m, radial side of the wrist joint of the left hand; gm, centre of gravity of the wrist; f, hip joint of the left leg, ϕ, a point on the longitudinal axis of the left thigh; s, knee joint of the left leg; s', knee joint of the right leg, p, ankle joint of the left leg; π, a point near the end of the foot. Frequency - 90 exposures per sec.

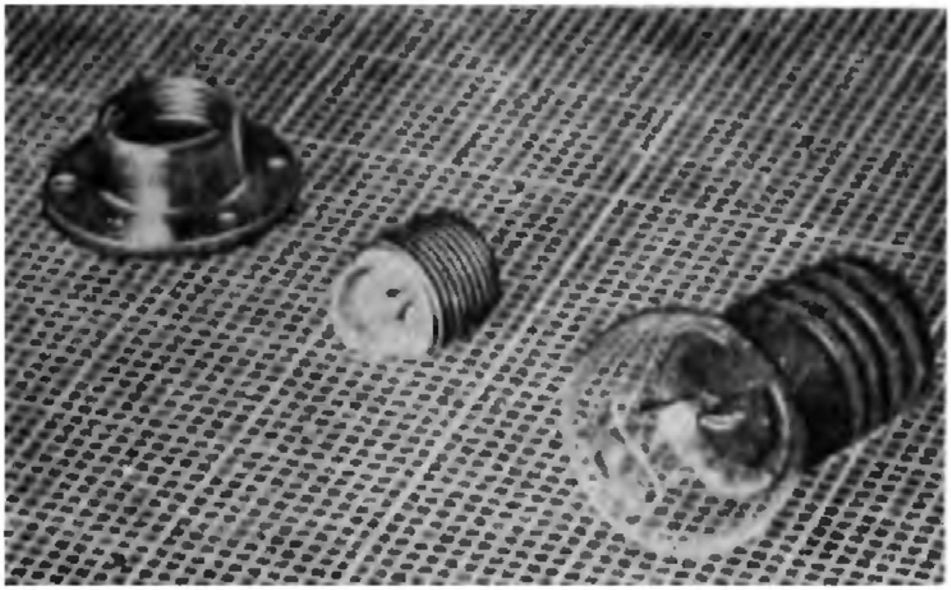

Fig. 6. Bulbs used for cyclography, placed on a millimetre grid so that their dimensions may be gauged. (Left) a Wolf socket; (centre) a Wolf-Bernstein bulb; (right) for comparison, the type of bulb commonly used in pocket flashlights.

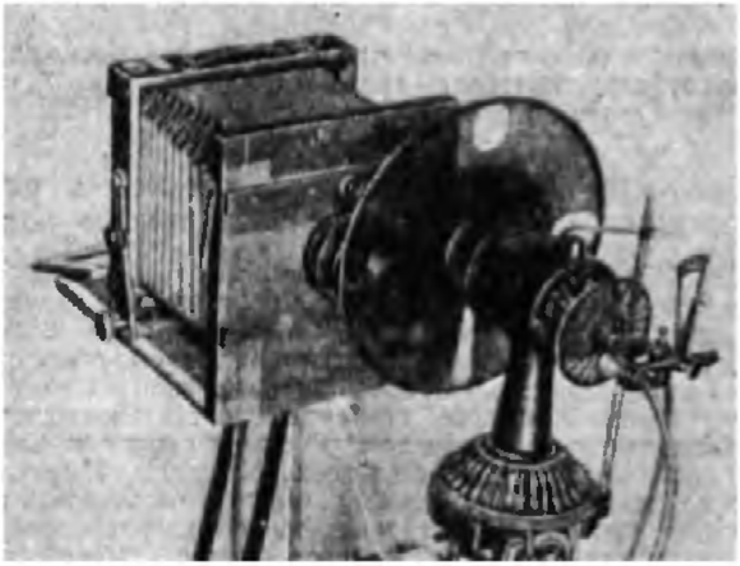

Fig. 7. Camera with rotating shutter, equipped with a siren so that its velocity of rotation may be estimated. In a later system used by the author the rotating shutter is semi-transparent; this provides faint lines on the cyclogram, uniting successive points on the same trajectory.

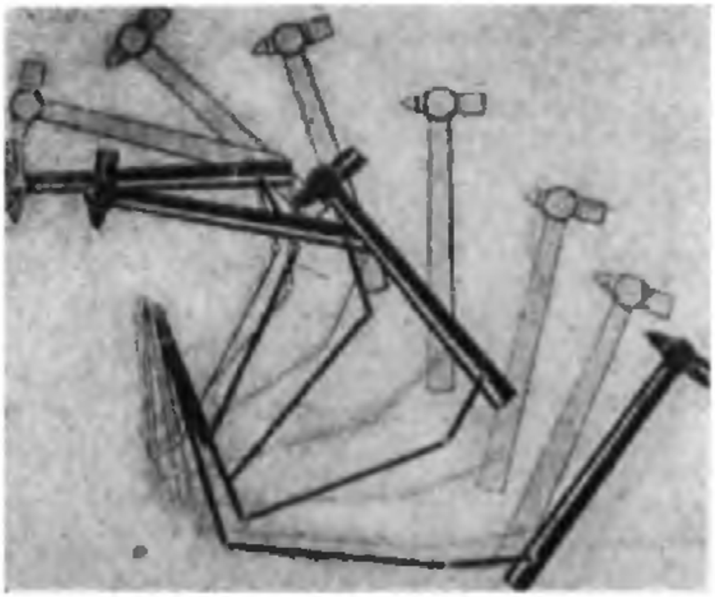

Fig. 8. Successive positions of the right hand and a hammer during correct striking with a chisel. The time interval between each phase shown is $^1/_{15}$ sec. The sketch was made from a cyclogram.

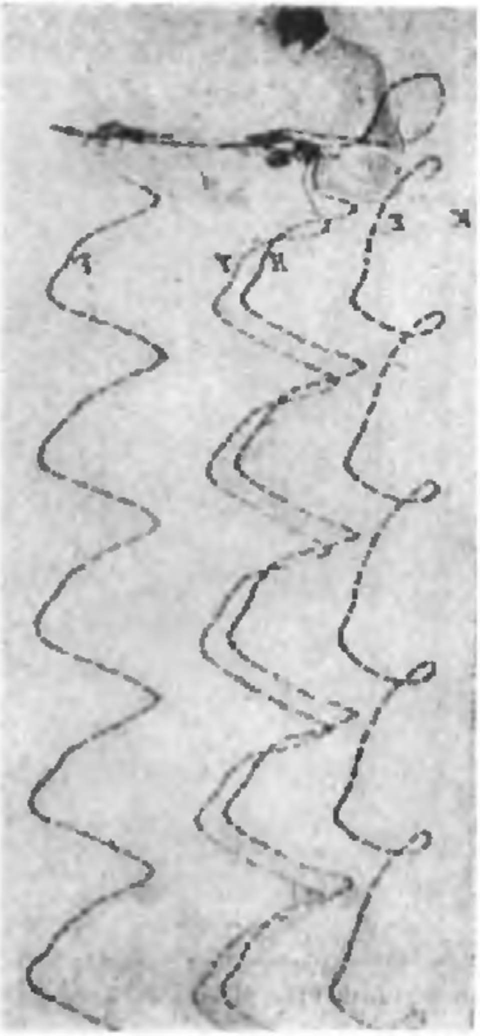

Fig. 9. A kymocyclogram of filing. The figure of the subject is visible at the top of the illustration, with a standard cyclogram of a single cycle of the movement of filing. This can be seen to be quite unanalysable. Below is a series of curves of the same movement, separated by being photographed on a moving film. *K*, a control bulb; *E*, the elbow joint; *H*, the radial side of the wrist joint; *F*, the fingers of the right hand; *F'*, the fingers of the left hand. Frequency - 73 frames per sec (1923).

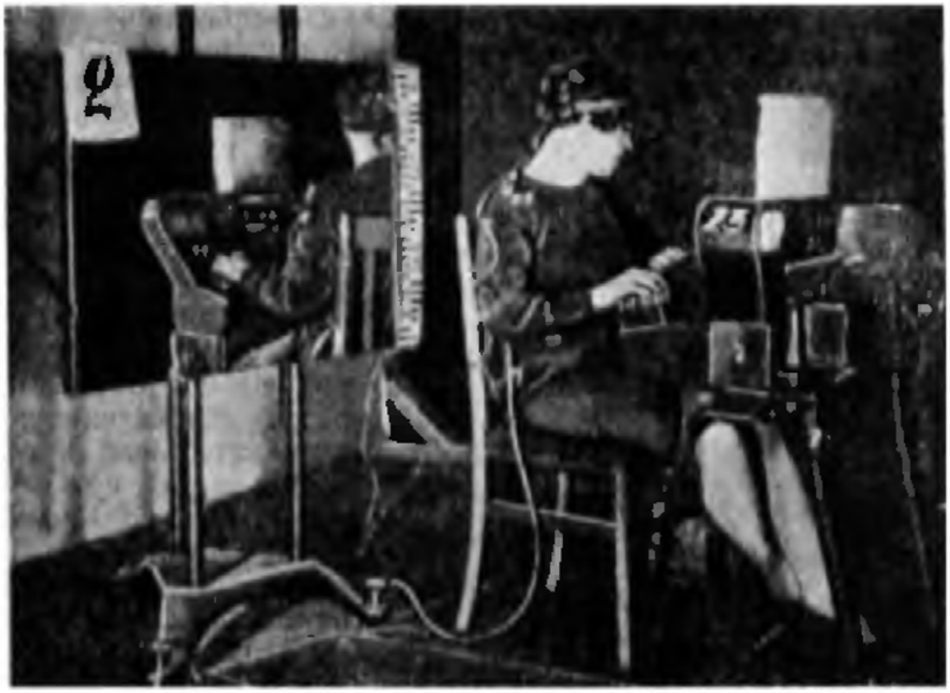

Fig. 10. Apparatus for mirror kymocyclography. The subject is operating a Powers perforator. On the left we have a mirror with a scale and the serial number (1929).

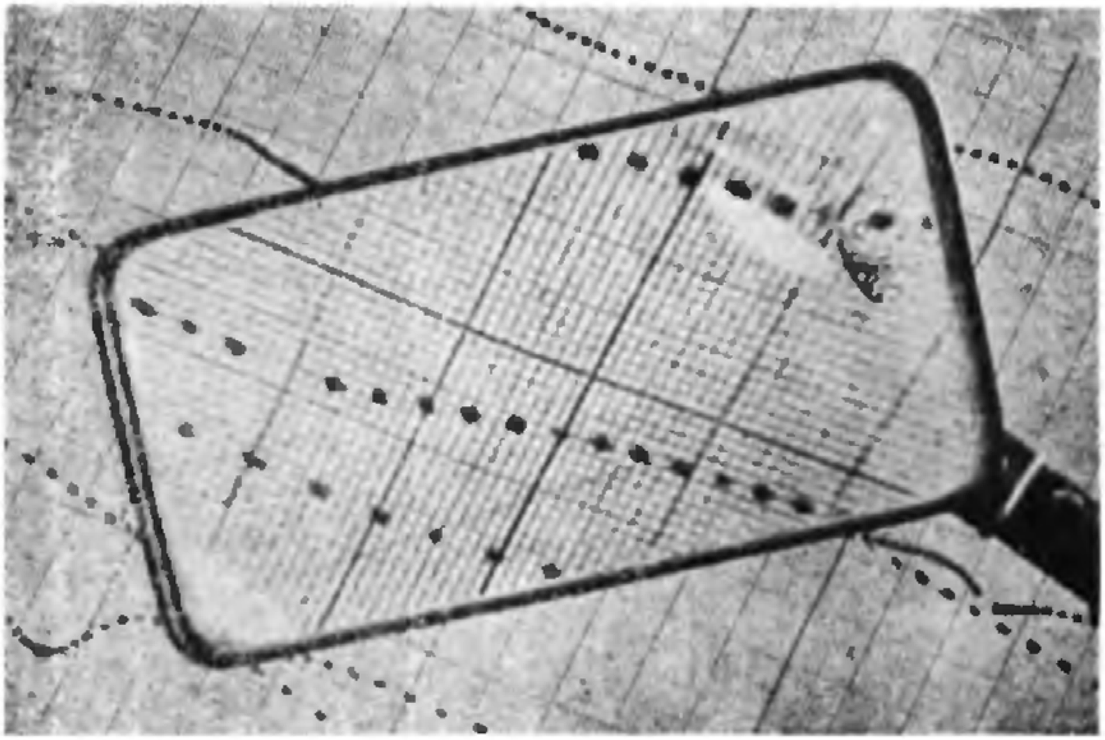

Fig. 11. A section of a photograph on a measuring grid and the means by which it is studied through a lens.

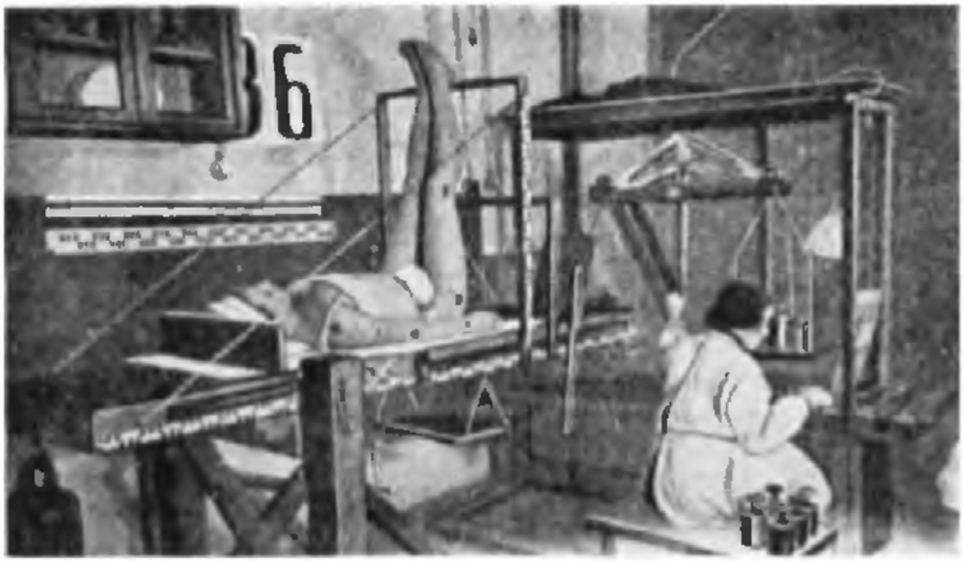

Fig. 12. An experiment on the determination of masses and the centre of gravity of the limbs by Bernstein's method. The subject lies in a predetermined position on a platform supported at two points, the placement of the head and the lower extremities being determined by upright boards. At the end the platform is fixed upon a fulcrum, at the lower extremity it is supported by one of the pans of accurate scales. The assistant balances the static moment of a given position of the scales, the position being photographed at the same instant on a predetermined scale.

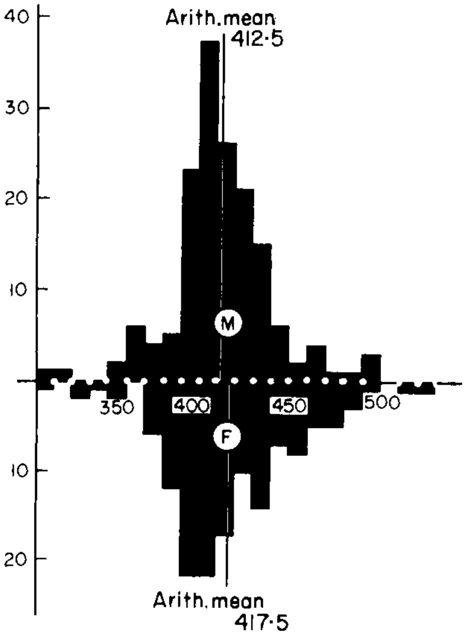

Fig. 13. Distribution of the values for the radii of the centres of gravity of the forearm from data obtained by the author and his colleagues. Above: the limits for men; below: those for women. The values of relative radii are plotted along the abscissa. The number of cases observed is plotted along the ordinate.

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H.T.A. Whiting (editor)

CHAPTER Ia

THE UNIT OF ANALYSIS

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The technique of recording movement in 'real-time' as a series of linked events were initiated by several investigators, but was most completely developed by Bernstein over a period of approximately 30 years. Some of the recording techniques are equipment specific, but on the whole tend to measure the time-course of a particular movement as opposed to its simple result. Depending upon the particular effect desired, the results can be: the temporal record of the movement of a specific joint or angle; a composite of many such chronologically and sequentially related points; or a derivative as in the case of certain accelerative functions taken through time. These direct measures enabled Bernstein and others to view the time-course of a movement in its entirety and to examine its production from both an absolute and a relative basis. From these initial observations Bernstein developed a view of movement as important as it was unique in the study of voluntary, overt, guided behavior. The importance of his works to our present knowledge will be examined in later chapters, but suffice to say that his observations are still relevant today.

Since Bernstein's demise, several advances in both measurement and experimental procedure have taken place. The development of commercially available computer software packages of mathematical and statistical programs, has meant that virtually every investigator has access to very powerful analytic tools. As a consequence, much of Bernstein's original work has been surpassed, particularly by the biomechanists. However, a much more fundamental change has taken place in the study of movement. The manipulating of experimental factors has extended our knowledge far beyond the horizon that Bernstein sought. The bases of this expansion are many, but they are tightly bound to the rise of cognitive psychology. Of particular interest is the emphasis that is placed upon the theories of memory and decision-making that underlie our present view of performance. Movement cannot be meaningfully studied without taking cognizance of the task the subject is attempting to to perform. As a result, a substantial amount of information regarding movement has arisen from the comparison of movement behavior under varying experimental conditions. Often the crux of the experiment involves making inference from comparative results - thereby turning the dependent variable into a 'secondary' indicator of an unobservable underlying phenomenon. Both the biomechanical and the experimental developments are examined more fully in the remaining paragraphs.

BIOMECHANICAL DEVELOPMENTS

The natural evolution of Bernstein's approach to direct measurement has resulted in a sophistication of measurement technique that was neither technically feasible nor economically possible at an earlier time. The most recent advances in the measurement of skilled performance have arisen from the study of the biomechanical aspects of movements. Those interested in biomechanics for example have been responsible for several cinematographical, mathematical, and mechanical engineering applications to the study of movement. These advances have had their basis in Bernstein's original work and should be considered as extensions rather than original contributions.

Plagenhoef (1968) and McLaughlin, Lardner and Dillman (1977) assessed the minematic and kinetic parameters of human movement from a two dimensional or planar viewpoint. Further investigations by Miller and Petak (1973) and Shapiro (1976) have assisted in the development of cinematographic techniques to calculate three dimensional coordinate data relative to such kinematic features as the centre of mass displacement and the absolute angular displacement-time history of a given movement. This new method is known as 'the direct linear transformation' (Walton, 1979; Miller, Shapiro & McLaughlin, 1980). Variations of Newtonian and Lagrangian equations of motion have also been advanced to describe movement paths through three dimensions, following the establishment of angular displacement-time data relative to their specified axes when defined in space.

A recent parallel development has been the adaptation of spline functions to describe the path of movements arising from experimentally-produced data. The functions result in fitted curves that can more accurately follow a movement's path than do those arising from the more commonly used interpolated polynomials (Gerald, 1973). Although the spline functions tend to smooth the data during the curve-fitting process, a more versatile smoothing procedure has arisen from the development of digital filters. Such filters can be set to 'pass' frequencies of a certain range while rejecting and/or attenuating others. This selectivity allows the researcher an opportunity to remove unwanted noise from the experimental data, so that the desired signal can be seen more clearly (Winter, 1979; Zernicke, Caldwell & Roberts, 1966).

These most recent developments indicate a trend away from the simple recording and measurement of the time course of a movement to a position where the movements can be described in terms of mechanical and/or mathematical statements. Although these attempts to model the movements of limbs and bodies are still in their infancy, they do mark a change in emphasis from one of mere analysis to one of projection and simulation. For example, a number of procedural developments have occurred which allows the experimenter to define the total position of a body and its posture within an inertial frame of reference for a specific environment. Predicted movement paths then become probability statements with the attendant confidence limits providing a basis for prediction success (Reynolds & Hubbard, 1980). Extensions of this approach to computer modelling of movement is not only likely, but obvious. Some initial work by Hinton (1981) elucidating movement behavior based upon an alternate solution to Bernstein's 'degrees of freedom' concept, shows how powerful that tool can be to the understanding of movement.

PSYCHOLOGICAL DEVELOPMENTS

The significance of the change from simply recording and measuring movements to the act of modelling and simulating those same movements should not go unnoticed. These differences in approach, parallel to a large extent, the two different traditional approaches to experimental psychology, namely functionalism (associationists) and structuralism (cognitivists). The differences between the two approaches are most apparent in the unit of analysis which the experimenter adopts. Studies which attempt to measure and define simple movements in isolation, e.g. Bernstein (1967), Bizzi, Polit and Morasso (1976), Houk (1979) and Hoffer and Andreassen (1981) focus upon the control factors which come into play when the movement is initiated. Unravelling the integrative processes that occur between the conscious initiation of a movement and the actual occurrence of that movement is of prime interest. The problem with such an approach is that it is difficult if not impossible to obtain a functional equivalent between the phenomena noted when such movements are made in isolation, than when they are made as part of a larger integrated movement pattern. Clearly the behavior of emitting a simple response is not always functionally the same, and it suggests that the functional significance of a simple response is dependent upon the way in which the behavioral output is patterned or organized. For example, in certain instances the simple movement of the head to left or right may constitute an independent functional unit, but when combined with eye movements and postural adaptations it becomes integrated into the well known 'observing response' - an entirely different functional unit. The neck movements in both instances may arise from the same anatomical and neurological bases but they could only be considered equal in the fact that each one is a conscious act. This view suggests that a given movement can be considered as a functional unit in one context and not in another, and secondly that a functional unit may not be just a simple response but may indeed involve a very complex and structured behavior.

The functionalist and structuralist approaches may address issues and problems specific to their viewpoints as well as problems in common. The measurement of observable and unobservable phenomena is an issue of concern to both of these traditional approaches. Nowhere is this issue more clearly defined than in the recent work on the behavioral patterning, segmentation and organizational sequencing of movements in a list. It may be helpful at this point to examine some studies in which the unit of analysis was of paramount importance in the complete examination of a subject's performance.

In this first experiment by Restle and Burnside (1972) the effects of list repetition are inter-related to the organization apparent in the behavioral output. Previous work by Restle (1970, 1972) and Restle and Brown (1970) had shown that subjects tended to group serial movement list items into sub-units. Although Restle did not develop a measure of behavioral patterning such as did Tulving (1962, 1964), he was able to demonstrate that more errors were made by subjects when transiting between sub-groups than were made within such units. Restle and Burnside (1972) required subjects to track the stimulus as it moved from one location to another in a continuous sequence. A serial pattern learning paradigm was used and Restle and Burnside (1972) found that subjects tended to break the total list into sub-groups that had the characteristic of a 'run' (abc) or (cba), 'trill' (aba) or (bab), or 'repetition' (aaa). This tendency to form sub-units increased as the number of repetitions increased and led Restle and Burnside to the conclusion that the new functioning unit of analysis was no longer a single movement, but one composed of a number of such movements. These results are certainly in agreement with the Handel and Todd (1981) finding that, when left to their own devices, subjects will segment undifferentiated lists in a variety of ways.

In general, those who support a functionalist point of view would not have interpreted the data this way. The functionalists (associationism) would have focussed on the increase in the number of remembered items as a result of trials with each correct item being considered as the functional unit. Any patterning that may have resulted would be viewed as a by-product of an increase in the strength of the invariant units, i.e., the individual movements. The structuralists' viewpoint, on the other hand, would focus on the degree of clustering (sub-group consistency) that took place as a function of trials. That is, it would focus on the emergence of new behavioral patterns. In this sense the increase in the number of movements remembered would be viewed as a by-product of the emergence of new patterns, the true functional unit.

Bernstein's approach to both the measurement of movement and its experimental methodology, follows almost completely the functionalist viewpoint. Although Bernstein eventually came to the conclusion that very little of movement behavior could be described by the measurement and observation of single movements, his research methodology and measurements did not by itself provide such evidence.

The unit of analysis in the Restle and Burnside study was aimed at the relationship of movements within and between sub-groups, and could be derived from experimentally-produced data. In the following class of experiments the unit of analysis is manipulated by the experimenter; usually by provisionally varying the level of the independent variable. As Pachalla (1974) observed, much of the structuralist (cognitive) research is carried out by making inferences about unobservable mental events, and by studying the subsequent behavioral results that follow as a consequence. The reaction time score is perhaps the most widely used measure of such unobservable activity, with variations in latency being primarily due to changes in the amount, difficulty, or complexity of the information processing task demands.

Cognitivists such as Sternberg (1969) have used reaction time data to support their hypothesis that the search through memory for an encoded probe character is a sequential process with each comparison between the probe item and the to-be-remembered list items being equated to a single mental operation.

The reaction time latency should then reflect the number of mental operations (list length) being required of a subject. The reaction time score is then a function of a sequential pair-wise comparison process and as such can be set by the experimenter in different experimental conditions. From a structuralist's (cognitivist's) viewpoint the unit of analysis is not the simple latency that accrues from different experimental conditions, but is rather the item-to-item sequential pair-wise comparison.

Because the associationist viewpoint would not recognize memory in the sense that the cognitivists view it, the increase in latency as a function of list length would not be interpreted in the same way. Variations in reaction time latencies would be construed to arise from differences in associative strength or perhaps stimulus-response compatibility. While both the structuralists and functionalists would view the mechanisms underlying the reaction time latencies as being unobservable, the associationist's view would be essentially atheoretical.

The measurement techniques used by Bernstein were quite capable of producing reaction time data in terms of 'lag' time and that such system response times could monotonically vary as a function of practice, task difficulty and/or task complexity. Bernstein however, does not appear to have varied his subjects' behavior in order to determine the part played by decision making and memory in the production and control of movement. While it is quite clear that Bernstein did not study memory per se, it is nonetheless true that a contemporary view of movement demands an understanding of the control feature that guide the intended movement, and the cognitive aspects that first generated, then initiated it.

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Human Motor Actions – Bernstein Reassessed
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CHAPTER Ib

ON METHODOLOGY IN THE STUDY OF HUMAN MOVEMENT

H.J. Woltring

1. Introduction

The dichotomy between material content and methodology in any field of scientific endeavour is artificial in the sense that neither can justify itself in isolation: without an external purpose, methodology is sterile and a potential, 'academic' waste of effort, while facts worthy of the qualification 'scientific' can only be obtained through a proper, generally acknowledged methodology. This does not imply that speculation and intuition do not deserve a place in science, as is borne out by the existence of academic journals such as *Speculation in Science and Technology**.

Yet, there is a tendency that methodology is relegated to a secondary position, with the risk that incorrect procedures remain unrecognized. In other instances, methodology and material results are reported separately, in that order, and here the risk exists that the earlier publication is taken for granted. It is, therefore, appropriate, that the editors chose Bernstein's *Techniques for the Study of Movement*, originally published in 1934, as the first chapter of the 1967 book, and it is intriguing that the editors of a German translation of a selection from his work adopted a somewhat different view (Bernstein, 1975, p. 15),

> The works of Bernstein and his collaborators which appeared in the years 1927 through 1936 in German periodicals and in Abderhalden's *Handbuch der biologischen Arbeitsmethoden* could only give a very incomplete and therefore single-minded impression of the creations and ideas of this important Soviet scientist. Through these, the German reader was given the impression that this work was merely concerned with a continuation of the old movement research line which is more or less characterized by the names of B. and E. Weber, Muybridge, Marey, Braune and Fischer. It

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* *Speculation in Science and Technology*. An International Journal Devoted to Speculative Papers in the Physical, Mathematical, Biological, Medical, and Engineering Sciences. Elsevier Sequoia, ISSN 0155-7785.

> was merely the methodology which appeared more perfected through kymocyclography, cyclogrammetry, and the spatial data acquisition through mirror registration, while biomechanical data processing was rendered more accurate and simple.

They went on to state that "the important and general results of investigations during many years on biomechanical fundamentals (cf. the chapter on Biodynamics of Locomotion)" did not become known in Germany. On the other hand, Gurfinkel (Moscow) wrote in the introduction to the same book (Bernstein, 1975, p. 10),

> Important steps were undertaken in order to improve the technique of movement registration and the methods for the evaluation of experimental results. Spurred by Nikolai Alexandrovitch, the cyclographic technique was rendered highly perfect and a cyclographic mirror method was developed allowing spatial movement registration. For cyclically repetitive movements whose trajectories would superimpose themselves upon each other, also the methods for cyclogramme evaluation were further developed, and nomogrammes were constructed rendering the calculations more simple. This total complex of methodological and technical procedures enabling a 'microscopy of movement' was given the designation 'cyclogrammetry'. A detailed presentation of this material is contained in the monograph 'Techniques for the Investigation of Movement' which appeared in 1934 under the redaction of N.A. Bernstein.

In his chapter on Biodynamics of Locomotion (p.178), Bernstein himself states

> All these circumstances determined the third and, in principle, the most important aspect of our approach to the study of locomotion. We refused to theorize about our object or investigation in advance by forcing it to fit one or other analog in the field of general mechanics. We regarded the locomotion process as a living *morphological object* of inexhaustible complexity and set as our primary task the necessity of observing and describing it as closely as possible.

It is from this point of view that Bernstein's work is subject to both praise and criticism. His achievement in measuring a considerably larger amount of movement data than Braune & Fischer*, with a temporal resolution ranging from 60 to 190 frames per second constitutes a significant improvement with respect to his predecessors, and he was right to criticise

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* Condensed translations of two key publications Braune & Fischer (1889) and Fischer (1906) are available as Braune et al. (1963).

them for their *a priori*, subjective smoothing of displacement data, sampled at only 26 Hz. Moreover, he was far ahead of his time in conducting sensitivity studies on the stochastic measurement errors affecting his estimates for velocities and accelerations (Bernstein, 1936). On the other hand, he failed to relate these sensitivity studies to his data on the Biodynamics of Locomotion, and he did not realise - like many investigators who followed him, even up to the present day - that the finite difference algorithm adopted for estimating velocities and accelerations was quite unsuitable in view of its deviation from a true low-pass differentiating filter in which signal and noise properties of the observed data are *both* taken into account. This point will be discussed more fully in a later part of this commentary.

Furthermore, in rejecting the earlier investigators' approaches to model the leg as a pendulum, or the foot as a class I or II lever, he did not acknowledge that his own approach to the estimation of centre of gravity forces, joint forces, and joint moments was based on another, more general mechanical model, namely the rigid body for which a theoretical structure was readily available even in his day. As far as this author is aware, the non-rigid character of the soft tissues in the body segments was hardly mentioned by Bernstein, and the occurrence of local landmark movements caused by skin shifts with respect to the underlying skeletal structure not at all.

Considering Bernstein's views, it seems reasonable to conclude that he was influenced by the traditional empiricist interpretation of the ethology of the research process. Medawar (1963, 1967, 1969) gives a lucid account of the empiricists' view that scientific research is an *inductive* process starting with "simple, unbiased, unprejudiced, naïve, or innocent observation", out of which an orderly theory will somehow emerge. In his opinion, scientific discovery or the formulation of a scientific idea on the one hand, and demonstration or proof on the other, are two essentially different notions. In this positivist, *hypothetico-deductive* view of the research process, hypotheses may be obtained in any fashion available to their proponent, for subsequent, rigorous testing in the deductive phase. It is in this latter phase, that a well-founded, generally acknowledged methodology is essential, particularly in applied fields drawing on a number of parent disciplines.

The iterative character of the research process is a natural consequence of this *Popper*ian view of science: each finding evokes new hypotheses to be scrutinized in turn by appropriate methodology, and any theory or interpretation remains on probation until rejected in later research. In this sense, Bernstein's work may be characterized as an inductive attempt to collect human movement data in as unbiased a fashion as possible, using the most general biomechanical model currently available, in order to allow the formulation of hypotheses with minimal prejudice.

It might be argued that methodology is the most 'dated' aspect of Bernstein's work in view of current availability of automatic data acquisition and processing technology; however, few investigators have taken the trouble to attempt to measure human kinematics at the same degree of accuracy. It is, therefore, in honour of Bernstein, that the following sections of this commentary present a synopsis of developments in kinematic data acquisition and processing, mass distribution parameter estimation, and kinetic modelling. Suitable introductory texts in this field are those by Miller & Nelson (1973), Grieve et al. (1975), and Winter (1979).

In this commentary, the data *acquisition* orientation of chapter I is transcended. However, biomechanics suffers, as so many experimental sciences do, from a lack of sufficiently direct measurement facilities. The investigator measures what he can, and bridges the gap to what he desires to know by *modelling*. Data acquisition and modelling thus become complementary, and should be treated as such. By consequence, measurement methodology and 'biomechanical fundamentals' should be viewed in a wholistic manner, when assessing the reliability of the data acquisition and processing chain: estimated forces, moments, and energies are affected by both aspects.

2. Techniques for biokinematic data acquisition

Cinematography and Stroboscopic Photography. It is perhaps appropriate that Bernstein's photographic and cinematographic methodology is commented on by someone who was originally concerned with *avoiding* these as a data acquisition tool! Although cinematography (henceforth to include stroboscopic, multiple-exposure photography) is the perfect means for recording time-fleeting movements, conversion of these records into a quantitative, processable form is notoriously laborious and error-prone. Therefore, the perseverance of Bernstein's group and of his predecessors in digitizing large amounts of movement data is to be commended. Besides, even today, the large variety of electrogoniometric, ultrasonic, and optoelectronic methods for automatic acquisition, identification, and digitization of kinematic data have not obviated the need for cinematography in certain movement studies. Particularly in sports (Marhold, 1979) and in ethology (Lehner, 1979), where affixing measurement devices or artificial landmarks for increased observability may be tantamount to destroying essential parts of the movement process, high-speed cinematography remains a unique approach, recently complemented by high-speed video recording technology (e.g., Terauds, 1981). In biomechanical impact research, requirements concerning spatio-temporal resolution have caused that cinematography has been retained as the major technique for kinematic registration.

In sports biomechanics, digitization of kinematic data is still largely done by hand. On the other hand, in the case of impact studies in car crash research it is usually possible to affix special landmarks to dummies, cadavers, animals, or volunteers, and this facilitates automatic identification and digitization of the points of interest in individual film frames, when used in conjunction with current pattern recognition techniques (e.g., Aten; 1975; Niederer et al., 1981; MacKay et al., 1982).

In his 3-D studies, Bernstein used mirrors to obtain multiple views from different vantage points on a single film, thus avoiding the problem of synchronizing multiple cameras which today still arises in high-speed cinematography. However, the disadvantages of this method are that mirror planarity errors and, in the case of back-coating, refractive errors may assume significance.

As an alternative to mirror approaches, the work of Baum (1980, 1983) on anaglyph *Motography* qualifies him as a worthy successor to Bernstein. Here, the use of carefully chosen optical filters and light sources in the visible, infrared, and ultraviolet ranges allows simultaneous registration of cyclographic trajectories and instantaneous movement snapshots on a single photograph. When flashing light bulbs are used as landmarks, approximate velocity information is made available, and 3-D perception is possible by means of anaglyph spectacles (red/green glasses). Quantification of these data remains a problem, and the major utility of this approach is in making direct, qualitative judgments.

Electrogoniometry. Most contemporary methods for non-cinematographic/photographic acquisition of biokinematic data rely on electrogoniometry, ultrasound, or various optoelectronic methods, which were not used by Bernstein, or - it would seem - available to him. More recently, electrogoniometry has been used extensively for joint or body segment kinematics monitoring (Lamoreux, 1971, 1978; Chao, 1980; Zarrugh & Radcliffe, 1979).

Typically, potentiometric linkage systems are affixed to body segments on either side of a joint, and the potentiometer rotations are registered electronically. Remote measurements have also been carried out in this fashion, using strings or rubber bands attached between the subject and an external transducer. The advantages of these systems are their low cost and easy application; disadvantages include the load imposed on the subject and (in the case of potentiometric linkages) the limitation to relative joint movement measurement. Absolute body segment measurement, as required when dynamic analysis is to be performed, is not possible in this way, and other methods are required if full estimation of joint or segment forces, moments, and energies is contemplated.

Ultrasound. One 'wireless' approach for biokinematic measurement has been based on the Doppler-effect. Whenever a sound source emits a continuous wave of frequency f_s, the frequency f_v perceived by a receiver depends on the velocities of the sound source and receiver with respect to the medium through which the sound is propagated. In the case of a stationary receiver and moving sound source, e.g., an external microphone and an ultrasonic emitter attached to the wrist of a sportsman, the wrist velocity component v_s towards or away from the microphone will be

$$v_s = c \, \frac{f_r - f_s}{f_r} \tag{1}$$

where c is the velocity of sound in the medium (in air, c = 343 m/s at 20° C). The frequency change can be measured by techniques similar to those used in conventional UHF/FM receivers. In an ergonomics context, this approach was used by Nadler (1955) and by Nadler & Goldman (1959) who derived 3-D velocities, translations, and accelerations by further signal processing. Hennig & Nicol (1976) reported a multichannel system based on ultrasonic emitters in the 25-40 kHz range, and stated that errors on the order of 0.5% may be expected as a result of turbulence and variations in temperature and humidity of the air. Furthermore, reflections from neighbouring surfaces may cause errors by interfering with the 'direct sight' waves.

An alternative technique is based on pulsed ultrasound. Here, long microphonic 'bars' detect the travel time of the sound resulting from the generation of an electric spark between two adjacent contacts. Originally designed for manual image digitization purposes, such systems have been used for direct measurement of 3-D wrist kinematics *in vivo* (Brumbaugh et al., 1982).

Opto-electronics. With the emergence of video technology, many attempts have been made to adapt standard closed-circuit TV equipment for biokinematic data acquisition (Furnée, 1967; Jarrett, 1976; Whittle, 1982; Taylor et al., 1982). Typically, retro-reflective landmarks affixed to the subject are stroboscopically illuminated by light sources close to the observing cameras. The light is reflected back along the line of sight, irrespective of the landmark attitudes, and special circuitry detects the circumference of the landmark images. Images blur is prevented because of the short, stroboscopic flash synchronous with the image scanning rate. From the circumference measurements, the horizontal and vertical coordinates of the landmark images' centroids are calculated with a much higher resolution than the basic scanning resolution of the video system (Taylor et al. (1982) claimed a figure of 1:2000 per image axis). Temporal resolution is essentially limited by standard video conventions, namely 50 fps in Europe, and 60 fps in North-America.

Such TV systems only allow the measurement of x-y coordinates, not the identification of landmarks (e.g., wrist, elbow, hip, knee). For the latter purpose, some form of interactive *pattern recognition* is required. In the commercial VICON[†] system, this is achieved by means of extrapolative prediction, for each camera independently, in partial interaction with an operator at a graphics terminal. In the GALATHEA system (MacKay et al., 1982), identification is further facilitated by correlating unidentified landmarks in a particular view with comtemporaneous observations which have already been identified in different views.

TV systems suffer from low resolution in time, since the standard line-by-line scanning norm causes most time to be spent on deciding where the landmark images are *not* located. By means of a 'random-access' approach, the landmark prediction algorithm described above for non-real-time identification may be use, on-line, for the definition of a suitable search area. Image dissector tubes and solid-state arrays are currently used for real-time tracking of single and multiple targets, usually in a military context, and such technology may be straightforwardly adapted for biomechanical purposes.

Macellari (1983) has described a video system for 3-D measurement of up to 8 infra-red light emitting diodes (IR LEDs). This CoSTEL* system consists of a spatial arrangement of three linear, 2048-elements charge-coupled

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[†] VICON (VIdeo CONverter for Biomechanics). Trademark of *Oxford Dynamics Ltd*, ABINGDON, Oxon, United Kingdom.

[*] CoSTEL (Co-ordinate Spaziali mediante Transduttori Elettrici Lineari). An Italian acronym for "spatial coordinates by linear electrical transducer".

photodiode arrays in combination with cylindrical lenses. As is the case in the SELSPOT system discussed below, the LEDs are operated in *time-division multiplex*, at a sampling time of approximately 100 μs per LED. By means of a spatial averaging technique, an image resolution of 1:4000 is attained.

A completely different class of sensing systems does not require scanning of an image area at all. Here, the incident light distribution is directly converted into position-dependent signals (cf. Woltring, 1974). An example which has led to actual biokinematic equipment is based on the *lateral photoeffect* (cf. Woltring, 1975) - see Fig. 1. In this approach, the image of an external light source is focused onto the surface of a special photodetector having lateral contacts on both faces. When properly connected, the generated photocurrent divides itself between the two contacts per face of the detector, each of the currents being a linear function of either the x- or y-coordinate of the incident light distribution's centroid. These currents can be processed to compensate for signal light intensity (which varies with distance), and for background light influences (Woltring & Marsolais, 1980).

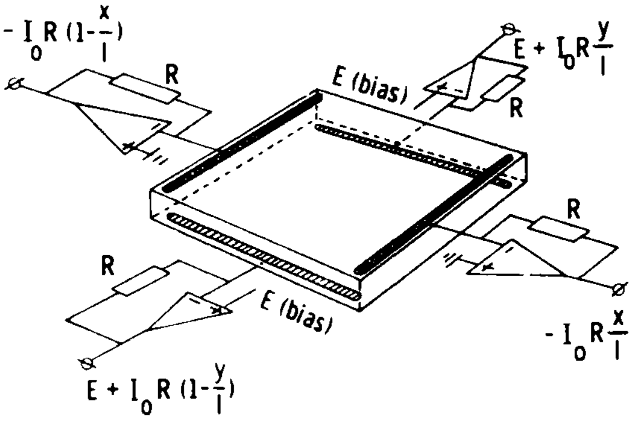

Fig. 1. Dual-axis, duo-lateral photodiode for 2-D, linear position detection of the centroid of an incident light distribution (from Woltring, 1974, 1975).

A multi-landmark system based on this principle was originally developed by Lindholm (1974), and has since been made available as a commercial, multi-camera system SELSPOT[†] (cf. Woltring & Marsolais, 1980). Recently, a substantially improved version SELSPOT-II has become available with a

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[†] SELSPOT (SElective Light SPOT recognition). Trademark of *SELCOM Selective Electronic Company AB*, PARTILLE, Gothenburgh, Sweden.

spatial resolution of 1:4096 per image axis (i.e., 12 bits), 100 µs sampling time per landmark (the system uses IR LEDs which are operated in time-division multiplex), and improved compensation both for background light interference and observed intensity variations caused by landmark orientation and distance changes. Intensity dependence may be compensated by means of a feedback system controlling the light power of the LEDs, although at the expense of increasing the sampling time per LED by a factor equal to the number of cameras in operation.

A fundamental problem with the lateral photoeffect is its sensitivity to reflections. Whenever false images occur on account of reflection of the IR LEDs' light at neighbouring surfaces (ground, walls, ceiling, adjacent body segments), the integrating character of the lateral detector will cause substantial and irreversible errors. Furthermore, the system uses *active* landmarks, with concomitant encumbrance to the subject, and the possibility of crosstalk to any neighbouring EMG-wires unless all cables are carefully shielded. On the other hand, major advantages of the system are its real-time characteristics, the automatic identification of multiple landmarks, and its high spatio-temporal resolution in comparison to TV approaches (unless the number of time-multiplexed landmarks becomes inordinately high). These features allow its use in real-time situations, such as in biofeedback paradigms; furthermore, if clinical use puts high emphasis on short turn-around times, the system is superior to TV-approaches. However, further data processing is also time consuming, for any data acquisition system.

Another approach is based on scanning mirrors. Güth et al. (1973) and Heinrichs (1974) reported a method in which a V-shaped light image is projected onto a rotating mirror. The mirror sweeps the reflected image through space, and the legs of the V repeatedly hit small photodiodes affixed as landmarks to the subject's body. The times at which a photocurrent pulse occurs are converted into 2-D direction information, with a claimed resolution of better than 1 mm at 8 m distance, and 40 Hz sampling frequency. More recently, a commercial system CODA-3*, based on similar principles, has been announced. Unlike its predecessor (Mitchelson, 1975), it uses three rotating mirrors, two of which rotate about a vertical axis, with a stereobasis of 1 m between the rotation axes, the remaining one rotating about a horizontal axis at a position halfway between the other two mirrors. An 1-shaped light image

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* CODA-3 (Cartesian Optoelectronic Digital Anthropometer). Trademark of *Movement Techniques Ltd*, BARROW-UPON-SOAR, Leicestershire, United Kingdom.

is projected onto the three mirrors by means of fibre-optics, and the subject wears retro-reflecting, *coloured* prisms. The times at which the light is reflected back to the mirrors is read out, and converted *on-line* into 3-D, cartesian coordinates. Identification is brought about by the distinct colours of the prism landmarks. At the present time, the basic resolution per mirror is reported to be 1:16000, over an angle of 40°, and at a scanning frequency of 600 Hz. For the given stereobase, the resolution (in mm) at a distance Z (in m) from the scanner is quoted as 0.1 Z for the transverse and vertical axes, and as $0.1\ Z^2$ for the longitudinal axis, at a field width (in m) of 0.8 Z - 1. Low-pass filtering will improve these figures, e.g., by a factor of $\sqrt{12}$ at 50 Hz. Currently, the number of landmarks which can be simultaneously accommodated is 8, but an increase to 12 - 16 is to be expected.

All these optoelectronic systems are complementary in terms of active/passive character and number of landmarks, spatio-temporal resolution, real-time properties, and subject encumbrance; none is superior to the others in all respects, and the choice between them depends on the individual application.

Accelerometry. As discussed in the section on data processing, displacement measurements tend to be unreliable if velocities and accelerations are to be derived from them, because of the amplification of high-frequency noise due to, e.g., quantization and electronic signal processing. For this reason, direct measurement of accelerations has been preferred by various investigators especially in impact studies (Morris, 1973; Becker & Williams, 1975; Padgoankar et al., 1975; Voloshin & Wosk, 1982). As with aerospace navigation methodology, these data may be integrated to yield velocities and displacements, both translational and rotational (Mital & King, 1979). Unfortunately, integration of measured data results in amplification of low-frequency errors (drift), and much effort in the area of aerospace navigation has been directed to the optimal combination of displacement, direction, and distance measurements with direct measurements of velocities and accelerations. Recently, such methods have begun to appear on the biomechanical scene (Seemann & Lustick, 1981), as discussed in the next section.

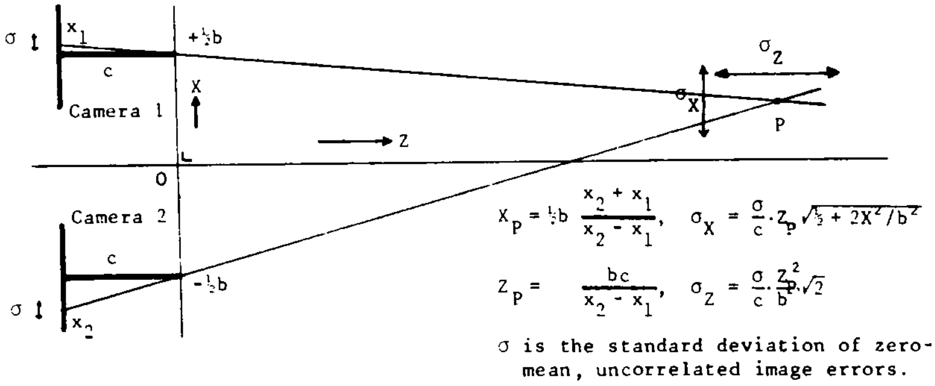
3. Biokinematic Data Processing

As a prerequisite to the estimation of movement kinetics (forces and moments in body segment and joints), and movement energies and power, a substantial amount of calibration and kinematic data processing is required. As discussed in the preceding section, the measurement of 3-D human movement through projective imagery involves the calibration of a multi-camera or camera-mirror configuration, the estimation of individual landmark positions in a laboratory-defined coordinate system, position and attitude estimation of body segments marked with at least three non-collinear landmarks having known coordinates with respect to local, body-fixed coordinate systems, and the estimation of translational and rotational velocities and accelerations. The term *estimation* is used here in the statistical sense: all measurements relating to the movement process are susceptible to error, and an important issue is the effect which these errors may have on calculated positions, attitudes, velocities, accelerations, energies, forces and moments.

All components in the signal processing chain involve a certain amount of implicit or explicit *modelling* of the real world (with concomitant modeling errors), and definition or calibration of certain model parameter values. Unlike contemporary system identification methodology (e.g., Eykhoff, 1974), biomechanics suffers to a considerable extent from the use of insufficiently validated models, particularly where the underlying assumptions are concerned. Thus, cameras are assumed distortion-free, optical axes in stereo configurations to intersect at exactly 90°, and simple, intuitively appealing algorithms are used without criticism. It is the purpose of this section to evaluate Bernstein's data processing work in this light, with special reference to the work he published in 1930, 1935 and 1936, and to outline more appropriate methodology which has been developed in a number of parent disciplines during the past 30 years.

Bernstein's Photogrammetry. Bernstein's papers in the German literature contain a wealth of methodological considerations and should be digested by anyone who intends to do experimental work in movement analysis. Amongst other things, he analyzed the influence of observation distance Z_P (see Fig. 2) and stereo base b on position reconstruction, utilizing planar triangulation for the conventional case of parallel stereophotogrammetry, and with specified resolution of the image data. For given camera field angles, the stereometric 'overlap' between the cameras for an acceptable ratio of depth-to-parallel resolution, $\sigma_Z/\sigma_X \leq 2Z_P/b$, was found to be a serious limitation, and Bernstein developed his mirror-method in order to improve in this respect. Also the mirror-camera configuration was extensively analyzed in terms of image error influence. Furthermore, in order to minimize the influence of systematic lens distortion errors, he advocated using the same lens for observing the movement and for projecting movement data onto a digitizing table.

In Bernstein's day, automatic equipment for processing his large amount



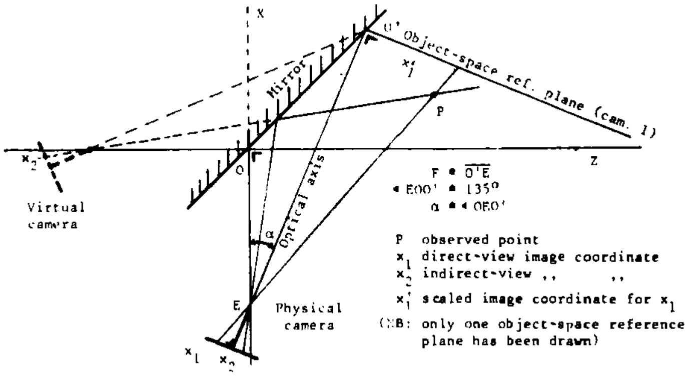
$$X_P = \tfrac{1}{2}b\,\frac{x_2 + x_1}{x_2 - x_1}, \quad \sigma_X = \frac{\sigma}{c}\cdot Z_P\sqrt{\tfrac{1}{2} + 2X^2/b^2}$$

$$Z_P = \frac{bc}{x_2 - x_1}, \quad \sigma_Z = \frac{\sigma}{c}\cdot\frac{Z_P^2}{b}\cdot\sqrt{2}$$

σ is the standard deviation of zero-mean, uncorrelated image errors.

Fig. 2. Error propagation in the normal, stereophotogrammetric case.

of measurements did not yet exist, and his group devoted considerable effort to the design of *nomogrammes* for the graphic conversion of image data - scaled to a reference plane in object space - into 3-D coordinates. For constructing these nomogrammes, certain configurational constraints had to be met, and certain parameters had to be quantified by calibration. In particular, the camera had to be level, and the mirror vertical; these requirements were verified by means of a spirit level and a plumb line. Furthermore, the distance F between the camera and the mirror, along the camera's optical axis, and the angle α (see Fig. 3) were required. Contrary to the situation in conventional geodetic surveying, Bernstein found that direct angle measurement was insufficiently accurate in his close-range situation, and he resorted to an indirect approach in which F and α were calculated from measured distances between the camera lens, mirror vertices, and a vertical yardstick somewhere in the observation field, in combination with image data on these targets.



F = $\overline{O'E}$
∠EOO' = 135°
α = ∠OEO'

P observed point
x_1 direct-view image coordinate
x_2 indirect-view ,, ,,
x_1' scaled image coordinate for x_1
(NB: only one object-space reference plane has been drawn)

Fig. 3. Geometry of Bernstein's mirror photogrammetry.

In Bernstein's work, the cameras and mirrors were assumed to be free of distortion; similar assumptions have been entertained by many recent biomechanicians.

Contemporary Photogrammetry. Unverified assumptions on model appropriateness are largely avoided in contemporary photogrammetry (e.g., Ghosh, 1979; Slama, 1980), and more general camera configurations and image error models are used. An essential notion in this context is that of *redundancy* or *overdeterminacy*, i.e. the use of more measurements than is minimally required, in order to improve on the accuracy of estimated variables and parameters by means of some form of averaging. Bernstein was apparently not aware of the advantages of redundant measurement, although another explanation might be that he ruled this out on account of the already overwhelming amount of work required for processing nonredundant data. For example, the horizontal X-Z coordinates in Fig. 3 are exactly determined by the real and virtual, horizontal image coordinates, but the vertical coordinate Y is overdetermined in that both the real and virtual, vertical image data yield estimates for the Y-coordinate, for prior estimated X- and Z-coordinates. Bernstein did acknowledge that the discrepancy between these Y-estimates could be used as an accuracy measure, but did not mention the possibility of calculating some weighted average as an improved estimate.

In contemporary analytical photogrammetry, camera calibration and 3-D target position reconstruction are often treated in a dualistic or in a simultaneous manner. A highly popular approach in biomechanics today is the one based on the *Direct Linear Transformation*, or DLT, originally developed by Karara and his associates (cf. Miller et al., 1980) which accommodates all forms of linear image distortion, and which allows virtually arbitrary camera positioning with respect to a given laboratory coordinate system. The relation between an object point P, with object coordinates $\underline{X}_p \triangleq (X_p, Y_p, Z_p)'$, and the corresponding image point p, with image coordinates $\underline{x}_p \triangleq (x_p, y_p)'$ is described by the bilinear ratio

$$\begin{aligned} x_p &= (a_1X_p + a_2Y_p + a_3Z_p + a_4) \;/\; D \\ y_p &= (a_5X_p + a_6Y_p + a_7Z_p + a_8) \;/\; D \qquad (1) \\ \text{where} \quad D &= (a_9X_p + a_{10}Y_p + a_{11}Z_p + 1) \end{aligned}$$

For a given 3-D distribution of at least 6 *control points* having known coordinates $\underline{X}_{pi}$, and corresponding measured image coordinates $\underline{x}_{pi}$, the DLT parameters $\underline{a} \triangleq (a_1 \ldots a_{11})'$ may be determined in a statistically optimal fashion, for a given measurement error model. Conversely, for given parameter vectors $\underline{a}_j$, the coordinates $\underline{X}_p$ of some unknown point P may be determined from image coordinates $\underline{x}_{pj}$ in at least two cameras (j= 1,2).

Particularly in the study of locomotion, the construction, maintenance, and (trans)portability of a sufficiently large and stable, 3-D calibration object is rather problematic. One should note in this context that the calibration distribution should encompass the full observation volume, in order to minimize the influence of extrapolation errors. Recent developments in photogrammetry have resulted in procedures which avoid the need for a precisely known 3-D calibration distribution. The most general of these is called *Analytical Selfcalibration*. Here, a stationary 2-D or 3-D distribution of targets with unknown coordinates is observed by a camera with stable but unknown internal parameters (e.g., focal length), while held in different *attitudes*, and possibly in different positions, and both the unknown targets and the unknown camera parameters are recovered. In this approach, it is merely necessary to define a coordinate system and a yardstick, e.g., by defining one target to be the origin of object space, a second target to be at a certain position on the X-axis, and a third target, non-collinear with the other two, to have Z=0. For each individual 'frame', i.e., the set of image coordinates for the camera in a specific attitude and position, determinacy does not obtain, but the combination of at least three different attitudes yields a unique, highly overdetermined solution. Since the positions of the targets are usually the eventually required quantities, the name "selfcalibration" has been adopted for this approach.

Such calibration and reconstruction procedures impose limited constraints on the equipment being used in that only *stability* of the complete configuration is required. In cinematography, this may be a problem on account of mechanical vibration and sprocket tolerances in the film transport mechanism, but opto-electronic systems generally meet this requirement. On the negative side, the complexity of some of the more advanced photogrammetric methods has deterred many non-photogrammetrists from exploring this field, despite the availability of comprehensive software packages.

A number of investigators in biomechanics have opted for solutions intermediate to the DLT and Analytical Selfcalibration. Woltring (1980) has described a procedure using a known, *planar* distribution, to be held at various different and numerically unknown attitudes throughout the observation field, for complete calibration of internal and external camera parameters. Dapena et al. (1982) used a distribution of known linear 'yardsticks' placed at unknown positions throughout the field, while estimating internal camera parameters in a separate method.

The reconstruction of landmark positions in these procedures follows similar lines. Ideally, iterative adjustment calculus is used to find those spatial coordinates for which a statistically motivated costfunction on image residuals (i.e., discrepancies between observed image data and predictions following from estimated 3-D positions) is minimized. A slightly biased solution is the one which optimizes in terms of object-space distances, i.e., that particular and usually unique point for which a (weighted) sum of squared distances to direction lines emanating from the cameras is minimized. For given camera positions $\underline{P}_{c_i}$ and observed direction lines $\underline{S}_i$ (both expressed in terms of a laboratory-defined coordinate system), the ODLE-solution (for *Object-Distance Least-squares Error*) becomes

$$\hat{\underline{X}} = \left(\sum_i Q_i \right)^{-1} \cdot {}_{i}\, Q_i \underline{P}_{c_i}, \quad \text{where } Q_i \triangleq I - \underline{S}_i \underline{S}_i' \,/\, \underline{S}_i' \underline{S}_i \qquad (2)$$

Rigid-Body Kinematics. The estimation of individual point coordinates is merely an intermediate phase in kinematic calculus. For complete movement analysis, translations and rotations of body segments are sought; these may be evaluated once global and local landmark coordinates are known (e.g., Spoor & Veldpaus, 1980). Alternatively, these variables may be evaluated directly from observed image coordinates by means of iterative adjustment calculus, even when not all landmarks are completely observed so as to allow their individual reconstruction (Miller et al., 1980; Woltring, 1982).

The calculation of these kinematic variables is quite costly, and this presumably deterred Bernstein from carrying out a complete 3-D analysis involving at least 3 non-collinear landmarks per body segment. Instead, he limited himself to landmarks close to the joint 'centres' per body segment, thus rendering axial rotation unobservable, for each segment. For normal locomotion studies, or as one phase in the iterative conception of research, this simplification was quite viable; only recently has axial rotation been deemed important in the study of pathological gait in such fields as neurology and orthopaedics. Besides, the precision with which axial rotation can be determined is a linear function of the distance of a third landmark to the axis defined by the joint landmarks, and this has given rise to practical difficulties in the case of typically oblong arm and leg segments.

Two other important issues are, first, the validity of the rigid-body concept which underlies these kinematic models, and secondly, the definition of local coordinate systems, since no two body segments are exactly equal. These questions will be addressed more fully in the section on body segment description and parameter estimation. On the kinematic level, deviations from the rigid-body model such as may be caused by skin shifts can be ascertained from the intermarker distances calculated from reconstructed point positions; these discrepancies can then be used to adjust the local landmark coordinates.

Derivative Estimation. As apparent from Newton's laws, velocities and accelerations are required input data for dynamic modelling and data processing. The optimal estimation of these derivatives seems to be a perpetual bone of contention in experiment biomechanics. There would appear to exist two reasons for this situation. First, *stochastic* and *deterministic* errors are confounded, and secondly, the Shannon sampling theorem is taken to be a *sufficiency* criterion instead of a *necessary condition*. Furthermore, certain intuitively appealing, 'simple' algorithms for derivative estimation are taken at face value, without proper analysis.

As regards the first point, it is often argued - as Bernstein did in criticizing the need for Braune & Fischer's 50 μm resolution in estimating landmark coordinates - that the large discrepancies found in the case of redundant measurement do not warrant such high resolutions. However, suppose that all measurements are affected by the *same* constant error, differences calculated from these data as used in derivative estimation will be error-free. By analogy, low-frequency errors such as those due to skin shifts during movement will have comparatively little influence on the estimation of velocities and accelerations. On the other hand, uncorrelated, wide-band errors have a considerable influence, since differentiation in the time domain is equivalent to multiplying the frequency spectrum by the frequency, thus emphasizing higher frequencies in the data. Quantization errors tend to belong to this class, and Bernstein actually investigated their influence on his derivative estimators.

As regards the second point, the sampling theorem (formulated since Bernstein's day) prescribes that a signal should be sampled at a rate which is at least twice the largest signal frequency contained in the data. Since physically realizable signals are not strictly band-limited (cf. Slepian, 1976), and always affected by some wide-band noise, one should sample at a frequency which is at least twice as large as the cross-over frequency beyond which the noise level is dominant. In this way, frequency 'aliasing', i.e., the occurrence of stroboscopic effects, is minimized. On the other hand, the sampling theorem does not say anything about the utility of sampling at much higher frequenceis. If the signal is disturbed by wide-band (i.e., largely uncorrelated) noise, it can be advantageous to sample at a much higher frequency, and then pass the data through a suitable low-pass filter. In this way, the influence of noise is reduced by averaging. For a strictly band-limited signal (bandwidth $\omega_B \triangleq 2\pi f_B$), sampling interval τ, and additive white noise with standard deviation σ, Lanshammar (1981, 1982) has shown that an ideal low-pass estimator for the k-th derivative (or for the signal proper, with k=0) has output noise with standard deviation

$$\sigma_k = \sigma\sqrt{\tau}.\omega_B^{2k+\frac{1}{2}}/\{\pi(2k+1)\}^{\frac{1}{2}}, \quad k = 0,1,2, \ldots \qquad (3)$$

This ideal estimator is perfect up to the frequency ω_B, and a perfect attenuator for higher frequencies.

The model (3) is ideal in three respects: (i) no signal is strictly band-limited, (ii) noise cannot be perfectly white, and (iii) a filter cannot have a perfect discontinuity in its frequency response. However, the ideal filter may be approximated arbitrarily closely. From (3) one can see that increasing the sampling frequency, i.e., decreasing τ, will result in smaller values of σ_k. In fact, the term $\sigma\sqrt{\tau}$ demonstrates the utility of defining a combined *spatio-temporal resolution criterion* in addition to the sampling theorem requirement. On the other hand, the white-noise assumption underlying (3) may be significantly violated if τ is chosen too small when quantization noise is the dominant error source: in this case, adjacent samples may remain within the same quantization interval, and thus contain perfectly correlated errors.

The availability of a spatio-temporal resolution criterion allows the evaluation of different measuring systems such as the various optoelectronic systems discussed in section 1 of this chapter. In order to render a comparison valid, stereometric configurations must be defined in such a fashion that the different systems, with their different viewing angles, encompass the same field of view. For example, Braune & Fischer used equipment with σ = 50 μm and τ^{-1} = 26 Hz, which was definitely superior to Bernstein's in terms of the $\sigma\sqrt{\tau}$ criterion, but they failed to process their data properly for derivative estimation, as demonstrated by Bernstein (1935). In view of Bernstein's claim that sampling frequencies higher than 26 Hz were necessary for the detection of minor but stable 10 Hz signals in his locomotion data, it is of interest to compare the signal and noise transmission properties of his derivative estimators with the ideal case (3). In one experiment, he reported a sampling frequency of 90 Hz, and a measurement accuracy of ±0.5mm. If the quantization noise is assumed to be uncorrelated (i.e., 'white'), and uniformly distributed throughout the quantization interval, its standard deviation may be calculated as $1/\sqrt{12}$ mm. Substitution of these values into (3) yields $\sigma_1 = 4.94$ mm/s and $\sigma_2 = 0.24$ m/s² for ideal, single and double differentiators up to 10 Hz, and perfect attenuators beyond this frequency. Now, Bernstein (1935) used the following estimators for the first and second derivatives,

$$\hat{v}_i = (x_{i+2} - x_{i-2})/4\tau, \quad \hat{a}_i = (\hat{v}_{i+2} - \hat{v}_{i-2})/4\tau \tag{4}$$

where i is the sample index. The noise output from these estimators, again assuming white noise with $\sigma = 1/\sqrt{12}$ mm, can be calculated as $\sigma'_1 = \sigma\sqrt{2}/4\tau = 9.19$ mm/s and $\sigma'_2 = \sigma\sqrt{6}/(4\tau)^2 = 0.358$ m/s², respectively; these figures are considerably higher than those for the ideal differentiators. Furthermore, Bernstein's estimators appear to be significantly *biased* when analyzed in terms of their frequency response. From (4), it may be shown that the transfer function of Bernstein's differentiator for $\hat{v}_i$ is $j.\sin(2\omega\tau)/(2\tau)$, whereas the ideal characteristic is $j\omega$, with $j \triangleq \sqrt{-1}$. The ratio $H_1(\omega\tau)$ of these functions, and its square $H_2(\omega\tau)$ for the double differentiator are

$$H_1(\omega\tau) = \mathrm{sinc}(2\omega\tau) \triangleq \sin(2\omega\tau)/(2\omega\tau), \quad H_2(\omega\tau) = \{\mathrm{sinc}(2\omega\tau)\}^2 \tag{5}$$

and these approach unity for the limiting case $\omega\tau \to 0$. However, at higher frequencies, considerable attenuation occurs as shown in Fig. 4 for the double differentiator. For the above numerical values, $H_1 = 0.71$ and $H_2 = 0.5$. Thus, the signal-to-noise ratio (SNR) of Bernstein's acceleration estimator is worse by a factor $q_2 \triangleq H_2.\sigma_2/\sigma'_2 = 0.334$, in comparison with the SNR of the ideal low-pass differentiator. As discussed in the section on kinetic data processing, this fact is relevant when assessing the significance of some of Bernstein's force estimates. The bias in (4) explains in part Bernstein's criticism of Braune & Fischer's 26 Hz frame rate. As Bernstein (1935) demonstrated by means of Fourier analysis, the investigation of higher harmonics is facilitated when dealing with accelerations and forces, since higher harmonics are amplified by a larger amount during differentiation. Using $\tau^{-1} = 26$ Hz and $f_B = 10$ Hz, the low-pass gain H_2 in (5) becomes 0.042, i.e., smaller by a factor of 11.8 than that obtained for $\tau^{-1} = 90$ Hz. On the other hand, Braune & Fischer attained a resolution of 50 μm, which was better by

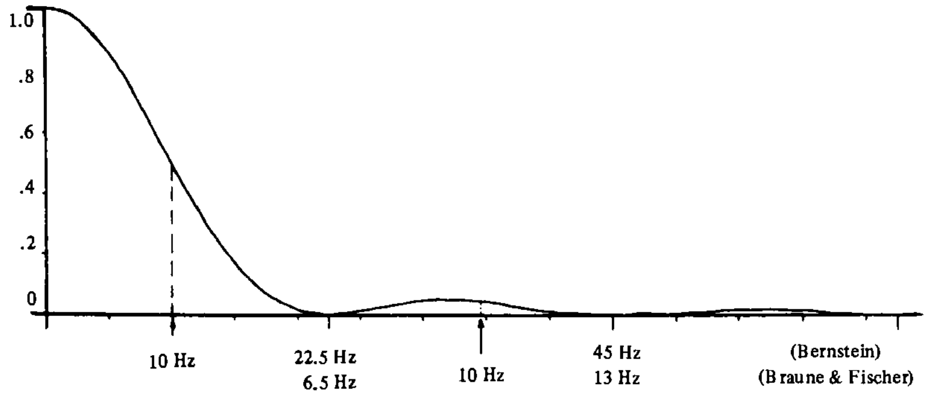


Fig. 4. Low-pass characteristic $H_2(\omega\tau)$ in Bernstein's acceleration estimator.

a factor of 10 than Bernstein's, thereby producing an actual SNR proportional to H_2/σ'_2 of about the same magnitude. This explains why Bernstein (1935), having processed some of the raw data provided by Braune & Fischer, found harmonics similar to those in his own data!

Since Bernstein's day, considerable progress has taken place in the domain of digital signal processing, and a number of efficient algorithms have been described which afford suitable balancing of passband and stopband transfer characteristics (see, for example, Oppenheim & Schafer (1975), and especially Rabiner & Gold (1975)). One of the rare applications in biomechanics is that of Lesh et al. (1979). Other variable approaches include the optimal regularization method of Andersson & Bloomfield (1974), introduced into biomechanics by Hatze (1981b), and the use of spline functions (Soudan & Dierckx, 1979; Wood & Jennings, 1979). Spline functions are more general than the preceding algorithms since they do not require equidistant time intervals; as a result, they combine the techniques of low-pass filtering, derivative estimation, and interpolation in the case of temporary data loss due to shadowing effects between a landmark and an observing camera.

For all these algorithms, information on signal and noise properties is required. To some extent, this distinction is semantic insofar it may be the investigator's purpose to decide what is signal, and what is 'noise'. However, technical limitations, particularly those imposed by the spatio-temporal resolution criterion, are often dominant, and this justifies the use of an effective cross-over frequency, beyond which a flat frequency spectrum is deemed to be mainly due to measurement artefacts.

In summary, the selection of a proper data acquisition *and* processing chain is essential for reliable estimation of numerical derivatives. Of course, Bernstein can hardly be blamed for not fulfilling this desideratum, since most of the theory underlying the criticism of his derivative estimation was developed after his time, and he did attempt some comparisons with more elaborate filtering algorithms. For example, the finite difference (4) was shown to be the slope at x_i of a parabola fitted to the points x_{i-2}, x_i, x_{i+2}, and Bernstein did look into improvements on the *displacement* level when higher order polynomials were fitted. However, he did not compare the error levels in the slopes of these functions, and merely looked at the errors for his finite difference model (4). Presumably, he choose the ±2 increment in (4) after finding that the use of adjacent samples resulted in excessively noisy derivatives. Furthermore, the noise terms in adjacent acceleration estimates $\hat{a}_i$ and $\hat{a}_{i+2}$ are strongly correlated ($\rho = -0.67$). By interspersing uncorrelated estimates derived from odd and even samples, it is possible that the data would have appeared less noisy.

State-Space Modelling. In most data processing approaches discussed hitherto, the various stages of landmark position reconstruction, rigid-body calculus, derivative estimation, and so on, were treated as independent, sequential operations. For more or less ideal data, this approach is quite efficient, but experimental data are unfortunately by no means ideal. In particular, the temporary loss of sight of moving landmarks and the non-rigid nature of the body segments cause considerable problems. Thus, rotations and translations may become *unidentifiable*, or - in the case of redundant measurements - residual error changes caused by the combination of skin shift and reappearence of a landmark may entail sudden, apparent acceleration peaks. This will be discussed more fully in the section on kinetic data processing.

Recent developments in engineering system identification and state/parameter estimation, particularly in control engineering and aero-space navigation, have resulted in comprehensive models and algorithms for the simultaneous estimation of relevant variables and parameters from whatever measurements are available. Typically, a statistically motivated *state-space* description of the dynamic process under study is provided, for instance, in terms of positions, rotations, and velocities. These *state variables* are viewed as stochastic quantities which propagate themselves according to a given set of dynamic or kinematic equations, and they are corrected by any incoming measurement in accordance with the statistics of the measurement and process models. In this way, it becomes possible to carry out, at the same time, the estimation of rigid-body kinematics and its derivatives, the interpolation for lost data, and the correction for discrepancies between the rigid-body model and the real body segment under study.

When using state-space models, the measurement system may observe different entities such as directions and accelerations which are then merged in a statistically optimal fashion. This is one of the most common situations in aero-space navigation, and recent work in crash biomechanics has been along similar lines (Seemann & Lustick, 1981). In this way, the limitations of each data source are corrected by the complementary data source: accelerations errors entail low-frequency errors in velocities and displacements obtained via integration, whereas velocities and accelerations obtained via differentiation of noisy displacement data suffer from high-frequency errors.

Recommended texts in this field are those by Eykhoff (1974), Gelb (1974), Maybeck (1979), and various AGARD publications. It is to be expected that system identification methods will find increased use in biomechanics (cf. Hatze, 1980a), in view of their succesful application in other fields of engineering and medicine (cf. Bekey & Beneken, 1978; Murray-Smith, 1982).

4. Kinetic Data Acquisition and Body Segment Description

Once the kinematic data processing phase has been completed, the kinetics (forces, moments) of the movement process may be investigated. For this purpose, the mass distributions of the body segments must be known in relation to local segment-defined coordinate systems. Furthermore, explicit force and moment measurements are necessary in the case of *closed kinematic chains*, e.g., during the double stance phase of locomotion when strong 'circulating' forces may exist within a closed chain of body segments and the environment; such forces cannot be determined from the kinematics of the movement, or from stationary posture.

Bernstein confined himself to the investigation of those inertial and gravitational forces and moments which are identifiable from kinematic measurements. Bernstein appears to have been particularly interested in the high-frequency microstructure of the movement process, and in its significance for the underlying movement control processes. By contrast, some investigators in recent years have neglected the inertial components, by estimating total joint forces and moments 'from the ground up', starting from force plate measurements and assuming that the masses of the legs may be neglected. Since these investigations are usually for orthopaedic purposes, with special interest in maximum joint loadings during slow pathological gait, the neglect of inertial effects in the legs may be acceptable, particularly in most of the stance phase where the forces are largest, and the accelerations smallest. Furthermore, the assumption of negligibly small inertial effects renders the data acquisition and processing phases much simpler, and this is an attractive feature for routine clinical purposes. On the other hand, the alternating character of maximum and minimum loading may eventually prove to be a much more relevant factor in mechanical stress and fatigue analysis of components of the locomotor system. In addition, the occurrence of co-contractions between flexing and extending muscles over a joint causes much higher joint loadings than can be identified from kinematic and kinetic measurements. This point will be discussed in a later section.

From the 'prospective' point of view, model simplifications should be minimal, and the aim should be to analyze all kinetic components of human movement. In the following sections, the use of force plates and the acquisition of body segment parameters for the comprehensive analysis of movement kinetics is discussed.

Force Plates. Simple force plates for ground reaction force measurement were already known in Bernstein's time (Amar, 1916), and it is unclear why Bernstein did not use, or even refer to these measurement tools. Today, force plates of various sizes, based on piezo-electric transducers or strain-gauges, are commercially available. Typically, they provide force and moment information integrated over their surfaces (cf. the integrating nature of the lateral photoeffect detector depicted in Fig. 1), that is, vertical and horizontal force components, vertical moment, and mean point of impact. Some disagreement exists as to the need for high sampling frequencies in the case of impact transients, such as heel- and toe-strike during gait, since the recorded peaks might be caused by mechanical resonance ('ringing'). Furthermore, the mounting of force plates requires proper damping of vibrations from other sources. The possibility to place multiple force plates in different positions with respect to each other is useful when it comes to accommodating different gait patterns, e.g., childrens' versus adults', or normal versus 'scissors' gait where one foot is placed partly in front of the other.

When conducting comprehensive movement studies using both kinematic and kinetic measurements, careful calibration of, and synchronization between the data sources is necessary. Thus, the force plate coordinate system must be known with respect to the kinematic coordinate system, and the integrity of the data collection system must be verified. Some of the kinematic data acquisition systems described in section 1 of this chapter incorporate the possibility of synchronized data collection from other sources.

Body Segment Description. It is particularly in the quantitative description of body segments that the intimate relation between mathematical modelling, data acquisition, and data processing becomes obvious. In all kinematic and kinetic analyses, modelling of the human body as a linked system of rigid bodies is perhaps one of the crudest approximations to reality. Bernstein (1936) recognized the limitations of the rigid-body model when reproducing Fischer's data on the proximal and distal, relative distances (0.44 and 0.56, respectively) for the 'by no means rigid' trunk, in addition to the data in Table 2 of Chapter I. For lack of better information, he nevertheless used these parameters to estimate body segment forces and total centre of gravity forces from landmarks observed at the hips and shoulders (Bernstein, 1935). However, he demonstrated that the influence of random errors in his acceleration data on the force estimates was much stronger than the non-rigid nature of the body segments.

Since Bernstein carried out his investigations, considerable attention has been paid to quantitative anthropometry, particularly in sports, aerospace, and crash biomechanics where high accelerations do not allow the neglect of inertial effects (see, e.g., Miller & Nelson (1973), Chandler et al. (1975), and Herron et al. (1976) which give an adequate account of the literature). Hay's biomechanical bibliography (1981) contains about 10 pages on anthropometry, and only 4 on data smoothing and general mathematical modelling! Current methods for body segment anthropometry include volume determination by immersion, biostereometrics, and tomography, under the assumption of certain mass densities for the estimation of inertial properties. Furthermore, a variety of dynamic system identification methods are used for estimating inertial properties from observed kinematics, e.g., the swing time of a freely swinging leg modelled as a pendulum. More recently, inconsistencies in estimated kinematics and kinetics have been used for the correction of mass distribution parameters (Vaughan et al., 1982).

Comprehensive mathematical models have been presented by Hanavan (1964) and, more recently, by Hatze (1980b). Hatze's 17-segment model of the human body requires 242 measurements to be taken on the subject, and this renders his model impractical for routine use. However, the availability of such comprehensive models will allow systematic evaluation of those measurements and parameters which may be neglected in the estimation of total movement dynamics.

As discussed in the next section of this chapter, considerable attention is currently being given to the *redundancy* or *indeterminacy* problem in biomechanics. Mass distribution parameters, kinematic measurements, and reaction force measurements with the environment only provide total, *external* forces and moments, not the contributions of individual muscles, ligaments, and other components of the musculoskeletal system. For the latter, *internal* forces, a number of models are currently being developed, and these require information on muscle and ligament attachment points, ligament stiffnesses, and geometry of the articulating surfaces. Originally, this information was mostly inferred from anatomical textbooks, but more recently, Röntgen measurements and anthropometric scaling procedures have been used (Brand et al., 1982).

5. Kinetic Modelling and Data Processing

Under the rigid-body assumption, the kinetics and energetics of the movement are comparatively simple as explained in the 'Technical Note' of Hardt & Mann (1980). If each segment is viewed as a free body, with forces and moments acting at its proximal and distal ends (apart from gravity forces which work at its centre of gravity), the following force and moment balance equations based on Newtson's second law apply,

$$\begin{aligned} \underline{F}_{cg} &= m\,.\underline{\ddot{x}}_{cg} = \underline{F}_1 + \underline{F}_2 + m.\underline{g} \\ \underline{M}_{cg} &= I_{cg}.\underline{\alpha}_{cg} = \underline{M}_1 + \underline{M}_2 + \underline{r}_1 * \underline{F}_1 + \underline{r}_2 * \underline{F}_2 \end{aligned} \qquad (6)$$

with

$\underline{F}_{cg}$	the total force acting on the segment's c.o.g.
$\underline{M}_{cg}$	the total moment acting at the segment's c.o.g.
m	the mass of the segment
I_{cg}	the inertia matrix of the body at the c.o.g.
$\underline{x}_{cg}$	the position of the segment's c.o.g.
$\underline{\ddot{x}}_{cg}$	the acceleration of the segment's c.o.g.
$\underline{g}$	gravity acceleration vector ($g = 9.81\ m/s^2$)
$\underline{\alpha}_{cg}$	the angular acceleration of the segment
$\underline{F}_1$, $\underline{F}_2$	distal and proximal external forces acting on the segment

$\underline{M}_1$, $\underline{M}_2$ distal and proximal external moments acting on the segment

$\underline{r}_1$, $\underline{r}_2$ the moment arms of the proximal and distal forces with respect to the segment's c.o.g.

where all quantities are expressed with respect to a common, inertial coordinate system (NB: the inertia matrix I_{cg} is a function of the segment's attitude). Starting from the distal end of a segment with known $\underline{F}_1$ and $\underline{M}_1$ (these are zero in the case of a freely swinging extremity, or otherwise given by a force-place measurement), the proximal values $\underline{F}_2$ and $\underline{M}_2$ follow from (6), for given kinematics and mass distribution parameters. By virtue of Newton's third law (action = reaction), the distal forces and moments of an adjacent segment are equal to the estimated proximal forces, albeit with opposite sign. Thus, all segments may be processed sequentially in the case of open kinematic chains or closed chains with explicit kinetic measurements.

The vector and matrix products in the moment equation of (6) indicate the limited validity of separate analyses in the frontal and sagittal planes, since the moments acting in a given plane may be affected by force components acting outside that plane. Complete, 3-D models for kinetic analysis (e.g., Hatze, 1980a) have recently become available, so it will now be possible to verify the sagittal analyses of Bernstein and many later investigators. In particular, the lateral 'swing' at the hip level in ordinary gait by means of which weight is transferred to the stance leg, is a significant source of extrasagittal kinetics (cf. Bresler & Frankel, 1950).

Bernstein's model and results. Bernstein (1936) regarded the complete 3-D calculation of the product $I_{cg} \cdot \underline{\alpha}_{cg}$ as "one of the most tiring and complicated calculations of applied mathematics", and went on to simplify his model, by neglecting axial rotation in the oblong body segments, and by modelling the mass distribution of the segments in terms of two point masses, one on each side of the segment's c.o.g., at 3/10 of the segment's length. In this way, he was able to separate sagittal from extra-sagittal components, and could confine himself to planar, sagittal analysis with one observing camera per body side.

Adopting these simplifications, Bernstein estimated segment forces and total body forces during locomotion, with particular emphasis on the microstructure of his curves, and it is here that serious doubt exists as to the significance of his results. In the sagittal analysis, the position of a segment's c.o.g. is derived as a linear function of the landmark positions at the segment ends, using Fischers' data (cf. Table 2, Chapter I) as weight factors. Total body c.o.g. forces were subsequently calculated as a weighted sum of individual segment c.o.g. forces, using Fischer's relative masses (Table 1, Chapter I) as weight factors. Mathematically, this results in a formula of the form

$$\underline{F}_{body} = \sum_{i=1}^{20} w_i \ddot{\underline{x}}_i M/g \qquad (7)$$

where $\ddot{\underline{x}}_i$ are the estimated landmark accelerations, w_i the weight factors calculated from Fischer's tables, M the total mass of the subject, g the gravitational acceleration, and $\underline{F}_{body}$ the total body force expressed in kgf. Now, if the $\ddot{\underline{x}}_i$ are disturbed with additive, uncorrelated measurement noise, with standard deviation σ_2', the standard deviation of the components in $\underline{F}_{body}$ follows as

$$\sigma_{\underline{F}} = wM\sigma_2'/g, \quad \text{with } w \triangleq \left(\sum_{i=1}^{20} w_i^2 \right)^{\frac{1}{2}} \qquad (8)$$

According to Fischer's tables and the other data in Bernstein (1935), w= 0.33. Using M= 70 kg and $\sigma_2' = 0.358$ m/s², as derived in the section on kinematic data processing, we find $\sigma_{\underline{F}} = 0.8$ kgf. Considering the amplitudes of the various peaks in, for example, the horizontal components of whole-body c.o.g. forces (Fig. 22, Chapter III), one might well question Bernstein's elaborate account of the minute details in his curves. As pointed out above, if he had better differentiating algorithms at his disposal, his signal-to-noise ratio might have been better by a factor not larger than 3.

This criticism is even more serious in view of the fact that Bernstein (1935) used an additional landmark at approximately the c.o.g. of the thigh. Whenever the freely swinging hand of the subject obscured the hip landmark, he relied on this additional landmark to recover the hip's position, by formally extrapolating from it and from the knee landmark. Not only does this increase $\sigma_{\underline{F}}$ to 1 kgf if one hip landmark is obscured, and to 1.2 kgf if both are simultaneously obscured (it is assumed that the additional landmark was halfway between the hip and knee landmarks), but residual error changes, due to skin movement, in the transition from one model to the other may also have caused sudden, apparent acceleration peaks. This effect is most likely to occur in the case of γ^{ζ} peaks in Fig. 22 and the γ peaks of Fig. 23 (Chapter III; cf. Bernstein, Fig. 11 & 12, Expt. 331).

Unfortunately, it is unclear from Bernstein's description whether he allowed for the possible occurrence of such artefacts.

Comparison with later research. During the 1940-s and early 1950-s, a long series of investigations on normal and prosthetic gait was conducted in the Biomechanics Laboratory of the University of California at Berkeley (Anonymous, 1947; Inman et al., 1981). A frame rate τ^{-1} = 30 Hz was used, and a digitization error of 10^{-4} inch was attained, corresponding to a quantization noise standard deviation σ= 21 μm in object space (assuming that image blur was negligibly small, and that the digitizing operator was perfectly capable of locating the centroid of a landmark image). Numerical derivatives were estimated from finite differences of *adjacent* samples, a procedure which results in a low-pass transfer function $H_2(\omega\tau) = \mathrm{sinc}^2(\omega\tau)$. Using f_B= 10 Hz as was done in the case of Bernstein's data above, this yields $H_2 = 0.171$, $\sigma_2' = 0.046$ m/s², and $H_2/\sigma_2' = 3.69$ s²/m, as compared to Bernstein's $H_2 = 0.5$, $\sigma_2' = 0.358$ m/s², and $H_2/\sigma_2' = 1.40$ s²/m. Thus, the SNR of the Californian acceleration estimates for a 10 Hz bandwidth was better than Bernstein's by a factor of 3.69/1.40 = 2.64. For a 5 Hz bandwidth, this factor increases to 6.28, and it is gratifying to see that most of the acceleration curves published by the California group show quite significant waves in the 5 - 10 Hz range, e.g., in the horizontal components at the ankle, knee, greater trochanter, and iliac crest. Thus, there is some basis for Bernstein's conclusion that "characteris and stable dynamic waves (oscillations)" with a frequency of 10 Hz occur during normal locomotion.

In direct contrast to these conclusions are those of Winter (1982), who claims that human movement at normal walking speed contains no significant frequencies beyond 5 or 6 Hz, and that there is no need to sample at frequencies beyond those of the standard 24 fps cine camera or 30 Hz TV system. He bases these conclusions on his earlier finding that virtually no signal power is contained in the *displacement* data beyond the sixth harmonic of the stride frequency. However, double differentiation of ideal, noisefree measurements may be expected to show quite a different picture, and this may not have revealed itself to Winter since the noise level of his equipment was considerably higher than the noise level of the equipment used by Bernstein or the California group (cf. Winter, 1983). In fact, most contemporary research on the biomechanics of locomotion seems to suffer from much lower spatio-temporal resolution than was achieved by the German, Russian, and Californian investigators, and a suitable research topic would be to repeat some of the old experiments with contemporary, high resolution equipment in combination with proper forms of low-pass derivative estimation. The accelerometric findings of Wosk & Voloshin (1981) concerning the presence of a major frequency component in the 25 - 35 Hz range at the *head*, during normal locomotion, are quite striking in this context.

Contemporary neuromusculoskeletal models. The free-body model (6) may be generalized to more complicated systems if anatomical information on muscles, ligaments, and joint surface shapes is included. The most important, mechanical models are based on the Lagrangian and Newton-Euler methods (Hemami et al., 1975). The Lagrange method is based on the difference between total kinetic and potential energy, expressed in terms of generalized, independent coordinates and their time derivatives, and allows direct calculation of active (external) forces and moments. The Newton-Euler method is directly based on Newton's laws, and leads to equations similar to those in (6); it admits interdependencies between coordinates, and allows the calculation of both active and reactive *constraint* forces and moments. An advantage of the Lagrange method is its smaller number of equations. On the other hand, the Newton-Euler equations have a simpler structure, with less changes being required when the model is changed, e.g., in the transition from single to double stance in locomotion. In recent years, the advantages of both approaches have been combined by Wittenburg (1977) in a method based on d'Alembert's principle of virtual work. Hatze (1981c) and Hatze & Venter (1981) used similar methods in their comprehensive models for analysis and synthesis of human movement.

An important topic in current *bio*mechanics is the study of the indeterminacy problem. Many muscles and ligaments cross a single joint, and various muscles cross multiple joints. By consequence, the kinematic/kinetic analysis of a mechanistic rigid-body model does not reveal the distribution of forces over all anatomical components. A customary hypothesis is that the redundancy in the neuromusculoskeletal system serves as a safeguard, so that the organism may redistribute its activity over different elements in order to compensate for fatigue and damage, such compensatory effects being hardly or not at all observable on the kinematic level, if only for aesthetic reasons. If compensatory effects involve multiple joints, e.g., when transferring load from a painful joint to another, this can be revealed by kinetic analysis (Winter, 1981b; Brown et al., 1981), but if they are limited to a single joint, other measurement techniques are required, e.g., for assessing the relative contributions of ligaments and muscles in joint stabilization. Such measurements may be non-invasive as in the case of surface-EMG, which can indicate the contributions of individual muscles (Hof & van den Berg, 1981), or invasive, as when strain gauges are used in ligaments or artificial joints. Naturally, the use of invasive methods is constrained to animal or volunteer work, or to those clinical situations where surgery is indicated for medical reasons.

Because of these constraints, much research has gone into further modelling. Various engineering models for optimal force distribution based on minimal energy or equal force density across muscles have been used. However, these techniques were used because they happened to be available, rather than because there were sound reasons for believing in the physiological rationale of their optimization criteria; cf. Crowninshield (1978), Hardt (1978), Chao & An (1978), Hatze (1980a), and Brand et al. (1982). Hatze (1980a, 1981a) has been one of the few to model the neural innervation process in complex human movement.

6. Trends and Speculations

One of Bernstein's criticism of his predecessors concerned the limited amount of data on which they based their conclusions. Bernstein (1935) reported that 120 out of 800 cyclographic photographs were analyzed by his group, corresponding to more than 500 single steps, and about 400,000 digitized coordinates. In contrast, Braune & Fischer only analyzed 3 out of 12 cyclographic photographs, corresponding to about 5000 digitized coordinates. After processing the measurements with his chosen biomechanical model, Bernstein attempted to recognize a pattern in the estimated forces and moments, particularly in their microstructure. It is in this sense that Bernstein's work constitutes a unification of two major trends in contemporary studies of human movement, locomotion in particular. One of these is the use of 'simple', direct measurements (electrogoniometry, force plates, EMG) in statistical, empirical studies on large numbers of subjects (Donath, 1978; Chao et al., 1980). The other is the inclusion of more 'complex' measurements (3-D kinematics, mass distribution parameters) with the object of carrying out deterministic, biomechanical analyses on a small number of subjects (Winter, 1981; Brown et al., 1981). In view of the indeterminacy issue, both approaches seem too limited: the 'simple' features used in the former approach are insufficiently discriminative, while the limited amount of data in the latter do not allow generalization to the population at large. Ideally, both approaches would be combined through the development of both deterministic, biomechanical models, and statistical, empirical pattern recognition procedures with suitable data base management. For the latter purpose, current developments in interactive pattern recognition (Gelsema, 1980) are promising.

In orthopaedics and physiotherapy, the investigation of functional load distributions within and between joints is an important, long-term issue. In particular, orthopaedic implant failure and irradiation of arthritis to other joints requires longitudinal studies on the kinetic level, over large numbers of patients. Once an estimation of *in-vivo* loading during functional activity has been made, the study of dynamic stress distributions within the bones and in the interface between bones and prostheses becomes feasible. Until now, stress analysis of biomechanical structures has been predominantly static and *in vitro*, using standard software packages developed in the domain of structural mechanics (Huiskes & Chao, 1983).

Other issues in orthopaedics include those of critical timing for surgery, pre/postoperative assessment, and even diagnostics - which is the most elusive of all applications. In particular the identification of pathological and compensatory effects on the kinetic level may render biomechanical analysis of human movement an efficient clinical tool. The notorious pathologies following professional athlete achievements may also be investigated in this way, and hopefully prevented.

In applications of a psycho-motor nature (rehabilitation, sports training), biomechanical analysis can provide essential information about causes and consequences in the generation of movement patterns, both as regards optimization in the direction of a given norm ('normal' gait, peak sports performance), and comparison of alternative norms (structural limitations in a damaged body, different high-performance sport styles); here again, the biomechanically oriented *feature extraction* should be followed by a suitable pattern recognition policy.

At the present time, rehabilitation of specific muscle groups through biofeedback methods is performed via EMG or kinematic measurements. Neither data source provides unequivocal information about the effective contribution by the muscle group under investigation, because of co-contractions, compensatory effects, and limited validity of dynamic EMG-to-force transformation models. It is surmised that kinetic models either by themselves or in combination with EMG data will provide a reliable information source.

Practical problems in all these applications include the large amount of developmental and computer processing effort required. Fortunately, both measurement equipment and suitable software packages are available. The validation of biomechanical models is an important aspect, in view of the healthy distrust on the part of clinicians of 'black-box' models of biological reality. However, collaboration between medical and engineering participants will allow the inclusion of anatomical information, and thereby change the model into a 'gray box' (Murray-Smith, 1982). Validation or refutation will follow from the explorative use of these models on large numbers of subjects, i.e., in the pattern recognition phase. In this fashion, the hypothetico-deductive conception of the research process outlined in the introduction to this chapter on Bernstein's methodology will find its proper realization.

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SECTION 2

Chapter II **THE PROBLEM OF THE INTERRELATION OF CO-ORDINATION AND LOCALIZATION**
N. Bernstein

Chapter IIa FREQUENCY ENCODING IN MOTOR SYSTEMS
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CHAPTER II

THE PROBLEM OF THE INTERRELATION OF CO-ORDINATION AND LOCALIZATION

N. Bernstein

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1. THE BASIC DIFFERENTIAL EQUATION OF MOVEMENTS

The relationship between movements and the innervational impulses which evoke them is extremely complex and is, moreover, by no means univocal. I have already undertaken an analysis of this relationship in a series of previous studies (8, 9, 14, 15) and for this reason I shall present here only a short summary of such statements as may be regarded as firmly established at the present time. The main object of this summary is to serve as an introduction to a further discussion.

The degree of tension of a muscle is a function, in the first place, of its innervational (tetanic and tonic) condition E, and, in the second place, of its length at a given instant and of the velocity with which this length changes over time. In an intact organism the length of a muscle is in its turn a function of the angle of articulation α; for this reason we may write that the momentum of a muscle with respect to the joint is

$$F = F\left(E,\ \alpha,\ \frac{d\alpha}{dt}\right). \tag{1}$$

On the other hand, we may assert that the angular acceleration of a limb controlled by a given muscle is directly proportional to the momentum of the muscle F and inversely proportional to the moment of inertia of the limb I. In this way

$$\frac{d^2\alpha}{dt^2} = \frac{F}{I}. \tag{2}$$

If there are other sources of force than the muscle operating on the limb, the situation is a little more complicated. Let us limit ourselves for simplicity to only one external force, namely gravity. In the simplest case which we have just described, where we are considering the movement of a single limb segment in relation to a second fixed one, the momentum due to gravity G is, like the momentum of the muscle, a function of the angle of articulation

$$G = G(\alpha). \tag{1a}$$

The angular acceleration of the limb segment under the influence of both momenta together is expressed by the equation

$$\frac{d^2\alpha}{dt^2} = \frac{F + G}{I}$$

If we introduce into this equation expressions (1) and (1a) for F and G, we obtain a relation of the following form:

$$I \frac{d^2\alpha}{dt^2} = F\left(E,\ \alpha,\ \frac{d\alpha}{dt}\right) + G(\alpha). \tag{3}$$

This is the fundamental equation for the movement of a single limb in a gravitational field under the influence of a single muscle where the level of innervation is E. In cases where the moving system consists not of one but of several limb segments and where we are obliged to take into consideration the activity of several muscles, eqn. (3) becomes extremely complicated, not only quantitatively but also qualitatively as considerations of the mechanical effect of one muscle upon others also enter into the problem and the moment of inertia of the system becomes a variable term. However, in spite of the fact that the complications which arise in this case are so great that equations of type (3) cannot always be written even in the most general form, the physiological aspects of the problem differ only slightly, and the complications essentially involve only the mathematical and mechanical aspects of movement. For this reason in the present context we may limit ourselves only to the consideration of the most simple equation (3).

The basic equation is a differential equation of the second order which may be integrated if the functions F and G are known. Solutions of an equation of this type, that is to say, the determination of the movement which will take place in each given case, will be different depending on the so-called initial conditions of integration: that is, the initial position of the limb segment determined by the angle α_0 and on its initial angular velocity $d\alpha_0/dt$. By altering these initial conditions in various ways we may obtain very different effects of movement from one and the same governing law (3), i.e. for the same functions F and G.

It must first of all be noted that eqn. (3) directly bears on the cyclical character of the relation between the momentum of the muscle F and the position of the limb α. The limb segment changes its position as a result of the operation upon it of the momentum of effort F and this momentum in its turn changes because of the changes in the angle α. A cyclical chain of cause and effect operates in this way.

This chain would be ideally cyclical if the momentum (eqn. (1)) depended solely on α and $\mathrm{d}\alpha/\mathrm{d}t$, that is, if the movement were completely passive (for example, the falling of the arm). But, as in eqns. (1) and (3) given in this report, the value of F also depends on the degree of excitation of the muscle E, which appears most clearly from the areas lying outside the circle which we have just described. It is apparent that there are two possibilities here; either the degree of excitation E depends wholly or partly on the values of α and of $\mathrm{d}\alpha/\mathrm{d}t$, or it is quite independent of them and is solely a function of time t.

The choice between the two possibilities indicated here is clearly of great physiological significance as may be revealed with sufficient clarity only by further discussion in this chapter. At the moment I shall only indicate some of the consequences of each of the hypotheses we have raised.

If the degree of excitation E is simply a function of position and velocity and not a function of time, then eqn. (3) will take the form of a classic differential equation,

$$I\,\frac{\mathrm{d}^2\alpha}{\mathrm{d}t^2} = F\left[E\left(\alpha, \frac{\mathrm{d}\alpha}{\mathrm{d}t}\right), \alpha, \frac{\mathrm{d}\alpha}{\mathrm{d}t}\right] + G(\alpha), \tag{3a}$$

the partial integrals of which depend only on the initial conditions. In this case, consequently, a movement must occur if the required initial conditions are fulfilled (from without), and once having begun it must proceed with the same uninterruptable regularity with which a s ring will oscillate if displaced to a precisely determined initial position and then released. It is clear that this hypothesis does not correspond to physiological reality and in effect completely ignores the role of the central nervous system.

On the other hand, it may be supposed that the degree of excitation E is a value which changes with time and depends entirely on a predetermined sequence of impulses from the central nervous system without any relation to the local conditions operating in the system of the moving limb being studied. If, as in the hypotheses formulated above for the elastic oscillation of a string, the muscle can be compared to some sort of independent spring or rubber band, then in the second hypothesis it may be represented as a sort of solenoid which attracts its core solely in relation to the potential of the current which is supplied to the coil from an external source. The law of the variation in this current must be represented in the system of eqn. (3) as a function of time; in fact, whatever may be the real causes of these changes, the changes themselves are presented to system (3) in a completely finished and independent form as quite unalterable data. Equation (3) in this case takes on the form

$$I\,\frac{d^2\alpha}{dt^2} = F\left[E(t),\ \alpha,\ \frac{d\alpha}{dt}\right] + G(\alpha), \tag{3b}$$

which does not permit of any concrete solution.

It is important here to draw attention to the following. In spite of the fact that the degree of excitation E, as has been hypothesized, is independent of α and of $d\alpha/dt$, the momentum of the muscle F is dependent on them as before. Meanwhile, as we have shown above, the operation of this momentum, that is, the entire picture of the course of a movement, will vary with the initial conditions which in no way enter into the expression for the degree of excitation E and consequently do not in any way affect the course of its changes in time. It follows from this that the general results of interactions from eqn. (3b) cannot be foreseen or regulated in advance because the changes in excitation will be involved in the interplay of forces and dependencies which can in no way alter the further course of these changes following a fully independent law. Movements which are regulated according to the law (3b) will necessarily be ataxic.

And so we are left with the hypothesis that the excitation of a muscle E must be both a function of time and a function of position and velocity, and must be described in eqn. (3) in the form

$$I\,\frac{d^2\alpha}{dt^2} = F\left[E\left(t,\ \alpha,\ \frac{d\alpha}{dt}\right),\ \alpha,\ \frac{d\alpha}{dt}\right] + G(\alpha). \tag{3c}$$

This purely analytical deduction of the functional structure of muscle excitation permits of exceptionally simple translation into physiological terms. The dependence of the variable E on time, proceeding from the absurdity of the opposite hypothesis (3a), underlies the necessity for the changes in excitation which are directly effected by the activity of the motor areas of the central nervous sytem. The dependence of the excitation on the position of the limb α and its angular velocity $d\alpha/dt$ is the proprioceptive reflex so well known in physiology. It necessarily follows from the preceding analysis that both position and velocity directly and independently influence the changes in the degree of excitation of the muscles, and in reality both these effects have been subjected to precise physiological investigation.

Turning to clinical evidence we may say that (3a) is the equation of movement for an extremity in a case of central paralysis and that (3b) represents the equation of movement in a case of proprioceptive ataxia.

In this way we have stated in the basic equations of movement a superposition of two cyclical connections of different orders and related to different topics. The first cyclical connection is the mutual interaction of the position α and the momentum F, and exists purely mechanically as has been pointed out above. The second connection constructed on the first one, is a similar interaction between the position α (and also of the velocity) and the degree of excitation E; this connection is effected by means of systems of reflexes and is related to the activity of the central nervous system.

The principal significance of the general conclusions examined above may easily be deduced. The customary older representation implicitly accepted and, until the present, retained by many physiologists and clinicians, describes the skeletal link as being completely passive under the control of the central impulses and as being unequivocally subservient to these impulses. In this scheme the central impulse a always produces movement A, and impulse b always produces movement B, from which it is easy to proceed to a representation of the motor area of the cortex as a distribution panel with push-buttons. However, eqn. (3b) indicates that one and the same impulse $E(t)$ (ignoring the periphery) may produce completely different effects because of the interplay of external forces and because of variations in the initial conditions. Equation (3c) shows, on the other hand, that a determinate effect is possible for a movement only in a case where the central impulse E is very different under different conditions, being a function of the positions and the velocities of the limbs and operating very differently in the differential equation with various initial conditions. Parodying the well known tag on nature we may say that *motus parendo vincitur* (movement is conquered by obedience).

It must be pointed out, finally, that the external force field does not consist of the force of gravity $G(\alpha)$ alone and it may even occur that this latter expression does not enter at all into the basic equation in such a simple form. Because it necessarily affects the position and the velocity of a system, and because in the norm these latter affect the changes in E, we may say that the *parendo* of the central impulses must sometimes go to

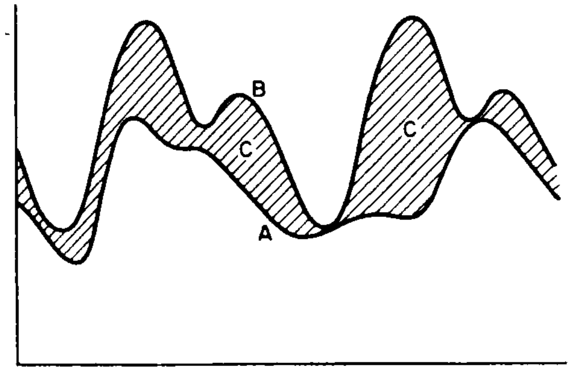

Fig. 14. A semi-schematic representation of the course of a single central impulse during a rhythmical movement. *A*, the non-rhythmical curve of changes in external forces; *B*, the summed rhythmical result; *C* (Hatched area), an impulse bridging the gap between the curve *A* and the result *B*.

great lengths. It is obliged to adapt to all internal and external forces operating in the system while forces which do not directly depend on the operation of the impulse *E* may frequently play a decisive part in the general balance of forces affecting movement. In such cases (Fig. 14), if for a given movement the required changes of forces at the joint are represented by the curve *B* and the resultant forces in the external field are represented by the curve *A*, then the central nervous system will be obliged to provide only the additional fraction *C* so that the sequence of impulses cannot maintain even a remote correspondence to the contours of curve *B* and frequently even less correspondence with this curve than to the changes in the external field *A*. These supplementary compensatory impulses sometimes appear in an indirect way from cyclogrametric observation.

And so, not to enter into details discussed in the studies mentioned above, it may be said that:

(a) a unequivocal relationship between impulses and movements does not and cannot exist;

(b) the relation between impulses and movement is the further removed from unequivocality the more complex is the kinematic chain operating in the movement under consideration;

(c) movements are possible only under conditions of the most accurate and uninterrupted agreement - *unforeseen in advance* - between the central impulses and the events occurring at the periphery, and are frequently quantitatively less dependent on these central impulses than on the external force field.

2. THE INTEGRITY AND STRUCTURAL COMPLEXITY OF LIVE MOVEMENTS

In the treasury of experimental physiology there are vast collections of experimental observations and facts characterizing the course of single impulses or of the more simple patterns of impulses. There are most detailed studies of all aspects of excitation, inhibition, *parabiosis*, *chronaxie*, and so on, for a single nerve pathway. However, we have up to the present only two major advances along the lines of investigation of the total or systemic operation of impulses: Sherrington's principle of reciprocity and A. Ukhtomskii's principle of dominance. But even these groups of data are very far removed from the areas we touch upon - the problems of the study of structures of movements as integral formations.

It is, however, precisely this integration of movements that is the most important feature implied by 'motor co-ordination'. The fact of this integration may be investigated in many experimental situations and significant connections and correlations are observed in all these cases between the various components of the integrated processes. The simplest and most easily observed phenomenon in this category is the appearance of gradual and smooth redistribution of tensions in muscular masses, which is particularly clearly expressed in cases of phylogenetically ancient or highly automatized movements. A muscle never enters into a complete movement as an isolated element. Neither the active raising of tension nor the concomitant (reciprocal) inhibition in antagonistic[*] subgroups is, in the norm, concentrated in a single anatomical muscular entity; rather, there is a gradual and even flow from one system to others. I shall suggest a short experiment; stretch the arm out anterolaterally and describe a great circle with the hand as shown in Fig. 15, and then find out by means of anatomical analysis how the change in muscular innervation and the process of inhibition of the antagonists are accomplished during this movement. Exactly the same process of gradual

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[*] The concept of antagonism may be applied unconditionally only to cases of muscles operating on joints with a single axis and, further, to those which cross only this one joint. The number of muscles of this type is extremely small; in the skeletal extremities we find as examples of this type only m. brachii and internus, m. pronator quadratus, the short position of m. triceps brachii and m. vastus femoris. All other muscles may be only functionally antagonistic in a single situation and in quite different relationships in other situations.

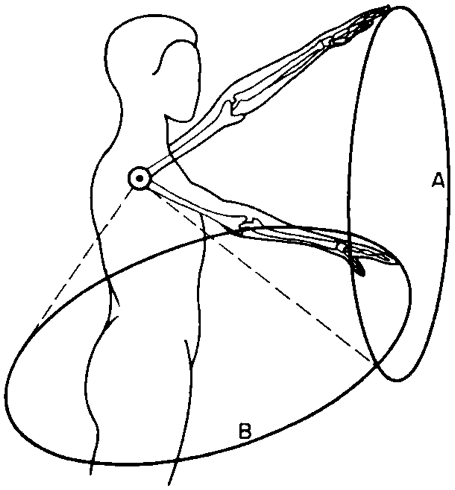

Fig. 15. Circular movements made with the arm extended in various positions are accomplished by completely different innervational schemes for trajectories of the same type.

transfer of innervation may be clearly observed in any plastic movements; with more accurate means of registration this may be observed as a general phenomenon.

The integration discussed above is even more clearly demonstrated in the analysis of automatic rhythmical movements by the appearance of extremely marked reciprocities in the interrelationships of the components. I have described examples of such reciprocity in other studies (9, 19); I shall here merely refer to the fact that in rhythmical work with a hammer the direction of the trajectory of the elbow (forwards or to the side) gives a close correlation with such phenomena apparently far removed from the elbow as the relationship of the maximal velocities of the head of the hammer in the movements of raising and striking, the angle of inclination of the hammer to the horizontal in raising, the relationship between the length of the trajectories of the hammer and of the wrist, and so on. It seems to me that the effects of changes in tempo and the whole construction of a movement are relevant here, as I have discussed in other studies (14, 19) on striking piano keys and on locomotion. A movement never responds to detailed changes by a change in its detail; it responds as a whole to changes in each small part, such changes being particularly prominent in phases and details sometimes considerably distant both spatially and temporally from those initially encountered.

Finally, it is necessary to comment on a fact to which I have drawn attention in a number of places (14, 15, 19, 21); that the course of rhythmical live movements may be presented in the form of rapidly converging trigonometric series. I have been able to demonstrate that a diversity of rhythmical human movements (walking, striking with a hammer, filing, piano-playing, etc.) may be interpreted to an accuracy of within a few millimetres in the form of a sum of three or four harmonic oscillations, the so-called Fourier trigonometrical sums:

$$r = A_0 + A_1 \sin \frac{2\pi}{T} (t + \theta^1) + A_2 \sin \frac{4\pi}{T} (t + \theta^2)$$
$$+ A_3 \sin \frac{6\pi}{T} (t + \theta^3) + \ldots \qquad (4)$$

The rapidity of convergence of these sums may be seen from the numerical examples of Table 7.

TABLE 7.

Walking	Absolute amplitudes				Relative amplitudes (A_1 = 100%)		
	A_1cm	A_2cm	A_3cm	A_4cm	A_2%	A_3%	A_4%
Longitudinal displacement of the point of the foot	38.50	9.09	0.80	0.67	23.6	2.08	1.74
Longitudinal displacement of the centre of gravity of the whole arm	7.60	0.81	0.15	0.07	10.65	1.98	0.92
Longitudinal displacement of the center of gravity of the whole leg	14.47	1.22	0.49	0.22	8.42	3.39	1.52

The fact that such an interpretation is possible is of great importance to the question under discussion. If one complete cycle of a movement lasts for 1 sec, and in this case may be represented with an accuracy of within 1-3 mm as the sum of three sinusoids, this means that all the details of this movement must have been organized with the required degree of accuracy a full second beforehand. Further, its period being known, the sinusoid is determined by two parameters, that is, it can be determined from two points.

The sum of four sinusoids may thus be theoretically determined from eight points; in other words, it is possible to reconstruct from a small section of a movement of the type which we have represented, to within a fraction of one per cent in the form of the sum of four sinusoids, the entire movement as a whole with the same order of accuracy. This experimental fact is evidence in its most cogent form of the organizational interaction and mutual reciprocity of rhythmical movements in time while the mutual interdependence between the elements of the movement which I studied (of striking with a hammer) suggests a similar interaction in terms of spatial components.

If the external expression of co-ordinational activity provides a picture of such a high degree of reciprocity and interrelatedness, then, on the other hand, its anatomical structure in terms of our present knowledge also displays a picture of no less highly organized complexity. The extreme variety of clinical studies of damage to the motor area suggests that a large number of different subordinate and variously interacting systems co-operate in order to make possible a movement in its entirety. The spinal system alone contains up to five independent centrifugal pathways (pyramidal, rubrospinal, vestibulospinal and two tectospinal pathways). In the cortical region we have a very large number of centres which in one way or another appear (most often from pathological evidence) to be necessary components in a complete movement. All attempts to describe their activity in the norm are necessarily limited at the present time to very general statements and hypothetical descriptions, but there is no doubt whatever of their synthetic activity. For example, the attempts of Bianchi, Brown, Dupré, Foerster (44), Goldstein, Gurevitch, Homberger, Jacob, Lashley, Lewy (52), Magnus (55), v. Monakow (59), and many other investigators to describe in one way or another the functional interactions of various parts of the cortex must be mentioned here with recognition of the great service these authors have rendered to the physiology of movement*. A brief summary of what these authors have uncovered in areas directly bearing on the objectives of this report would be approximately as follows. An impulse reaching the terminal plates in a muscle

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*Bianchi, *The Mechanism of the Brain*, etc., Edinburgh, 1922; T.G. Brown, *J. Physiol.* 10, 103; Dupré, *Revue neurol.* 1909, p. 1073, 1910, No. 13; K. Goldstein, *Deutsche Ztschr. Nervenheilk.* 70, P, p. 7; M. Gurevitch, *Ztschr. ges. Neurol. Psych.* 93, 1924; *Ibid.* 108, 1927; Homberger, *Ztschr. ges. Neurol. Psych.* 85, and *Arch. f. Psychiatrie* 69; Jacob, *Ztschr. ges. Neurol. Psych.* 89; K.S. Lashley, *Brain* 41, 255; *et al.*

from the centrifugal fibre of the last neuron is the resultant of a whole series of separate central impulses which reach the synapses of the anterior horn by different pathways. Among these latter we must recognize the significant innervational independence of pyramidal impulses (the cortico-spinal tract) and the combined impulses from the striopallidal groups of nuclei (c. striatum-gl. pallidus-nucleus ruber-tr. rubro spinalis) which are found in close co-operation with centres whose functional relationships are less apparent (substantia nigra, Dark-schewitsch's nucleus, corpus Luysi, and so on). Centripetal proprioceptive impulses give rise to answering effector impulses from the cerebellum and from other many stations related to the spinal cord through the quadrigeminal system. Finally, the decisive role in the production of a movement must be referred not to a centrifugal but to a central-informational system (an older physiology would have termed this 'commissural-associative') interplaying along the lines of the frontal pontocereballar pathways. It would be possible to list the general characteristics of the functional peculiarities of each of these anatomical stations (often the details given by different authors are contradictory), but this is not my aim at present. It is important here to point out a single peculiarity which is common to all these characteristics and which has been stated as un undisputed fact for the last 20 years.

All the clinical observations noted above, as well as those of many other authors, agree on the position (quite foreign to the ideas of physiologists of the last century) that these central nervous subsystems have one and the same object of excitation at the periphery - the same muscles and most probably the same peripheral conducting pathways. The idiosyncracies and differences in the operations of the pyramidal, striopallidal, cerebellar and other systems lie not in differences and peculiarities in the peripheral objectives on which they operate but solely in differences in the forms of influence exercised on these objectives. The pallidum is concerned with the same musculature as is the brain cortex; it is not the objective but the manner of excitation which is specific. None of the data from contemporary physiological investigations contradicts the reliability of the fact that, for example, both flexion and extension in any single-axis joint can be achieved through both pyramidal and the striopallidal systems; both these systems may and do give the effect of reciprocity. In 'gross pathology' this is carried out separately and in the healthy norm both systems in some way co-operate in a rhythmical process. As accounts of the way in which this co-operation may be effected we have in the literature many observations, impressions, and deliberations which are often quite persuasive and in many cases not contradictory. What is common to all these descriptions is not in general important; what is important, with the object of a formal examination of material, is the general tendency found in all of them - the recognition of the common presence in all cases of the qualitatively peculiar operation of central subsystems on one and the same peripheral objects.

Closely related to these considerations is the currently established picture of the multiplicity of projections of peripheral organs in the central nervous system. Along the lines of the exceptionally detailed knowledge of cortical projection of the motor periphery, which was already under investigation in the 19th century, and which has been worked out in very great detail in our time (Fig. 16), we are now obliged to hypothesize localized projections of the same periphery both on the globus pallidus and on the cerebelli vermis. It is immaterial to our purpose in what way this multiplicity has occurred as a result of phylogenetic stratification and superimposition. In the human being, it is presented to us as given data, a given problem, and our task lies in the search for non-contradictory explanations of the mechanisms of a multistaged functioning of this sort.

If we attempt an examination from the point of view of such multiplicity of the equations for elementary movements (3c) which we have deduced above, we necessarily arrive at the following. A nerve impulse E, which, as appears on close examination of peripheral processes, is not related in a univocal way to its consequence - a movement - and is therefore restricted to the most precise concordance between its evolution and the proprioceptive input for α and $d\alpha/dt$, is at the same time the sum of (or is in some other form of linked equivalence to) a series of impulses which have very different points of origin in the brain. Each of these impulses arises in a separate area in the brain that is distinct from other centres. Each of these centres has its particular interrelationships with other centres in the brain, its own conducting pathways, a particular degree of relationship with and form of connections to the receptors, and, finally, as clinical practice in nervous disorders shows, its own mode of operation in time and its own particular means of interaction. The difficulty of co-ordinating all these facts is very great indeed as I shall attempt to demonstrate. If the impulse E were to follow the pattern in eqn. (3b), that is to say if it had the form $E(t)$, it would not in principle be difficult to represent a series of independent sources (a very high degree of agreement between them being

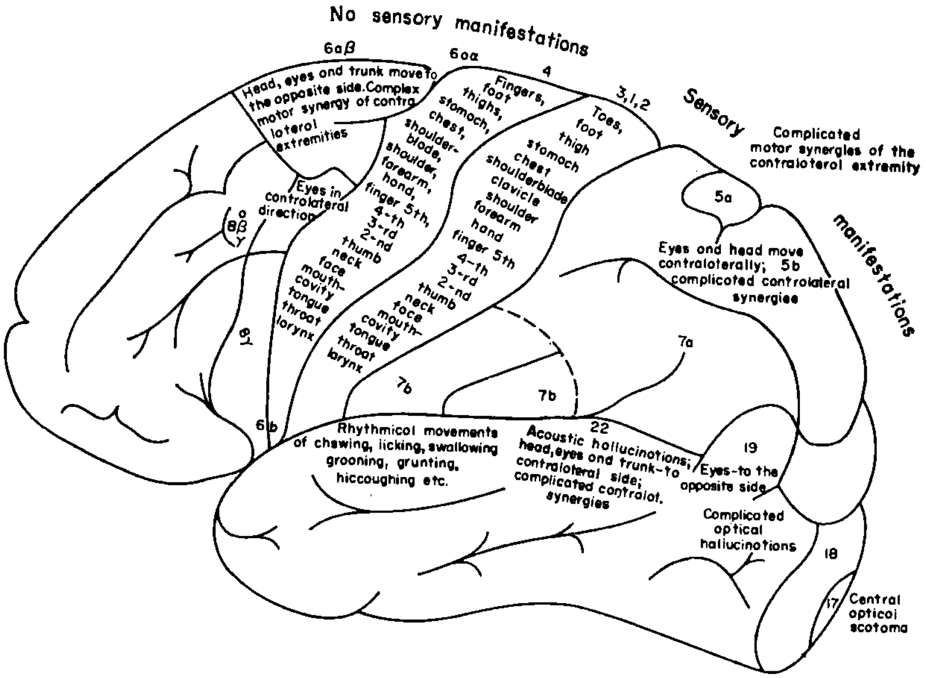

Fig. 16. Summary of data given by O. Foerster on results of stimulation of various points of the human cortical hemisphere (1926).

implied) which would ultimately unite in a single common path of a motor nerve and transmit the result of their common action $E(t)$ through it to the end plate. In exactly the same way it would not be difficult to conceive of the possibility of multiple action of impulses in a case where the actual form was (3c), that is, $E(t,\ \alpha,\ d\alpha/dt)$, if each of the separate impulses had its own particular object of excitation at the periphery (in the form, for example, of a particular muscle which was controlled by it alone). If we suppose, for example, that control of a flexor group at a joint is concentrated in the centre A and control of the extensor group of the same joint in centre B while the proprioceptive connections secure the possibility of either centre being able to react to α and to $d\alpha/dt$, then the mechanism in this case would be merely quantitatively and not qualitatively difficult to comprehend. The actual situation - that is, a system of impulses without unequivocal correspondence to the movement, and controlled by proprioception, being sent to the object from a number of sources - is one which does not permit us any simple escape from the question. The sole (apparent) possibility of explanation for a structure of this type lies in referring the proprioceptive "perception" to only one of the effector centres of the brain, for example, to the cerebellum, and supposing that the other effectors function purely according to type $E(t)$. The mathematical expression of such a structure might be regarded in the following way: the summed impulse E is made up of a series of centrel impulses E_1, E_2, E_3,...

$$E\left(t,\ \alpha,\ \frac{d\alpha}{dt}\right) = E_1(t) + E_2(t) + \ldots + E_n\left(\alpha,\ \frac{d\alpha}{dt}\right). \tag{5}$$

However, this combination appears to be unacceptable for a number of reasons. In the first place, we have no guarantee whatever that E is the sum of E_1, E_2, ..., E_n, and not some other function of them such as would considerably alter the case and complicate for the co-ordination centre E_n the possibility of suppressing the "blind" impulses E_1, E_2 with precisely proportinal additions. In the second place, the centripetal impulses are directed in the spinal cord not through one channel, but through a large number of channels, and reach by direct pathways at least two central nuclei, cerebellar and thalamic, attaining a further series of areas by indirect pathways; but the problem lies precisely in the interpretation of the possibility of proprioceptive multiplicity. To visualize this is about as difficult as to imagine the movement of a two-seater bicycle each seat of which is equipped with its own separate set of handle-bars. It is clear that the presence of two effector centres responding to proprioceptive input requires the closest possible co-ordination between them. I would deny neither the possibility that such connections may exist nor that they exist in fact. My aim in the first two sections of this chapter is merely to indicate the great difficulties which confront functional explanations of the co-ordination of movements. It is already apparent that eqn. (3c) is quite different from our usual, qualitatively simple models of the interaction between the centre and the periphery; when, however, we are obliged to confront their complex interaction as a result of the mutual activity of entire systems of organs which, anatomically and clinically, display varying degrees of independence, then the resulting great structural complexity becomes more obvious still. Yet this is fruitful, since a failure to realize the difficulty of a problem frequently defers the moment of its solution.

3. THE INTERRELATIONSHIP BETWEEN CO-ORDINATION AND LOCALIZATION

The discussion in the preceding sections has already largely revealed the close connection between problems of co-ordination and localization. It is clear from all that has been said above that no nuance of a single impulse $[E(t)]$ can serve as an explanation of even the simplest case of repeated accomplishment of automatizing movements, and still less as an explanation of the involved complexity of natural movements carried out by many muscles, each of which involves control from many centres. To digress: at the beginning from the indubitable presence of functional "inter-departmental" connections between brain centres which organize these latter into hierarchical order (*Über- und Unterordnung*), the following innervational scheme for effector impulses will be obtained (Fig. 17). It is clear that co-ordination is determined not so much by differences in the effect of each of the impulses *Aa*, *Ab*, ..., *Ba*, *Bb*... taken separately, but also by the systematic modes of their common operation and joint effect. The term co-ordination hints at the common actions of separate elements. The solution to the problem of co-ordination lies not in analysis of the tonal and expressive resources of a single instrument in an orchestra but in the technical construction of the score and in the mastery of the conductor.

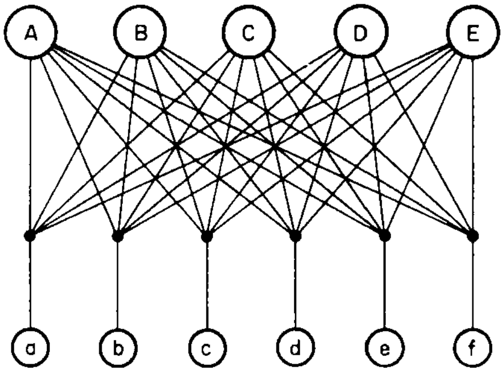

Fig. 17. The multiplicity of efferent pathways for the control of six muscles *a*, *b*, *c*, *d*, *e*, *f*, by five effector centres *A*, *B*, *C*, *D*, *E*, gives, even in this intentionally simplified case, a complex structural scheme of innervation.

The basic guiding thesis for investigations of co-ordination must for this reason be formulated in the following way. Co-ordination is an activity which guanrantees that a movement shall have the homogeneity, integration and structural unity which has been described above. This activity is principally based not on particular processes in individual neurons, but on the determinate *organization* of their common activity. This organization must necessarily be reflected in the anatomical plan in the form of localization.

This seems to me to be an extremely expedient way of formulating the question. On the one hand, organization and the forms in which it exists must inevitably be represented in the structural forms of localization. In the same way as we may derive from an examination of a diagram of an electrical circuit some idea as to the nature of its function, so data of the localization-anatomical type may serve at least as circumstantial evidence in the consideration of the new experimental problems I have put forward - *the structural physiology* of movement. On the other hand, such a structural analysis of movements should aid considerably in critical evaluation of existing and future conceptualizations of the type and structure of cerebral localization. It is impossible to visualize a situation in which localizational structure would be found to contradict structural organization.

A pertinent point must be made here. One must not in any way confuse localization with topography. Topography is the geography of the brain, the study of the spatial distribution of its functionally existing points. Localization is the structural plan of anatomical interrelationships between these functional points. If we shuffle in Fig. 17 the positions of the centres *A*, *B*, *C*, *D* and *E*, this will change the entire topographical picture, but will not alter their localizational structure. The distribution (topography) of the elements in a diagram of a receiving set are completely different in the diagram from the topography of these elements in an actual apparatus constructed from this diagram. On the other hand, for one and the same topography completely different schemes are possible.

Figure 18 makes the latter clear in regard to circuit-diagrams which are extremely convenient for illustrating our problems; diagrams (a) and (b) in this figure have exactly the same structure for different topographies, schemes (b) and (c) have the same topography but different structures. The problem which obsessed our physiological forefathers, that of the inversion of the retinal image, and in particular whether this inversion is transmitted in exactly the same way to the cortex, and if so, how it is compensated, appears to us now to be childishly simple-minded. We still remember how some of their contemporaries hypothesized, to explain the matter, that the soul was located in the brain with its feet uppermost, without however determining more precisely whether souls have feet. Nowadays we hypothesize with great facility much more complex transpositions of elements in the representation of the retina on the cortex without experiencing structural difficulties from this fact; indeed, in a central telephone station, for example, we do not have to worry whether the commutator links for subscribers from the northern and southern parts of a town are located respectively at the northern and southern ends of the switchboard. However, this old question permits of new and less childish formulations; are there, nevertheless, limits to this type of transposition? And if such limits exist, what are the borders which separate transpositions that do not change structure from transpositions which inevitably destroy structure? An analysis of the problem when framed in this way shows us first of all that different structural schemes may show different degrees of tolerance to transpositions, but this aspect of the problem will be developed a little later.

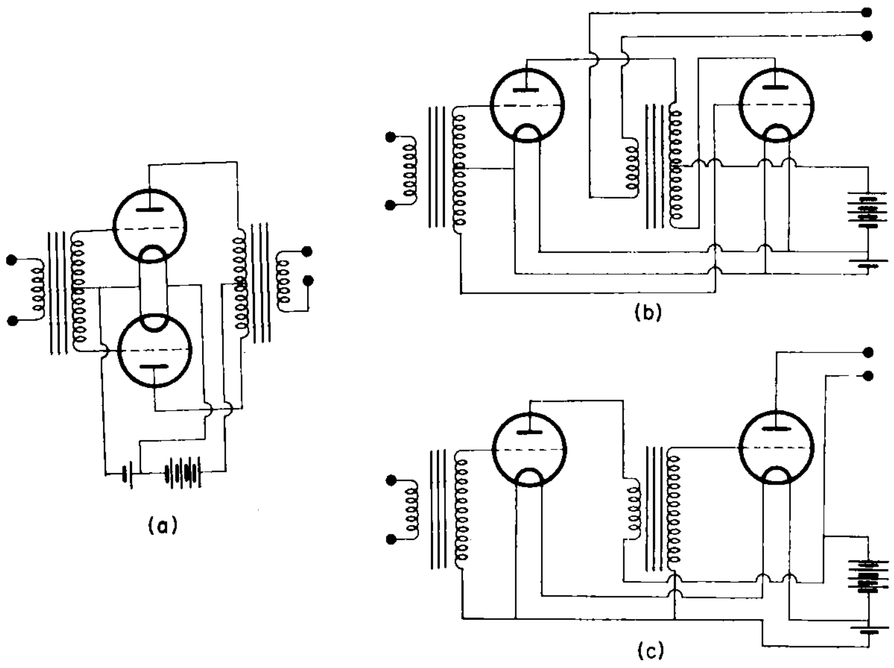
(a)
(b)
(c)

Fig. 18. Three diagrams for two-tube audio-frequency amplifiers. Diagrams (a) and (b) (a single-cascade push-pull block) are identical in all details; that of (c) (a double-cascade amplifier with transformers) is very different in principle from the other systems.

Thus, in the problem of localization what is important for our purpose is not precisely where in the cortex one or another peripheral object or function is reflected, but *what* is represented, and *how*; and what are the distinguishing characteristics of those objects represented in the cortical hemispheres and in subcortical centres. Topographical problems are for the most part clearly unrelated to the analysis of the co-ordinational structure of movements, while the problems of localization are of paramount and principal significance.

This significance may be very well explained from the example of the old conception of localization which has already been mentioned in Section 1. This conception would answer perfectly to reality if every central impulse unconditionally governed a single determinate movement, that is, if there existed a one-to-one correspondence between impulses and movements. In this case the effector impulses would be able to operate purely as a function of time $E(t)$, giving always one and the same effect independently of what occurred at the periphery; the push-button control-board model of the cortex, similar in plan to an organ keyboard, would be suggested in the types of explanation we employed. But, on the contrary, this type of one-to-one correspondence does not exist and the cerebral motor area organizes responses by deftly adjusting and balancing between resultant external forces and the manifestations of inertia, constantly reacting to proprioceptive signals and simultaneously integrating impulses from separate central subsystems, so that ten successive repetitions of the same movement demand ten successive impulses all different from each other; and the presence in the cortex of localizational equipment of the Hitzig or Foerster type begins to seem a very dubious interpretation. I would like to recall here the failure in 1923 of the invention of "a symphony of whistles". An attempt to convert steam whistles into a musical instrument with an organ keyboard failed because any given whistle could not be relied upon to sound the same on every occasion, and its pitch would vary with the pressure of steam, with the number of whistles sounded simultaneously, with the degree to which the steam-channel was clear, and so on, so that it was impossible to obtain a one-to-one correspondence between the keyboard, on one hand, and the frequencies of the tones obtained, on the other.

It is understood that a statement of complexity, of "impractivality" from our point of view, is not in any sense a decisive argument for the acceptance or rejection of any physiological hypothesis. There is no reason to suppose that physiological structure should be maximally rotational from our technico-social anthropomorphic point of view. The localizational structure of the cortex according to Foerster's scheme (Fig. 16) does not directly contradict eqn. (3c) with its proprioceptive cycle and lack of simple one-to-one relationships; it only makes the problem of functioning of its cell centres extremely difficult. The decisive argument against the theory of direct representation of muscular systems on the cortex comes from quite another, perhaps unexpected, direction. I present this argument in its most general formulation below; here I shall employ only one of its partial modifications as applied to a particular case.

Let us suppose that the cells of the gyrus centralis are in reality the effector centre for the muscles. Let us further suppose that the activity of these cells must be (as is inevitable in the given hypothesis) sharply different from instant to instant on the multiple repetition of a given movement, in relation to changes in the external force field and proprioceptive signals. If we suppose for clarity that we may represent each excited effector cell in the cortex as lighting up like an electric bulb at the moment when its impulse is transmitted to the periphery, then under such an arrangement the effecting of every movement will be visible to us on the surface of the cortex as a zig-zag discharge. The absence of one-to-one correspondence and all the considerations which have been described above as consequences of eqn. (3c) will be obvious in this case because on every repetition of a given movement the zig-zag discharge will be visibly different. Now suppose that this repetitive movement is an automatized act, the realization of a habit of movement, in other words, a conditioned motor reflex. From the discussion above it follows as an inescapable deduction that the conditioned reflex of movement operates each time through a new zig-zag - through new cells; in other words, we arrive at the conclusion that the hypothesis of cellular localization of muscles necessarily leads to a denial of cellular localization of conditioned reflexes. One of the two chess pieces must here be taken, and it is here a very pertinent question which of the two the old-fashioned localizationalist would rather sacrifice.

I do not for a moment imagine that I can overthrow the old localizational concept at a single blow, but it is not possible to disguise the fact that it is already threatened in very serious ways. The experiments of Bethe (30) and of Trendelenburg on the extirpation of cortical tissue in monkeys has indicated the possibility of far reaching compensations and, moreover, the extensive investigations carried out by Lashley, experimenting on rats and observing the appearance of compensation and of the re-establishment of conditioned reflexes for the most varied and topographically different sites of extirpation, and very persuasive evidence in favour of a radical re-examination of the old conceptions. Lashley could not discover any clear relationship between the topographical loci of the areas he destroyed and the degree to which the conditioned reflexes could be re-established; he found, on the contrary, that there is a strong correlation between the time required for re-establishment and the quantity of brain substance removed, without reference to its locus. These results cause him to favour the theory that there is no cortico-cellular individuality of operation, in which, it seems to me, he is quite mistaken. His data are extremely dangerous for the old localizational theory, but they far from disprove the possibility of any form of localization in general terms.

Lashley's error sets off very well the opposite error made by Gall in his time. Nobody now believes that phrenology was doomed to failure because the very principle of cortical localization was found to be defective. Nobody ascribes its downfall to the fact that Gall did not localize avarice or ambition to areas in which they were located in reality. Gall's theory was essentially faulty not because of the topography he assigned but because of the principles of selection underlying those categories for which he thought that he could find discrete localizations in the cortex. The categories suggested by Fritsch, Hitzig, Foerster, and others appeared to be more physiological and nearer to reality than Gall's fantasies, which were impregnated with the moral rationalism of the 18th century, and appeared, as it were, to be the next approximations to the discovery of reality. The evidence which has accumulated against these Foersterian categories up to the present time must inevitably lead to their abandonment, but this does not yet threaten the fall of the principle of localization in general. It should be recalled that immediately after the abandonment of phrenology the idea of localization also appeared for a long while to be compromised until it gradually became appararent that it was possible that the baby had been thrown out with the bath water. Now, again, after the development and establishment of the understanding of conditioned reflexes, to deny the structural anatomically engraved specificity of the brain would amount to an affirmation that its nature is absolutely beyond knowledge.

Our experimental aim at present lies in the correct formulation of categories which are really represented in the brain centres. The key to this search for the true categories clearly must lie in structural analysis: of the recepter moment, as it appears in experiments with conditioned reflexes; and of the effector moment, as it appears in the co-ordination of movements.

4. ECPHORIA OF THE ENGRAMS OF MOVEMENT

So far I have touched in this report only on those phenomena which point to the momentary, extensively structured nature of the co-ordination of movements. It appeared important to me to demonstrate that a movement could not be understood in terms of some nuance in operation of a single impulse, but that it is the result of the simultaneous co-operative operation of whole systems of impulses, while the structure of this system - its structural schema - is important for the understanding of the result. It is only a short step from this to the central argument in this report, that the innervation and localization of this structure is in reality not only not contradictory to the observable structure of the movements of the organism but is necessarily an exact representation of the latter. To proceed further it is now necessary to attend to another side of the phenomenon, that is, its duration in time. It is necessary to elucidate experimentally whether a simple parallelism exists between the duration in time of a series of system-related impulses or whether there also exists on the co-ordinational time axis the same mutual structural interdependence as has been described above for every separate moment of force.

This formulation of the question may be clarified by the following illustration. In order to achieve a given co-ordination at a given moment, we have Schema I (for example that illustrated in Fig. 18). Is it possible to regard all co-ordinations over all possible durations of time as uninterrupted functionings of Schema I, or do they exist, and may be regarded as a sequence of changes of Schema I to some other schema, qualitatively different from it (Schema II, and then to Schema III, Schema IV, and so on), while the law of the transition between the schemas and their order of transition, in its turn, has its own determinate structural features? Our factual data on this problem are so far extremely scanty but some observations may still be made.

Firstly, we must turn to the facts described above of the homogeneity of a movement and its unity in terms of the interrelations of its parts in space and in time. Having established our model for a rhythmical movement in the form of a three-four term trigonometric series of the type of eqn. (4), it is possible to prove beyond doubt that this homogeneity also exists in time, and that this particular homogeneity is indeed not peripheral or mechanical but certainly originates in the operation of the central nervous system. This demonstrates that there exist in the central nervous system exact formulae of movement (*Bewegungsformeln*) or their engrams, and that these formulae or engrams contain in some form of brain trace the whole process of the movement in its entire course in time. We may affirm that at the moment when the movement began there was already in existence in the central nervous system a whole collection of engrams which were necessary for the movement to be carried on to its conclusion. The existence of such engrams is proved, however, by the very fact of the existence of habits of movements and of automatized movements.

A problem of considerable structural significance now arises. Let us suppose that to a given co-ordinated movement there correspond, in the brain n engrams by means of which it is ensured that the movement will take place with successive ecphoria in a determinate time sequence and with determinate tempo and rhythm. All these n engrams exist in the central nervous system at any given moment as the habit of movement exists, but they exist in a hidden, latent form. How are we to explain the facts that, firstly, they do not all undergo ecphoria simultaneously but in sequence, secondly, they do not lose their order of ecphoria, and thirdly, they observe determinate time intervals between ecphoria (tempo) and quantitative relationships in their duration (rhythm)? There are here two basic possibilities, two "temporal structures"; either (a) each successive ecphoria of the engram (or perhaps a proprioceptive signal of its effect at the periphery) serves as an ecphorator for the next engram in order; or (b) the mechanism for ecphoria, the ecphorator, lies outside the engrams themselves and directs their order by a hierarchic principle of *Überordnung*. The first hypothesis may be called the "chain" hypothesis, the second the "comb" hypothesis (Fig. 19).

Very weighty considerations may be found to support both these hypotheses. The chain hypothesis brings to the fore proprioceptive moment, and in this connection it explains independently and satisfactorily the observation of tempo and rhythm referring them to a regular synchrony with events occurring at the periphery. Because, on this hypothesis, the stimulus for the arousal of each successive ecphoria is the existence of the preceding one, it is possible to explain both the maintenance of an order of succession and the impossibility of separate links being left out of a succession of ecphoria in this way. Finally, the hypothesis recommends itself by its simplicity, and by the fact that it is unnecessary to postulate any particular structure for the ecphorator.

The arguments in favour of the comb hypothesis are no less cogent. The presence in the C.N.S. of "the plan of a movement", the homogeneity of its

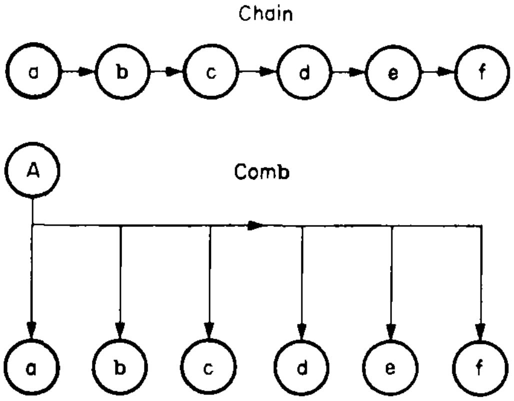

Fig. 19. Two theoretically possible schemes for successive ecphoria of motor engrams a, b, c, d, e and f.

formula and the homogeneity of the movement itself and of its course from beginning to end does not answer to the hypothesis that a movement is fractioned in this way, or that there is no guiding principle of succession among elements of the chain type other than events at the periphery. We do not see in this case any sign of a guiding principle unifying the whole. Further, if we recall the facts discussed above which indicate that the central impulses merely adjust to, and compensate for, the external force field so that the pattern of the impulses over time may have very little in common with the picture of the movement, the comb hypothesis finds a new and important reinforcement. At the same time, a glance at Fig. 14 shows that the central impulse C, which is represented by a hatched area (and is entirely unlike the summed rhythmical equilibrium B which is finally achieved because of the presence of external disturbing forces A), shows a succession of elements which in no case resemble each other. The possibility of obtaining the homogeneity and regularity of B on every repetition, agreeing at the periphery with the law expressed in eqn. (4), necessarily requires the existence in the C.N.S. of some type of guiding engram which encompasses the entire law of succession of B. If a guiding engram of this type exists (we may refer to it as the motor image of a movement) it must have a dual nature; it must contain within itself, in some way uniquely and simultaneously existing like an embryo in an egg or track on a gramophone record, the entire scheme of the movement as it is expanded in time. It must also guarantee the order and the rhythm of the realization of this scheme; that is to say the gramophone record mentioned above must have some sort of motor to turn it. To pursue this metaphor, what I have called the gramophone record is the directing engram, and what I have compared to a gramophone motor is the ecphorator*.

Both of the hypotheses which we have examined are completely bound up with the structure of the peripheral impulse which we have deduced above in terms of eqn. (3c), that is, its dependence on the form $E(t, \alpha, d\alpha/dt)$, but they only illuminate this dependence in different ways. On the chain hypothesis the critical agents which determine the development of the process are the dependence of E on α and $d\alpha/dt$, that is, their proprioceptive relationships, while the temporal moment of the relationship to t is determined in this case only by the tempo and by the maintenance of each individual element of the chain a, b, c (Fig. 19). In the comb hypothesis, on the other hand, the dominant relationship is $E(t)$, that is, the independent initiative and the regulating activity of the C.N.S., and proprioceptive effects merely play the role of correctors to the general whole.

It must not be forgotten that the hypothesis of the necessity for an effector mechanism which is distinct from the engrams themselves and is in some sense dominant over them, is not necessarily related to the comb hypothesis but is necessary in equal degree for both hypotheses. Whatever we may ascribe to the regulating engram in the comb hypothesis and to the elementary engrams a, b, c, ..., in the chain hypothesis they are all alike bound to contain in latent form the impulse E in dependence not only on α and $d\alpha/dt$ but also on t. It makes no difference whether the central mechanism of tempo - this "gramophone motor" - is related in its action to the duration of the physico-chemical intercellular reactions or to some other physiological rhythms;** they must in any case exist as some functions which differ from the collection of engrams which they activate, because real time cannot be incorporated in the latter. A decision between alternatives in favour of one or other of

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* It is interesting to note here that the question which I have raised of the ecphoria of movements in a chain system or a comb system is a repetition in new terms in the area of the physiology of movement of the ancient psychological dispute of association (Bleuler, Adler) versus action (Berze) in the manner in which psychological processes are carried on. The chain model corresponds to the concept put forward by the associationists and the comb model is very similar to Berze's hypothesis. I am in no sense a partisan of the latter opinion in view of its deeply idealistic basis (the psychology of voluntarism), but I cannot deny that the attacks made on the opinions of pure associationists were extremely opportune.

** For example, the velocity of the dispersion of waves of excitation through the C.N.S., time phenomena related to the interference of these waves, rhythmical heart activity, etc.

these two hypotheses, or perhaps in favour of some other more complex organizational synthesis which incorporates both of them, is a topic for further investigation. At present is is important for us to discover what, in principle, is implied in the actual manner in which the problem is framed.

What is important is that the motor image of a movement (that has been termed by neurologists "the program of a movement", *Bewegungsformel*, *Bewegungsgestalt*, and so forth) must necessarily exist in the C.N.S. in the form of an engram. This directional engram does not merely exist on the comb hypothesis; indeed, the same fact of successive "stamped-in" connections between elementary engrams a, b, c, d, e, ..., in the chain hypothesis is also the engram in the other scheme, only in this case it is represented by an arrow rather than by a circle; this is the engram that determines the law of systematic succession of ecphoria and that consequently controls it. This motor image corresponds to the real, factual form of the movement, that is, *to the curve B* in Fig. 14, and in no way to the curve of the impulse C; it is indeed true that its presence makes it possible to control the course of the impulse C so that, as a result, a smooth performance of the movement habit B is achieved. Therefore it is necessary that there should exist in the supreme nervous organ an exact representation of what will later occur at the periphery; meanwhile, the unfolding of the activity in the field in the intervening operational stages and the realization of the impulse C (which by the argument given above is accordingly dissimilar to the peripheral effect), must therefore also be dissimilar to the contents of the controlling engrams. We may use the following metaphor: it is as if an order sent by the higher centre is coded before its transmission to the periphery so that it is completely unrecognizable and is there again automatically deciphered. In Section 3 above I have said that the possibility of a habit of movement, of the establishment of a conditioned motor reflex, necessarily implies its unitary localization in the central areas, and that a unity of this type cannot be related to the theory of the representations of muscles in the higher centres of the cortex. The considerations which have just been raised once again confirm this theses, on this occasion from the point of view of the time structure of movements; that level of the C.N.S. in which the centrifugal impulse C is formulated and in which we might consequently expect to find a representation of the muscular system is not the supreme level of the C.N.S., but is in fact that level at which the elementary engrams a, b, c, ..., etc., of the comb hypothesis are located. Between the mechanism represented by the comb hypothesis and the mechanism involved in the case of muscular representation we are obliged to insert another process of the coding of the image of the movement and its presentation in the form C. In the terms of our equation this coding process is the transformation of the relationship $E(t)$ in the pure form prevalent in the higher level into a full dependence of the form $E(t, \alpha, d\alpha/dt)$; that is, the adaptation of the impulse to proprioception*.

In this way, the analysis of the course of a movement in time again brings us to a recognition of the structural complexity of an act of movement, and consequently also of the complexity of its representation in terms of localization. Here, also, the recognition of the necessity for the existence of directional engrams and mechanisms of ecphoria demands that we postulate a series of hierarchical levels, each of them, inevitably, having a degree of qualitative independence.

5. TOPOLOGY AND METRICS OF MOVEMENTS. THE MOTOR FIELD**

If we now turn from the temporal moment to the spatial it will be necessary to touch on two considerations: the distinction between the metric and topological properties of physiological space, and the peculiarities of the motor field of the central nervous system. Because of their fundamental importance these two points should really be the objects of separate reports.

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* The formation and development of new habits of movement, that is, the engraphy of conditioned reflexes of movement, also appears to be a structurally complex process in the light of the analysis undertaken in this report. It is in fact the case that new directional engrams with their spatio-temporal details must be built up in the C.N.S.; however, those auxiliary proprioceptive mechanisms which I have just described as "coding" the impulse, and which provide the higher engram with the possibility of an actual detailed existence, must also be built up. The fact that the habit of movement is not engraphed in those centres in which the muscles are localizationally represented is at once demonstrated by the fact that an acquired habit may exist while incorporating very different muscles in various combinations. When a child learns to write he can only form large letters, but a literate adult can form either large or small letters with equal facility and write either straight ahead or sideway, etc. Apparently the motor directional engrams are developed, generally speaking, later than the auxiliary coding mechanisms and correspond to a higher degree of mastery in the acquisition of a habit.

** The term "topology" as used here does not coincide exactly with the strict mathematical definition. For lack of a more adequate expression I have adopted this term for the whole of the *qualitative* characteristics of space configurations and of the form of movements in contrast to the quantitative, metric ones. The more detailed definition of what is meant here under the term topology will be understood from the text.

I will for this reason discuss them only as much as is necessary to develop my basic thesis.

In any geometrical representation we may make a distinction between topology and metrics. By the topology of a geometrical object I mean the totality of its qualitative peculiarities without reference to its magnitude, form, any distortion in its reproduction, etc. As topological properties of a linear figure, for example, we may discuss whether it is open or closed, whether the lines composing it intersect with each other as in a figure eight or whether they do not intersect as in the case of a circle and so on. Besides these properties, in the determination of which quantitative considerations are irrelevant, we may also consider such topological properties as incorporate the concept of number, not, however, including the concept of measure. Among these properties we may refer, for example, to that of quadrangularity, membership in the group of five-pointed stars, and so on. I shall arbitrarily describe this group of properties as topological properties of the first order while the former may be considered zero order properties. All figures in the upper row in Fig. 20 belong to one and the same topological class of figures of the first order (being, however, completely dissimilar in metric relations); they are indeed identical in respect to the numbers which characterize them. All of them have five angles or points, all of them display five intersections of the lines composing them, and so on. No. 6 in this illustration belongs to another class of the

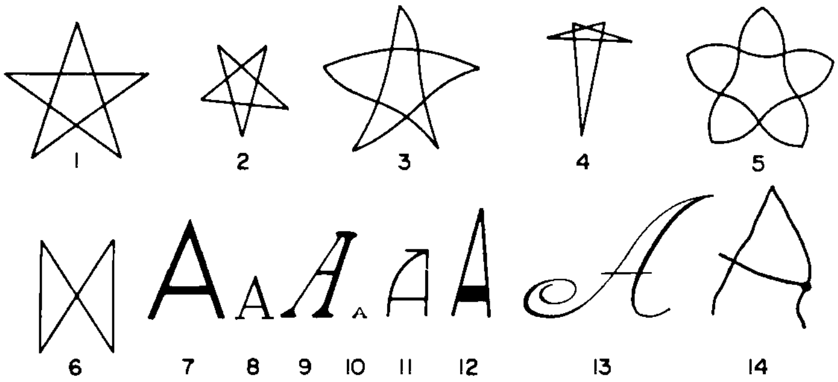

Fig. 20. 1-5, topological class of five-pointed stars; 6, topological class of figure-eights with four angles; 7-14, topological class of letters *A*.

same order containing four angles and one intersection, but it is located, as are the first five figures, in the class of zero order, being as they are a closed figure with intersecting lines. In order to illustrate characteristic properties of the first order with an example with which we are all familiar I shall point out that every printed letter is a separate topological class of the first order, while to the single class of letter *A* there belong letter *A*'s of all dimensions, scripts, outlines, embellishments, etc. (see 7-14 in Fig. 20), though we ignore certain additional details of purely calligraphic significance. The chalk figures for the game of "hopscotch", which appear in great numbers on our pavements every spring, are also all representatives of one and the same topological class of the first order for each equivalent representation of the game, and in this respect the scale of the figure or the age and skill of the draughtsman do not matter. The habitual scheme on which a given child draws a house or a face is also usually a determinate topological class and nothing more.

After this general introduction we may turn from geometry to psychophysiology. If we draw the attention of a psychologist or a teacher to our collection of letter *A*'s in Fig. 20, he will immediately remark that the whole set displays a common characteristic in terms of its essential sign, that is to say without circumlocution, that the topological characteristics of a figure are of paramount psychologico-pedagogical importance in comparison with properties of a metrical sort. Our psychologist or teacher will be quite right, because the recognition of the letter *A* does not require the presence of any metrical properties and is, on the contrary, entirely dependent on the presence of determinate topological cues. This great affinity between the process of recognition and topology, which has also been noted and studied for some considerable time by adherents of Gestalt psychology, is certainly a psychophysiological phenomenon and may even be of general biological significance, but in any case it cannot be deduced from purely geometrical considerations. The biological characteristic of the predominance of topological categories over metric ones may be pursued in a multiplicity of examples. A maple leaf differs from a birch leaf in respect to topological properties of the first order, while at the same time all maple leaves belong to one and the same topological class in spite of all the thoroughly investigated biometric variation between separate specimens. The structure of the brain and the disposition of the main convolutions of the cortex again provide an example of an object having the same topology for all possible metrical variations. It is possible to say with certainty that in the area of biological morphology those cases in which metrics is of importance together with topology (for example, the lens of the eye) are rare exceptions*. This overwhelming importance of topology in the case of living objects should be attentively compared, for example, with the morphology of crystals where the essential relationships are all metric ones.

A whole series of biologically important morphological signs must unquestionably be referred to topology, although they cannot be numbered either in the zero or in the first order. Every child will naturally distinguish between a cat and a dog; the distinction is certainly not made on the basis of anatomical considerations such as the comparative structure of the claws and the teeth, on which topic he may be fully ignorant, but on the general appearance - on a certain *je ne sais quoi* - that indubitably appears to be a topological category. However, the difference between the appearance of a cat and a dog cannot be related to topological signs of the first order. It is likely that some higher orders which await future analysis are operating here.

Insufficient attention has so far been given to the fact that the movements of live organisms, to no less a degree than their perceptions, are determined by topological categories. This is illustrated with great clarity by the example of drawing, perhaps because this type of movement leaves a record which may be conveniently studied. It is easy for everyone to draw a five-pointed star, but we can say with certainty that this picture is made by using only topological and not metric relationships. As proof on this I suggest the experiment of drawing ten such stars in succession and comparing the pictures. I doubt if it is at all possible to make a metrically perfect copy of a similar object without the help of a compass and a ruler, that is, the human motor system cannot attain any high degree of metric proficiency, but it can be said that our motor system is very sensitive to topological distinctions of higher orders than one and zero. It is sufficient, for example, to draw attention to *handwriting*. I pointed out above that the letter *A* belongs to a single topological class of the first order no matter how or by whom it is written. Besides this, all letter *A*'s written in my hand are similar to each other and are simultaneously different from letter *A*'s written by second and third persons. The similarity between my *A*'s is far from metrical, but is topological; the differences between my *A*'s and those written by other hands must in the same way be related to topological

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* It may never cross the mind of an anatomist or a topographical anatomist that all his life he considers only various topological categories - a new variation of Molière's M. Jourdain!

differences involving higher orders than the first. The topological propensities in our perception seize upon what may be regarded as common features within the limits of a given handwriting - once again in a form which it is not easy to subject to analysis in terms of impressions - a *je ne sais quoi*, the analysis of which is not yet practicable for us because of our lack of acquaintance at present with whatever may constitute higher topological orders and what properties we must ascribe to them.

Such of our movements as do not leave a trace upon paper have drawn less attention to themselves in the manner indicated. Parallel with the knowledge on handwriting there exist structurally similar bodies of knowledge on gait, touch in music and accent of voice, although the analysis of these phenomena has not been carried very far. One thing may, however, be already affirmed with certainty; all attempts to draw distinctions of this type in terms of quantitative metrical signs (as can be done, for example, with pitch) are doomed to failure in advance. It is here necessary to make new discoveries in qualitative geometry, but consideration of the perspectives which are now unfolding in this direction would be for us at present too far from our main goal.

A circumstance of great and immediate interest in the structural analysis of movements is the fact that topological peculiarities in visual perception display marked similarities to some signs of idiosyncrasies in the topology of motor organization. So, for example, the category of dimension is equally indifferent to visual perception and to movement. I find it equally easy to recognize a triangle, a star or a letter whether it is presented to me in a large or in a small form. The same indifference to the absolute dimensions of a geometrical object was demonstrated in dogs by Pavlov and in rats by Lashley. In precisely the same way I find it equally easy to draw a star or write a word large or small, and to do this on a piece of paper or on a classroom blackboard. It would be interesting to make a study of the quantitative relationship between the variation in these drawings and their size; but we may say in any case that, whatever the size, they retain their topological properties not only of the first but also of higher orders; so, for example, all the characteristics of handwriting which are peculiar to a given person when writing on paper are also apparent in writing on a blackboard, although, in a word, the entire muscular structure of the movement is absolutely different in the two cases.

Visual perception, however, shows great sensitivity to such concomitantly metric cues as symmetry, a category that is at the same time completely ignored by the motor system. On the other hand, the metrical category of extensity is, without doubt, of greater importance to the motor than to the visual receptors because the estimation of dimension in perception (for example, visually) is always ultimately based on deep-seated kinaesthetic associations related to the field of sensitivity of the receptor. The perceptual and motor systems are to all appearances equally indifferent to the category of position in space (right, left, above, below), which is of exceptional interest for the structural analysis of localization. In fact a figure which may be placed in the most diverse portions of the visual field is recognized with equal facility as being the same*; in the same way the process of carrying out an habitual action, for example, writing a word or playing over a passage which one has learnt by heart on the piano, is carried out with approximately the same facility and with the same degree of accuracy independently of the position of the hand or of the register on the piano. It is interesting that the purely metrical abilities of the kinaesthetic apparatus (for example the estimation of length or of distance) are characterized by gross differences in various zones of the spatial field.

Both perceptual recognition and motor reproduction are extremely sensitive to the orientation of a figure in space. The identification of a triangle after it has been rotated 180° is incomparably more difficult than the identification of triangles of different size with the same orientation. In just the same way it is extremely difficult to draw figures upside down with a pencil.

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* I consider this fact to be an extremely clear illustration of the structural complexity of every conditioned reflex, even what is apparently the most simple one. In presenting a conditioning stimulus visually to a dog neither the head, and still less the eyeballs, are in any way immobilized and for this reason the visual stimulus may fall on the most various points of the retina, and consequently on different points of the first visual centre. If the visual stimulus is, for example, a triangle, on each presentation of this stimulus to the animal's retina a whole series of sensory elements are excited, and here, every time the animal turns its head and eyes this series is either wholly or partially different. The appearance of a single reaction in all these cases proves, it would seem, that the engram for a given conditioned reflex is not located at those points (the primary visual centre) on which the separate lines and points are represented but at some structurally higher centre, the connection of which to the former is very similar to that which has earlier been described in the case of the successive levels of the centres of movement. Here we find an example of the same fact which was employed above (section 3, p. 29) as proof of the impossibility of localization of the muscles and of conditioned reflexes in one and the same centre.

It is an important fact that in a very large percentage of cases children draw the mirror images of letters, that is to say they change about the right and left sides although they never turn the letter upside down. Another fact is also interesting (being indicative of some sort of structural hierarchy); children never either read or write an entire word from right to left, they only do this separately and successively for individual letters of the world. It is clear that in both these cases there must exist different mechanisms which cannot be compared to each other.

These analogies and differences may be pursued to great lengths: a study of these and others promises to be extremely fruitful. At present, however, it is necessary only to summarize all that has been said above about the topological properties of perception and movements.

First of all, it may be stated that the totality of the topological and metrical characteristics of the relations between movements and external space can be generalized under the term *motor field*, analogous with the concept of the visual field recognized by psychologists. An immediate task of physiology is to analyse the properties of this motor field. The preceding statements permit us to assert that the physiological motor field is as widely different from objective external space as is the visual field. Its typical differences from theoretical Euclidian space consist, first of all, in an obvious preference of the motor field for topological categories as compared with metric ones, in the presence of some evident tropisms, or tendencies toward certain directions, in the absence of bilateral symmetry (which is peculiar to the visual field), and so on. The predominance of topology is revealed also in the fact that straight lines and their distinction from curved ones are not proper to the motor field (in this it differs from the visual), nor are stable, identical lines. In biomechanics this is manifested in that successive

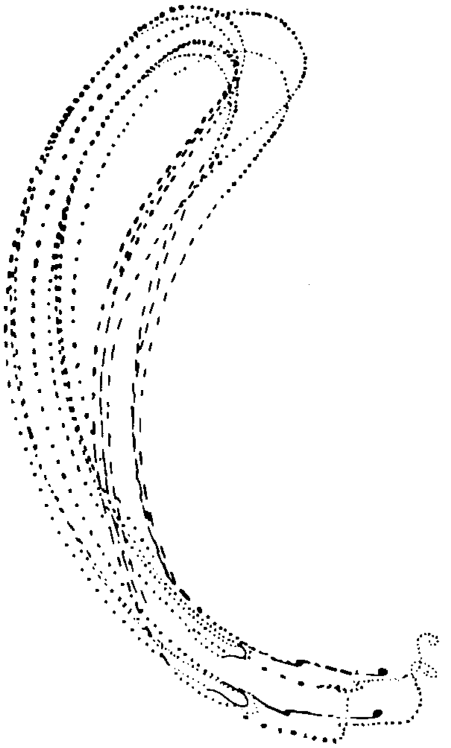

Fig. 21. Cyclogram of a series of successive poorly automatized movements taken on the same plate.

movements of cyclical nature never exactly repeat themselves (Fig. 21). The co-ordinational net of the motor field must be regarded, in distinction to a net in Euclidian geometry, firstly as non-rectilinear, and secondly as oscillating like a cobweb in the wind. Its "oscillation" does not, however, in every case proceed so far as to destroy topological relationships either of zero order (for example the category "between") or of the first and perhaps of even higher orders.

Some of the properties of the motor field which we have now disentangled are of great interest for the theory of localization. Firstly, there is the deeply seated inherent indifference of the motor control centre to the scale and position of the movement effected, as we have discussed above. It is clear that each of the variations of a movement (for example, drawing a circle large or small, directly in front of oneself or to one side, on a horizontal piece of paper or on a vertical blackboard, etc.) demands a quite different muscular formula; and even more than this, involves a completely different set of muscles in the action. The almost equal facility and accuracy with which all these variations can be performed is evidence for the fact that they are ultimately determined by one and the same higher directional engram in relation to which dimensions and position play a secondary role. These engrams, which determine the muscle structure of each of the concrete variations, clearly lie still lower than those described in section 4 and in particular in the area *C* of Fig. 14, whereas the engrams of dimension and spatial position may already be referred to area *B*. We must conclude from this that the higher engram, which may be called the engram of a given topological class, is already structurally extremely far removed (and because of this also probably localizationally very distant) from any resemblance whatever to the joint-muscle schemata; it is extremely geometrical, representing a very abstract motor image of space. This makes us suppose - for the time being merely as an hypothesis though it forces itself upon us very strongly - that the localizational areas of these higher-order motor engrams have also the same topological regulation as is found in external space or in the motor field (and that in any case the pattern is by no means that which maintains in the joint-muscle apparatus). In other words there is considerable reason to suppose that in the higher motor centres of the brain (it is very probable that these are in the cortical hemispheres) the localizational pattern is none other than some form of projection of external space in the form present for the subject in the motor field. This projection, from all that has been said above, must be congruent with external space, but only topologically and in no sense metrically. All danger of considering the possibility of compensation for the inversion of projection at the retina (section 3) and many other possibilities of the same sort are completely avoided by these considerations. It seems to me that although it is not now possible to specify the ways in which such a topological representation of space in the central nervous system may be achieved, this is only a question of time for physiology. It is only necessary to reiterate that the topological properties of the projection of space in the C.N.S. may prove to be very strange and unexpected; we must not expect to find in the cortex some sort of photograph of space, even an extremely deformed one. Still, the hypothesis that there exist in the higher levels of the C.N.S. projections of space, and not projections of joints and muscles, seems to me to be at present more probable than any other.

6. THE PRINCIPLE OF "EQUAL SIMPLICITY"

It is now time to give a general formulation of an heuristic principle which I have already presented in a partial modification in section 3 above, and to examine its application in terms of a few examples. I shall call it the *principle of equal simplicity*.

I begin with non-physiological examples. I have three devices with which it is possible to draw a circle; a circular template, a compass and an ellipsograph. A circle of the same radius as the template may be drawn as easily with the template as with the compass; a circle may also be drawn with the ellipsograph, as it is a particular case of an ellipse, but it will be a little more complicated to do this than with either the template or the compass. If we have a circle of some other radius the template immediately becomes useless. The compass draws circles of all radii with equal facility. A given actual ellipsograph may only describe a circle of a single determinate radius, and for this reason it is eliminated together with the template. If we wish to draw an ellipse we may do this using the ellipsograph with exactly the same difficulty, no more or less than we had in drawing the circle, but both the compass and the template are useless.

In this example we are considering a set of curves of the second order which differ (a) in radius and (b) in eccentricity. One of our instruments, the template, gives us with great simplicity one curve and no others. The second instrument gives us equally easily all variations in radius, but only a single eccentricity, zero, peculiar to a circle. The third instrument gives us with equal ease - although in absolute terms the process is slightly more complicated than in the case of the compass - all eccentricities, but only one radius. A circle of the same radius as the template may be drawn by means of all three instruments but the functional relationship between their simplicity and the possible variations in all three cases are quite different. The type of this functional relationship is determined with great accuracy by the scheme of construction of the instrument.

In mathematical language the preceding example may be presented in the following way. We designate the degree of simplicity (for example the speed with which the task can be completed, or unity divided by the time of completion, etc.) by S, the radius of the circle by r and the eccentricity by e. Then, for all our devices,

$$S = F(r, e). \qquad (6)$$

For the template of radius r we have:

$$F(r, e) = 0; \qquad F(R, O) \neq 0. \qquad (6a)$$

For the compass:

$$F(r, e)_{e \neq o} = 0; \qquad F(r, 0) = \text{const} \neq 0. \qquad (6b)$$

For the ellipsograph:

$$F(r, e)_{r \neq \mathrm{R}} \qquad F(R, e) = \text{const} \neq 0. \qquad (6c)$$

Equations (6b) and (6c) may be represented by a line; eqn. (6a) is the point of intersection of the lines (6b) and (6c).

Examples are possible in which the degree of simplicity does not change abruptly from zero to some final value as in the preceding case, but changes from one value to another with a certain regular continuity. So, for example, in multiplying numbers with Odner's calculating machine the degree of simplicity (or the speed of the work) decreases in parallel with an increase in the number of multiplication signs and with the number of units involved with each of these signs. At the same time the degree of simplicity is invariable with respect to the number of digits multiplied. In the Millioner calculating machine the degree of simplicity is invariable in respect to the number of digits in the multipliers and depends only on the number of multiplication signs. Finally, on a slide rule the degree of simplicity is almost invariable with respect to both components involved.

In all these cases we encounter the same fact, that different structural schemes may carry out the same set of operations but the differences in their structures are always accompanied by differences *in the form of the function*

S. We may say with certainty that the more marked are the changes in S in the transition from one element of the set to another adjacent to it, the smaller is the degree of the adaptation to this transition possible with the structural peculiarities of the system in question. On the other hand, for any given system there are "lines of equal simplicity", that is, those transitions from one element of the set of possible tasks to another which do not result in any change in the simplicity of manipulation, corresponding to transitions which are most closely related to the structural scheme of the device.

We arrive from this case to the following formulation of the principle of equal simplicity: for every system which is capable of undertaking a set of different elementary processes of a given range, the lines of equal simplicity correspond to those directions in this range along which movement does not involve any change either in the structural principles or in the principles of operation of the system. Instead of the expression "simplicity" which does not have any concrete association we may insert a whole series of parallel expressions in relation to the case under investigation: the expressions of speed of completion, degree of accuracy, degree of variance and so on. For a general formulation I have selected the term simplicity as being the most general in spite of its lack of concrete associations.

We may extract an heuristically valuable principle from the discussion above. If we are concerned with any given system, the structure of which is unknown to us but whose operation we may observe under a variety of conditions, then by a comparison of the changes *in the variable* S (speed, accuracy, variation, etc.) encountered as a function of each of the variables in the conditions, we may come to determinate conclusions as to the structure of the system which are unattainable by direct means.

Let us imagine, for example, that we are invited to see a film without having any idea of how cinematography works. We may suppose that we are attending it in a puppet theatre (as our grandfathers might have). We are astonished by the wealth and variety of the material we are shown, exceeding by far all that could be met with in these days, but we still have an indisputable right to consider that we are watching only marionettes which have been greatly improved in principle. It is true that in a puppet theatre we never, for example, saw the sea; but then (says grandfather) it is obvious that here they have only an extremely cunning mechanical imitation of the sea. In the old puppet theatre figures could not be made to diminish as they grew more distant, as this occurs in the cinema, but once again it is possible to consider this as a new achievement of the technology of the marionette theatre. All this, although extremely difficult, is possible. It is, however, very easy to show that we are not in a puppet theatre and to do this precisely with the help of the principle of equal simplicity. It is sufficient, for this purpose, to select two objects which are sharply different in their difficulty of representation in the puppet theatre, for example, a rotating wheel and a stormy sea, and without reference to cinematic technique (let us suppose that its technical structure is inaccessible to us) to turn to the studio's accountant and ask how much it would cost to obtain representations of both these objects on the screen for one minute. As soon as we discover that a strip of 20 m of film costs about the same for either object (or, to put it more accurately, that the cost of the film is related to some other arbitrary factors and in no way to the mechanical properties of the objects on the screen), the hypothesis of a puppet theatre collapses. In general, a skilful interview with an accountant may give many positive technical details - we may recall that it was in just this way that Mendeleev discovered the secret of an important French explosive.

For the puppet theatre all is possible (at least potentially) that is possible to the cinema. But the "all" is, in principle, unattainable with the same degree of simplicity as operates in the cinema. We encounter the same interrelationships between the gramophone and such talking machines as were experimented with a hundred years ago by von Kempelin, for example. The whole structural nature of the gramophone lies in the fact that the sounds to be reproduced make no difference to it, whereas von Kempelin would have been obliged to construct a new mechanical gullet for each new pitch. A rural deacon in L. Andreev's story was brought into an extremely amusing collision with the principle of equal simplicity as applied to the gramophone when he could not conceive how the gramophone could reproduce with equal ease both a music-hall song and the voice of the Son of Man. The principle described proves to be extremely fruitful in its application to the structural analysis of the function of the central nervous system, both in its receptor and in its effector aspects. In section 2 I used a circular movement of the extended arm of the type shown in Fig. 15 as an example of the smooth redistribution of muscle pull. We may return to consideration of the same movement from a new point of view. If a circle is described with the arm directly to the front, then directly out to one side and then about some intermediate axis, both the muscle and the innervational schemes of the three movements will be sharply different. However, all three movements are subjectively very much alike in terms of difficulty and objectively they display approximately the same amount of accuracy and of variation. This allows us to conclude with a high degree of probability that the structure of the central complex which governs the production of a given series of movements is much more closely related to spatial form than to muscle scheme, because all three variations of the circular movement which we have attempted lie on lines of equal simplicity in regard to the properties of the movement and the properties of their forms, but not the properties of the muscular schemes. This conclusion may be made more clear from the following example, which I have thoroughly analysed in another study (23). In order to carry out with precision any given automatized movement, for example, cursive writing, the positions and the means of fixation in the intermediate links of the arm are almost completely indifferent. I write with the same handwriting and with almost equal ease when I rest my forearm on a table-top and when my arm supports its own weight, as well as in a variety of positions. All these variants are sharply different from the point of view of muscle structure and if it were assumed that the object of the working out of a habit of movement were one of these structures we would be obliged to suppose that the others would lie completely outside the range of this habit, that is to say, on a quite different level of simplicity. The fact of identical simplicity and the retention of the characteristics of the habit is immediate evidence that the habit of writing is not a habit of the muscle scheme and consequently that the traces in the C.N.S. which govern these habits are closely related to the topology of handwriting and considerably removed from joints and muscles. All these, and many similar examples, must be experimentally analysed both qualitatively and quantitatively and each such analysis allows us to arrive at new basic conclusions as to the structure of the activity of the motor centres of the C.N.S.

An extremely interesting example of the application of the principle of equal simplicity may be taken from the psychology of perception which is, at the moment, far more developed than the structural physiology of movement. This example refers to a theory of hearing. A whole series of hypotheses have been put forward in order to explain the mechanism operating in the inner ear and allowing us to discriminate sounds (Hemholtz, Ewald, Hering, Gray) among which the most popular at the moment is Helmholtz's hypothesis. On this hypothesis each of the numerous fibres of the basilar membrane is supposed to act as elastic string tuned to a particular frequency. When this particular frequency operates on the organ of Corti and the basilar membrane the given fibre goes into a condition of resonant oscillation and mechanically stimulates the auditory receptors attached to it. In this way each of the sensitive endings of the acoustic nerve are stimulated only by a single sound frequency and the recognition of the frequency in the C.N.S. is achieved by the same process which effects the perception of tactile local signs (*Lokalzeichen*). Complex sounds or harmonics are analysed in this way which explains the recognition of pitch and the discrimination of chords.

Many serious psychological objections have been raised against this hypothesis. Additions and corrections were soon made. Helmholtz himself was not able to explain in these terms the perception of consonance and dissonance for which he was obliged to hypothesize the presence of a separate system perceiving beat (*Schwebungen*). There have been numerous later additions and emendations (F. Alt, A. Gray, L. Hermann Waetzmann, Budd-Feldafing, W. Köhler, G. Revesz, F. Brentano, and others), and the very fact of their necessity has cast serious doubt on Helmholtz's hypothesis. It is very probable that should a new hypothesis appear which adequately explains all the requisite phenomena and is at the same time simpler, it would be preferred to the older hypothesis on the principle that the true explanation is the simpler one* (although there is nothing objective in this guarantee). However, no hypothesis of this type has so far appeared. Meanwhile, there are serious objections to Helmholtz's hypothesis independent of its simplicity or complexity. It is only necessary for this purpose to show (and there are in the literature an enormous number of experimental and clinical facts pointing in this direction) that the lines of equal simplicity are distributed in an essentially different way for the function of auditory perception and for a resonant harp. So as not to encumber this report I shall limit myself to two points.

We are making a comparison between a system whose functional operation is unknown to us, the apparatus of auditory perception and a known physical model - a set of resonators which for the sake of vividness I have called a

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*This conviction may very easily be false. Contemporary physics provides various examples of this. The theories of de Broglie, Einstein, Heisenberg, Schrödinger or Dirac are far more complicated than the concepts which they have supplanted. To set up simplicity as a criterion of reliability would be to affirm in principle that the categories of logic and psychology dominate the categories of objective reality and determine them, and we have no authority for apriorities of this type.

resonant harp. For this latter structure the simplest of all operations is the determination of the absolute frequency of a tone; this follows from its very structure. The determination of the relationship between the frequencies of the components, and is for this reason more complicated. However, the statistics of musical pedagogy (J. v. Kries, O. Abraham, G. Revesz) show that the possession of absolute pitch is a very rare occurrence while a majority of people have relative pitch. In other words, for the organ of hearing relative determinations of intervals are easier than those of absolute tones.

On the other hand, a pure musical tone is simpler in its acoustic structure than the sounds of the human voice - vowels with their numerous formants, and consonants with their characteristic phonation. For a resonating harp these can be recognized in no other way than by their analysis into simple components and only after the determination of these components; consequently, on this model the discrimination of speech sounds is more complicated than the discrimination of pure tones and is based entirely on the latter process. As far as the human organ of hearing is concerned, *many* people have musical (relative) discrimination while *all* understand and perceive speech. Very striking cases of tone deafness have been described (L. Alt, W. Köhler). Köhler's patient not only did not understand what was meant by a melody, but was even unable to distinguish between a low and a high tone, while he could distinguish all shades of speech and accent very well, indeed, imitating provincial accents quite well in telling anecdotes (such persons have no physical defects of hearing). We again find an inversion of the levels of difficulty with respect to the resonator apparatus which we have hypothesized.

It is clear from both comparisons that the organ of hearing gives an essentially different gradation of simplicity than that of the hypothesized resonator mechanism - a gradation amounting in some examples to a direct transposition of the order of difficulty. It is this circumstance which is critically dangerous for Helmholtz's hypothesis, independently of its simplicity or complexity.

The discussion in section 3 of the example of the lack of correspondence between the theory of muscle localization in the cortex and the idea of the localization of conditioned reflexes is clearly a particular case of the use of the principle which has here been described in full. Further experiments and observations on changes in the accuracy of movements in their different variations and for corresponding changes in the irradiation of a habit of movement may disclose for us a whole series of structural regularities in the motor field, and the motor functions of the brain in their entirety - regularities which cannot be foreseen at present. Only one thing may already be foreseen with certainty. Every new discovery in the field of co-ordinational structure will at the same time be a new discovery along the lines of localizational structure; and on that day when we understand the one we shall be able to say that we understand the other.

Symbols Used in this Chapter

I. SPATIAL COORDINATES

x	longitudinal (sagittal) coordinate.
y	vertical coordinate.
z	transversal coordinate.

For x, positive direction is forward.
For y, positive direction is upward.
For z, positive direction is to the left.

II. SYMBOLS FOR PARTS OF THE BODY

Initial letter	Limb		Centre of gravity of the limb segment	Proximal joint of limb segment
	Latin term	English term		
c	caput	Head	gc	-
b	brachium	Upper arm	gb	b shoulder joint
a	antebrachium	Fore arm	ga	a elbow joint
m	manus	Hand	gm	m wrist joint
t	truncus	Trunk	gt	-
f	femur	Thigh	gf	f hip joint
s	sura	Shin	gs	s knee joint
p	pes	Foot	gp	p ankle joint
H	homo	The whole body	gH	-

III. TERMS FOR THE CENTRES OF GRAVITY OF SYSTEMS (EXAMPLES)

The centre of gravity for the system (shin + foot)	$g(sp)$.
The centre of gravity for the system (whole + leg)	$g(fsp)$, etc.

Other points along the long axes of the limbs are indicated by Greek letters corresponding to the initial letters of the Latin term for the limb. For example:

Φ a point on the longitudinal axis of the thigh (f).

π a point at the end of the foot (p).

IV. SYMBOLS FOR THE MECHANICAL FUNCTIONS OF MOVEMENT

S displacement (along a line described by real coordinates).

V velocity.

W acceleration.

F force.

M moment of force.

The symbols for joints or centres of gravity of a limb segment are attached to these letters as subscripts. Symbols for coordinates are given in parentheses. For example:

$S_a(y)$	the vertical component of the path followed by the elbow joint.
$V_s(x)$	the sagittal component of the velocity of the knee joint.
$W_\pi(z)$	the transversal component of the acceleration of the end of the foot.
$F_{gs}(y)$	the vertical component of the force at the centre of gravity of the shin.
$F_g(fsp)(x)$	the longitudinal component of force at the centre of gravity of the whole leg system.
M_f	the moment of force at the hip joint.

ADDENDA

1. A point on the facial plane of the head in the region of the upper edge of the aural helix is projected upon the centre of gravity of the head in profile photographs, and is provisionally termed the semi-centre of gravity on these photographs, and is designated by $gc/2$.

2. The angles mentioned in this handbook are designated as follows:

α the angle of the longitudinal axis of the thigh to the horizontal, directed forwards.

β the angle of the longitudinal axis of the shin to the horizontal, directed forwards.

ϕ the angle between the longitudinal axes of the thigh and shin.

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CHAPTER IIa
FREQUENCY ENCODING IN MOTOR SYSTEMS

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THE ISSUES

Introduction

> There is considerable reason to suppose that in the higher motor centers of the brain (it is very probable that these are in the cortical hemispheres) the localization pattern is none other than some form of projection of external space in the form present for the subject in the motor field. This projection, from all that has been said above, must be congruent with external space, but only topologically and in no sense metrically. All danger of considering the possibility of compensation for the inversion of projection at the retina ... and many other possibilities of the same sort are completely avoided by these considerations. It seems to me that although it is not now possible to specify the ways in which such a topological representation of space in the central nervous system may be achieved, this is only a question of time for physiology. It is only necessary to reiterate that the topological properties of the projection of space in the C.N.S. may prove to be very strange and unexpected; we must not expect to find in the cortex some sort of photographic space, even an extremely deformed one. Still, the hypothesis that there exist in the higher levels of the C.N.S. projections of space, and not projections of joints and muscles, seems to me to be at present more probable than any other (Bernstein, p. 109).

With these insights Bernstein set the problem which neurophysiologists must address if they are to relate the anatomical organization of the central motor mechanism to the organization of behavior. Neuroanatomists have demonstrated a somatotopic representation of muscles onto the cerebral cortex. But as Bernstein points out it is the topological representation of external space, not of projections of joints and muscles, that is needed if patterns of behavioral acts, the consequence of movements, and not just patterns of movements per se are to be explained. Bernstein, in his experiments, used

Fourier analysis to specify the topology of such behavioral actions and his specifications were sufficiently accurate to allow prediction of the patterns of continuing action.

The experiments reported here were undertaken to test the hypothesis that the Fourier approach might also be as useful in analyzing the physiology of single neurons in the motor mechanism as it was for analyzing patterns of behavioral actions. Support for such an approach comes from its success when applied to the analyses of the functions of the sensory systems. These analyses are reviewed in some detail in order to provide a background of expectations and of problems faced using this approach.

The Fourier Approach to the Sensory Systems:

The first suggestion that brain processing might involve a Fourier analysis was made a century ago for the auditory system by Ohm (1843), the same Ohm who formulated Ohm's Law of Electricity. This suggestion was adopted by Herman v. Helmholtz (1863) who performed a series of experiments which led to the place theory of hearing - essentially a view of the cochlea as a piano keyboard, whose keys, when struck by acoustic waves, would iniate nerve impulses to the brain where resonant neurons were activated. This view was modified in this century by George v. Bekesy (1959). His experiments showed the cochlea and peripheral neurosensory mechanism to operate more like a stringed instrument which is sensitive to the superposition of acoustic wave forms. This work led to the discovery that the initial stages of auditory processing can be described in terms of a Fourier transform of the acoustic input (Evans, 1974).

Bekesy went on to make a large-scale model of the cochlea composed of a row of five vibrators (1959). When the model was placed on the forearm and the phase of the vibrators adjusted manually, the phenomenal perception was that of a point source of stimulation which could be moved up and down the arm. When two such model "cochleas" were applied, one to each forearm, the point source appeared at first to jump alternately from one forearm to the other, and then suddenly to stabilize in the space between the two arms. The stimulus was "projected" away from the stimulating source and the receptive surface into the external world, much as sound is projected into the environment away from the source in audio speakers of a high fidelity system.

Both macro- and microelectrode studies performed in my laboratory have shown that multiple simultaneous vibratory stimulations of the skin also evoke only unitary responses in cortex (Dewson, 1964; Lynch, 1971). Just as in perception, the cortical electrical response does not reflect the actual physical dimensions of the stimulus. Bekesy noted that sensory inhibition, due to lateral inhibition in dendritic networks, might be the responsible agent in the transformations.

Evidence is therefore at hand to indicate that the input to the ear and skin becomes transformed into neural patterns that can be described by sets of convolutional integrals of the type that Gabor (1969) has suggested as stages in achieving a fully developed Fourier holographic process.

The manner in which such a stepwise process occurs is best worked out for the visual system. Recordings from units in the optic nerve (Rodieck, 1965) demonstrated that the moving retina decomposes the image produced by the lens of the eye into a "Mexican hat" organization which can be described as convolving retinal organization with sensory input. A second step in the process occurs at the lateral geniculate nucleus where each geniculate cell acts as a peephole "viewing" a part of the retinal mosaic. This is because each geniculate cell has converging upon it some 10,000 optic nerve fibres originating in the ganglion cells of the retina. The receptive field of the geniculate neuron is composed of a center surrounded by concentric rings of receptivity, each consecutive ring of sharply diminishing intensity and of a sign opposite to that of its neighbors (Hammond, 1972).

At the cortex the transformation into the Fourier domain becomes complete. Beginning with the work of Campbell and Robson (1968), Pollen, Lee and Taylor (1971), Maffei and Fiorentini (1973), and Glezer, Ivanoff and Tserhback (1973), investigators using gratings as stimuli (e.g. Schiller, Finlay & Volman, 1976; DeValis, Albrecht & Thorall, 1978; Movshon, Thompson & Tollhurst, 1978; Pribram, Lassonde & Ptito, 1981) have repeatedly confirmed that the cells in visual cortex are selectively tuned to a limited band width of spatial frequency of approximately an octave (1/2 to 1-1/2 octaves). Ordinarily the term frequency implies a temporal dimension, but the spatial frequency (or wave number) of a grating reflects the width and spacings of the bars making up the grating. When such widths and spacing are narrow the spatial frequency is high; when widths and spacing are broad the spatial frequency is low*.

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* The temporal dimension can be evoked by successively scanning across the grating (as, for instance, by walking across the path of illumination of a projection of a slide of such a grating). Conversion to the temporal dimension is, however, not necessary. The grating is a filter whose characteristics can be expressed either in the spatial or temporal dimension, or both.

These findings do not, however, mean that the visual system performs a global Fourier transform on the input to the retine (see also Julesz and Caelli, 1979). The spread function, as such transformations are called, does not encompass the entire retina: rather it is limited to the receptive field of a retinal ganglion cell. Similarly at the cortex encoding is restricted to the receptive field of the cortical neuron.

This patchy organization of the Fourier domain (Robson, 1975) does not impair its functional characteristics. The technique of patching or stripping together Fourier transformed images has been utilized in radioastronomy by Bracewell (1965) to cover expanses which cannot be viewed with any single telescopic exposure. The technique has been further developed by Ross (see Leith, 1976) to produce a hologram by which three dimensional moving images are constructed when the inverse transform is effected. Movement is produced when spatially adjacent Fourier encoded strips, which capture slightly different images are scanned (temporally) as, for instance, when frames of a motion picture are used as the image base for the Fourier transformation.

The Place of the Fourier Transform in Modelling the Cortical Microstructure:

The hybrid nature of cortical organization serves as a warning that any simply conceived "global-Fourier-transform-of input-into-cortical-organization" is untenable. Furthermore, the multiple selectivities of cortical cells in the visual (Spinelli, Pribram & Bridgeman, 1970; Spinelli, Starr & Barrett, 1968; Morell, 1972) auditory (e.g. Evans, 1974) and somatosensorymotor (e.g. Bach-y-Rita, 1972) projection areas clearly indicate that such cells serve as nodes in neural networks in which the Fourier transform is only one, albeit an important, process. E. Roy John speaks of "hyperneurons" constituted of the distributed system of graded potentials he records from the brains of problem-solving animals, while Edelman (1974) has proposed a degenerative group model, also based on an essentially random connectivity.

Thus, several attempts have been made to characterize mathematically such cortical networks in terms of their essential properties. Modifications of the Fourier model have been proposed. For instance, Longuet-Higgins (see Willshaw, Bunewman & Longuet-Higgins, 1969) derived an associative-net model from a Fourier transform base and Leon Cooper (1973) has developed this model into a self-organizing distributed net whose mathematical description contains as a special case the Fourier transform hologram. Barlow (1981; Sakitt &

Barlow, 1982) and Marcelja (1980; Kulikowski, Marcelja & Bishop, 1982) have presented evidence which makes them invoke the Gabor function in which the spatial interrelations among cortical neurons serve, via lateral inhibition, to constrain the frequency domain by placing Gaussian envelopes upon the spatial frequency responses inherent within each receptive field. This model and those of Julesz (1971), Uttal (1978), Borsellino & Poggio (1973), Poggio & Torre (1981), and, in my laboratory, Sutter (1976) relate the Fourier and other continuous function domains to the statistical. Thus, for instance, Uttal emphasizes spatial autocorrelation functions while Poggio and Sutter rely on Wiener polynomial expansions. In addition, Poggio treats the dendritic potential microstructure in terms of the Volterra solution of wave guide equations. His carefully worked out proposal includes a stage of Fourier analysis and another in which the Laplace transform occurs. Marr, Poggio and Whitman Richards (Marr, 1976a, b; Marr & Poggio, 1977; Richards, 1977; Richards & Polit, 1974) have developed a model based on repetitive convolving of Laplacians with a Gaussian distribution in terms of a zero crossing theorem. Cortical organizations have also been described in terms of Lie groups by Hoffman (1970), vector matrices by Stuart, Takahashi and Umezawa (1978), and tensor matrices by Finkelstein (1976). These modifications have attained sufficient richness and precision to allow comparisons the descriptions of processing in distributed systems in cerebellum and cerebrum. Pellionisz and Llinas (1978) have elegantly reviewed the evidence which indicates that cerebellar cortical processing operates as a distributed tensor matrix system. Further, they have invoked Taylor expansions to describe the convergence of Purkinje cell operations onto the dentate nucleus. Cerebellar cortical computations are time limited because of the 50 msec inhibitory sweep generated by the basket cells which wipes out prior computations thus leaving the cortex available for another round - much as in a computational buffer. Taylor expansions are ideal descriptors of such processes. By contrast, the basal ganglia-cortical interaction is constituted of a cyclic resonant loop (Purpura, 1976; Denny-Brown, 1976) in which oscillations, limit cycles and Fourier operators, are more likely to describe functional relationships.

On looking over these various proposals one finds one overriding issue which needs further inquiry: To what extent can brain systems be treated with linear (and reversible) equations and to what extent must nonlinearities be introduced to explain the available data? Good evidence is at hand that at least some properties of the primary sensory systems (as discussed above) are within broad limits essentially linear in their overall operations. The research conducted in my laboratory and reported here addresses this issue within the context of Fourier theory with respect to the primary motor systems (see also Granit, 1970).

Encoding in the Motor Systems:

The direction of this inquiry was derived directly from the experiments performed by Bernstein (1967). Bernstein clad people in black leotards with white spots over their joints, had them perform relatively complex repetitive acts such as running over rough terrain, hammering nails, writing, etc., and took cinematographs of their behavior. The cinematographic record showed continuous fluctuating lines - the representations of the white spots indicating the varying locations of the joints. Fourier analysis made it possible to predict the location of any next point on the line from the fluctuations of location of previous points on that line.

To understand, at the physiological level, how Bernstein accomplished this computation, we need to examine in some detail the functions of various levels of the motor system. Essentially, muscle contraction can be analyzed into two different modes: an isotonic mode in which tone, i.e. tension does not change but the motor unit shortens; and an isometric mode during which no change occurs in motor unit length while tone increases. Muscle spindles sense changes in length; Golgi tendon receptors are sensitive to changes in muscle tension.

There is evidence that to some extent length and tension are controlled by separate neural systems. Changes in length, as measured by the velocity of change in position, is dramatically influenced by cooling the dentate nucleus of the cerebellum (Brooks, Horvath, Atkin, Kozlovskaya & Uno, 1969; Kozlovskaya, Uno, Atkin & Brooks, 1970). Also experiments by Rushworth (as reported by Stark, 1968) have shown that when spindles are blocked by procaine, "the so-called cerebellar syndrome of hypotonia, asthenia, ataxia, overshooting, rebound, dysmetria, and postural drift" was produced.

As would be expected on the basis of the cerebellar input to the cerebral cortex, responses of some of the cells in the monkey motor cortex - which receive the output from the dentate nucleus via the thalamus - have been shown to correlate with the speed of motion of a tracking task (Humphrey, Schmidt & Thompson, 1970). However, recordings made from other cells in the sensorimotor cortex, especially those which give rise to the pyramidal tract do not show any such relationship to velocity of movement (Evarts, 1966).

The question arises as to which motor structures are involved in controlling muscle tension and changes in muscle tension. Clinical and experimental evidence suggests that the basal ganglia are especially involved in regulating postural bias (Kornhuber, 1971, 1974) and the tonic changes in bias due to slow muscle contractions called ramp functions (DeLong, 1971, 1972, 1974). Such biases maintain the normal relatively tremorless steady state of muscle tension. When the functions of basal ganglia cells are disturbed by disease, tremors at rest and other steady-state changes such as cogwheel rigidity result.

There is a convergence of cerebellar and basal ganglia functions onto the neurons in the motor cortex from which the pyramidal tract originates. Evarts (1966, 1967, 1968, 1969) has studied the functions of these neurons in great detail. In his experiments the monkey grasped a rod which could be moved in a 30° arc. To obtain a reward the monkey had to complete the movement through that arc and return by a flexion and extension of the wrist and forearm within a limited time. Electromyograms, displacement of the rod and the muscle tension caused by resistance to the load were measured. The pyramidal electrical activity produced by this manipulation did not reflect the displacement nor the velocity of the rod, but rather directly reflected the magnitude of the load. When additional loads were imposed the responses of the pyramidal neurons increased proportionately. Most of the cells responsed irrespective of the direction of movement; however, most cells were more responsive to flexion than to extension (though some showed the opposite responsivity) irrespective of load. The results also showed that load per se was not the only or even the main critical variable represented by the response of the pyramidal neurons. The typical response was an increase in firing at the onset of a loaded motion or when a load was <u>changed</u>. After a short interval the firing pattern always resumes a steady state, which varies in proportion to the weight of the load.

A Vector Space: Force as Defined by Load

The interpretation of these results has been that the pyramidal neurons encode "force". Force is ordinarily defined to vary as a function of the acceleration of a mass. Thus, in physiology, force has been conceived as the product or resultant of the organism's metric motor activity: "Force can be looked upon as the body's basic output quantity: velocity is thus the single integral of this and displacement the double integral" (Bates, 1947). But if one takes displacement as basic to a measure of acceleration (a spatial derivative), Evarts' (1966, 1967, 1968, 1969) failure to obtain responses to displacement argues against the interpretation that force is being represented. Rather as Evarts and Houk (1967, 1981) have pointed out muscle tension, not change in length, must be thought of as the generator of force. In such a view muscle tension is conceived as the equivalent of mass. Hoffer (1982) has suggested that changes in tension are represented as a function of their mechanical impedance which reflects their elasticity and viscosity. Just as it is not mass but the accelleration of mass which defines force, it is not tension per se but changes in tension measured as its derivative and even its second derivative - acceleration in changes of muscle tension - which produce the best correlation with the responses of pyramidal neurons. Thus, differentiation of muscle tension, i.e. changes in tension with respect to time, become the corrollary of "force".

More operationally, force is represented in terms of load and changes in load. Load adjusting mechanisms are well known in the sensory domain - e.g. in vision the mechanism of retinal adaptation (see Dowling, 1967) and in audition the mechanism mediated by the olivocochlear system (see Dewson, 1968). In the motor mechanism that function has been ascribed, as we shall see below, to the gamma system of muscle spindle control (Matthews, 1964).

Changes in load are reflected either as changes in tension (force) or in the length of motor-units or both. One way to determine whether these load changes will result in a compensatory change in the innervation of motor-units is to look at the ratio of change in muscle force to muscle length*, which has been labelled stiffness (e.g. Houk, 1981). A change in this ratio, dependent as it is on the elasticity and viscosity of the muscle, therefore defines the fluency of control. We will make this clear through the following derivations.

The direction and magnitude of compensatory changes in motor-unit innervation will be determined by the balance between changes in muscle length, registered by muscle spindle afferents and changes in muscle force, registered by tendon organ afferents. Summarized in the following equation this means:

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* Relationship between muscle and motor-unit: The basic unit of activation is the motor-unit, being one alpha or gamma motoneuron with its connecting muscle fibrers. Muscle spindles and Golgi tendon organs register changes in length and tension (or force) of individual motor-units. However, force-length relationships are recorded from whole muscles. These gross recordings should be thought of as representing the integrals of force and of length provided by some group of individual motoneurons.

$$\Delta e = g_s \Delta x - g_t \Delta f \tag{1}$$

where Δe represents the compensatory change in motor-unit innervation, the parameters g_s and g_t the gains or sensitivities of respectively muscle spindle and Golgi tendon organ receptors, while x and f stand for muscle length and muscle force.

Consider next the ideal case in which there would be no need for a compensatory change in motor-unit innervation, i.e. $\Delta e=0$, solving for Δf gives:

$$\Delta f_i = (g_s/g_t) \cdot \Delta x \tag{2}$$

in which the subscript i indicates this is an ideal change in muscle force, not resulting in a compensatory change in motor-unit innervation. Usually changes in muscle force are not ideal and made up of mechanically and neurally mediated components:

$$\Delta f_i = K\Delta x + A\Delta e \tag{3}$$

where K represents the mechanical stiffness of a muscle, or the slope of the force-length relationship is muscles and A is an activation factor converting a compensatory change in motor-unit innervation into a neurally mediated component of force change.

Again, assuming there is no need for a compensatory change in motor-unit innervation, i.e. $\Delta e=0$, substitution in equation (3) yields:

$$\Delta f_m = K\Delta x \tag{4}$$

where the subscript m indicates the purely mechanical nature of his response. Looking back upon equations (1) to (4), we see that the postulated $\Delta e=0$ will occur only if the change in mechanical force Δf_m equals the ideal force-change Δf_i. Comparison of equations (2) and (4) indicates that this condition is met, if:

$$K = g_s/g_t \tag{5}$$

Next assuming $\Delta e=0$ for equation (1) we find:

$$g_s/g_t = \Delta f/\Delta x \tag{6}$$

When there is no need for a compensatory change in motor-unit innervation, the mechanical muscle stiffness K equals the ratio of change in muscle force to muscle length which in turn equals the ratio of receptor properties g_s/g_t.

This means that fluency as determined by ongoing innervation of motor-units, leading to a change in stiffness, is determined by the ratio of change of the sensitivities of muscle spindles to Golgi tendon organs.

The equality between the ratio of change of muscle force to muscle length and the ratio of receptor properties g_s/g_t is especially important when one considers how the central nervous system encodes placement or displacement of limbs. When the central nervous system encodes either of these it will want to do so without the need for any change in motor-unit innervation compensatory to the encoded placement or displacement. This can be accomplished fluently by encoding the changes in stiffness, i.e. changes in the ratio of change of muscle force to muscle length $\Delta f/\Delta x$, since this ratio is equal to g_s/g_t the ratio of the sensitivities of muscle spindles to Golgi tendon organs. We will call either of these ratios the reference ratio. The evidence is that the CNS encodes reference ratios in the following manner.

At the spinal cord, the activity recorded from e.g. alpha motorneurons is cyclic and can be thought of as constituting a central pattern generator (Grillner, 1975; 1981). These oscillatory rhythmic activities are brought under cortical control by way of pyramidal and extra pyramidal pathways to produce a motor-program (Schmidt, 1980). This motor-program operates in such a way, that to move a limb segment from one position to the next terminal position it encodes just the terminal position as the equilibrium point between tensions of agonist and antagonist muscle groups (Asatryan & Feldman, 1965; Bizzi et al., 1976, 1978, 1979; Schmidt & McGown, 1980). When more than one limb segment is involved the motor-program must also encode the timing or phase-relationships among different limb segments or different limbs in order to specify new equilibrium points for each joint (Schmidt, 1980).

Each equilibrium point is determined by the intersections of force-length relationships between agonist and antagonist muscle groups (see e.g. Schmidt, 1980, Fig. 1 & 2). It is, therefore, easy to infer that these equilibrium points actually are equal to the mechanical stiffness K of the muscle groups and are, by means of equations (5) and (6), equal to the reference ratio g_s/g_t and $\Delta f/\Delta x$. However, the organism does not encode a reference ratio once and stop, but continuously encodes new reference ratios. Fluency is thus a measure of the alteration of a current reference ratio g_s/g_t to a new reference ratio g_s/g_t.

In the reference ratio, the g_t, which is the sensitivity of the Golgi tendon organs, is fixed and very high (see e.g. Houk et al., 1967; Jami et al., 1976; Stuart et al., 1972). By contrast the sensitivity of the muscle spindles g_s is variable. Dynamic gamma or fusimotor-axons greatly sensitize the primary endings of the muscle spindle to dynamic stimuli, while the static fusimotor-axons greatly sensitize both primary and secondary endings to static stimuli (Matthews, 1981). It is thus by means of gamma motoneuron innervation, changing the sensitivity of muscle spindles, that new reference ratios are reached.

As already noted, motor-programs are generated when the cyclical activities of the spinal central pattern generators are brought under cortical control. This implies a higher order encoding of the g_s/g_t or $\Delta f/\Delta x$ ratio. Therefore change in muscle tension, which is the corollary of Δf, the change in force, would be expected to provide the best correlation with pyramidal neuron responses. Changes in muscle length Δx as well as changes in g_s the sensitivity of muscle spindles, would then be expected to operate within the fixed value of g_t and influenced soley by the descending pathways projecting onto the gamma motor-neurons.

Conceived in this fashion, the failure to find cortical units responsive to spatial position, in Evarts' (1966, 1967, 1968, 1969) experiments reflects a bias to viewing the motor-cortex in terms of a representation of changes in muscle length rather than in terms of a vector-space constituted of reference ratios which are primarily determined by changes in muscle tension. This vector space of reference ratios reflects, as we have seen, the sensitivities of muscle spindles operating within the matrix of sensitivities of Golgi tendon organs.

To relate the ideal muscle stiffness ratios g_s/g_t to Bernstein's Fourier analysis of joint motion, we observe that for a body in simple harmonic motion (motion under the influence of an elastic restoring force in absence of friction), the restoring force is a function of displacement according to Hooke's law: $F = -kx$, where x is the displacement from the equilibrium position and k is the stiffness, a constant dependent on the elastic properties of the material. From Newton's second law, we have for a body of mass m

$$F = -k\,x = m\,d^2\,x/dt^2,$$

a differential equation for the displacement, x, whose solution is the elementary harmonic

$$x(t) = x(o)\ \exp\left[i\ (k/m)^{1/2} t\right],$$

where x(o) is the initial placement; x(t) is the displacement after time t, and exp denotes the exponential function whose real coordinate is the cosine and the imaginary coordinate is the sine of the product of angular frequency (k/m) times time. Thus by adjusting the stiffness setting $k = g_s/g_t$, the motor system controls the frequency of joint motion to an equilibrium point dependent on the external load m.

There is further evidence that resection of the motor cortex impairs the sensitivity to changes in load which define the fluency of behavioral acts, the consequences of movements, rather than movements per se (Pribram, Kruger, Robinson & Berman, 1956). The results of these experiments showed that in addition to encoding the anatomical spatial arrangement of muscles (e.g. Chang, Ruch & Ward, 1947; Woolsey, 1952) and the physiological patterns of movements (Phillips, 1966) the motor systems must also encode the topological arrangement of the space in which actions, the consequences of movements, take shape (Pribram, 1971). In short, the motor cortex is truly a sensory cortex for action in that it receives multimodal input (e.g. Malis, Pribram & Kruger, 1953; Albe-Fessard & Liebeskind, 1966; Welt, Aschoff, Kameda & Brooks, 1967) via the dorsal thalamus which is a sensory way station. Thus it is feasible for this part of the motor mechanism to furnish the vector space coordinates (an "image") for an action to be achieved.

The current study was undertaken to furnish more direct evidence that the cells of the motor cortex encode such coordinates defined by the consequences of muscular contractions and to provide a plausible mechanism as to how this is achieved. The relationship between an image and a motor program is detailed in Plans and the Structure of Behavior (1960). Essentially, the motor program is an operation which effects congruance between input and image.

As detailed above, there is thus the possibility that the motor cortex shares this ability to Fourier-analyze and predict locations of joints with respect to some externally constrained vector space - i.e., a task - as in Bernstein's experiment. Within such a vector space, fluency in control over a rhythmic central pattern generator by a motor program which must simultaneously, compute several equilibrium points related to one another by phase differences, is readily accomplished in the frequency domain. By contrast, such computations would be extremely tedious if not entirely impossible if ordinary Newtonian mechanical dimensions such as changes in length per se were encoded.

Thus, in a series of experiments we explored the possibility that the response characteristics of cells in the motor systems of the brain (such as the basal ganglia) might reflect changes in muscle activity in Fourier or related frequency terms. Further, the question was posed as to whether the cells of the cortex are so tuned as to provide a representation of the equilibrium points which specify the matrix of locations within which action takes place.

METHODS

Surgical Procedure

Six cats weighing from 3 to 5 kilograms were used. Each animal was anesthetized initially with an intramuscular injection of equal parts of Ketamin and Rompun (20 mg/kg) and placed in a stereotaxic frame. During the operation, anesthesia was maintained using Fluothane. A midline incision was made and the skin and temporal muscle reflected laterally to expose the dorsal surface of the skull. Two openings of 10 mm in diameter were trephined with stereotaxic coordinates so that an electrode passing through the center of one opening vertically would intersect the desired section. The coordinates for caudate nucleus were A15, L5, and H4-8; and for sensorimotor cortex were A27, L5, and H10-12. The approximate coordinates for the center of the opening over the caudate nucleus were A15, L5, and over the sensorimotor cortex were A27, L5. A block with two nuts was fixed to the skull on the midline and posterior to both openings. This was used to restrain the head during recording. The dura was left intact and a stainless steel chamber was held vertically over each opening and secured with a dental acrylic cement. Non-penetrating holes were made on the surface of the skull, and on half a dozen of these holes small screws were installed to made the bond between the cement and skull stronger. Prior to the application of cement the surface of the skull was cleaned thoroughly with dental cavity primer. The skin was closed with surgical clips anterior and posterior to the chambers-block-acrylic assembly. The chambers were filled with Elastomer. After the operation 300,000 units of antibiotic (Bicillin) was administered intramuscularly.

Animal Restraint and Recording Apparatus

The animal was placed on a half section of a 7" diameter PVC tube with openings for legs and padded with foam for one hour every day until he became familiar with the environment. After one week of posoperative recovery the animal was lightly Ketaminized (15 mg/kg) and placed on the restraint device with his head fixed to the stereotaxic device via two bolts to the block with two nuts on it. During the length of the recording the animal was kept lightly Ketaminized by intramuscular injections when it seemed necessary (see Figure 1).

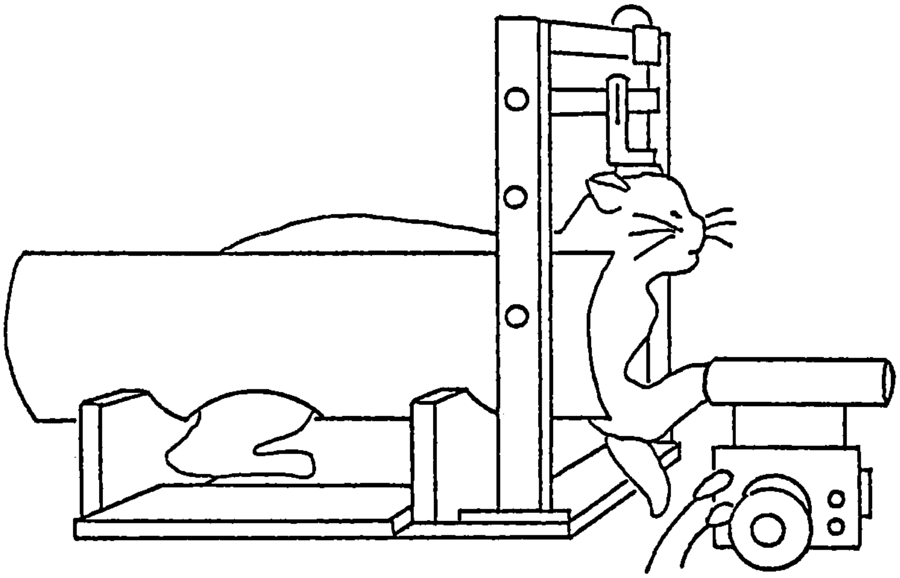


Fig. 1. Schematic diagram of restraining device for the cat while recording.

Epoxylite coated tungsten electrodes made by Fredrick Haer & Co. were used. They were 7.5 cm long and the diameter of tungsten was 0.010". The diameter of exposed tip was 15μ, the length of exposed tip was 50μ and tested impedance was 12± 2Mμ. The Elastomer was removed from the chambers and one electrode was lowered vertically until its tip touched the dura. Then the chamber was filled with Bacto-Agar solution. After this solution settled the electrode was pushed through the dura using hydraulic drive (Trent H. Wells, Jr. Mechanical Developments Co.) until the tip of the electrode reached the desired area.

Movement Apparatus

Movements were generated by a DC motor. The shaft of the motor was coupled to a 360° continuous potentiometer. The other side shaft of the potentiometer was coupled to a mechanical sine wave generator which decomposed a rotary movement to its vertical and horizontal components. Thus a constant speed rotary movement would be decomposed into a vertical and a horizontal sinusoidal movement. The frequency of movement was changed by changing the DC voltage applied to the motor. A load cell (Interface model 50-B) was connected between the moving section of the vertical element and the paw holder to measure the tension/load on the paw holder.

Data Collection

The extracellular activity was picked up by the microelectrode. It was amplified by a very low noise, very low bias current and high impedance preamplifier with a gain of 1000. The output of this preamplifier was then amplified (gain between 1 and 10), high passed filtered (f_o = 300Hz) and passed through a window discriminator which gave a pulse of fixed duration (1 msec) whenever the input exceeded a certain voltage.

The experiment started with a control period (C) when there was no movement. Following this control period there was a movement period where the right front paw was moved passively up and down sinusoidally. This was followed by another control period and the sequence repeated so that immediately before and after each movement period there were control periods. Each movement period was distinguished from the others by the DC voltage applied to the motor during that period. The voltages were 10, 15, 20, 25, 30, 35, 40, 45 and 50 volts corresponding to 0.27, 0.49, 0.71, 0.93, 1.11, 1.33, 1.48, 1.60 and 1.74 Hz. The sequence of these voltages was sometimes randomized to see if any significant change would occur. The control and movement periods were equal to 25.6 seconds, which we call a frame. Analog data (the outputs of the potentiometer and the load cell) were sampled at a 40 Hz sampling rate. Thus there were 1024 points per analog data channel per frame. The digital data (the output of the window discriminator) indicating neuron activity was recorded by counting the number of pulses between sampling instances (25 msec) using the interrupt system of the AR-11 system on the PDP-11/34A computer. Before starting to record from one cell, enough care was used to ensure that the cell would remain normally active during the whole recording period, and moreover, would have a reasonably high signal-to-noise ratio. The level of window discriminator was set according to the cell "height" for each individual cell and was untouched during the recording period for that cell. If during the recording period the cell became abnormal, the program was aborted and a new cell was "fished".

Anatomical Verification of Electrode Placements

When good recordings could no longer be obtained, the cats were given an overdose of barbituate intraperitoneally and perfused intracardially with normal saline solution followed by 10% formalin containing potassium ferrocyanide. The brains were blocked in the stereotaxic plane, removed from the skull and placed in formalin. Later they were removed to 20% alcohol. Frozen section were cur at 50µ saving every fourth section. These were mounted and stained with thionine.

All subcortical electrode tracts were found to traverse the head of the caudate nucleus. Two of the cortical placements were found to be post-cruciate; all others were pre-cruciate. The post-cruciate placements accounted for 9 recordings of narrowly tuned cells while the pre-cruciate placements accounted for 26.

RESULTS AND DISCUSSION

Frequency:

Three hundred and six cells from six cats were recorded and analyzed. Of these 144 cells were recorded from caudate nucleus and the remaining 162 from sensorimotor cortex. As can be seen in Figure 2, a sizeable number of cells in both caudate nucleus and sensorimotor cortex are selectively responsive to only a limited range of the frequencies with which the forelimb is moved. The response can be either in the form of increasing or decreasing the average spontaneous spike activity when a movement with a certain frequency occurs. Thirty-three out of 144 cells (approximately 23%) in the caudate nucleus and 35 out of 162 cells (approximately 22%) in the sensorimotor cortex were narrowly tuned to a specific band. Narrowly tuned is defined to be cell whose maximum or minimum activity at a certain frequency is at least 25% higher (lower) than the average baseline activity which remains relatively steady over the entire range of frequencies. Further, narrowly tuned cells are defined as responding only with a band width limited to less than 1/2 octave.

In examining the movement frequency tuning curves of cells, we find a wide range of maxima (minima). This range in tuning would be necessary if one were to build a system of band pass movement frequency filters which

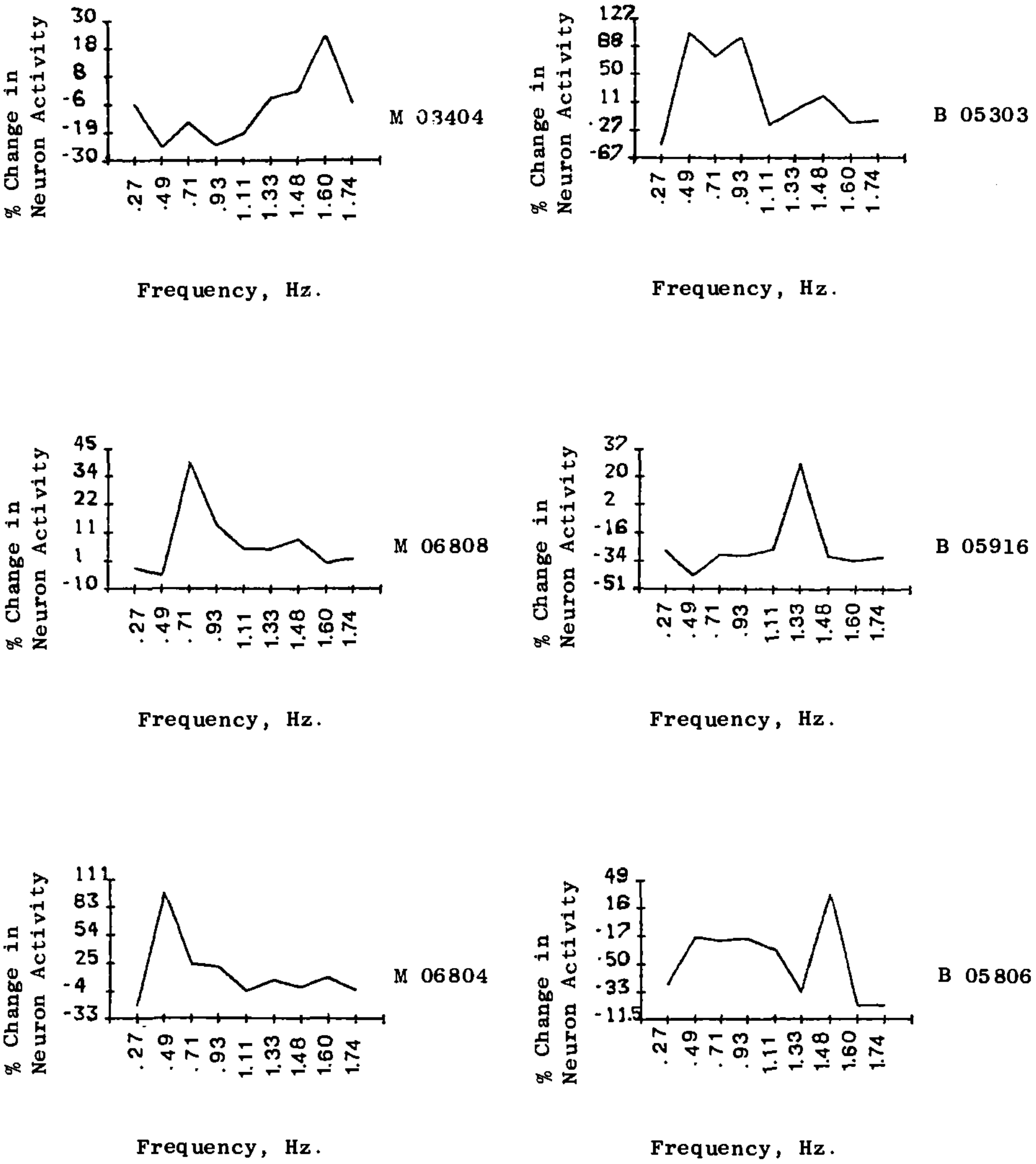


Fig. 2. Frequency tuning curves for narrowly tuned cells. The frequency of the mechanical sine wave generated by the motor is represented on the x axis; the % change in response over baseline is represented on the y axis. The six letter/digit alphanumeric code at the righthand side of the tuning curve gives the following information.

M 1 6 2 1 2

Recording site: Cat nr. Cell number for this site
B: Basal Ganglia
M: Sensorimotor Cortex

covered the whole range of frequencies. In support of such a filter model is the finding that all the remaining cells were not selective to a single frequency band, but rather showed maxima (minima) within several frequency bands.

Of 35 narrowly tuned cells in the sensorimotor cortex, 18 (11%) of the total of 162 cells, showed inhibitory responses to their tuned frequencies, that is, they acted like a notch filter (Figure 3). Most of these (16) and those which showed biphasic responses were obtained from placements which were shown post-mortem to be located in the post-cruciate gyrus. Only eight out of 44 or 5.5% of the total of 144 cells in the caudate nucleus showed similar behavior.

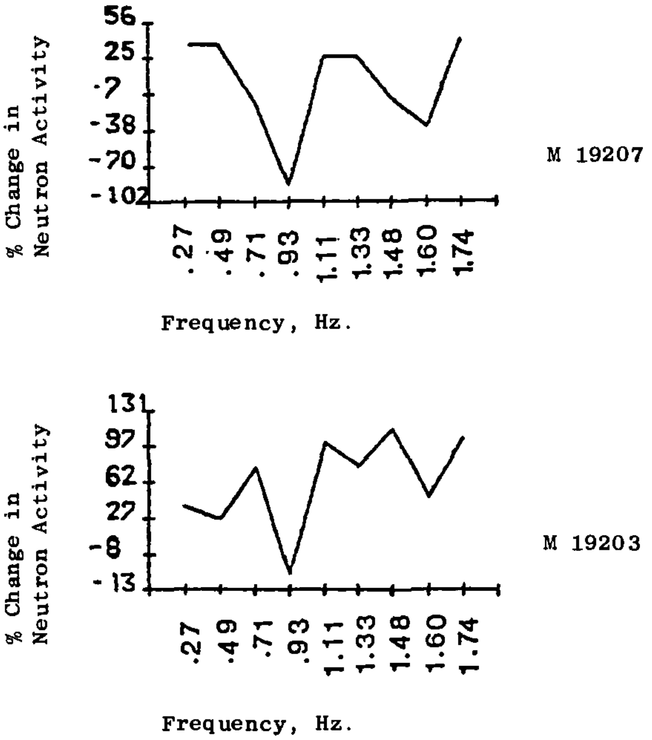


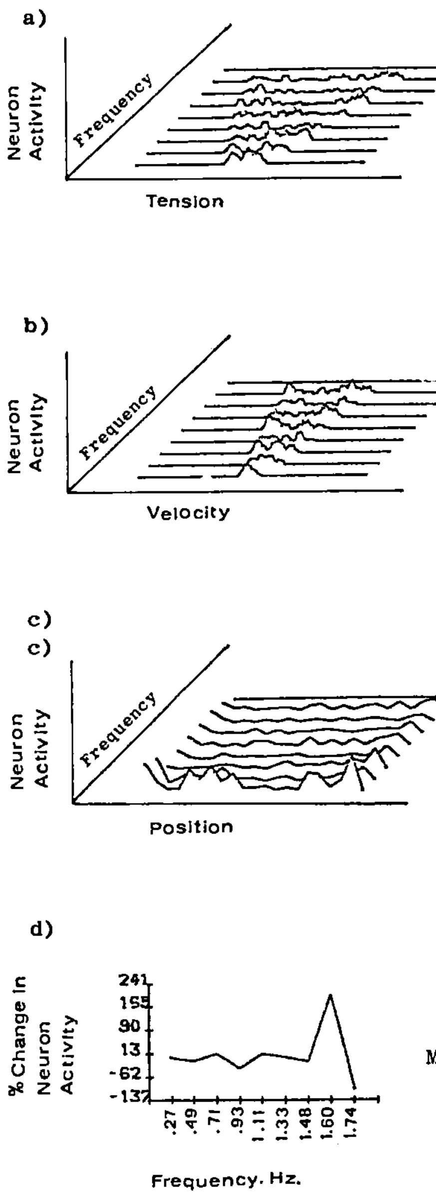
Fig. 3. Frequency tuning curves for narrowly tuned "notch filter" type cells.

The primary thrust of the experiment thus proved fruitful. There can be no question that some cells in the caudate nucleus and some cells in the sensorimotor cortex of the cat responded selectively to only a limited range of movement frequencies. The number of such cells compares favorably with the number of cells in the visual system which are classified as having simple or complex properties (Hubel & Wiesel, 1959; Spinelli, Pribram & Bridgeman, 1970), properties which have been shown to be related to the spatial frequency of visual patterns (Schiller, Finlay & Volman, 1976; DeValois, Albrecht & Thorall, 1978; Pribram, Lassonde & Ptito, 1981). In the visual cortex approximately 10-12% of cells are frequency specific, tuned to a range of 1/2 to 1-1/2 octaves; in the sensorimotor cortex approximately 20-22% of cells are tuned to a 1/2 octave band width of the frequency with which a forearm moves.

Although it is clear that some cells in the motor system "resonate" to movements at certain frequencies, the results presented so far do not indicate 1) whether these resonances are spurious in the sense that other variables conjoin to produce the effect, or 2) whether these resonances reflect the encoding of frequency per se as would be demanded by the model of a set of band pass filters for movement which was suggested above. In order to initiate the search for variables other than frequency to which these cells might be responsive three likely candidates were chosen: velocity, tension and position. The results of observations on these variables follow.

Velocity and Tension:

In order to test whether the frequency tuned cells were responsive to other variables, the neuronal activity of narrowly tuned cells was plotted against position in the cycle of movement, velocity of movement within cycles of various frequencies and the tension at different frequencies. As can be seen in Figure 4, the minimal increase in activity when velocity or tension are plotted is spread over all frequencies and fairly broadly within each frequency (especially at the higher frequencies). Since velocity and tension vary as a function of frequency, the nonspecificity of even this minimal increase in activity indicates that the cells were not responding to these variables. To make this clear, picture the velocity of movement when the frequency of the cycle is low: velocity is also low, the forelimb moves slowly. Now we increase the frequency of the movement cycle: velocity is now high, the muscles of the forelimb are contracting and lengthening rapidly. If velocity (or acceleration) were being encoded by these cells this would be reflected in a region of selective increase in their activity which would be localized within each frequency and the location of the increased activity would move in a systematic fashion across the various cycle frequencies. This



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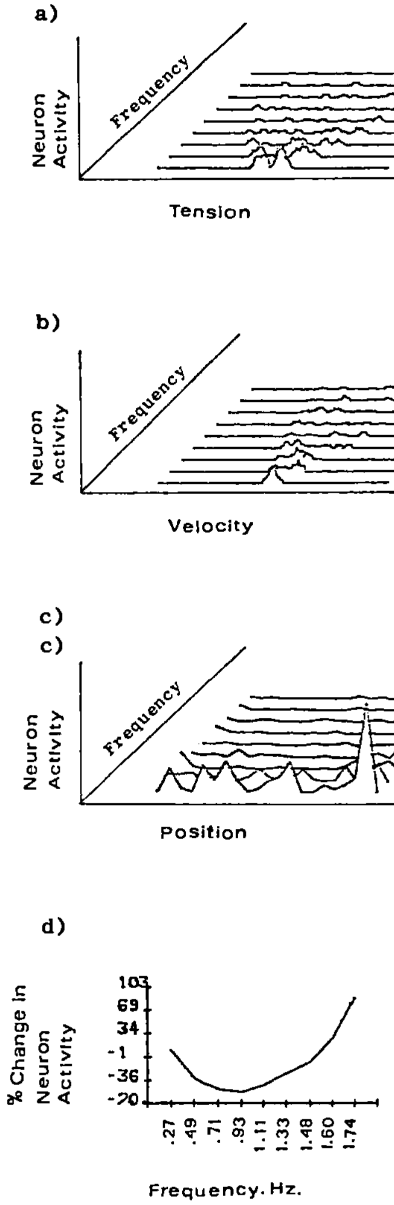

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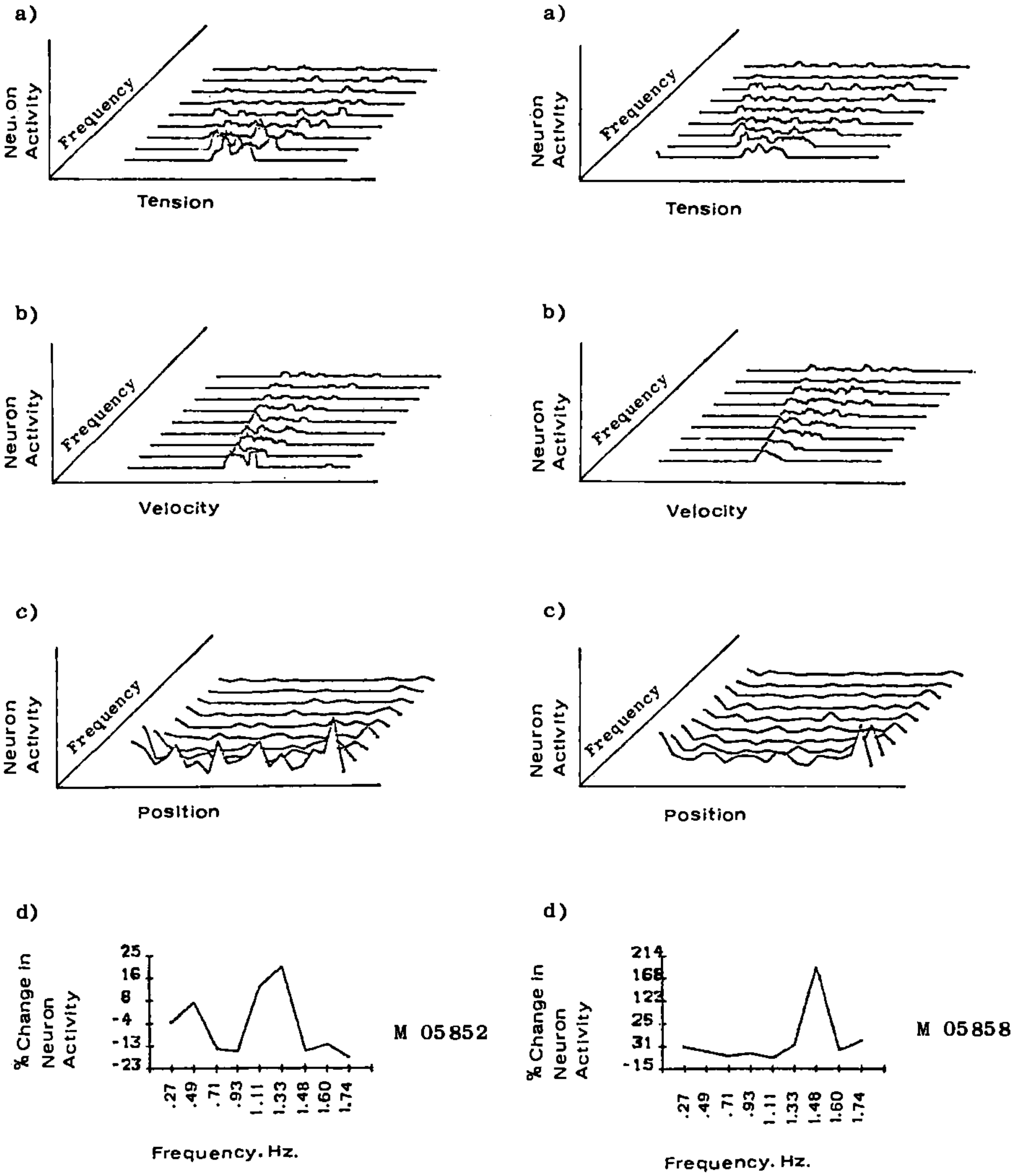

Fig. 4. Characteristics of some narrowly tuned cells:

a. Neuron activity versus tension (measured by the load cell) for each frequency.
b. Neuron activity versus velocity for each frequency.
c. Neuron activity versus position for each frequency.
d. Tuning curves for narrowly tuned cells.

behavior was not observed.

The same argument applies to tension. Response to tension and change in tension should be reflected in a localized increase in cell activity which systematically moves across the various cycle frequencies. This phenomenon was not detected. These frequency selective cells are insensitive to velocity and tension passively imposed on the muscles of the forelimb. This result appears to differ from results obtained by others (Brooks, Horvath, Atkin & Kozlovskaya, 1969; Evarts, 1968) who have found velocity and tension sensitive cells in the motor system. However, in their experiments active rather than passive movement was involved. Furthermore, in the current experiments no search was made for cells that selectively encode velocity or tension, although the non-narrowly tuned cells of the current sample were subjected to the same procedure and no velocity or tension specificity was found. The purpose of this part of the experiment was thus accomplished: it has been established that the frequency selectivity which was observed for this population of cells is not an artifact of their selective sensitivity to velocity and/or tension.

Position in the Movement Cycle:

By contrast, as can be seen in Figure 4, narrowly tuned cells in the sensorimotor cortex show sensitivity to the position of the forelimb in the movement cycle. This sensitivity occurs almost always at the ends of the cycle of movement where the maximum phase shift is taking place. But it must be emphasized that these cells are not sensitive to phase shift (or change in direction) per se since the increase in the cell's activity does not occur at all cycle frequencies. Rather, these cortical cells encode phase shift, i.e. position in the cycle, for a particular frequency band. This supports the hypothesis that these cells are indeed encoding frequency per se since at the encoded frequency phase shift is also responded to.

The encoding of phase shift was found to be a property primarily of cortical cells. Only one cell of the total population of those recorded in the caudate nucleus was found to encode position in the cycle and that cell was not maximally responsive to the ends of the cycle where the maximum phase shift occurs. Once again, this result is reminiscent of results in the visual system where cortical cells but not those below cortex show a selectivity to the orientation in space of a pattern. The results of the current experiments can be similarly expressed: below cortex the cells encode the frequency of movement regardless of phase; at the cortex, shift in phase - i.e. in the orientation of the movement at a certain frequency - is an additional consideration.

CONCLUSION

These experiments were undertaken to find out whether some cells in the motor system behave in a manner similar to some cells in the auditory system and in the visual system. Recall that in the introduction evidence was reviewed which suggested that a frequency analytic approach to sensory function in the nervous system has proved rewarding. Furthermore, evidence was reviewed that a similar frequency analytic approach to the motor behavior in the intact organism had been equally effective. It therefore seemed reasonable to apply this approach to an analysis of the motor systems of the brain. Thus the question was directly posed as to whether there are cells in the motor system which respond selectively within a range of frequencies of a cyclic up-down passive movement of a forelimb. The results of the experiment showed that a 20% portion of a total of 306 cells sampled are tuned (i.e. increase or decrease their activity at least 25% over baseline spontaneous activity) to a narrow (1/2 octave) band of the range of cycle frequencies.

Tuning could be due to a spurious convergence of factors relating to the basic properties of muscle as discussed in the introduction: metric displacement and tonicity or stiffness. An examination was therefore undertaken of variables related to these basic properties, variables such as velocity, change in velocity (acceleration), as well as tension, and change in tension. These factors in isolation were found not to account for the frequency selective effects. This does not mean that other cells in the motor system are not selectively sensitive to velocity and changes in tension. But is does mean that the frequency selectivity of the cells described is dependent on some higher order computation of the metric and tonic resultants imposed on the foreleg musculature by the external load.

The other variable investigated was position in the cycle of movement. Position is encoded by cortical cells (but not by caudate nucleus cells), but only at the site of phase shift and only for a particular frequency. This result supports the hypothesis that the cortical cells are in fact frequency selective in that any sensitivity to phase shift presupposes an encoding of phase and therefore of frequency. Furthermore, the fact that the cortical cells respond to position suggests that they are directly involved in the computation of the vector space coordinates within which actions are achieved.

There is thus no question that an approach to analysis of the functions of the motor system in frequency terms is useful not only in studying the overall behavior of the organism as initiated by Bernstein but also in studying the neural motor mechanism. That some such an approach is required is amply documented in a review of the field, initiated by R.B. Stein in an article entitled "What muscle variable(s) does the nervous system control in limb movements?" which became available in the December issue (Vol. 5, No. 4) of The Behavioral and Brain Sciences just as our chapter was being sent to press. In the peer commentary the following statements appeared:

Discussing servo assistance in the regulation of muscle contraction by way of their afferents, Granit (1982) points out that an "error has no existence in midair" and that thus "an error detector can act sensibly only if it is related to the purpose of the movement in which it operates. ... Clearly the concept of error is tied to the concept of goal. More often than not, the nervous structure incorporating the goal will be cortical".

Masao Ito (1982) takes this argument a step further: "...There is no straightforward extrapolation from elementary reflexes to complex movements such as postural adjustment, locomotion, skilled hand and finger movements, or speech. An idea diametrically opposed to the Sherringtonian view may be that the CNS is basically a multivariable control system handling a set of parameters, and that only in an extreme condition, in which all parameters but one are fixed, does it appear to have a reflex for that single parameter". He adds that "Perhaps we have difficulty in understanding the mechanism of motor control because we still know little about the structure of the CNS as a multivariable control system, about how the operation of the system is organized toward a certain superordinate goal above the level of the various parameters, each of which may be related only partially to the goal".

Partridge (1982) discusses the requirements which such a superordinate organization must meet: "Motor control, like operations of other mechanical systems has been treated in terms of force, velocity, position, and mechanical impedance. It is almost never described in other equally valid coordinate systems from mechanics, those with dimensions such as momentum, power, work, kinetic energy, potential energy, etc. (e.g. Brooks, 1981). In favor of such an alternative coordinate systems is the constancy, over a wide range of velocities, of the relationship between neural drive and power output from muscle. Similarly, neural drive adjusts the ratio of energy absorption with muscle stretch also independent of stretching velocity (Partridge & Benton, 1981). Yet these alternative coordinate systems are almost as limited for describing motor control as the elementary Newtonian descriptions. Encouraging these forms is the unrecognized assumption that evolution must have chosen to operate in one of the coordinate systems from a mechanics text while ignoring the infinte variety of other equivalent systems. Evidence from the study of receptor responses indicates that, at least at the input level, mechanical information is carried in a non-Newtonian frame of reference (Partridge, 1978)".

Partridge (1982) goes on to suggest that the only way that such a frame of reference can be properly "described as a single variable using conventional dimensions is by choosing for that variable the magnitude of a vector represented nonlinearly within those dimensions". In those coordinates the orientation of the vector depends more on load than on neural activity". Thus, "The neural representation of the problem requires conversion into a neural representation of the solution by a single set of transformation rules - a difficult enough problem without the necessity of transforming the signals into arbitrary intermediate coordinate systems".

Stark (1982) addresses the problem of what those transformation rules might be. He points out, as does Stein in the target article, that servo control theories as initially proposed by Merton and embellished by reference to alpha-gamma coactivation, have their limitations. His experiments have led him to embrace sampled-data control theory in which "the actuator intermittently moves the plant in an open-loop mode, alternating with a closed-loop mode of control". Ashby (1960) and Pribram (1971) have suggested that open-loop, feedforward operations of this sort coordinate the operations of several closed loop servos by virtue of the parallel processing configuration of neural control. Such a configuration demands the establishment of optimization criteria to operate its control-signal structure, just as the simpler servo loops demand goals in order to determine what might constitute an error. Stark notes that "Recent studies in robotics suggest that control of hand movement, for example, is apparently much simpler in the three dimensional visual-motor world than in the complex kinematic joint-angle, muscle-force, and muscle-length world. This is particularly true because the multidimensional interactions among changing interrelations with varying loads and the corresponding calculations become overwhelming, even for a good sized computer".

Terzuolo and Soechting (1982) come to a similar conclusion in reviewing their own work: "Perhaps if one were ...to concentrate on the rules which govern the organization of different types of movements, the nature of the control process would begin to unfold. Indeed, both the organization and control of movement are aspects of the same problem, which can only be addressed by examining the global properties of the system during tasks with specific constraints and under physiological conditions".

What these global processes, these constraints which are best described in terms of the visuomotor spatial world might be is the topic addressed by Stelmach and Diggles (1982). These investigators point out that the most convincing evidence for the shortcomings of attempting to understand the motor control process by reductionist techniques which isolate motor units or groups of units from each other, is the evidence provided by motor equivalence. They note that, for example, "individuals retain the same unique style of writing with a pen and paper or with chalk and board (Stelmach & Diggles, 1982)". Transfer functions must describe progressive hierarchies of control which become distributed among "collections of neurons that may interact to achieve the desired goal". Stelmach and Diggles do not specify what these transfer functions might be but suggest that "The mechanisms which provide input to these distributed control systems are neural maps. They are viewed as control surfaces from which the spatial-temporal patterns for movement may be derived. Neural maps are used as control surfaces, with the map providing information about the topographical relationship one item shares with several others. Thus information from one item will provide relative information about other items, suggesting a certain economy in storage and a basis for cooperative computation. It is then plausible that a layered motor controller exists in which position of input on the control surface, determined by the 'center of gravity' of an array of activation points, encodes the target to which musculature will be directed. In this manner each activation point contributes to target determination, and responsibility for the course of movement is distributed among these points".

Stelmach and Diggles' (1982) control surfaces, their neural maps, have identical properties to the "Images of Achievement" which were suggested to be constructed by the operations of the motor cortex by Pribram (1971). Further, the proposal was made that such maps of Images with distributed properties resulted from the operation of transfer functions which transformed the Newtonian dimensions based in space/time coordinates into phase operations in the frequency domain. Such transformations are known as spread functions since they distribute information across a surface. The proposal was derived from the work of Bernstein, much as reviewed in the introduction to this chapter.

A similar approach is taken by Kelso and Saltzman (1982) who, though they make important additional points, fail to come to grips with the nature of the transfer functions by which any control system has to operate. Nonetheless, their contribution is an important one: they affirm the spontaneous oscillatory aspects of neuronal functioning which demand more than feedback regulation and homeostasis. Emphasizing the mass-spring model of muscle-joint operation with its controllable equilibrium length, they discuss the homeokinetic and equifinal properties of such systems. In such systems "control is dynamically effected by means of coupled ensembles of limit cycle oscillatory processes. Limit cycles represent the only temporal stability for non-conservative, non-linear systems... Limit cycles are manifestations of thermodynamic engines and quantize action (formally the product of energy and time; Iberall, 1978) at every level of the system".

Kelso and Saltzman (1982) indicate that "For spectrally distributed limit cycle regimes to be observed and for new spatiotemporal organizations to emerge, certain necessary conditions must exist. Among these are the presence of many interacting degrees of freedom, nonlinearities, a relatively constant source of potential energy, and the requirement that the energy be dissipated. Given such conditions, and subject to critical scaling values, constraints emerge that are capable of mashalling the free variables into coherent functions". Kelso and Saltzman propose that the problem of motor control "might be attacked more effectively by seeking out a single set of physical principles that can apply at all levels, rather than by positing different units at all levels".

We believe that the results of our experiment as reported in this chapter show that we met "the certain necessary conditions" for "spectrally distributed limit cycles to be observed". We also believe that it is the transfer functions which transform the variables operating in Newtonian mechanics into those operating in the frequency domain, which are the "single set of physical principles" which allow neural control systems to operate as they do. The fact that such transfer functions are spread functions, and that the overwhelming conclusion of the analyses reviewed in the article in The Brain and Behavioral Sciences is that control is a function of distributed variables in distributed systems, additionally supports this conclusion.

Nonetheless, the results regarding the motor systems of the brain presented here are but a beginning. The question remains as to whether the frequency transform shown to operate at the forebrain level is linear. In order to show that the Fourier or any other linear process is involved in motor behavior and the motor functions of the brain, the requirement of superposition must be met. Thus, the interactive effect of two cyclic sinusoidal frequencies of motion of the forelimb need to be tested.

Furthermore, all of the experiments run in this series used passive movement. Much movement is active and the question remains unanswered as to whether the frequency approach is directly relevant to active movement. The results of the current experiments do indicate, however, that the frequency approach can yield useful results in understanding the coordinate matrix which passive movements (and posture, temporary equilibrium positions of phase shift) provide to form the context within which active movements are undertaken.

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CHAPTER IIb

FROM ACTION GESTALTS TO DIRECT ACTION

E.S. Reed

INTRODUCTION

If he had published nothing else but "The problem of the interactions of coordination and localization" Nikolai Bernstein would still be one of the most important students of bodily movement in recent history. Although he discussed in detail only two problems and emphasized the simplified case of coordinating a single limb-joint system, a science, like a movement "never responds to detailed changes by a change in its detail; it responds as a whole to changes in each part" (p. 84). The general outlines of the theory of action have been affected profoundly by Bernstein's important detailed insights.

It is both surprising and humbling to read this essay - written half a century ago - and find in it still-provocative analyses of some of the most contested issues of contemporary movement science. Bernstein anticipated (and often improved upon!) recent discussions of distributed brain function, the articular system as a "mass-spring" of sorts, the centrality of phase relations to understanding bodily coordination, the importance of understanding the functional hierarchies of the nervous system, the fundamental role of "sensory" processes in "motor" control, the importance of dealing with bodily movements and postures as a whole, not merely with motions of sections of the body.

The various detailed contributions made by Bernstein in this chapter are brought together by his principle of the "lines of equal simplicity" (p. 112). This principle still has important implications for both theorists and researchers today. In the first part of this response I will use this principle to highlight Bernstein's own achievements. In the second part I will show how the lines of equal simplicity principle can help to deepen and extend Bernstein's own ideas.

BERNSTEIN'S ACHIEVEMENTS

The Principle of Lines of Equal Simplicity

"Our experimental aim at present", Bernstein wrote, "lies in the correct formulation of categories which are really represented in the brain centers" (p. 96). Our goal still is to discover the proper taxonomy of action: what movement tasks are really of the same kind, and what kinds of actions are truly different? As simple-minded as this sounds it is tremendously important experimentally and theoretically. A line of equal simplicity is revealed when a "transition from one element of the set of possible tasks to another" has a negligible effect on performance (p. 112). "Simplicity" here is used as a general term, meant to cover such empirical variables as "speed of completion, degree of accuracy, degree of variance and so on" - in short, empirical measures of a movement's intrinsic structure and function (p. 112). For example, it is equally simple for most of us to write at various scales with the same (preferred or non-preferred) hand, although these tasks involve totally different sets of muscles. However, it is not equally simple for most of us to write with our non-preferred as opposed to our preferred hand. This is not some absolute simplicity (cf. Bernstein's note at p. 115) but an empirical one. It is likely, judging from some of my own informal observations, that the phasing and timing of writing movements varies negligibly with changes in either scale or lateralization, whereas the accuracy and variance of the traces will vary with lateralization far more than with scale changes.

At the beginning of this chapter (pp. 77 - 81), Bernstein uses a logical argument, based on classical mechanics, to show that animal movements are neither passive responses to environmental influences or to commands from the brain. Following this, Bernstein uses his principle of equal simplicity to show how properly to classify and describe movements without assuming that individual muscles are commanded to act or called into play by reflexes. The argument is straightforward and widely accepted by modern researchers (who often, however, try to avoid its implications): if the brain controls movements by means of controlling the particular muscles involved in them, then transfer experiments in which new muscles and nerve centers are involved in achieving previously learned effects should prove difficult. As Bernstein notes (p. 96), Lashley's (1929) experiments show that considerable compensation is possible. The experiments (unknown to Bernstein) of Gibson

(Gibson, Jack & Raffel, 1932; Gibson & Hudson, 1935) and Wickens (1938) directly confirm this hypothesis: learned movement patterns (in these experiments, conditioned reflexes) transfer easily to alternate muscle groups. Unlike Lashley, however, Bernstein did not use this point to argue against localization; on the contrary, he used it to help explain what it is that is controlled in action (p. 93).

What is Specified by "Motor Commands"?

If specific muscles are controlled by specific brain centers, then learned functional movements (conditioned responses or more complex actions) which are equally simple when transferred to two or more muscle groups cannot be so specifically controlled. "The hypothesis of cellular localization of muscles necessarily leads to a denial of cellular localization of conditioned reflexes" (p. 95) and any other higher movement functions. With some caution, Bernstein uses this ingenious argument to move outside of the sterile localizationist debate into the far more important issue of what functions must be controlled if coordination is to be achieved. Anticipating modern theories of "distributed control" (Arbib, 1981) and neural redundancy (Diamond, 1979) by five decades, Bernstein raised the problem of sensorimotor integration as follows. Stimulation at the periphery before and during movement is carried to diverse regions of the CNS (Merzenich & Kaas, 1980); sensory, motor and association cortical regions are all multiply interconnected, embodying redundant representations of the periphery (Asanuma, 1981; Diamond, 1979; Masterton & Berkley, 1974). Therefore, "we are obliged to confront (coordination) as a result of the mutual activity of entire systems or organs which, anatomically and clinically, display varying degrees of independence" (p. 90). This seems bewilderingly complex to those who - like all classical and many modern motor theorists - think of actions as comprised of specific muscle movements caused by specific motor commands.

Once again, Bernstein applies his principle of lines of equal simplicity: it would be incredibly complex to control each and every individual muscle of a body and keep up to date on their proprioceptive inputs as well (Turvey, Shaw & Mace, 1978). But the task of the nervous system is not to produce specific commands to achieve specific movements, because "a determinate effect is possible for a movement only where the central impulse is very different under different conditions" (p. 81). Taken separately, the idea of diverse innervation patterns producing specific movements or diverse neural centers with variable involvement coordinating into specific actions seems complex and confusing. Taken together, these concepts support each other, and begin to offer a fruitful new way of looking at the functional principles of the motor system.

In this chapter Bernstein did not take his concept of mutual or distributed control farther then a critique of "keyboard" theories of brain localization. In the end, he himself appealed to the concept of a "motor engram" which embodied an "image of achievement" - the intended movement pattern. Although he was certainly aware that no motor engram alone could determine a specific movement pattern (not without the cooperation of other brain centers and a properly integrated proprioceptive system - see p. 101) Bernstein nevertheless wrote of this motor image or engram as though it were a new kind of motor keyboard - as if motor commands were not commands to muscles, but something closer to motor gestalts. The historical comparison with gestalt theory is extremely close, and worth some consideration.

Bernstein's Gestalt Theory of Movement

This chapter played the same historical role in the theory of motor control as Wolfgang Köhler's pathbreaking "On unnoticed sensations and errors of judgment" (Köhler, 1913, 1971) did in the theory of sense perception. Somewhat oversimplified, Köhler's argument was that the specific effects of sensory stimulation could not give rise to determinate perceptions. Köhler showed that, if sensory inputs have constant effects, then actual perceptions cannot be based on sensory input alone (e.g., the perceptual effects, the perceived color, of a specific optical wavelength vary depending on the surrounding wavelengths) or one must give up the idea of sensory constancy altogether. (That this argument is strictly analogous to Bernstein's is demonstrated by the latter's critique of Helmholtz's resonant harp theory of hearing. Bernstein uses this critique to illustrate his principle of lines of equal simplicity and in so doing recreates Köhler's argument against Helmholtz). To explain perception one thus has to resort to (often ad hoc) hypotheses about non-sensory "cognitive" processes which correct and adjust the sensory input.

Bernstein argued that if motor processes cannot determine specific movements, then one must appeal to non-motor (proprioceptive or cognitive) processes to explain action, or abandon the hypothesis that central commands have univocal peripheral effects. As mentioned above, Bernstein explicitly states this as the dilemma for localizationists: either muscle movements are univocally commanded and more complex movements are not, or vice versa, but not both (p. 95). It is for this reason that Bernstein invented the gestalt-like concept of a motor engram of the "image of achievement" which he thought of as a central determinant of specific complex movements (p. 100).

Like Köhler, Bernstein developed a form of neural isomorphism theory. Speaking of the motor engram he wrote, "it is necessary that there should exist in the supreme nervous organ an exact representation of what will later occur in the periphery" (p. 101). Such a representation would be able to give rise to determinate commands for specific movements, although at the cost of having to encode a practically limitless amount of environmental and peripheral contingencies. There is no evidence that such specific representations do exist, and Bernstein's own arguments about the non-univocality of central commands for muscles calls the very possibility of such engrams for the muscular details of complex movements into question. Bernstein's theory here is limited in a way quite analogous to Köhler's - both fail to recognize that perceiving/acting is not a relation between the bodily periphery and the CNS, but an inter-relation of an animate organism (with both sensory and motor processes) and its surroundings. (Bernstein and Köhler also failed to realize that it is environmental information that is specific to perception and action, not internal representations, but the arguments to demonstrate this cannot be developed in this small space - see Gibson, 1966, chaps. 2 & 4; 1979, section II; Reed & Jones, 1982, chaps. 4.6 and 4.7).

Just as Köhler realized that specific properties of sensory processes cannot be the basis of perception, so Bernstein realized that the specific properties of motor processes could not be the basis of action. Yet neither of these thinkers were able to extend their insights by re-thinking the concepts of sensory and motor processes to take into account the implications of their arguments. Both these writers assumed that if particular sensory/motor processes did not lead to specific perceptions/actions, then perhaps multiple sensory/motor processes - such as a motor engram - could fit the bill. Bernstein, far more than Köhler, acknowledged the need to rethink the whole sensori-motor concept, but on the fundamental issue of whether action/perception is based on efference/afference both Bernstein and Köhler failed to reject incorrect orthodoxy.

Are there "Motor Commands"?

Bernstein's own arguments can be used against his own position in this instance. If a "central command" has no univocal peripheral effect, then it is not a "command" but a variable factor or influence. (Similarly, because proprioceptive "signals" have no univocal central effects, they are not "signals" as Bernstein claims). Furthermore, a combination of these variable factors is still a variable factor - not leading to a specific action - unless those factors which underlie invariant results are also included. Yet, as Bernstein himself proves in this chapter, any specific action is in part determined by the environmental field of forces, initial conditions of the movement, and perceptual factors. A "motor engram" that does not include these factors cannot yield invariant results - as Bernstein himself later argued (see p. 445). It has been found, to mention a single example, that the illusory perception of bodily support or non-support results in postural adjustments similar to those engendered by actual support or non-support (Nashner & Cordo, 1981).

The goal of research on action should be to explain how specific kinds of actions are determined. By accepting that such entities as 'motor commands" existed, Bernstein in 1935 placed many crucial factors in the process of coordination outside of what he would have called action, locating these factors in the environment or in the perceptual process. Later on, when he began to replace the concept of the motor engram and the motor command alike with the "motor problem" (p. 344, p. 445) Bernstein began to rectify this mistake. In his later work, Bernstein began to see than the implication of this early essay was not that one should replace muscle localizationalism with a gestalt theory of movement, but that one should abandon altogether the idea that within the motor system proper there are "commands" in the sense of patterns of efference that specify peripheral results. This difference is of the greatest theoretical significance. I have chosen to characterize this distinction between the earlier and later Bernstein as the difference between a theory of indirect action on the environment and a theory of direct action.

TOWARDS DIRECT ACTION

Bernstein's great insights in this chapter show how powerful his principle of lines of equal simplicity was in revealing new and fruitful ways of understanding bodily movement. Yet, as I have stressed above, Bernstein's conclusions left unresolved a number of problems which forced him and other students of action to extend and modify his early ideas. Most importantly, Bernstein began to replace the motor engram concept with a more functionally defined concept of the "motor problem". Elsewhere I have argued that this insight needs to be deepened into a full-fledged account of actions as specific to biologically evolved functions of an animal in its environment (Reed, 1982; in press). I haven't the space here to argue this case in full, so I will instead merely explain the significance of this transition from an indirect to a direct theory of action, without attempting to justify a theory of direct action.

According to David Hume (1779, 1948, p. 186) "no animal can move immediately anything but the members of its own body". As William James (1890, II, p. 480) puts it, "the only direct outward effects of our will are our bodily movements". This assumption should be called the theory of indirect action, because it holds that we act on our environment only indirectly, by causing our bodies to move. This theory, which goes back at least to Descartes' Passions of the Soul (1649; see Lawrence, 1972, chaps. 2 & 3; Reed, 1982b), underlies most of our thinking about action. Bernstein's arguments showing the impossibility of univocal peripheral results of central "commands" should have led him to reject this entire approach, but even he did not see this implication in 1935, and he only saw it dimly as late as in the 1960s.

Animals act, I would argue, by changing the relations between themselves and their surroundings. These changes are effected by bodily movements and postures, but we do not ordinarily move our bodies in a specific way and thus cause the desired change. On the contrary, we cause the specific desired change by whatever movements and postures are needed to get the job done (see Reid, 1785/1969, p. 50). The specificity of the action lies in the meaningful changes wrought, not in the pattern of the movements made. This theory, by contrast to the above, should be called the theory of direct action on the environment.

Most theorists have seen some merit to thinking of actions both as bodily movements and as their external effects. This has led to a number of variations on the idea that actions are bodily movements plus something else - learned habits, volition, goals, and so on. This something else is supposed to ensure that specific movement patterns also have specific desired environmental effects. In his essay of 1935 Bernstein favored the idea that actions were movements caused by motor engrams embodying images of achievement - and even in his work of the 1950s and 1960s he still maintained something like the concept of a mental goal as causing movements and thereby creating specific acts (the "sollwert").

Yet if an action is a specific way of altering an animal's relation with its environment, then what makes it just that action is the precise change effected, not the mechanisms or motions leading to the change. Nothing about the causes of a movement can make it bear a specific function - except that it fulfils the desired goal or purpose. The lack of morphological stereotypy of natural movements has been amply demonstrated time and again (so much so that many ethologists treat "fixed action patterns" as - at best - statistical distributions of a varying pattern).

The mistake made here by Bernstein is to reason as follows: when an animal exhibits a definite movement habit, then all the determinants of that habit must exist in the CNS "in a hidden, latent form" (p. 98). A potential, the ability to act in a determinate way, is thus turned into an actuality (a latent engram) and the actuality is claimed to cause all instantiations of the potential. (This mistaken argument concerning action has been subjected to a book-length critique by Lawrence, 1972). This is a muddle into which all existing theories of indirect action seems to lead. The solution requires abandoning the assumption that the brain causes the body to move and that actions are the environmental consequences of such movements.

The determinants of a specific action (any specific environmental accomplishment of an animal) are a complex relation of the forces in the environment surrounding the body and the initial physical and biological conditions of the animal. Moreover, if animals have access to perceptual information specifying their relations with their surroundings, and if they can use that information to regulate their posture and movement, then one can explain action without any appeal to "commands" causing specific effects. On this view, perceptual information serves to establish ranges of tolerable variation for the action system. For example, Lee and his colleagues (Lee, Lishman & Thomson, 1982; Lee & Reddish, 1982; Lee & Thomson, 1982) have shown that optical information specifying the time it will take an observer to contact an environmental surface is used by animals and humans to regulate the pace of their locomotion. Because optical information is not in the nervous system (but in an external energy field) it cannot issue any commands at all to the neuromuscular system (which would not respond invariantly to the same influences at different times anyway). However, the availability of such information allows active observers to regulate relevant parameters of their behavior (e.g., force output, rhythm, timing) according to the specific demands of a situation.

The theory of direct action is still in its infancy and relies on a number of hypotheses that require further testing. Gibson's theory of ecological information as specific to an animal's environmental situation (Gibson, 1979, Section II) still needs further development. Lee's hypothesis that information parameters can constrain movements has been tested only for a small number of cases. The simple-sounding idea of treating action as an animal's means of directly affecting its surroundings needs to be developed through experimentation on functional parameters of movement - an area consipicuously under-researched at present. Here Bernstein's lines of equal simplicity principle will be very fruitful: we need to design experiments that test whether actions are indeed functionally specific - whether it is more "simple" for animals to accomplish the same movement or to accomplish the same environmental effect. For example, Forster's (1982) studies of jumping spider predation suggest that the pattern of each pounce is adjusted to the "ballistics" of each case, and Olive's (1980, p. 1134) work showed that the attack mode chosen is specific to the size, shape, and behavior of the prey. These spiders apparently do not jump in order to predate, they predate by jumping.

CONCLUSION

Bernstein's great contribution to movement science in this chapter was to use his principle of lines of equal simplicity to raise profound questions about what action is, and how it is controlled and coordinated. If I have argued here that we must go even farther than Bernstein in reconceptualizing action, it is only because his own principles can still be developed in fruitful ways. When Bernstein wrote this chapter - and to some degree this is still the case - action was conceived as the passive response of the body to environmental stimuli or to central commands. It is only because of Bernstein's insights that we now realize that animal movements and postures are cooperative phenomena, resulting from an active interlocking of forces and processes throughout the nervous system, the bodily periphery, and the environment as well. Current research on the coordination of complex movements, on phase relations in action, on complex actions as the result of abstract "schemata" or "images", on the intricate sensori-motor integration of the spinal segments and higher centers, on the redundancy of cortical sensori-motor representation, on the central anticipation and "tuning" of peripheral motor processes - all this important work was prefigured in this chapter from 50 years ago.

Less obviously present in this essay, but nevertheless visible dimly, is the idea of direct action - of action as being the means whereby animals change how they relate to their surroundings, not how the CNS causes the limbs to move. Bernstein used his lines of equal simplicity argument to disprove the classical idea that central commands cause specific peripheral movements. I have argued that this same principle refutes even the idea that what is commanded are specific complex peripheral movements. The very idea of a "motor command" is called into question by Bernstein's principle. Action systems would seem to function not merely by outflow, or even by outflow as modified by feedback, but via a circular process of adjustment in which parameters of information set constraints on action. If this information is specific to the animal's situation, then the action so constrained will also be specific and appropriate, despite the lack of outflowing commands or inflowing signals. The division of labor in the nervous system is not between receptive (sensory systems and effective motor systems, but between information-seeking perceptual systems (themselves involving both afferent and efferent processes) and environmental-adjustment seeking action systems (involving both efferent and afferent processes as well). To come to understand how such action systems work we can do no better than to follow Bernstein in experimentally seeking the functional properties of purposeful action in the environment.

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SECTION 3

Chapter III **BIODYNAMICS OF LOCOMOTION**
N. Bernstein

Chapter IIIa HOW CONTROL OF MOVEMENT DEVELOPS
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CHAPTER III

BIODYNAMICS OF LOCOMOTION

N. Bernstein

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1. SOURCE MATERIAL, POINTS OF DEPARTURE, TECHNIQUES

On beginning the cyclogrametric investigation of locomotion in 1926, we set ourselves a very restricted and practical aim: the investigation of force phenomena at the centre of gravity of the whole body in walking and running in order to deduce dynamic coefficients for use in the design of footbridges*.

In subsequent years we came to the conclusion that locomotion could also provide very extensive and interesting material for investigations from the physiological point of view.

The advantages of locomotion considered as an object for investigation of the processes of movement are essentially as follows.

Firstly, locomotor movements, in particular walking, are amongst the most highly automatized of movements. The most rigid succession of all details is followed from cycle to cycle and these details are extremely repetitive for each particular subject. This allows one to fix one's attention and to adopt constant criteria for the discrimination of the random from the regular.

Secondly, locomotor movements display an extremely widespread synergy incorporating the whole musculature and the entire moving skeleton and

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*The principal result of this research was the finding that the amplitude of vertical dynamic forces at the centre of gravity of the body in walking, in other words, the amplitude of vertical reactions of the supporting surface is proportional to the square of the step frequency: $A= \pm P.Qn^2$, where P is the static weight, n the number of single steps per sec. $Q = 0.095 \pm 0.005$ for normal gait. In the case of carrying a load (up to 120 kg) on the back the value of Q rises to 0.100-0.110. Running is characterized by the inequality $A > P$; in fast running the maximal values of vertical dynamic force can reach more than $3P$. The longitudinal components of force in walking and running depend on the length of the step, and we could not find simple expressions for them.

bringing into play a large number of areas and conduction pathways of the central nervous system. We may therefore expect in them the most clear-cut reflection of the interaction of central and peripheral processes, with a great abundance of detail characteristics of the process of movement in general.

Thirdly, locomotor movements have generality. Their selection as an object of investigation assures the investigator of a large number of subjects who have all mastered, for example, the act of walking incomparably better and more completely than any of their individual professional skills. The beginning and the development of locomotion in normal subjects is achieved with no less regularity and orderliness than is found in morphological development and the development of tissues and organs. This generality and regularity guarantees material for broad comparisons and opens the way to a study of the ontogeny of movements, their growth, establishment, development and involutional disintegration.

Fourthly, locomotor acts belong to the category of extremely ancient movements. They are phylogenetically older than the cortical hemispheres and have undoubtedly affected the development of the central nervous system in the same manner as have the distance receptors mentioned by Sherrington; for example, some clinicians have recently assumed a direct connection between the development of the corpus Luysiand the transition from quadripedal locomotion to the erect posture. This antiquity assures for the investigator the existence of deep organic connections between locomotor processes and the most varied structural levels of the central nervous system and allows us to hope that we shall observe in the very course of the locomotor act traces of these different levels and of successive phylogenetic stratification.

Fifthly, and finally, locomotion presents an unusually stable and typical structure. This could not be assumed in advance; but it had already become apparent to us by 1934 that all the basic details of normal walking may be found in all normal adult subjects without exception, and that individual differences between subjects depend neither on differences in the structure of the locomotor act nor on the assembly of details encountered in it, but occur only in the rhythms and amplitudes of the ratios between these details. This result allowed us to build up a detailed nomenclature for the structural elements of the locomotor act*, incorporating many dozens

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* *See Investigations into the Biodynamics of Locomotion*, Vol.I,Ch.III,1935.

of stable phenomena which were invariably characteristic for each adult normal subject. The material collected by us (21) showed that the stability and generality of the structure of the locomotor act is even more widespread than we could then suppose, and that its roots may be clearly followed in early ontogeny and phylogeny in qualitative changes of locomotion, such as running or marching, and finally in a number of pathological disturbances. This will be discussed later.

All these circumstances-automatization, degree of synergy, generality, ancientness- and their staibility of character make locomotor processes extremely favourable objects for investigations in the general physiology of movements.

In order to make clear in what lies the main interest in movement as a physiological topic we may recall the general features of its structure.†

The movements of an organism from the mechanical point of view arise because of changes in the conditions of equilibrium in the force field encompassing the animal's organ of movement.

In the case of spontaneous movement equilibrium is destroyed because of redistribution of tensions in the animal's muscles, within the organ of movement itself or outside it. Once the movement of an organ has begun changing both its position in relation to external forces (the force of gravity is foremost) and the degree of tension of the muscles connected to this organ, the relationships between the forces in the surrounding field continue to change until they approach to, or move still further away from, a state of equilibrium. During this process changes in muscle tension bring about a movement and the movement affects the condition of the muscles by shortening or stretching them causing further changes in their tension. We call the reciprocal connection which we have described here *the peripheral cycle of interaction.*

A mathematical analysis of similar relationships between forces (of the muscles) and movements (of the organs) shows that this form of interaction *does not presuppose a one-to-one correspondence between force and movement,* that is, that one and the same sequence of changes in forces may produce different movements on successive repetitions. This absence of one-to-one correspondence is due to the fact that the biomechanical relationships between forces and movements may be expressed in the form of a differential equation of no lower than the second order, demanding among the conditions of its solution no fewer than two values independent of the equation itself.

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† The thorough analysis here has relationships with our report The problem of the interrelation of co-ordination and localization (Ch.II,p.15).

These independent constants of integration (the original position of the organ, its initial velocity, the condition of the force field as a whole, etc.) may change from moment to moment, resulting in completely different effects from the same initial innervation.

Adequate co-ordination and a correspondence of the movement to the animal's intention are only possible under conditions in which the central nervous system constantly receives information as to the state of these independent parameters of integration and adapts its effector impulses in an exact relationship to changes in the latter. This flow of information primarily involves the proprioceptive system and provides a second ring of reciprocal connections - in our terminology *the central cycle of interaction.*

In this cycle the effector impulses change the tensions in the muscles so as to bring about acceleration of the limbs and of the system, the acceleration results in changes in positions and velocities, and the latter, like the changes in muscle tension, give rise to *proprioceptive signals.* These signals affect the course of the effector impulses introducing necessary corrections and allowing the central effector apparatus to adapt itself with *plasticity* to changing conditions at the periphery.

In this way the connection between movements and the activity of the nervous system is at once very close and very complex.

The cyclogrametric method of investigation of movements here provides invaluable opportunities for the investigator. It permits the complex registration of the movements of a whole organ or even of the whole body, giving a picture of changes on a space-time grid of coordinates for any required number of points of the body simultaneously. It allows us to pursue this description in terms of very brief time intervals by the use of shutter frequencies of the order of 150-200 per sec and higher. And what is still more important, it allows us to obtain accurate quantitative data from these pictures.

As has been said, the central nervous effector impulses do not immediately reveal themselves in a movement. The concept in elementary textbooks that excitation of the *flexor* muscles results in flexion and that stimulation of the *extensor* muscles results in extension of the joints which they control, was seen to be erroneous even when the cyclogrametric study of movement was first begun. What is incomparably more essential and more frequently encountered is the connection between the active onset of muscular contraction (produced by the effector impulse) and the *acceleration of* the moving organ connected to the muscle. This connection was known to Fischer (43), thoroughly investigated for simple objects by Wagner (75), and taken as a starting point by our investigational group. For cases where this form of connection is applicable the cyclogrametric method gives all the data necessary for investigation, as by this means we may investigate simply, and with a high degree of accuracy, the acceleration of the movement of any given portions of the body in which we are interested. In cases of this type the curves of acceleration may give a very reliable picture of the course of effector impulses to the extremities on their final common pathway.

In more complex and general cases, where there are large-amplitude movements of complex kinematic linkages (for example, a whole arm or a whole leg), the acceleration of particular points may be quite unrelated to the course of the muscular contractions involved. We are here obliged to turn to another form of mathematical description - the *resultant moments of forces* in joints, which can be obtained in the same way from experimental material by cyclogrametric methods of analysis. These force momenta, the method of calculation of which was first developed by us in 1928, are values which are almost directly proportional to the resultant of contractions of all the muscles around a given joint. For this reason the curves of the changes in muscular force momenta in one joint or another provide us with a picture which is extremely close to the real characteristics of the course of effector impulses in every distinct neuromuscular biomechanical group.

It is now possible to turn to locomotion, in particular to walking. The analysis of the muscle momenta in the joints of the leg in walking shows beyond doubt that the curves of muscle force momenta in walking in the majority of their details are *very close to the curves of the longitudinal component of the dynamic forces at the centres of gravity of the limb segments and systems* (i.e. for the component in the direction of the sagittal axis of the body, from back to front, which we have designated by the symbol X). The dynamic forces which we calculated on the basis of the linear acceleration of points by Fischer's method are much more easily obtained from cyclogrametric material and are numerically more reliable than muscle momenta. The close correspondence between both forces and muscle momenta in the act of walking can be explained mainly by the fact that the actions of the force momenta are always directed perpendicularly to the longitudinal axes of the limbs. In walking, the deviation of the limbs of the leg from the vertical is not great; that is to say, lines perpendicular to them deviate only slightly from the direction of the longitudinal coordinate axis X. It follows from this that the curves of the longitudinally acting forces in the leg during walking give quite a reliable *qualitative* picture of the course of the neuromuscular effector impulses. The curves of the *acceleration* of centres of joints observed in walking, in their turn, show very close and regular correspondence to the curves of dynamic forces at the centres of gravity of the limbs. This allows us, proceeding with critical circumspection and constantly taking technical precautions, to come to conclusions as to the course of neuromuscular impulses during walking, not only from the curves of longitudinally acting dynamical forces, but also from their closest derivatives - the curves of acceleration for these same elements. The analysis of the material presented by us (21) has been carried out in this way, in cases where the calculation of dynamic forces was not possible without information on the masses of the limb segments and the locations of their centres of gravity. For running, in which we observe considerable and sharp deviations of the limb segments from their vertical axes, it was not possible to proceed with an analysis based only on the accelerations, or even, without special critical techniques, on dynamic forces, and here the conclusions of the whole work are based entirely on the calculation of the curves of the momenta.

It will appear below how complex and varied are the inter-relationships in various dynamic situations in walking, between the effector impulses, their most direct reflections - the force momenta - and their more distant functions - forces and accelerations - and how many characteristic signs may be recognized in these types of connection which already allow us to reach conclusions as to the central nervous origin and character of these and other impulses. It is important here to make another point. *The structural elements of the dynamics of a locomotor act may certainly be deciphered by means of more or less complex mathematical and physiological alphabets which permit the revealing through them of underlying central nervous processes.*

In the 1890's, in the epoch of the brillant flowering of studies by Marey, Braune and Fischer, it was not expected that so much would come out of a study of movement. Marey was chiefly enthusiastic about his new processes of recording, by the possibilities of halting the fleeting moment and examining, by this means, what could not be seen by means of the naked eye. Braune and Fischer were concerned, on the other hand, with correcting the observations which had accumulated in preceding years and drawing parallels between them and the more accurate material which they had collected, and, on the other end, with solving some problems in the field of the theoretical mechanics of movements, regarding the leg in terms of levers, pendulums and the other accessories of general theoretical mechanics.

The main advances of our experimental group from those of Braune and Fischer must be briefly stated*. We sharply increased the shutter frequences we employed (from 26 exposures per sec, as used by Braune and Fischer, to 60-190 exposures per sec); we increased by many times the quantity of material we employed, we simplified, mechanized and verified our methods of investigation. All this allowed us to study movements in incomparably greater detail than was possible for Braune and Fischer and to obtain highly reliable data because we had more methods of verifying our experimental material and, in particular, because we used a great amount of experimental material, while Braune and Fischer were obliged to restrict themselves to three experiments on a single subject. In close connection with these differences are also the basic differences between our and Fischer's points of view.

Firstly, Fischer's approach was essentially *retrospective*, that is, he was mainly motivated by a desire to order critically the basic information available on the mechanics of the act of walking which had been gathered up to his time. Our approach may be termed *prospective*, as we were not particularly concerned with which details might be found to be reliable and which false in the work of the older authors, but attempted to provide a more reliable and comprehensive *descriptive* basis for a subsequent broad extension of investigation into the genesis and pathology of locomotion. The main object of our investigations into locomotion from the first was to provide for future work as detailed a standard of average normal locomotion as possible, which might then be compared with whatever material might be accumulated in the future.

Secondly, Fischer was interested in the course of walking in the most general and basic terms. Therefore he ignored the details, referring a whole series of details on his photographs to errors of measurement**. Fischer supposed *a priori* that the act of walking must display a high degree of mathematical simplicity and dynamic regularity. Apparently for this reason he was satisfied with his very low shutter frequencies. Even those irregularities in the curves which could be retained on the coarse 'sieve'

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*For a more detailed discussion of this, see *Investigations in the Biodynamics of Locomotion*, Vol. I, p. 30, Moscow, 1935.

**See *Investigations in the Biodynamics of Locomotion*, p. 11, 1935.

that resulted from his low frequencies - and which, as our investigations revealed, are indicative of most important biodynamic processes - were smoothed out by Fischer and declared non-existent.

We, in contradistinction to this, gave most careful attention to these details - which on our high-accuracy, high-frequency sieve were retained much more clearly. If Fischer had *a priori* been sure that the movement of walking is maximally smooth and simple, then we, on the other hand, being aware of what complex synergy is involved in walking, expected from the very first to encounter a process of a degree of complexity which had been completely unrecognized beforehand, and which was in every case certainly impregnated with live micro- and macroscopic details, just as is living organic tissue. Our expectations in these respects were fulfilled in the highest degree. 'The biodynamic tissue' of live movements - of locomotion - appeared to be full of an enormous number of regular and stable details. In the course of one complete cycle of movement - one double step - each of the moving organs appeared to participate in a complex melody of scores of dynamic waves which followed each other with precision and regularity. Amongst these there were large and powerful waves, for example, the waves of the forward and backward thrusts which would have been visible even through Fischer's weak 'telescope' if he had not smoothed them out of the curves together with the 'dust-spots' and waves of the second order and, finally, the smallest objects which are located at the very limit of the resolution power of our present technology. The transition between stars of the first magnitude and the faintest visible objects is such a gradual transition, that without any doubt a multitude of stable particulars in locomotor processes are still invisible to us; there is considerable reason to suppose that it is precisely these ultratelescopic objects - these still unresolvable details of biomechanical tissue - that will seem the most interesting in the future.

All these circumstances determined the third and, in principle, the most important aspect of our approach to the study of locomotion. We refused to theorize about our object of investigation in advance by forcing it to fit one or other analog in the field of general mechanics. We regarded the locomotor process as a living *morphological object* of inexhaustible complexity and set as our primary task the necessity of observing and describing it as closely as possible.

As early as 1928 (15), I put forward in general terms the concept that the movements of a living organism could be regarded as morphological objects. That they do not exist as homogeneous wholes at every moment but develop in time, that in their essence they incorporate time coordinates (in ways different from tissues and organs, for example), and that they are in no way dissimilar to objects in the morphological field of investigation. On the contrary, the concept that movements are in many respects similar to organs (existing, as do organs, in a system of space-time coordinates (x, y, z, t)) appears to be extremely fruitful, particularly when the discussion concerns such stable and general forms of movement as locomotion.

The basic vital properties which exist in the movements of living beings clearly confirm their close analogy to anatomical organs or tissues. Firstly, a live movement *reacts* and secondly it regularly *evolves and involutes**.

I noted and described the former of these properties as early as 1924 (8, 9). Studying the biodynamics of movements involved in cutting with a chisel I was able to show that it is impossible to alter selectively any one given detail in this movement without affecting others. If, for example, the trajectory of the elbow is slightly altered, the form of the trajectory of the hammer is also unavoidably changed, as are the relationships between the velocity of the swing and the impact, and between the velocities of the wrist and of the hammer head, and a whole series of other nuances of the movement.

In subsequent years it was possible to establish from material obtained on walking that the reactivity of movements is extremely selective. Movements react to changes in one single detail with changes in a whole series of others which are sometimes very far removed from the former both in space and in time, and leave untouched such elements as are closely adjacent to the first detail, almost merged with it. In this way *movements are not chains of details but structures which are differentiated into details*; they are structurally whole, simultaneously exhibiting a high degree of differentiation of elements and differing in the particular forms of the relationships between these elements. This justified the analogy

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*The reactivity of the live structure of movements cannot be simply mechanically related to the reactivity of live organic tissues which take part in the given movement. The discussion does not concern the point that movement is a basic substrate - this would be completely false - but that the forms of the reaction of the material substrates of movement, which determine by their existence the course of a living movement, have quite particular qualitative characteristics.

which I made earlier between the characteristics of living movements and tissues, calling them both biodynamic tissues.

A second property of the movements of a living being has been established for a long time - they develop and involute. However, the problems of the ways in which they develop, and the stages they pass through in this process, etc., have been very little investigated. We will give some account below of the most important results obtained in this direction.

2. THE BASIC STRUCTURAL COMPONENTS OF THE LOCOMOTOR ACT

We shall discuss in general terms the basic biodynamic characteristics of the process of walking (19). These must serve as points of departure for further analysis.

The movement of walking consists, for each leg, in alternation of periods of *support* and *swing-through*. The swing phases are of shorter duration than the periods of support (for running the pattern is inverted), for which reason there are intervals during which one leg has not yet completed and the other has already begun its support phase. We call these intervals periods of *double support*.

The dynamics of the legs in their interactions with the supporting surface have their most clear-cut reflection in the behaviour of the centre of gravity of the whole body. In fact the forces which operate during walking at the centre of gravity of the whole body are equal in magnitude and opposite in direction to the forces which are applied to the supporting

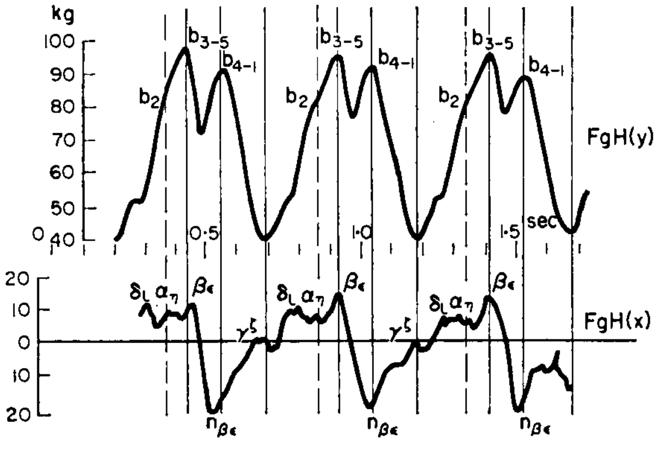

Fig. 22. Curves of forces at the centre of gravity of the whole body during normal walking. Above: vertical components. Below: horizontal components.

surface by the supporting leg or legs. By the principle of the equality of action and reaction, these forces in the legs are, in their run, equal and opposite to the support reaction, that is, the reaction of forces in the surface supporting the body of the walker. For this reason the curve of the dynamic forces at the centre of gravity of the whole body is exactly the same as the curve of the support reaction, that is, the curve of the interaction between the lower extremities and the supporting surface.

Graphs of dynamic forces at the centre of gravity of the whole body (Fig. 22) are among the types of evidence which may be quite easily obtained from live movements by the cyclogrametric method. These graphs incorporate a series of clear-cut and reliably stable elements which are evident in all normal human subjects without exception. The vertical component of stresses in the centre of gravity of the body as a whole $F_{gH}(y)$ displays to high peaks, h and v, which we have termed the backward and forward thrusts (*h-Hinterstoss*, *v-Vorderstoss*). The rear thrust is delivered by the leg, standing behind at the beginning of the period of double support; the front thrust is delivered towards the end of this period, about 100-200 msec later by the front leg. In this way every period of double support in walking begins with a rear thrust (with one leg) and ends with a front thrust (with the other leg). The support period for each leg begins with its front thrust and ends with the rear thrust. The entire sequence of events is clearly represented in Fig. 23.

Shortly before the rear thrust the leg that is behind delivers another vertical thrust of varying magnitude - the auxiliary thrust h'. The middle

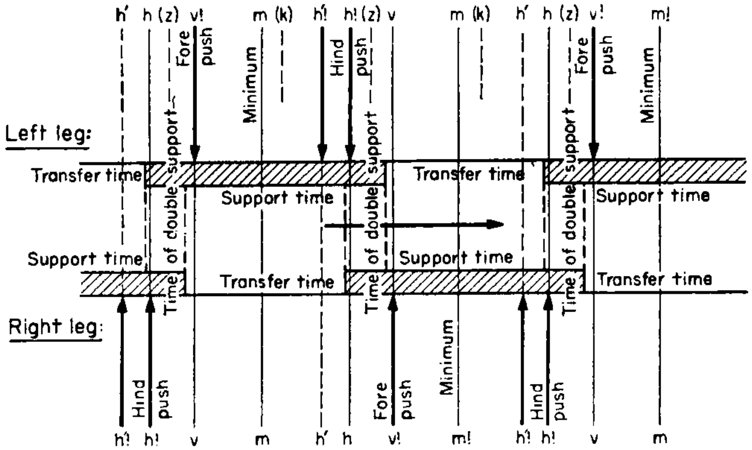

Fig. 23. Diagram of alternation of principal thrust forces in normal walking (the figure is corrected by data obtained from recent studies by O. Saltzgeber).

of the single support time - that moment when the swinging foot moves past the supporting foot - corresponds to the principal minimum m of the vertical component $F_{gH}(y)$. At that moment the pressure of the foot on the supporting surface is significantly less than the static weight of the walking subject, whereas, at the moments of the two principal thrusts h and v, it is significantly greater than this weight.

The longitudinal components of the forces at the centre of gravity of the whole body $F_{gH}(x)$ (Fig. 22) are significantly less constant than the vertical components in form, though not in the dynamic elements which they incorporate. These latter always appear in a strict sequential order varying only in their magnitudes and in the nuances of their rhythmical interrelationships. The most significant in amplitude are the direct (that is, forward-directed) thrust β_{ε} which occurs almost simultaneously with the rear thrust h, and the reverse (directed to the rear) thrust $n_{\beta\varepsilon}$ which appears a little before the front thrust v. If we examine both components in their entirety, that is, if we consider the forces at the centre of gravity as a vector, then the alternation in the waves which has been described will be visible in this way: The rear thrust manifests itself at the centre of gravity as a vector directed forwards and upwards which displays a small oscillation forward (β_{ε}) immediately after its maximum (h); the front thrust is a vector directed upwards and backwards ($n_{\beta\varepsilon}$) which deviates slightly further forward at the moment of its maximum (v).

The intermediate stable elements of the curve of the longitudinal components γ_{ζ}, δ_{ι}, χ, α_{η}, are of great value in the co-ordination of the movements of the legs and determine all the details of these movements, but their relation to the movements of the centre of gravity of the whole body is not at present very clear. The wave α_{η} occurs close in time to the auxiliary thrust h'.

The curves of the dynamic forces at the centre of gravity of the segments of the leg and at the centre of gravity of the whole leg may be considered as indicators of the locomotor structure on one side of the body. While the curves of forces at the centre of gravity of the whole body F_{gH} show the periodicity of a single step and reflect in equal measure the dynamics, now of one, and now of the other side of the body (this is the reason for the two-letter symbols which I have given to elements of the longitudinal components), the curves for the segments in the leg display periodicity over a double step. As examples in the present brief discussion of the curves of focus for the legs we may consider the force vector for the thigh (F_{gf}) which is shown in Fig. 24 as curves for the vertical $F_{gf}(y)$ and longitudinal $F_{gf}(x)$ components. This vector is the richest of all the force-vectors in the legs in structural details, for which reason we draw attention to it. The vector for the thigh is, for two reasons, in a particularly favourable position to allow a large number of biodynamic structural details to appear in it. Firstly, the thigh is located at the centre between the 'shin-foot' system at one end and the trunk at the other so that it is simultaneously affected by the operation of both systems and is an arena for the interplay of reactions of support and of forces which originate in the trunk and the contralateral leg. Secondly, its moment of inertia in relation to the hip joint is significantly less than the moments of other distal segments relative to this joint, i.e. the lower leg and foot; for this reason the thigh reacts more sensitively to all the nuances and shifts in the thrust impulses communicated to the leg by the hip musculature.

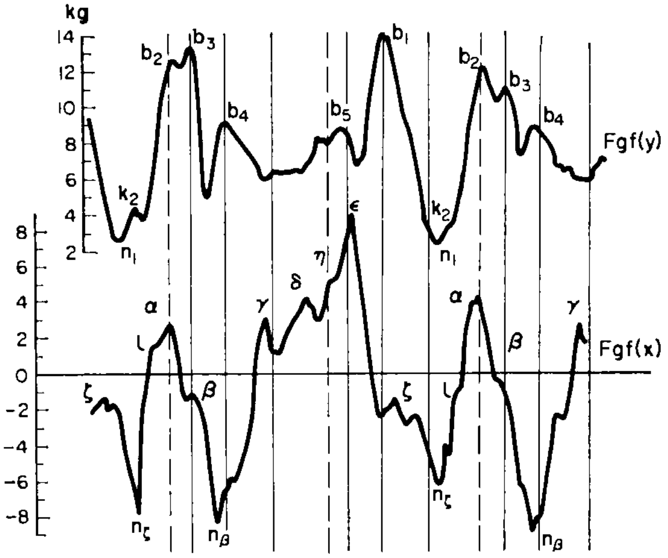

Fig. 24. Force curves at the centre of gravity of the thigh in normal walking. Above: vertical components. Below: horizontal components.

The vertical component of forces at the centre of gravity of the thigh $F_{gf}(y)$ (Fig. 24) is made up of an alternation of high groups of peaks with the raised plateau of the support period, and the deep troughs (n) of the transfer period. The group of peaks after the swing period minimum includes the peaks b_2, b_3, and b_4 if we do not consider the small irregular wave k_2 at the very bottom of the trough n . The peak b_2 coincides in time with the auxiliary thrust h' and is apparently caused by it. The peak b_3 is an echo of the rear thrust h of the opposite leg, that is, it directly precedes the beginning of support by the given leg. The peak b_4 , which is always much lower than both the previous peaks, is the effect of the operation of the front thrust by the given leg. The powerful wave of this thrust is transmitted through the thigh, and the small peak b_4 is a part of this thrust deposited into the curve of the thigh itself.

The support plateau is terminated by the poorly differentiated low group b_5 (sometimes b_5-b_5') reproducing the thrusts h' and h on the given side in the same order as b_4 reproduces the front thrust. There then follows the high wave b_1 - the reflection of the front thrust on the opposite side - after which comes the swingphase minimum n_1 .

And so the vertical component of the forces F_{gf} incorporates the main waves b_2, b_3, b_1 corresponding to the main thrusts on the opposite side and the lower peaks b_4 and b_5 corresponding to the same thrusts on the given side. All these elements are stable in normal adult subjects and are repeated in the same order in the curves for the action of forces in the shin and the leg as a whole. Except for these force elements, nothing stable and characteristic is observed in the curve $F_{gf}(y)$ and $F_{g(fsp)}(y)$.

In this way the vertical components of forces in the leg during walking basically incorporate only elements which reflect the struggle of the moving organism as a whole with the force of gravity: the after-effects of its supporting thrusts and the reflections of thrusts on the opposite side. This completely agrees with what has been said above in section 1; the manifestation of local muscular activity in walking must be observed mainly in the forces visible in the curves of the longitudinal components, while in the vertical components we find only very remote and generalized phenomena directly related to the integral dynamics of the centre of gravity of the whole body.

For all these reasons it seems that the most comprehensive and interesting neurodynamic material available to us must be the *curves for the longitudinal components in the legs*, and this expectation is fully verified. We may recall the nomenclature which we have adopted for the longitudinal components of forces.

The most distinct direct (inclined forward) force waves of the curve $F_{gb}(x)$ have been indicated by the first letter of the greek alphabet in their sequential order: α, β, γ, δ, ε, ζ. We have designated the reverse waves by the symbol n with the addition of an index to indicate the prior direct wave, for example, n_{ε} is the reverse wave immediately following the direct wave ε, and so on.

This nomenclature could not be retained in such a simple and schematic way. New smaller waves and previously unnoticed details, etc., are constantly being observed. For this reason we now regard the entire nomenclature we have given as only a series of proper names and we do not seak greater rationality in them than may be found in geographical or astrophysical terms.

The support period of the legs (see Fig. 24) commences with the very marked reverse wave n_β. This wave occurs immediately before the front thrust of the given leg and essentially represents the braking effect of the planting of the foot on the supporting surface. In the curve for the forces at the centre of gravity of the body at this moment we observe the appearance of the reverse wave $n_{\beta\varepsilon}$.

During the course of the support period itself there is a gradual stepwise rise in the curve $F_{gf}(x)$. It displays a small rise close to the zero level, γ (between $v!$ and $m!$),* a direct wave δ of moderate amplitude height, another higher region with poor relief η (close to h') and, finally, a large direct peak ε at the moment of the rear thrust on the same side.

The gradual jagged rise γ-δ-η corresponds to the steady increase of the horizontal velocity of the thigh, particularly its distal end; that is, it reflects the increase in the activity of the supporting leg pressing on the ground. The wave ε, the last and distinct direct peak of the curve $F_{gf}(x)$, occupies precisely the same dominant position in the curves for the shin and foot, and sharply falls after the rear thrust. From this point on, that is to say, from the beginning of the swing period on, the forces in the thigh display a course which is quantitatively different from those at the centres of gravity of the whole leg and its lower segments, but there nevertheless remain the same structural elements common to all moving organs, which are only disposed differently in relation to the axis of the abscissa (Fig. 25).

The direct element of the curve $F_{gf}(x)$ following immediately after ε is the jagged region ζ located between the front thrust v and the minimum m for the opposite leg. In the curve for the centre of gravity of the whole leg this region is located a little above the zero level (in the direct waves), whereas in the curve for the thigh it lies below zero (in the reverse waves). It is preceded in the curve $F_{gf}(x)$ by the reverse wave n_ε

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*The exclamation mark is used to designate the main thrusts on the same (ipsilateral) side of the body.

and terminates in another reverse wave n_ε. The reason for the different disposition of the region ζ in different curves is explained below; at present it need only be said that the reverse waves n_ε and n_ζ represent the braking effect, which begins at this point, on the forward velocity of the leg (and of the knee joint) which is accompanied by increases in the longitudinal velocities of the foot and the shin. The muscle forces in the hip and knee joints during the period represented by the region ζ are *extensory* (Fig. 25) so that the braking of the thigh is reactively provoked by peripherally operative forces.

Following n_ζ in the curve $F_{gf}(x)$ there appears a high and compact group of waves, the summit of which constitutes the wave α. This direct wave only exists in the thigh; for the shin and the foot we have at this point the reverse wave n_α. At that moment there are also reverse waves in the curves of the force moments at the hip and knee joints (Fig. 29). In this way the wave α for the thigh is not a resultant of muscle action, but is reactively produced by the periphery in the same way as is the braking effect on the knee in area ζ which has been noted above. The wave α and its functional pair n_α correspond closely in time with the supplementary thrust in the opposite leg h'.

Almost merging with α in the curve for the thigh we find the trace of a sharp peak β (the phase h in the opposite leg), in which, however, all the signs indicate quite another process than that at α. Firstly, this rise is observed in the form of a direct peak in all curves for the leg while there is no trace whatever of α in the curves for the shin and the foot. Secondly, as will be shown below, the whole history of the development of this peak β is radically different than that for α. By these means we ascertain that it is related to its neighbour not functionally, but as 'an optical pair'*.

This, in its most general features, is the framework of the structure of the longitudinal forces in normal adult walking. We may deduce from the inventory of the primary properties of the force impulses, which determine the stepping movements of the leg in walking given above, that these are certainly not limited to one simple pair of impulses, a forward impulse and a backward impulse, for each double step. Examples of such reciprocal impulses might be, for instance, ε and n_β which operate practically

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**Translators note*: The distinction made in astronomy between physical and optical pairs in the case of double stars.

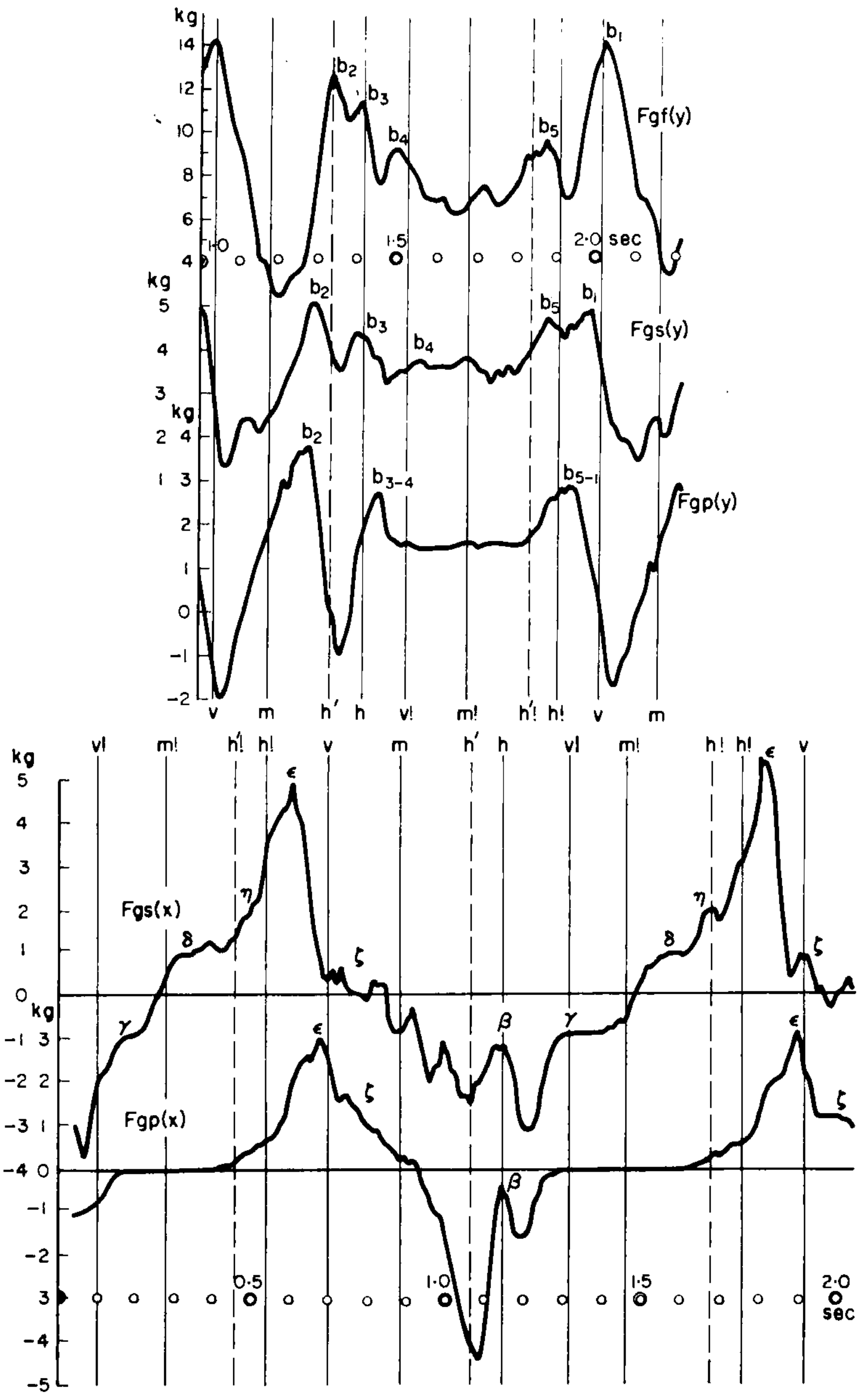

Fig. 25. Curves of thrust forces at the centres of gravity of the limbs of the leg in normal walking. Above: vertical components of the thigh, shin and foot. Below: longitudinal components for the shin and foot.

simultaneously in both legs in opposite directions to one another. The facts of the matter make evident that this most simple pair will not in any way exhaust the data. The multiplicity of elements interacting regularly with one another and making up a complex and idiosyncratic rhythm of alternation gives to the dynamics of each separate step a radically different physiognomy from that which may be observed in the simple stepping reflex in a decerebrate preparation. All these elements are strikingly constant and general, they occur in precisely the same unchanging sequence in all the longitudinal force curves for all the elements of the leg and the trunk in normal walking, they retain this constancy of structure in all normal subjects and they remain the same even in a whole series of central nervous system disorders. Further these very structural elements, in this precise sequence, are retained in a form of locomotion as qualitatively different from walking as running; and gross peripheral disturbances of structure, such as amputation, are necessary before they are lost to any significant degree. All this indicates that the structural elements of muscle action which have been described above are not in any way accidental, they are of essential co-ordinational significance for the locomotor act and that to all appearances they must consequently have a peculiar genesis, history and basis in the central nervous system or elsewhere. The confirmation of all these suppositions is the basis for the material discussed in this work (21); in subsequent sections we shall examine the facts which relate to these topics.

3. THE GENESIS OF THE BIODYNAMICAL STRUCTURE OF THE LOCOMOTOR ACT

The first fact revealed to us by our study of the ontogenesis of walking and running in the child was that *biodynamic structures live and develop.*

All the numerous elements which were briefly described in the preceding sections, and which were there irrefutably shown to participate in the co-ordinational formulation of an act of locomotion, are absent from the walk of a child which has just started to walk by himself. Considering the longitudinal acceleration curves of the child, in the first day of independent walking only the direct wave ε is present in the leg as a whole; the reverse wave n_{α} in the foot is accompanied by its functional pair α in the thigh; and the region for the hip joint is, as we see, entirely different from the adult ζ. There are no traces of β, γ, δ, η, θ, or ι.

This picture is retained during the whole of the 1st year of walking, that is to say, until nearly the 2nd year of life.

In other words, in the whole of the muscular-dynamic inventory of the child in the first months of the development of his walk we may observe only one pair of independent muscle impulses ε and n_{α},[*] the identical simplest reciprocal pair which we have described in the previous sections and which were believed by physiologists of the last century to explain the entire muscular dynamics of walking in adults. The other two waves which are observed in this early stage of ontogenesis have essentially a peripheral origin; namely, the purely reactive-mechanical α which arises in the thigh as a reaction to the active muscle stress n_{α}, and the reactive pair ζ-n_{ε} for which the same is true. The impulses ε and n_{α} develop first, and initially there are no others. I should term this level of development the *innervationally primitive* stage.

The following elements of the biodynamic curves develop much later: within 2 years of birth the infant has, besides the curves mentioned above, the waves γ and η in the thigh and signs of the beginning of the development of β in the knee and the ankle joint. These new elements are not yet clear-cut, and where, for example, tempo is increased, they have a tendency to disappear.

The whole inventory of dynamic waves develops very slowly, being complete by about the 5th year. The separate elements very gradually change over from the group of those which are not constantly encountered, and which are not observed in every step (γ) to the category of those which are constant for slow tempos, and finally to the category of unconditionally constant signs. The development of the adult forms of the structural elements is also completed slowly, and, for example, even by the 4th year the 'childish' ζ has not fully given place to the adult version.

In the first place, all this is evidence that the gradual appearance and consolidation of new structural elements is not in any way related to the elaboration of elementary co-ordination and equilibrium in walking. By the age of 3 or 4 the child is not only able to walk without difficulty, but can also run, hop on one leg, pedal a tricycle, etc. This means that the mechanisms of co-ordination for all types of locomotor movements and for the maintenance of equilibrium have been thoroughly elaborated for some time, whereas the inventory of the dynamic impulses for normal walking is

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[*] Not n_{β} as β still does not exist.

still far from complete. A little later on we shall see that the nervous mechanisms of elementary co-ordination and the maintenance of equilibrium are illuminated in quite another way by the curves obtained for children. The basic structural elements which we have just been discussing clearly have some other significance and are related to finer details of motor co-ordination.

The development of the structural elements of walking is completed in a manner which is far from direct. On the contrary, during ontogenesis a whole series of redundant signs which are not peculiar to adults appear, to disappear again at adolescence. These childish elements appear very early among the vertical components. By the 1st year of independent walking, while the child has only b_1 and b_3 among the whole range of adult components, there are also apparent, alternating with them, the 'infantile' waves k_1 and k_2. The second of these may also survive in adulthood in the form of a step k at the bottom of the wave b_2, although the homology of both these forms has not yet been rigorously proved; k_1 certainly disappears by the age of 7 appearing again only in cases of amputation in the transverse components of the forces $F(z)$. The infantile mechanical-reactive wave ζ in the thigh appears, as we have seen, very early and is maintained during the course of the entire 1st year of walking. It then falls away like a milk-tooth; the adult innervationally reactive form of ζ develops in its place only in the 4th-5th years of life.

An extremely interesting overemphasis of structural details occurs in the period of development between 5-8 years. During this period all possible variations of θ, η and ι often occur with very large dimensions; the vertical infantile thrusts k_1 and k_2 also attain very significant dimensions as do the horizontal details γ and ι which are still bound in synchrony to them. This overabundance energetically involutes between 8 and 10 years, but even by the age of 10 the process of formation of the adult structure has not been entirely completed.

In the course of the development of the biodynamic structure of walking there also appear qualitative deviations of another type which cast light upon the gradual mastery of the mechanisms of co-ordination and equilibrium. In the very first days of independent walking the vertical components of forces at the centre of gravity of the head of the infant already appear to be completely ordered and invariable from step to step, although primitive in form - only the waves b_1 and b_3 may be discerned in them. Meanwhile, the *movements* of the head (the curves $S_c(y)$ are extremely chaotic, uneven and involved. Towards the age of 2 the curves of the vertical forces for the head are gradually enriched by new waves and in extremely close connection with these the curves of *movements* $S_c(y)$ become increasingly more simple and stable. We may relate this to the fact that in various children we observe less intervariance in the curves of forces and accelerations of their heads than in the extremely diverse and individual curves of displacements.

An explanation of these phenomena follows directly from the basic conditions of the structure of movement which were discussed above. The given curve of changes in muscle forces (and it follows, in the simplest cases, of acceleration) cannot alone entirely determine the resulting movements as dependent parameters of integration*.

The effect of these independent parameters is stabilized in the normal development of the central nervous system by the proprioceptive apparatus; this signalling compels the motor areas of the central nervous system to adapt the form of their effector impulses. If the proprioceptive impulses are interrupted, as occurs, for example, in the classic case of tabes, we

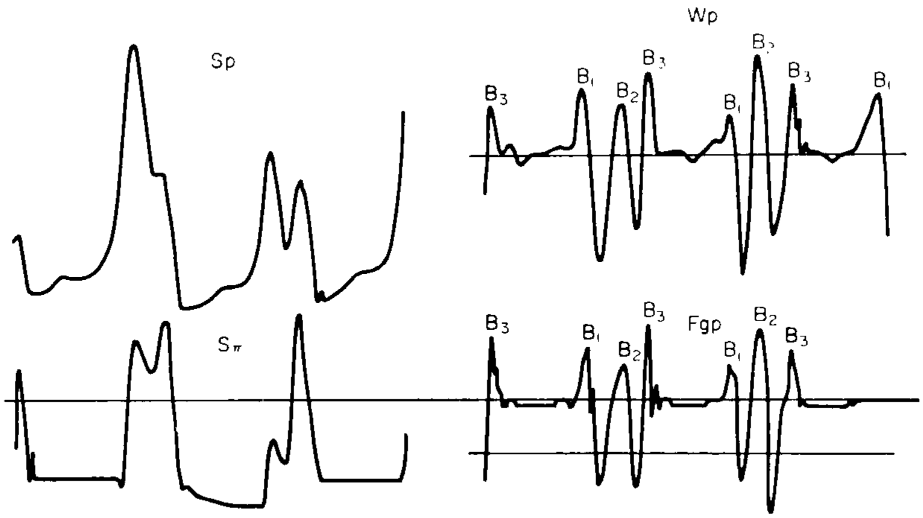

Fig. 26(a). Left side: vertical translations of the ankle and the toe end. Right side: vertical accelerations of the ankle joint and vertical efforts in the foot during two consecutive paces in walking of a patient with tabetical ataxia. It can be seen clearly that a moderate variability of accelerations and efforts in consecutive cycles of movement of a tabetic patient can result in strikingly different forms of translation curves in the same cycles.

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* Mathematically and not in a Sherringtonian sense.

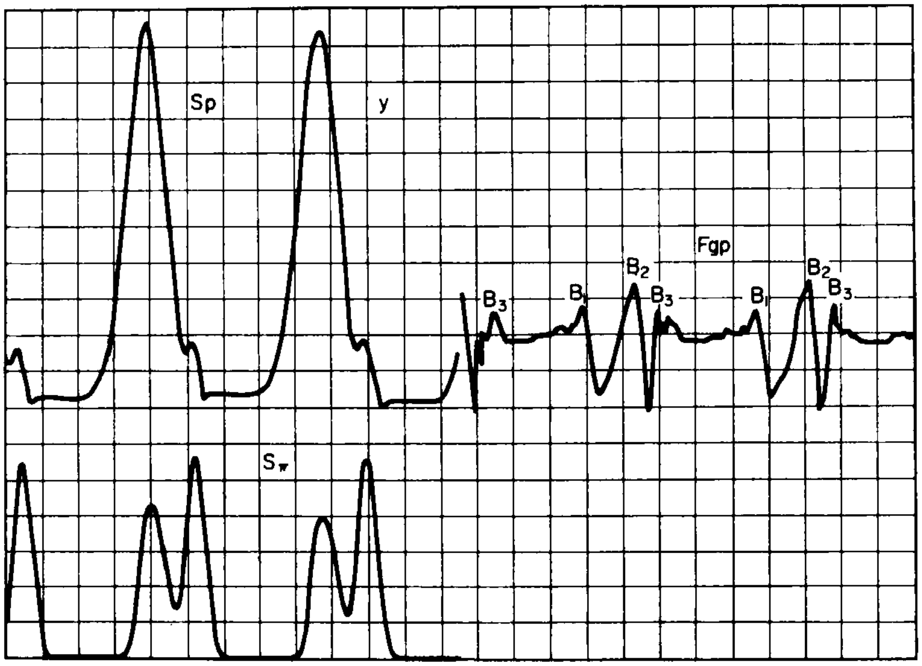

Fig. 26(b). Left side: vertical translations of the ankle and the toe end. Right side: vertical efforts in the foot during two consecutive paces in walking of a normal subject. The curve of efforts shows only a slightly lesser variability of cycles in comparison with those of the preceding figure, while the translation curves are absolutely identical from pace to pace.

obtain as a result extremely regular and constant successive cycles of *forces F* and *accelerations W* with completely disordered and dissimilar cycles of *displacements S*. We have material on tabes among our unpublished data which provides very clear examples of this type (Fig. 26). We may deduce from this that the principal and most general symptom of disturbances in proprioceptor signalling (proprioceptive ataxia) is the *disturbance in successive cycles of the constancy of S* with (sometimes marked) retention of *constancy of F and W*.

In small children we are dealing, naturally, not with disorders in proprioceptive signalling, but only with the slow and gradual appearance of the mechanisms of adequate response to this signalling. In cases where this mechanism has not yet been built up we may observe the ataxic symptom which we have described - the regularity of *W* and chaotic disorder of *S*. In this stage of ontogenesis the spontaneous effector impulses are consequently already developed (*W* is in order) while the innervational-reactive impulses do not operate in conjunction with them.

In locomotion of children there are a whole series of phenomena of this sort which are clearly observable at first, and later grow feinter and feinter. In children who have only just begun to walk there is as a rule no constant correspondence between dynamic phases and the positions of the body. Their phases, appearing in successive steps as the phase of one and the same force waves, are quite unlike each other. For adults, on the other hand, there is an exact repetition of positions in simultaneous dynamic instants (see Fig. 27(a)). Further, for some dynamic waves, for example ζ, the connection between positions and phases is extremely stable and is not even destroyed by very gross changes in gait. The absence of a similar correspondency in small children is naturally an indication of the same relationships seen in the chaotic forms of the S curves; the absence of correct response to proprioception.

In slightly later stages of childhood development when the most elementary mechanisms of locomotor co-ordination have already been mastered, the same symptom of inadequate mastery of proprioceptive signalling appears in a feinter and partly transformed aspect. The situation is the same in the case of the ζ wave, which, when all is considered, is one of the most complex and enigmatic of all the co-ordinational waves. T. Popova observed that the height of the ζ wave in children between the ages of 1½ and 3 years is in very close correspondence with the length of the stride and that apparently the inadequate participation in one or another separate step of the main ε impulses is compensated, so that a step may be correctly made by the incorporation of the ζ wave. In some cases, ζ in this context considerably overshadows the basic ε impulses and in this pattern of walking waves appear which vary considerably in magnitude from step to step. This observation discloses a connection between proprioception and the effector system, but now in a new scheme. Because of the inadequacy of the motor responses, which are not yet proportionally related to the amount and the exact graduations of the force ε (the auxiliary waves δ and η! are undeveloped), unequal steps are obtained for constant values of ζ, while for steps of constant length variations in ζ are necessary. In the adult norm it is both the case that ζ are of constant value and that steps are of constant length; this means that even *before the completion* of the main impulse ε an exact regulation of the direct longitudinal forces on the basis of propriosignalling is necessary in carying out steps of constant metric proportions. This preparatory regulation has not yet been developed in the child and it depends on subsequent compensation including the *termination* of the impulse ε. The proprioceptive

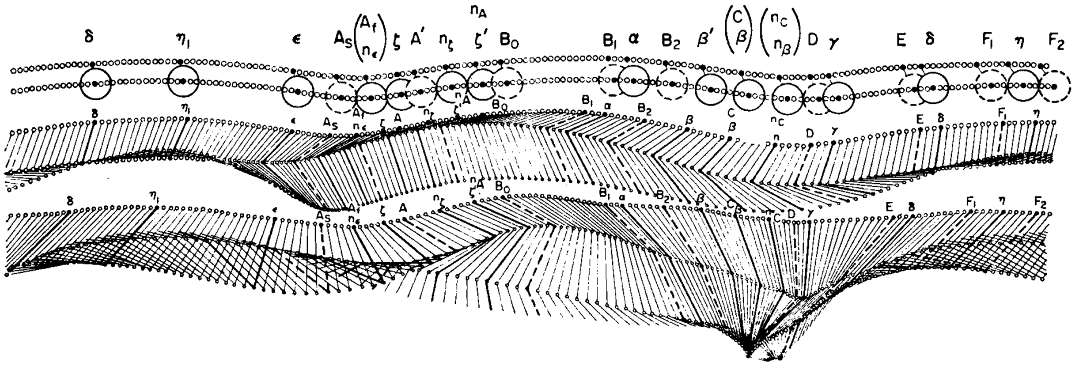

Fig. 27(a). Successive positions of the right side of T. Ladoumeg's body, taken at a frequency of 187/sec in experiment No. 731. Heavy lines mark phases of the movement corresponding to characteristic dynamic phenomena. Continuous heavy lines indicate longitudinal dynamic elements. Heavy dotted lined indicate vertical elements.

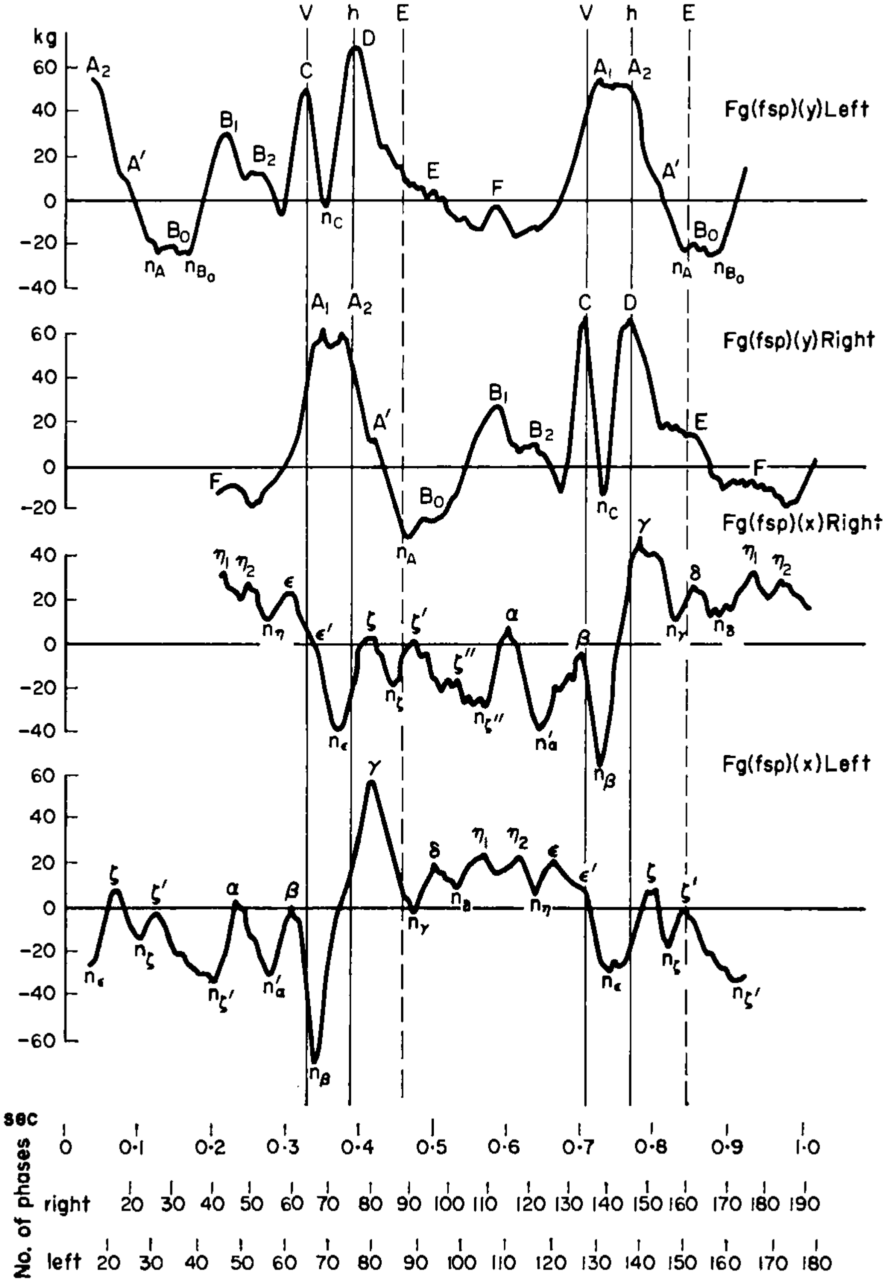

Fig. 27(b). Forces at the centres of gravity of Ladoumeg's right and left legs. Standard terminology.

activity is still inadequate to allow metric *planning* of movements but is capable (in distinction to the very earliest stages) of introducing metric *correctives*.

The evolution of the locomotor act involves nearly the whole of childhood and extends almost to the beginning of puberty. The last stage through which it must pass, which begins to be observable only after the age of 5, and which disappears only after the age of 10, is the conversion of an undifferentiated collection of biodynamic elements into a determinate structure. All the structural elements of walking have developed by the 5th year and, as has been said above, are still encumbered by a whole collection of infantile peaks and hillocks for the next few years. However, at the beginning of this stage of development all the 'teeth' in the curves, both the 'adult teeth' and the 'milk teeth' although they are already located in the correct sequential order, have not yet as a whole adopted the *form* which is characteristic of the adult curve. This form is finally apparent when regular and constant proportions are established between the elements of the curve, when the smaller 'half-tone' elements merge and are effaced, and the larger elements emerge into dominant positions. It is only then that the last and highest stage of development of co-ordination is completed: the displacements (S) become regular and of equal lengths, and here neither curves of forces (F), nor accelerations (W), necessarily vary in order to compensate for irregularities (as it were, *post factum*, in proprioception), so that it is possible to maintain a unity of movements without violating the unity of the force curves (as a result of proprioception *ante factum*).

A similar picture may be observed in the *running* of children. In giving an account of the adult run, we have been able to show that running considered as a *neurodynamic* structure displays many incontestable signs of similarity to walking. This relationship is clearly marked in an analysis in depth of running, and is in sharp contrast to running regarded as a *biomechanical* structure in which respect it is in many ways exactly opposite to walking. The problem of moving the body through space is solved in almost directly opposite ways in running and walking and a whole series of features of the structure of innervational processes undergoes basic changes in running in comparison with walking, notwithstanding the incontestable common genesis of both processes in the nervous system. This common origin may be studied and proved with the greatest clarity in the context of the evolution of running in children, where we clearly observe the common operation of locomotor structure as well as the gradual course of their biomechanical and neurodynamic *divergencies.*

In the very earliest phases of development of locomotion (in the 2nd year of life) it is impossible to observe clear differences between running and walking in the child. His run is quite free of the most characteristic sign of a true run - the interval of flight - and is only very slightly different from walking. Only a few dynamic elements similar to those encountered in true running appear at the proximal ends of the legs while the distal ends still present the picture characteristic of walking. All these changes are concentrated in the *support period* in the structure of which all the first signs of divergences are incorporated.

Very gradually, during the 2nd and 3rd years of life, parallel with the mastery of new elements in the curves for walking, we find the development of divergencies in the running curves. The front and rear thrusts of the supporting leg (*C* and *D* in Fig. 28(a)) are shifted nearer in time as compared with those observed in the walk of a given child; the longitudinal force waves γ and δ develop in place of the older primary wave ε which is reduced in running and changes its position from the support group γ-δ-η to the transfer group ζ-α; we also observe the first traces of *flight*. It is an interesting fact that the curves of the vertical dynamic components diverge earlier than the curves of the longitudinal components (Fig. 28(b)). If we remember that the former are closely concerned with the integral dynamics of the organism and with its biodynamic activity in its struggle with the force of gravity, whereas the latter mainly reflect the internal more intimate structure of the force impulses in the given extremity, an explanation of this phenomenon may be attempted. It seems to me that the delay in the appearance of divergency in the case of the longitudinal curves in comparison with the vertical is a sign that the reorganization of the movement begins with its biomechanics, that is to say, with the *peripheral parts* of the process (the reorganization of the support interval, the organization of the phase of flight, etc.); this biomechanical reorganization sets new problems for the central nervous system, to which it gradually adapts, mirroring that adaptation in subsequent changes in the longitudinal dynamic curves. This secondary character of *central divergency* is also very marked in another phenomenon which will be discussed a little later, that is, in the diminution of the amplitude of the longitudinal force curves in the qualitative development of running with increasing age.

Though it was necessary in the first stages of the development of running to use care in determining differences from walking, in subsequent

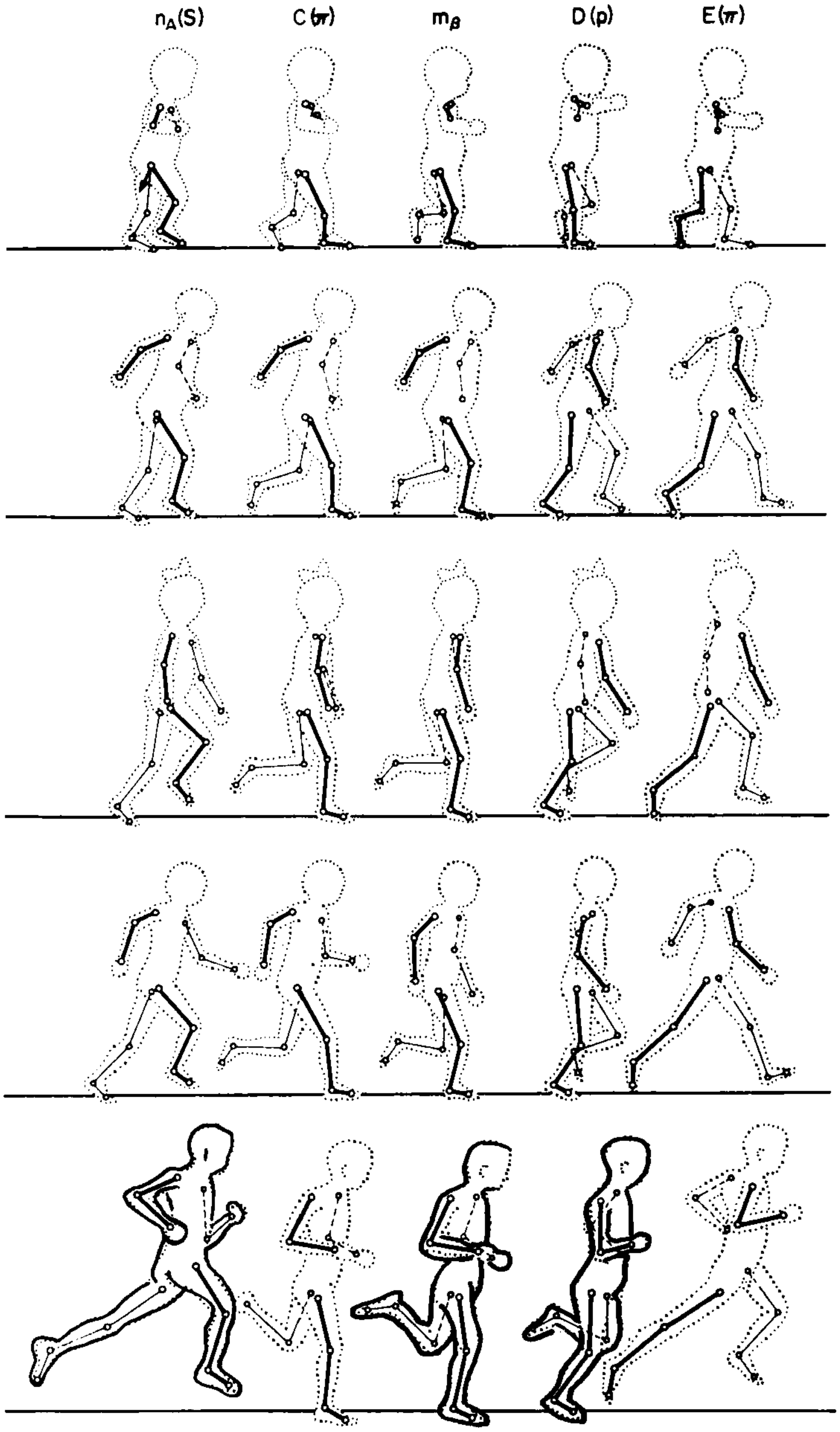

Fig. 28(a). Schemes for positions of the body during phases of the step. n_A downward push in the thigh of the rear leg; C, thrust to the rear by the rear leg; m, limit of raising of the knee to the rear; D, thrust to the rear; E, the last dynamic element of the support period.

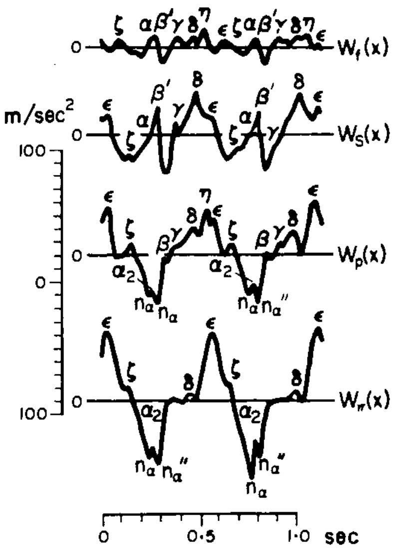

Fig. 28(b). Longitudinal acceleration of the hip (f), knee (s), and ankle (p) joints and of the point of the foot (π) during walking of a child ages 3 years and 5 months. (T.P.; experiment No. 963). The support period is indicated by horizontal lines at the bottom of the graph.

stages (from 2 to 5 years) the basic properties of running are quite incontrovertibly apparent. In this age group the organization of the *transfer period* in running has already begun (firstly the latter half of transfer, then the former). In particular, during this stage of development dynamic novelties appear in the transfer period and, curiously, the overwhelming majority of them appear at the *proximal* points of the lower limbs, while the distal points do not display noticeable signs of divergency for some time. From 2 to 5 years the longitudinal curves for the thigh already display the typical structure of the running swing-phase period in its entirety, while the curves for the foot are still not differentiated from those obtained for walking, even in respect to the support interval.

This prevalent course of evolution and of divergency from above to below, from proximal to distal points, leads to an interesting physiological generalization. It is very improbable indeed that the nerve dynamics of the distal musculature are so sharply different (by a whole year of development) from the dynamics of the proximal muscles. It is much more probable that there is another explanation here. The proximal ends of the legs (for example the hip joints) are surrounded by far more massive muscles than are the distal ends (the feet), while at the same time the moments of inertia of the former are much less than the moments of inertia for the latter. For this reason the muscles of the hip can move the upper sections of the limbs much more easily than the foot, since to move the foot they must oppose the inertia of the entire leg from top to bottom. There is also the fact that the (relative) velocities are higher as a rule for distal than for proximal parts, so that the kinetic energies of the former are higher and it is more difficult to overcome them. The distal parts play roles reminiscent of heavy flywheels in relation to the legs as a whole.

It follows that the nerve effector impulse at any given strength of its operation appears far more easily in the proximal curve and is mirrored there in the form of observable dynamic waves, because it does not have to overcome all the inertial resistance of the distal system. In order to be perceptible in this latter system, the effector impulse either must be very strong or must coincide exactly with the moment when conditions in the distal system are particularly favourable for its appearance. It is still to say what these particularly favourable conditions are, and here a wide field of investigation is opened. It is possible that we have here simply a convenient position of the extremities which offers the muscles the most favourable biomechanical conditions of operation; it is possible that this most opportune moment is a critical moment in the velocity of the limb when the inertial resistance is least perceptible; finally, it may be the case that it is at this moment that the degree of excitability of the muscle apparatus is most receptive because of some favourable concurrence of proprioceptive signals (this latter hypothesis may be successfully tested by electrophysiological techniques). In one way or another the control of the distal parts demands a greater deftness, a higher degree of co-ordinational technique in respect to the skill of selecting the optimal moment to give just the right impulse at just the right time. If this moment is lost, even by a fraction of a second (it must never be forgotten that all processes in running are measured in terms of hundredths and thousandths of a second), then the impulse will fail to penetrate; that is to say, it will not produce any noticeable effect at the periphery.

We must stress here that we are not discussing small co-ordinated movements of the distal sections such as finger movements, but global general displacements of distal portions of the body of the extrapyramidal type. The dynamics of these latter depend, in the last analysis, on the same hip muscles which control the dynamics of the proximal portions of the legs; the distal dynamics become, however, richly differentiated in biodynamic detail, not when these details are incorporated in the effector impulse and are first reflected in the dynamics of the pliant proximal points, but only when *functional integration of the receptors and effectors* is achieved and when the effector side of the nervous system learns to seize the fleeting moments of functional conductivity.

The reliability of this explanation is well illustrated by observations on the world-famous runner, Jules Ladoumeg. In his case, the dynamic curves of the distal limbs reach their maximum degree of resolution, far exceeding the degree of resolution observable in the curves of other runners; and, further, in his case this degree of resolution was accompanied by a particularly rich set of modulations of forces in the distal limbs corresponding to extremely accurate control of external, biodynamic processes. In the complicated multijointed pendulum, by which the leg may be represented in biodynamic terms, the dynamic interactions of the limbs, the play of reactive forces, the complex oscillations of the links, etc., are extremely varied and abundant; the fact that they are kept in the background in the case of this great runner, but have at the same time such abundant reflections in the dynamic curves, is evidence of the extremely delicate degree of adaptation to proprioceptive signalling achieved by Ladoumeg's neuromotor apparatus. It is precisely this adaptability which allows him a high degree of differentiation and control over the active dynamics of his distal limbs; in his case this is accompanied by a considerable degree of quantitative economy of force.

We may also cite in favour of this explanation an observation made by T. Popova. For a given value of the velocity in running in various children, such of them as have at the time of observation a more differentiated biodynamic structure give as a rule smaller amplitudes of acceleration, that is to say, a smaller range of dynamic forces. In order to arrive at the same final result the child with the qualitatively less differentiated dynamic picture must expend more energy. This can only mean one thing: that a higher degree of resolution of the distal force curves is a sign that the system is learning to seize the moments of least resistance or, to put the matter another way, to utilize the whole rich play of external forces and possibly also the entire physiological (involuntary) gamma reciprocals and other more complex reactive processes at the muscle periphery.

The further development of running in children after the age of 5 closely repeats that for walking and I shall not dwell on it at present.

An analysis of changes in walking in old persons undertaken by

P. Spielberg[*] has reinforced the data discussed above on the *development* of the structure of the locomotor act with interesting data on its *involution*. Spielberg distinguishes three involutional stages in the walking of old people. In the first of these stages some decrease in the normal activity of the structural mechanisms of walking can already be discerned, but this decrease is effectively compensated for by the involvement of the higher psychic functions in the realization of the act of walking: consciousness, voluntary attention, and so on. In the second stage of involution this alerting of consciousness gives way to a heightened degree of fussiness, hyperproduction of movements, hasty and short steps, etc. The inventory of the dynamic structural elements becomes poorer; the vertical wave b_2 for the foot gradually diminishes and then disappears (in early childhood this wave develops, on the other hand, last of all), then the peak of the longitudinal component β undergoes attrition. The reactive wave α is retained longer. In the third stage we observe gross dissolution of the structure of movement. The force curves become small in amplitude and impoverished in terms of their components. They lose element after element. Meanwhile, the equality between successive steps disappears and irregularity begins with alternation of relatively large and small steps and deeper signs of disco-ordination (disorder of the forms of the S curves). To this picture of decay we may also add P. Spielberg's observation of the disintegration of normal unitary co-ordination. The synergy existing in normal walking between the action of the arms and legs is destroyed, the movements of the arms become arrhythmical and the amplitude of these movements gradually diminishes to zero, after which the arms are stretched our rigidly slightly in front of the body - as if in constant readiness to support the body in case of falling. The vertical amplitude at points of the feet is still perceptible, but amplitudes for the upper portions of the body rapidly decline to zero, destroying the normal proportion between movements of the upper and lower portions of the body. In extreme senescence a man eventually has force curves which are as devoid of peaks as his jaws are of teeth. Among the force curves there survive only the earliest reciprocal elements ε and n_a (not counting α) and some vestiges at times of ζ, and at times of γ or η. The dying of the structure is clearly marked in this material[**].

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[*] This study was undertaken at the Laboratory of Physiology of Movement of the All-Union Institute of Experimental Medicine.

[**] In recent times, extensive and interesting studies of senile gait have been published by R. Drillis (81).

In this way the ontogenetic material has shown us beyond all doubt that the biodynamic structure of walking emerges, passes through a series of regular stages of development, and then regularly involutes in senescence. Most important in principle is the fact that this development is related to extremely determinate *qualitative* changes in the structure itself. In respect to its *morphology*, this structure passes in early ontogenesis through: (a) a reciprocal innervationally primitive stage; (b) a stage of gradual development of morphological elements; (c) a stage of abundant proliferation of these elements; and (d) a stage of inverse development of infantile elements and the final organization of complete and proportional forms.

In their relations to *motor co-ordination* the biodynamic structures of walking pass through a series of qualitatively different stages of development in exactly the same way.

(1) At the beginning we encounter the signs of general hypofunction of proprioceptive co-ordination. There is no correspondence between positions and dynamic phases; there is no unity of S for the existing adjustment between W and F; there is no similarity of S for various children.

(2) Later the child passes through a stage of development of proprioceptor co-ordination *post factum* (compensation by means of secondary co-ordination) and only significantly later on develops:

(3) Co-ordination *ante factum*, or more accurately, *in facto* (adjustment or primary co-ordination).

It is perfectly natural to compare these sequences in morphological and functional development. This comparison may give as a key with which to decipher the meaning of particular morphological phenomena.

We may first suppose that the initial ontogenetically earliest (and clearly also phylogenetically the most ancient) impulses ε and the infantile n_a are *spontaneous preproprioceptive impulses*. They represent the original and most ancient framework of movements, their rhythmic and dynamic basis. All the waves developing later, which are brought forth during the period of the development of compensatory co-ordination, are already indubitably effector responses to proprioceptive signals; we have termed these waves *innervationally reactive*. Among these waves we have, for example, ζ and β. The connections between the positions of a moving organ and its velocity must be biodynamically characteristic for these waves.

Finally, the appearance of forms and of concrete proportionalities in the dynamic curves is deeply involved with the development of compensatory co-ordination, that is, with the activity of those highly organized apparatuses of the central nervous system which ensure the completion of the entirety of a projected movement, the definition and accuracy of movements, etc.

And so we have established at least three forms of elements among the component waves of the biodynamic structure which differ from each other in essential ways: (1) *spontaneously innervated*, the earliest and most primary (ε and n_a); (2) *reactively innervated* (γ, ζ, β); and (3) *mechanically reactive* which do not have as their basis either innervational impulses or changes in muscular activity, but which arise entirely at the periphery as a result of the complex collision of internal and external forces in the kinematic linkage of the extremities. Amongst these mechanically reactive waves we may place α, which will be discussed further, and a whole series of other smaller waves.

4. A SKETCH OF THE QUALITATIVE ANALYSIS OF THE BIODYNAMIC ELEMENTS OF THE LOCOMOTOR ACT

In the 'constellations' of biodynamic elements which we have observed and studied, the degree of visibility and clarity of the individual elements and the degree of investigational interest they possess, are not always coincident. This has been the case in regard to astronomical constellations, where very often objects of the first magnitude, the 'alpha' of the constellations, have considerably less scientific interest than some quite faintly discernible phenomena such as 'delta' of Cepheus, which not so long ago was epochmaking in astrophysics. However, the large and bright objects are always noticed first and they provide the investigator with an impetus towards the examination of their fainter fellows.

This also occurred in the present study in the case of the first magnitude star of the α wave. At present this wave is of considerably less interest to us than such weak and nebulous objects as the ζ group in walking, or the A' and n_{B2} waves in running, but it originally guided our investigational group towards the study of waves in general. For this reason it deserves a short general characterization.

The tension of the flexor musculature at the knee results in the bending of the lower leg and foot backwards *relative to the knee*, though the movement in space of the knee itself and of the thigh with it, is not predetermined by this. According to the basic principles of biomechanics the musculature at the knee cannot displace the centre of gravity of the leg as a whole either forwards or backwards, but only along the straight line joining it to the hip joint, that is to say, in walking, only upwards or downwards. For this reason the isolated operation of the musculature at the knee moving the lower leg and the foot backwards inevitably moves the thigh forwards in compensation. The same *forward* displacement of the thigh is effected even in a case where it is itself pulled gently *backwards* by the action of the hip musculature, especially because this latter must surmount significantly higher moments of inertia than the musculature at the knee.

In this way the result may be that, should the moment of flexion in the hip muscles be not much greater than the moment of flexion at the knee,

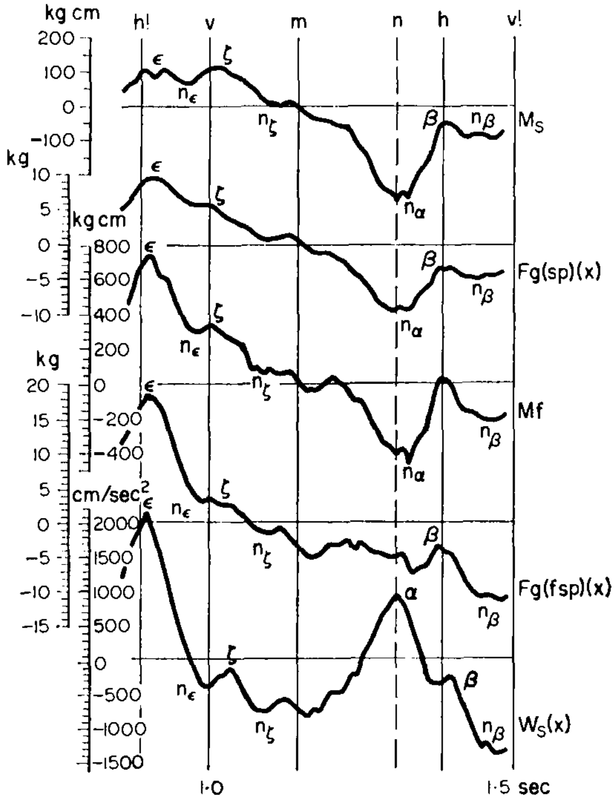

Fig. 29. Curves for the muscle force moments in normal walking compared with curves for the forces and accelerations of the limbs of the legs. From top to bottom: 1, the force moments in the knee joints; 2, the longitudinal components of acceleration at the centre of gravity of the shin-foot system; 3, the force moments in the hip joints; 4, the longitudinal components of the forces at the centre of gravity of the leg as a whole; 5, the longitudinal components of acceleration at the knee joint.

there occurs only a very slight backward displacement of the centre of gravity of the leg as a whole, whereas the momentum at the knee can effect considerable flexion of the more compliant shin-and-foot system relative to the knee. As a result the thigh will be displaced *forward* in spite of the fact that the hip muscles are pulling it backwards. If it were not for this pull the thigh would be displaced even further forward; for it not to be moved forward at all it would be necessary for the flexing force of the hip muscles to reach a significantly higher level. So towards the end of the swing period, forces which operate *forward* arise in the thigh in spite of the fact that the whole of the surrounding musculature, both at the knee and at the hip joints, is pulling *backwards* at that moment (Fig. 29). This constitutes the force wave α which we term *reactive-mechanical* for the quite logical reason that it arises in opposition to the muscle forces at a given moment, entirely as a result of the peripheral interplay of action and reaction in the complex kinematic linkage of the leg.

It is clear that if the knee joint were to be immobilized in any way the wave α would rapidly disappear.

This is confirmed in fact. In *Handbuch der normalen und pathologischen Physiologie*[*] is cited a cyclogrametric of the forms of the curves for the longitudinal velocities of the knees in the walk of a subject with strained ligaments at the knee joints. Because of pain in the joints this patient walked carefully avoiding flexing his knee joints (antalgic walking) and as a result there is no sign of the α wave in the curve obtained for his knee (this wave forms a second smaller prominence in the normal velocity curve for the knee (see Fig. 29, on left). We have also tested this position experimentally. A healthy subject had his knee joints splinted and bandaged, and as a result the α wave entirely disappeared from the curves or was at least very much reduced. The latter fact may be explained by some 'leakage' of the flexibility of the knee as a result of the bandages not having been tied tightly enough. It is interesting that under all these conditions the β wave which, in normal walking, almost merges with the α wave, and is difficult to distinguish from it with the naked eye on normal curves for the thigh, becomes completely isolated, and begins to tower above the remnants

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[*] W. Steinhausen, Mechanik des menschlichen Körpers, *Hdb.d.norm.u.path. Physiol.*, Vol.15, part 1, p. 215, from my papers in the handbook *Problems of the Dynamics of Bridges*, edn. 63, p. 67, Moscow. See also Ref. 15.

of the α wave. All this gives final and conclusive proof that α is a reactive-mechanical curve. We found an opportunity to put this problem to experimental verification, which now leaves no doubt in the matter.[†] A subject with a very short above-knee amputation had an artificial limb which replaced the lower section of the thigh, the knee joint and the shin and foot. It was possible to lock the knee joint or to give it some passive flexibility restricted on both sides by elastic buffers. When this subject walked with the knee joint locked, as can be seen in the cyclograms, the curves of movement do not show the smallest trace of α; when, however, the lock was left open, the artificial limb immediately began to reproduce the phenomenon of α in walking (Fig. 30). It would be difficult to prove more completely the essentially mechanical nature of this phenomenon[*].

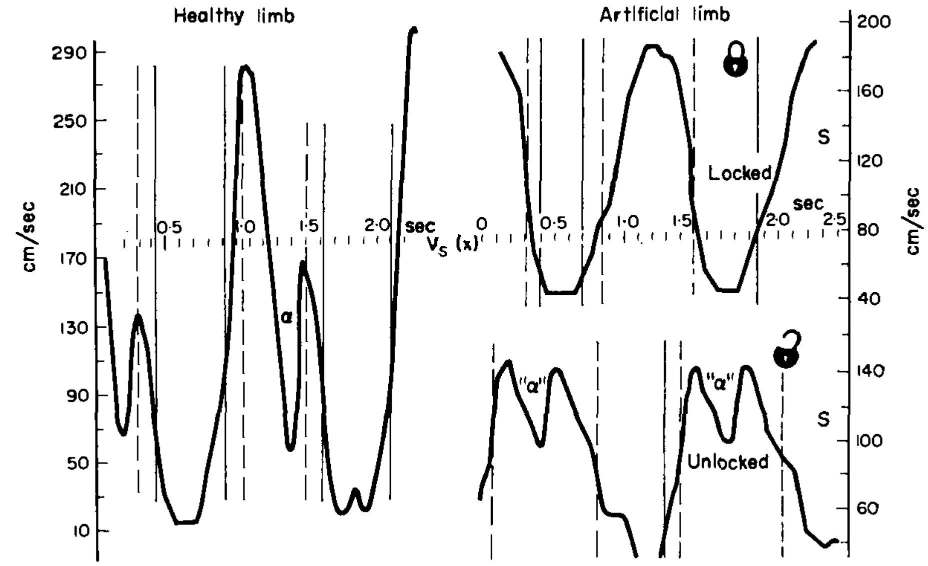

Fig. 30. Appearance of force and reactive wave α at the knee hinge of a prosthesis after unlocking of the hinge (from a study by O. Saltzgeber carried out at the Central Institute for Study of the Disabled).

We must at once admit that it appears, on the basis of our most recent observations, that the α phenomenon is not entirely *mechanically reactive*. Some accessory phenomena of an innervationally reactive type begin to be apparent in this wave. The appearance of α in running is not limited to the

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[†] A study by the Candidate for Medical Science, O. Salzgeber, carried out in the central Institute for Study of the Disabled, N.K.S.O.

[*] The flexing force of the knee muscles necessary for the appearance of the α phenomenon is replaced in this case by the force of the springs in the knee joint of the artificial limb.

single simultaneous pair of waves $n_a-\alpha$, but gives rise to an entire chain of successive waves. This might be expected as, in life, no phenomenon is exhaustively described by any single explanation.

It is possible to count very many mechanically reactive waves in locomotion and there certainly exist even more mechanically reactive components which are part of phenomena of another type. Among mechanically reactive waves we may enumerate, for example, the childhood ζ which is an echo of the force n_ζ (the beginning of the main reverse wave n_α); the waves n_α and n_ϕ of the Prussian goose-step, and so on. We may include among mechanically reactive components the lowering of the ζ area in the thigh as compared with the centre of gravity of the whole leg in normal walking, indicating the effect of reverse reaction in the thigh as a result of the tension ζ of the extensor muscles of the knee. We may also refer to this category the passive maximum flexion of the knee in running, which occurs in spite of the quite considerable and long-lasting tension of its extensor muscles. By thoroughly studying one representative of such a family we may now quite easily establish its position in the same family of less distinct objects. It is only necessary to formulate common characteristics by which these may be recognized.

The basic indication of mechanically reactive waves is the presence, in the curves expressing linear force or acceleration of a limb, of any waves which are absent in the corresponding moments in the curves of muscle momenta of the nearest proximal joints. This most essential sign answers the question of the mechanically reactive nature of a wave at once and without further argument. To employ here another of the terms popular with morphologists, the curves of momenta are the best reagents for the recognition of elements of this type.

The second indication is less rigorous and less general, but is nevertheless more conveniently used in the majority of cases, as the 'momentum reagent' is very time-consuming and therefore expensive. The condition is that the mechanically reactive wave always arises as an offshoot of another active wave, making itself felt in one of the intermediate limb segments. From the explanation of the α wave given above it is apparent that the mechanically reactive wave arises because it is impossible for the internal muscles of a system to displace the centre of gravity of the system. Because of this, if the internal muscles communicate an acceleration to a single link in one direction, another link will undergo compensatory acceleration in the opposite direction in a mechanically reactive manner.

The second indication mentioned above follows from this: if, in two adjacent limbs of a freely moving system (for example, in the swing of a leg), there exist two simultaneous waves in opposite directions, it is almost always the case that these are a mechanically reactive pair and that one of the elements represents a reaction of the recoil type.

It goes without saying that a very careful analysis of the surrounding phenomena is necessary here in order to recognize which of the two waves of a pair is active and which is reactive, and whether it is possible to regard the phenomenon as being entirely mechanically reactive or whether innervational components are incorporated in it.

The meaning of this analysis is quite clear. It is only after eliminating from the picture of a movement such components as are imposed upon it from the periphery that it is possible to say that it really reflects the internal processes of innervation. Braune and Fischer, in their time, thought that it was possible to go about the matter in the opposite, synthetic way by setting determinate dynamic conditions and attempting to resolve the manner in which the complex, many-linked pendulum of a limb must move under these conditions. The problem is completely unresolvable mathematically if approached in this way because of its extreme difficulty. We have chosen the other approach, which is much easier, and which is in any event always possible. We take the evidence of real live movements which are already established and integrated by nature, and we then gradually free them of all their external mechanical components, that is always possible by analytical methods. After this preparation only the central innervational core of a movement remains and this is appropriate for further purely physiological investigation to reveal how, and in what way, the discrete events occur.

At present we stand only at the very beginning of this study. Much time has been spent on the development of techniques and not a little on the formulation of the questions. Nevertheless, some observations, still difficult to systematize, have already begun to accummulate in directions that reveal the nature of biodynamic elements. Some of these observations I summarize here.

If the curves for the forces in various limbs of the body are examined together, in terms of the same somponent, then it is at once strikingly obvious that whole series of these elements are common to all limbs. In all the curves for a given moment there occurs, with a greater or lesser degree of synchrony, a distinct direct wave (maximum wave) or a reverse wave (minimum wave). In other cases this generality is not apparent, and sometimes the maximum wave of one limb or system corresponds more or less in time with the minimum wave of another limb or system. We designate waves of the first type as *univocal* (i.e. single-signed), and waves of the second type as *contravocal* (i.e. opposite-signed). Exceptions are almost unknown to the rule that the *point of the body on one side of which the waves are univocal, and after passage through which they become contravocal, incorporates the active dynamic source of these waves*. If at any given moment all points of the leg located below the knee display greater or lesser synchrony at the maxima of their force curves, while, above the knee, instead of the maximum there appears a minimum in the curves of the same components, this signifies that the dynamic cause of all the waves which have been examined lies in the musculature about the knee. We see an expression of this law both in the case of the reactive pair n_a–α described above and in the case n_ε-ζ in the walking of children.

It follows from this observation that if any given wave can be followed in the same form through the entire length of a given limb, the source of arousal of this wave is located outside this limb. In this way we may be sure that the sources of the b_1, b_3, b_4, b_5 waves of the main vertical thrusts in walking lie outside the leg, since they appear as univocal waves in all the force curves of the leg. Moreover, as these waves are univocal for both sides of the body they cannot be referred to the hip musculature of one leg - yet another proof of their high degree of integration.

The same cannot be said of the b_2 wave in the thigh (see Fig. 24), which we see to be in almost exact synchrony with the trough (minimum) n_2 in the curve for the vertical forces in the shin and foot. The change of signs takes place in the region of the knee, which means that the musculature of the knee is somehow involved in the appearance of this pair b_2–n_2 (it can hardly be involved on its own because the case is complicated by the absence of a strict synchrony between b_2 for the thigh and n_2 for the shin and foot).

Clearly, most longitudinal waves in the swing period (other than α) arise outside the leg; in the curves for running it is possible to follow the univocality of the majority of these waves over an interval approximately from δ to ζ with particular clarity. At the same time the origin of these waves is clearly established by the circumstance that the waves simultaneous with them in the other leg are opposite in sign to the former (see Fig. 27(b)); this means that the source of the force lies in the hip musculature – in the region joining one leg to the other.

There are signs which allow us to specify, in many cases, not only the muscular region where a given element appears but also its deeper innervational properties. We are obliged to consider a number of cases (running, marching, etc.) where the force wave rises and falls in strict parallellism with changes in the angle of articulation; the more strongly, for example, the hip joint is extended, the more intensive becomes the flexion moment in this joint. The maximum of the angular displacement and the maximum of the force or of the moment become almost absolutely synchronous, or the maximum of the force lags by a few milliseconds in relation to the maximum angular displacement. This relationship leaves no doubt of the fact that the given wave is a reflex produced by the extension of a muscle group; a reflex of the myotatic or Eigenreflex type, etc. Similar phenomena are also regularly encountered in running (the n_A wave), in the goose-step (n_2), and in stepping tot three-part time (n_1). Apparently these occur only under conditions of very considerable angular displacement of the limbs from their mean positions.

There are waves which display very little relationship to the degree of angular displacement, but a very close correspondency to position. As we have already said above, in general the majority of dynamic phases in normal healthy walking are very closely related to the specific positions in which they occur. This is an indication that proprioceptive elements are involved in all of them. Some waves are, however, more easily separated from their usual positions by changes in the type of locomotion than are others. So, for example, we may be sure that in the various forms of marching the ζ wave is so indissolubly linked with the positions normal to it in walking that even the usual order of succession of waves is destroyed. Usually, in normal walking ζ begins after the reflection of the front thrust b_1, but at the precise moment when the leg reaches the ζ position. The b_1 wave is displaced by the conditions of the change in gait, but the ζ wave is not.

In running, a wave which corresponds to ζ in all respects begins, however, at another position of the leg than in walking. It is possible that we are not justified in calling this a ζ wave and it is also possible that it is simply the case that there are in general no positions in running that resemble the ζ position for walking, so that the entire kinematics of the transfer of the leg are completely different in the two cases.

This question as to the extent to which it is permissible to apply to one or another wave the symbol of a wave which has already been investigated in the context of quite another form of locomotion leads on to a very important general question: to what degree are waves which we have designated by the same symbols in various forms of locomotion really homologous and not simply analogous?

This question has been analysed by us primarily in relation to three groups of objects: in walking of children, running and military marching. In all these cases we have come to the conclusion that among the dynamic waves of various forms of locomotion we undoubtedly encounter analogies which have only an external mechanical relationship with each other and gross differences in their nature (for example, the n_2 wave in the Prussian goose-step and the n_1 wave in the triple-time step); however, it is usually not difficult to distinguish these from true homologs. In general, all the data on the course of the development of movements, and a careful analysis of qualitative variations, indicate that the waves to which we have given a single name in all these types of locomotion are real genetic homologs in the overwhelming majority of cases. Nevertheless, the question, for example, of the nature of the integral waves of the vertical components is still sufficiently complicated for us not to be able to establish any homology between running and walking, so that we have given the vertical curve for running a separate nomenclature.

Important material for the determination of internal similarity of different waves may be obtained by observing the chronology of these waves. We have only just begun an analysis of components of this type, but it is necessary to say a few words on the topic at this point. If we study the waves of the curves of various points of the limbs it appears that waves which are absolutely synchronous at all points of the limbs do exist (for example, ε' or n_{B2} in running), but only as extremely rare exceptions. Any given wave usually runs along the limb from top to bottom, or from bottom to top; some waves pass through these stages faster than others. The classic central spontaneous waves ε and n_α move along the leg from top to bottom - from the centre to the periphery. Waves which are reactive thrusts from external forces spread from the point of application of these forces, for example, the supporting front thrust C in running. It is still difficult to say whether this is absolutely correct.

It is interesting that for a number of waves which have been studied the velocity of the passage along the limb is close to the velocity of passage of a nerve impulse along a neuron. This raises a series of very interesting problems which will definitely form topics of investigations in the immediate future.

5. CONCLUSIONS TOWARDS THE STUDY OF MOTOR CO-ORDINATION

It is necessary as a general summary of this chapter to say a few words indicating in what respect the material published here will offer new points of departure for the understanding of motor co-ordination.

The understanding of motor co-ordination, like many other scientific goals, has been achieved by a negative method - through observation of the phenomena of lack of co-ordination - and has been only gradually enriched by the accumulation of observations on pathological movement. Like all knowledge acquired by negative means it has constantly suffered, and suffers at present, from the absence of accurate determinations.

Knowledge about the processes of co-ordination is not obtained deductively from knowledge of the effector process. Until the present, while the moving periphery has been regarded as an exact somatic projection of the central effector apparatus, reproducing in the form of movements with particular exactitude and simplicity all those chains of effector impulses which operate in it, there has seemed no need for any special physiological organization in the form of co-ordination. If the centre transmits a regular and efficient chain of effector impulses to the periphery, it will appear at the periphery in the form of equally regular and efficient movements; if the chain of central effector impulses is irregularly and inefficiently organized, its peripheral projection will also be an irregular and badly organized movement. But in both cases the movements will be *co-ordinated*, that is, they will accurately reflect what is contained in the central impulse. Both of them will be accurately fulfilled (reflected) at the motor periphery, in exactly the same way as a grand piano reflects with equal accuracy the playing of a good or a bad pianist.

Knowledge of co-ordination ought to be applied to the explanation of the effector process only from the moment when it becomes clear that the motor periphery does not have such rigid mechanical connections with the centre as were taken for granted in the preceding examples. *Movements are not completely determined by effector processes.*

But, if it is inadequate to send to the periphery any completed effector impulse in this way, and it is still necessary to attend to the periphery to ensure that it has obeyed by being moved in the required direction, there must exist together with the initial effector system more or less complicated auxiliary systems which ensure constant and complete *control* of the periphery by the centre. The deeper the functional gap with the absence of univocality between the centre and the periphery, the more complex and unstable is the real relationship between impulses and movements; the greater (in mathematical language) are the number of degrees of freedom of the motor periphery relative to the central effector, the more complex and delicate must be the organizational control to which we have referred. This organizational control is motor co-ordination.

In this context the idea of co-ordination is in the closest relationship to the idea of *functional non-univocality* of the connections between the motor centre and the periphery, between impulses and effects. The more our knowledge of the forms and types of these univocalities increases, the deeper becomes our understanding of the co-ordination of movements.

At present a whole series of sources of this indeterminacy are known. In the first place we must recall the *anatomical* sources described earlier. The fact of the presence of a large number of degrees of freedom of movement at the joints, and more so in the complex kinematic chains found in the make-up of the organism, provides very many conditions for indeterminacy. Among these we may count the impossibility of the existence of fixed anatomical antagonists at many joints; the variation in the function of one and the same muscle group at a multiaxial joint in relation to the disposition of the limb segments; the multiplicity of action of muscles, first described by Fischer, where they act on more than a single joint, and so forth. Amongst anatomical sources we must also mention the fact of multiplicity of innervation of the skeletal muscles, resulting in their convergent motor dependence on a whole series of conduction pathways both in the central and in the autonomic nervous systems.

Next there are a number of sources of indeterminancy of *a mechanical* order such as we attempted to investigate in experimental terms. Among basic facts of principal significance in this respect we must include, first, the fact described above of a closed dependence between muscle tensions and movements - a fact establishing the presence of indeterminancy in strictly mathematical expressions and directly pointing to the necessity for at least two conditions of integration independent of the primary effector impulse.

The second principal fact in this group is the existence of a high degree of mechanical complexity in the multisegmental kinematic chains (of the limbs and, in particular, of the body as a whole) which conditions the great abundance of all sorts of reactive forces and moments in these chains and makes them extremely capricious and uncertain instruments for the fulfilment of movements. We should note here that the mathematical theory of pendulums with many links is extremely complicated and leads to solutions only for a few particular cases, and that so far we have been quite unable to employ multilinked kinematic chains in which more than a single degree of freedom is used at one time in contemporary technology.

The third group of sources of indeterminacy has begun to be described only during the last 10 or 20 years as new data have been obtained. This is the *physiological* group. In this group we must include all such data as give evidence of selective relationships between the motor periphery and effector impulses reaching it. If in previous years the pathway followed by the effector impulse from the giant pyramidal area of the cortex to the myoneural plates appeared to be (functionally) continuous and uninterrupted, now the matter must be considered in a new light. The selective and integrational character of the function of the synapses at the anterior horns has already been explained by Sherrington. The principle of isochronism of Lapicque may serve as a possible explanation of these phenomena. The observations of Adrian's school on the spinal transforms of rhythms when compared with the abundant material on lability and parabiosis emphasize still more the physiologically active nature of the latter synapses. The selective character of myoneural transmission has been demonstrated by Lapicque and has also been elucidated in an original way in the studies of Paul Weiss. The active filtering role of the motor periphery in respect to the impulses that reach it appears to hold true for all these data, as does the fundamental dependence of this activity not on the central relationships but on the afferentational field.

All these many sources of indeterminacy lead to the same end result; which is that the *motor effect of a central impulse cannot be decided at the centre* but is decided entirely at the periphery: at the last spinal and myoneural synapse, at the muscle, in the mechanical and anatomical changes of forces in the limb being moved, etc. It is thus obvious that the decisive role in the achievement of motor control must be played by *afferentation* and that it is this which determines the physiological conductivity of the peripheral synapses and which guides the brain centres in terms of the mechanical and physiological conditions of the motor apparatus. The central effectors achieve co-ordination of movements only by plastically reacting to the totality of the signals from the afferent field, adapting the impulses transmitted to the situation that actually obtains at the periphery.

Co-ordination is therefore a type of complex sensory motor reflex beginning with afferent input and ending with an exactly adequate central answer. The afferentational input part of this reflex is, however, itself a form of reaction of the periphery of the body to the beginning of course of a movement. We observe in this phenomenon a sort of inverted reflex where the effector pathway acts as the exciting arc and the afferent nerve pathway acts as the reacting arc. The point of closure of this reflex arc appears here as the functional connection between motor impulses and the resulting movements, i.e. the same area of physiological indeterminacy which was discussed above. For this reason this functional area deserves a metaphorical description as a peripheral synapse.

Because this is so we may guess that the structure of the co-ordinational reflex differs considerably in principle from the sensory reflexes known to us from other areas; the co-ordinational reflex is not an arc *but a closed circle with functional synapses at both ends of the arcs*. In this reflex the centripetal impulses as in all other reflexes are transformed above into centrifugal ones, but the centrifugal impulses going out to the periphery are there rapidly converted into new centripetal impulses.

Like very other form of nervous activity which is structured to meet particular situations, motor co-ordination develops slowly as a result of experiment and exercise. Since co-ordination is, as we have established, a means of overcoming peripheral indeterminacy, it is clear that the basic difficulties for co-ordination consist precisely in the extreme abundance of degrees of freedom, with which the centre is not at first in a position to deal. And, in reality, we observe as a rule that improvement in co-ordination is achieved by utilizing all possible roundabout methods in order to reduce the number of degrees of freedom at the periphery to a minimum. When someone who is a novice at a sport, at playing a musical instrument or at an industrial process first attempts to master the new co-ordination, he is rigidly, spastically fixed and holds the limb involved, or even his whole body, in such a way as to reduce the number of kinematic degrees of freedom which he is required to control. Invertebrate organisms have in their make-up a form of co-ordinational surrogate in mechanisms of muscular locking (*Sperrung*) which by physiological means eliminate such degrees of freedom as are unnecessary at any given moment. But we must add that all lower forms of vertebrates (up to birds inclusively), for which the striatum still predominates over the cortical hemispheres, possess analogous auxiliary muscular mechanisms and employ them widely. Lizards, snakes, many brooding birds (eagles, parrots, etc.) are as rigid as statues in the intervals between voluntary movements. Reptiles show particularly clearly a statue-like stiffening of the body as soon as successive voluntary movements cease. If a lizard turns head to tail its body and limbs are motionless as sculpture. Mammals, apparently in healthy condition, find similar locking completely superfluous, and return to it only in cases of diseased hyperfunction of the extrapyramidal system (catalepsia, catatonia, hypertonic symptom complexes in encephalitis). In the norm there is no rest in mammals and in human beings, and outside of deep sleep there is no similar immobility; careful observation of standing or sitting human beings, dogs or cats give evidence of this. Even the set immobility of a cat or a tiger is quite unlike the immobile period in a reptile (or a spider) - it is sufficient to watch its tail.

Artificial tetanic elimination of superfluous degrees of freedom which is observed in the form of tenseness and constraint gradually gives way during the course of training to complete freedom. Having mastered the first degree of freedom the organism increasingly raises its ban on further degrees of freedom. Where there has been a high degree of expensive tetanic fixation, now there comes a greater economy of movement and a diminution of fatigue. Here two successive stages of release may be observed. The first degree corresponds to the lifting of all restrictions, that is, to the incorporation of all possible degrees of freedom. They no longer interfere with the movements of the organism but introduce complicating reactive phenomena, additional oscillatory frequencies, and so on. The organism has learnt to extinguish them, not prophylactically (by eliminating the given degree of freedom as a whole), but in an innervationally reactive way (by means of single dynamic impulses). The second, highest stage of co-ordinational freedom corresponds to a degree of co-ordination at which the organism is not only unafraid of reactive phenomena in a system with many degrees of freedom, but is able to structure its movements so as to *utilize entirely the reactive phenomena* which arise. Our material allowed us to examine a great many such cases, both in great masters of movement and in advanced students of movement.

Apparently this second stage corresponds to that described by sportsmen and music teacher as 'relaxing', a phenomenon which they instinctively feel but which they do not know how to describe. The economical effect of the transition to this stage is apparent; not only is there evidence here of minimal use of physio-reactive-innervational impulses, but it is also the case that all those mechanical-reactive forces in the complicated link systems, which, in the best cases, occurred without damage at the previous level of co-ordination, are used in a positive sense. This second co-ordination level is a biological control of highest perfection which explains the great wealth of kinematic utility of the degrees of freedom in the higher mammals - evidence that a level of co-ordination is possible at which this wealth is of immediate use.

It is much more difficult to determine the means by which the organism so far overcomes the internal physiological indeterminacy of the periphery; it is, however, perhaps possible to make a few suggestions at this point.

All that has been said above on the initial suppression and later use of the multiplicity of degrees of freedom may be put in the following way. The movable limbs of the organism always do respond in the same manner to the impulse which is transmitted through the muscles. The secret of co-ordination lies not only in not wasting superfluous force on extinguishing reactive phenomena but, on the contrary, in employing the latter in such a way as to employ active muscle forces only in the capacity of complementary forces. In this case the same movement (in the final analysis) demands less expenditure of active force. It is possible to express this (by means of an analogy with simpler, but similar, electrical phenomena) in the form of a statement that in the second case the reactive resistance of the moving system to the neuromuscular impulses is less than in the former, or, which amounts to the same thing, that its reactive conductivity is higher in the second case than it is in the first. A higher degree of co-ordination secures a higher reactive conductivity to muscle force impulses at the periphery. We must note that, because this heightening of conductivity is entirely achieved by employing the *dynamic* transitory phenomena, such high conductivity cannot be a stable or constant value. The mastery of co-ordination must consist in the ability to give *the necessary impulse at the necessary moment*, seizing the fleeting phases of higher conductivity of force and avoiding those phases during which this conductivity falls to low values.

It is quite in order to draw an analogy here between the examples of reactive-mechanical conductivity which we have studied and those phenomena of changes in synaptic conductivity, mentioned above, which have been explained by neurophysiology in recent years. Observations both by Lapicque and by P. Weiss indicate that the effector impulse *may arrive but not pass through*. It is obvious that at this level also the most effective impulse will be that which arrives at a synapse at the moment of its attainment of a level of highest conductivity, or that which is, by its nature, most adequate to meet the conditions of conductivity (lability) at the final synapse (which from the point of view of the result amounts to the same thing). It follows that in this case also co-ordination will consist in the ability to order events so that the effector impulses will encounter conditions of the greatest physiological conductivity at the periphery and will not collide with phases of physiological refractoriness which would doom them to ineffectuality.

The role of co-ordination at this level must therefore consist in the *preparatory organization of the motor periphery in order to guarantee optimal selection of conductivity.* This opinion is extremely unusual but necessarily proceeds from the facts. As the effector impulse cannot in principle bring about by itself a co-ordination without being entirely dependent on afferent processes, we must not regard this impulse as somehow precisely differentiated - as there is no biological basis for this. There is no possibility that accurate *effector* differentiation can be developing here because, as has been shown above, the peripheral indeterminacy demands different effector connections for each successive repetition of a given movement. Co-ordination at the level described lies basically not in the character and the accuracy of a tetanic effector impulse but in the accuracy of some sort of preparatory (not tetanic) effector impulses *which organize and prepare the periphery* for the reception of the right impulse at the right moment. The co-ordinational process does not enter into the composition of the tetanic impulse, or follow immediately after it; it goes *before*, clearing and organizing the path for it, and therefore must operate through quite different paths and employ quite different innervational processes.

What may be the path along which the co-ordinational process reaches this given level and what are the non-tetanic effector channels which it employs we may at the moment only guess. Nevertheless, it is possible to put forward a few hypotheses in this area.

Firstly, there is no doubt that the co-ordination is certainly not organized independently at the periphery alone and that the preparation of the periphery for the selective transmission of 'the right impulse at the right time' discussed above is, to a very considerable degree, centrally determined (in Lapicque's order of 'sub-ordination' in its broader sense) by means of a proprioceptive reflex cycle. The co-ordinational process on the level described is obviously not a tetanic process, but it undoubtedly incorporates both receptor and effector components.

We find it very tempting to draw upon the concepts of *tonus* to explain the phenomenon described here.

The physiological data available on tonus has considerably extended the initial ideas on this topic which incorporated first only the idea of a condition of elasticity of the muscle fibres. Without any more accurate determinations tonus, in the vocabulary of physiologists, began gradually to cover a very wide range of facts beginning with decerebrate rigidity and extending to Magnus and de Klein's tonus which has already been understood as a very generalized state of the motor periphery of preparation (in particular of the musculature of the neck and body) for the accomplishment of positions or movements.

The older, static concept of tonus as physiological elasticity constricted and retarded the understanding of these phenomena. It seems that there is at present evidence enough to decide upon a judgement, perhaps preliminary, and to say the following about tonus:

(a) Tonus as an ongoing physiological adaptation and organization of the periphery is *not a condition of elasticity but a condition of readiness.*

(b) Tonus is not merely a condition of the muscles but of the entire neuromuscular apparatus, including at least the final spinal synapse and the final common pathway.

(c) Tonus, from this point of view, is related to co-ordination as a state is to an action or as a precondition is to an effect.

If taken as working hypotheses these suppositions allow us to explain much more.

Firstly, one is struck by the fact (which was not considered before, but which after these hypotheses are made becomes quite obvious) that not a *single case* of pathological co-ordination is known in which there is not at the same time a pathology of tonus, and that not a single central nervous apparatus is known which is related to one of these functions without being related to the other. The cerebellum has for some time been known as an apparatus which is of decisive importance for co-ordination, and it is also the most important effector for tonus. The same is true of the pallidum and the nucleus ruber. Disorders of the vestibular apparatus lead to functional lack of co-ordination and to destruction of tonus. Section of the posterior spinal tracts results simultaneously in ataxia and atomia; both of these are the basic motor symptoms of tabes. Experimental deafferentation rapidly results in the disappearance of co-ordination and the disappearance of tonus. The number of examples of this type could easily be extended *ad infinitum.*

Secondly, we now see the significance of the flexible and reactive tonic reactions studied by the school of Magnus, which were, however, studied because of inadequacies of techniques, mostly in the static supporting organs of the body - the neck and trunk. It is clear that these systemic reflexes of high degrees of plasticity are decisive co-ordinational prerequisites to movement or positions and that their physiological purpose is not limited to the communication of a necessary and simultaneous rigidity to the trunk of the body but incorporates the entire preparatory reaction of the periphery to the conditions of the external (static and dynamic) field of forces.

Thirdly, tonus and those centrally directed mechanisms which regulate the conductivity of the distal synapses is very clearly explained by the circumstance that the anatomical substrate both of Rademacher's tonical phenomena and of Lapicque's subordination phenomena are one and the same - that is, the lower stage of the extrapyramidal system and the nucleus ruber group. Section of the brain stem at this level gives a picture of a disorganization of sub-ordination - of the return of muscular chronaxie to its constitutional value. It also leads to a picture of decerebrate rigidity, that is to say a picture of set, non-reactive spread of tonus, with marked appearance of flexion in some cases and extension in others. This correspondence is certainly not accidental and allows us to discuss the position of the anatomical substrate of the co-ordinational layer, referring it to the nucleus ruber group and to the paleocerebellum functionally related to it.

The innervational paths for the control of tonus in our model would be the rubrospinal and the vestibulospinal tracts for the tonomotor effects, and the sympathetic tract for tonotropic impulses.

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Human Motor Actions – Bernstein Reassessed
H.T.A. Whiting (editor)

CHAPTER IIIa

HOW CONTROL OF MOVEMENT DEVELOPS

C. Trevarthen

Bernstein's study of walking and running beautifully exemplifies his method of research. The observations are restricted to bipedal locomotion on a level surface in a straight line. Nevertheless the principles Bernstein elucidates would apply not only to other less regular paths and ways of getting about, but to gymnastics in which the whole body is thrown through the air and to arm movements displacing or intercepting objects while the body is standing or seated. Most importantly, his account of changes in locomotion during development from infancy, and of its involution in senescence, help clarify his general theory of the interactions between the cerebral centres (neural structures) and the periphery (motor and mechanical structures and sense organs); that is, between what he calls the central and peripheral 'synapses' or 'cycles of interaction'. This assists appreciation of what is involved in the perfection of mature coordinated movements.

The central principle of Bernstein's method is accurate measurement, in time and in space, of the displacements of joints and body segments. His measurements by the cyclogrametric method were much more precise than those of any of his predecessors or contemporaries. Physical and mathematical analysis of forces and moments about joints and at centres of mass of body segments are then carried out to tease apart the causes of movement. From such a rich description Bernstein derived, already in the late '20s, an entirely new picture of motor control in which the interplay of cerebral, corporal and environmental factors could be appreciated as never before. In his subsequent writings, he states and restates the laws of movement he discovered, using locomotion as his example with such intuitive brilliance that one experiences a discovery of a general insight into the brain's creative role in governing the interplay of forces in the concealed world of living body mechanics. Thus Bernstein built a genuine psychology of movement on physiological foundations, with far reaching implications for the psychology of cognition, motivation, learning and intention. Bernstein's findings on the control of human locomotion may be extended to gain a richer and more productive view on how objects and events are conceived. I believe they also provide an important insight into inherent mechanisms of human communication.

The following synopsis of Bernstein's 'laws' of movement is made in preparation for an evaluation of his account of the development of walking and running and comparison with some recent studies of the same problems. Than I shall endeavour to apply his concepts to findings of the last 15 years on infantile reaching and grasping, a form of movement which brings into being a rich, indeed unbounded, interrogation of the environment that begins well before the baby walks. Finally I shall attempt to derive, in the same spirit, some as yet speculative ideas on the genesis of expressive gestures by which the mover can engage the minds of other persons and transmit messages into their motor coordinations at the deepest levels. It would appear that humans have some implicit mastery of Bernstein's laws from the day they are born, and that they use this 'knowledge' not only to gain knowledge of what their bodies can do by acting on the world, but also to gain close communication with others, perhaps even becoming responsive to the mother's rhythms of displacement and patterning of somatic muscle activity before birth.

These are the laws of Bernstein's 'biodynamic structures' that make adaptive movement possible:

1. Movements, while they are caused by changes in equilibria between muscle tensions, cannot be in one-to-one relationship to excitatory neural impulses or forces of muscular contraction.
2. Events at the periphery of a moving member generate external, non-neurogenic or mechanical forces that may contribute substantially to the movement while it is happening (the 'peripheral cycle of interaction').
3. Peripheral force vectors must be responded to accurately and smoothly with the aid of proprioceptive relay of their effects into the motor centres, if there is to be "adequate coordination and correspondence of the movement to the animal's intention".
4. The regulation of movement in plastic response to sensory feedback requires a central structure that can perform accurate <u>anticipatory</u> tracking of peripheral stimuli, by changing responsiveness in the motor centres, or by a change in sensitivity of the receptors commanded by the centres. (This coordination cannot be reflexive, motor response following proprioceptive signal in a fixed manner with an irreducible delay. The time taken to integrate new information from receptors into a movement, at least 50 milli-

seconds and often as long as 250 milliseconds, is much longer than the duration of some large force changes in the system. Reflexive correction would come too late and it must be exceptional).

5. Well-coordinated movements have a regular, automatic, rhythmic or oscillatory structure (manifest in the small number of harmonic components of force curves), in relation to which events in all moving segments of the body are virtually interdependent. (Cooperative reciprocities of motion and support in linked segments and the smooth flow of forces down the kinematic system rely upon this 'orchestration' of the force momenta which is necessary to, and an expression of, the accurate anticipation of resultant mechanical events at the periphery by the centre).

6. Adjustments to a change in goal or tempo for a limb movement must involve generation of a new force program transmitted throughout the body, since change of movement in one segment alters the relationships between forces everywhere.

These laws define the characteristics of a complex dynamic organization of motor centres which exploit the peripheral mechanisms and respond to their signals.

An important corollary of predictive programming of movements by anticipatory recoding of proprioceptive response (Law 4) is that information obtained through exteroceptors about impending contacts with exterior objects must be immediately assimilated to the plan for movement that has been built up to take in data from mechanoreceptors. Surfaces against which locomotor thrusts are made and objects picked up are capable of adding large forces at the periphery. These effects must be taken into the calculation of the movement plan before they arrive. Mechanoreception verifies the immediate condition of the body. Exteroceptive assimilation may track the outside world's potential for assisting or destroying the movement, or for providing a reference to detect departure from a planned course. Thus efficient planning of movement requires perception of external events and resistant media. There will be a need for reciprocal adjustments and associations between all sensory modalities capable of transmitting information on the impending interactions between the moving body and the environment. For example, vision of the path ahead can provide unique data to forecast how and when mechanical perturbations are to be met when the feet touch the ground. Conversely proprioceptive readiness to monitor effects of catching or picking up an object will constitute a part of the description of that object's properties, added to information obtained by visual perception of its size, substance and location or velocity.

David Lee's concept of visual exproprioception explicates the direct motor coordinative function of vision and corrects a traditional bias towards thinking of vestibular and mechano-receptor channels as the only direct inputs for control of posture and locomotion (Lee, 1978, 1980). In lower vertebrates tactile, vestibular and lateral line systems have a dominant role for coordinating the tight automatisms of their body action as they swim through water or crawl on their bellies. Exteroceptive senses, especially vision, set a spatial frame around this, guiding head displacement (Harris, 1965; Trevarthen, 1968). In more active vertebrates, with more complex multiarticulated bodies, problems of motor coordination are greatly multiplied and direct visual input is necessary to guide their much more dynamic posture with, as Bernstein describes it, many more 'kinematic degrees of freedom'. Visual projections to their more developed neocerebellar 'computer' of motor coordinative programs undoubtedly contributes to this control (Glickstein & Gibson, 1976). Cetaceans (whales and dolphins) offer an extremely interesting special case shere sonic rather than visual guidance of high speed and extremely agile propulsion is associated with an enormous development of the cerebellum and parietal cerebral neocortex.

Bernstein considered neuro-anatomical interpretation of the control system required by his laws, and his most significant conclusion was that systems with different control functions can only be segregated spatially in the brain if they have the tightest intercommunication so that they may act for the most part at the same time and in a predetermined relationship, as do the efferent and afferent components of the peripheral motor system. Predetermined and controlled sequences of action in different hierarchically related parts of the peripheral motor system (trunk, limbs, distal appendages, head) with different relationship to specialized receptor structures, express an hierarchy of control systems or modes of action in the brain (Trevarthen, 1978). Their coordinated activity would require the existence of a superior level of representation that can determine their joint effects at the periphery in engagement with the environment. A common cerebral organization with respect to the spatial and temporal framework in which behaviour takes place is therefore necessary. I have elsewhere considered the ways in which inherent somatotopic patterning (body mapping) in major cerebral systems provides this integration during brain development (Trevarthen 1974a, 1979a, 1980a).

After explaining the advantages of locomotion for the study of universal principles of motor coordination, Bernstein shows that the regular beat of thrusts to keep the body above the ground as the feet exchange support (the "struggle against the force of gravitation") is fully represented in vertical component of forces in body and limb segments. The longitudinal (front-back) component of the forces of leg swinging carries more information about the local muscular activity that rotates foot, shin and thigh round the joints, and about the reactive forces to which these rotations give rise. As the weight of the body is transferred from one foot to the other, forward and backward thrusts alternate in a fixed pattern. Other longitudinal force waves reveal how the limb is transported when it is off the ground. In running both feet are off the ground for most of the time. They move over a larger space at higher velocity with each step than in walking, and thrust harder and in a more horizontal resultant direction against the ground.

The alternating movements of a leg in walking are not driven by a simple succession of forward and backward swings matched to opposite movements in the other leg. There are many other "strikingly constant and general" elements in the dynamics of each step which appear in unchanging sequence in the longitudinal force curves and give it a "radically different physiognomy form that which may be observed in the simple stepping reflex of a decerebrate preparation". Bernstein concluded "The structural elements of muscle action ... are of essential coordinational significance for the locomotor act and ... they must consequently have a peculiar genesis, history and basis in the central nervous system and elsewhere". Thus he lays the foundation for discovery of the innate adaptive structures that govern motor coordination.

Taking each segment of the limb in turn and examining the cycle of forces about joints or at the centres of mass, attending even to minute regularities and events separated by as little as one or two milliseconds, it is possible to discern the place of origin of component events that drive the surprisingly regular displacements. Bernstein explains how he distinguished longitudinal accelerations and decelerations due to muscle contractions (e.g. the primary ε and $n\beta$ waves) from reactive inertial forces. The identification of the large α waves in the force momenta about the knee exemplifies this. This wave is reflected as a backward (extensor) thrust on the thigh from contraction of the extensors of the knee. As the shin and foot are thrown forward, the thigh is forced back against concurrent flexor activity at the hip. Immobilisation of the knee by a splint all but abolishes the effect. Thus, except for a small proprioceptive component in skilled running, the α wave proved to be purely mechanical.

Bernstein then describes how the attention of his "telescope" passed from such large events ("stars of the first magnitude") to more informative smaller features. Thus the succession of ζ waves, occupying about 200 msec, which are seen at centres of gravity of the thigh and shin of the leg that is flexing and moving forwards through the air while the body's weight is being transferred to the foot recently planted on the ground, were identified as 'reactively innervated' or responding to proprioceptive feedback. Presumably this discharge of pulses is part of an active balancing which the body must execute and the brain must record very precisely at this moment. One might expect that events coordinating the two legs in coming to a standstill, in moving over irregular and unstable footing and in changing direction would be strongly represented at this phase of the locomotor program - but Bernstein only makes passing reference to such variations in walking, stating that the standard ζ waves are not displaced in distorted walking such as three-part marching. The curves presented in Bernstein's Figure 27 for the skilled running of the athlete Ladoumeg have a regular succession of small waves, including the complex labelled ζ, of which Bernstein only says that they are exactly repeated in the same dynamic instants of limb displacement and they prove the existence of a highly efficient "correct response to proprioception".

I note that in this figure the longitudinal forces at the centres of gravity of Ladoumeg's right and left legs while running have remarkably even phases, but different frequencies of oscillation. Could there be different proprioceptive governors for the two legs? Was the difference (right leg = 18.5 waves/sec; left = 16.7 waves/sec) maintained continuously underneath the precise complementarity of major force waves in the two legs? Bernstein gives no comment on this phenomenon though he numbers the phases on the figure. Research by Grillner and Zangger (1979) with locomotion in spinal cats on a split belt with the two halves moving at different rates shows that the spinal locomotor system does indeed have separate left and right generators which, however, can be coupled. It would be interesting to know if right handed humans tend to have a slower system on the left side of the spinal cord. Does their inherent lateral asymmetry extend to spinal centres?

Bernstein's studies of walking and running in children demonstrate the rather surprising fact that precise cerebral control of the forces arising from movement is achieved on a time scale of decades comparable with the learning of language and 'skills of culture'. The order in which features of control are acquired casts light on the complex hierarchy of processes by which the brain increases its repertoire of 'soft ware' or schemata, not only for cognitive mastery of objects and actions, but also for control of the body in action. The full potentialities of the innate neural systems adapted to walking and running are achieved only after years of practice as limbs grow and muscles gain in power. A significant correlate of this development of agility and gymnastic skill in childhood is the increase in size and complexity of structure in the cerebellum which has a postnatal development comparable with that of the slow maturing 'uncommitted' cerebral cortex (Yakovlev & Lecours, 1967; Jacobson, 1970).

The force curves of the limbs of infants show only the primitive pair of impulses ε and n_α (later n_β) which produce an automatic pattern of stepping. At this stage the vertical forces at the centre of gravity of the head are "completely ordered and invariable from step to step, although primitive in form ... Meanwhile, the *movements* of the head are extremely chaotic, uneven and involved". Different children show similar force curves but "extremely diverse and individual curves of displacement". This is Bernstein's "innervationally primitive stage" in which highly irregular patterns of displacement are produced by regular motor output, as in the ataxia of a patient with loss of proprioceptive tracts (*tabes dorsalis*). Early walking, far from being driven by stimuli, is comparatively incompetent at responding to afference, like that of a deafferentiated patient.

At about two, new force elements appear, but they are weak signs and they disappear when the child walks quickly. The whole inventory of longitudinal force waves is only completed about five when the adult "innervationally reactive" form of the wave complex appears. Then there is what Bernstein describes as "an extremely interesting overemphasis of structural details" between five and eight years and "this overabundance energetically involutes between eight and ten years, but even by the age of ten the process of formation of the adult structure has not been entirely completed".

The first developments Bernstein attributes to maturation of proprioception, the "slow and gradual appearance of the mechanisms of adequate response to this signalling". In small children "the spontaneous effector impulses are already developed while the innervationally-reactive impulses do not operate in conjunction with them". In other words the plan for efficient assimilation of proprioceptive input has not yet been formulated. A poor tracking of body position is achieved, so the dynamic phases of force curves do not occur in constant correspondence with positions of the body as they do in adult walking. Thus in children under three unequal steps are obtained for constant values of the immature ζ waves and to obtain steps of constant length variations of ζ are required. In adults both stepping and the ζ waves are constant, and Bernstein concludes "this means that even before the completion of the main impulse ε, an exact regulation of the direct longitudinal forces on the basis of proprioceptive signalling is necessary in carrying out steps of constant metrical proportions. This preparatory regulation is not yet developed in the child".

I have repeated these extracts from Bernstein's text to emphasize that the most important contribution of this chapter is the definition it gives to a strategy of behavioural development which involves formation in the brain of a dynamic image of the moving body, and the achievement of precise temporal and metrical prediction of reafferent feedback effects.

Figure 1 reproduces Bernstein's curves of acceleration of the knee for a child of three years five months (A) and an adult (B) and puts them on the same time scale. I have added the periods of support for the child which are missing from Figure 28(b) in the translated paper. Two cycles are shown for the child which illustrate the variation, for example, in ζ and γ waves. These curves show numerous differences, but the most significant in relation to the whole story of the development of walking are the appearance of a period of double support, and two features of the transfer period. The childish ζ, which Bernstein claims is essentially a mechanically reactive curve, is replaced by the regular adult preprioceptively fed but preprogrammed ζ, and there is added the large mechanical α wave which has a significant role in the adult movement to assist extension. In other words the slower and more powerful adult program tracks mechano-receptor feedback closely and juggles the leg efficiently as a chain of penduli. Only part of the very regular force profile of adult walking cycle is shown.

Bernstein outlines how running diverges from walking in the second and third years of life. He suggests that this process works from the periphery to the centre. As with walking, the program for running acquires accurate central representation of peripheral events after the running form has been created. This he calls the "secondary character of central divergency". It could be described as learning how to cope with the motor problems created by emergence of a new form of movement.

At this point Bernstein brings in a most interesting consideration of the relationships between central control for proximal (i.e. near the trunk) and distal components of the kinematic chain of the limb. He considers that the earlier development of control for proximal segments follows from the

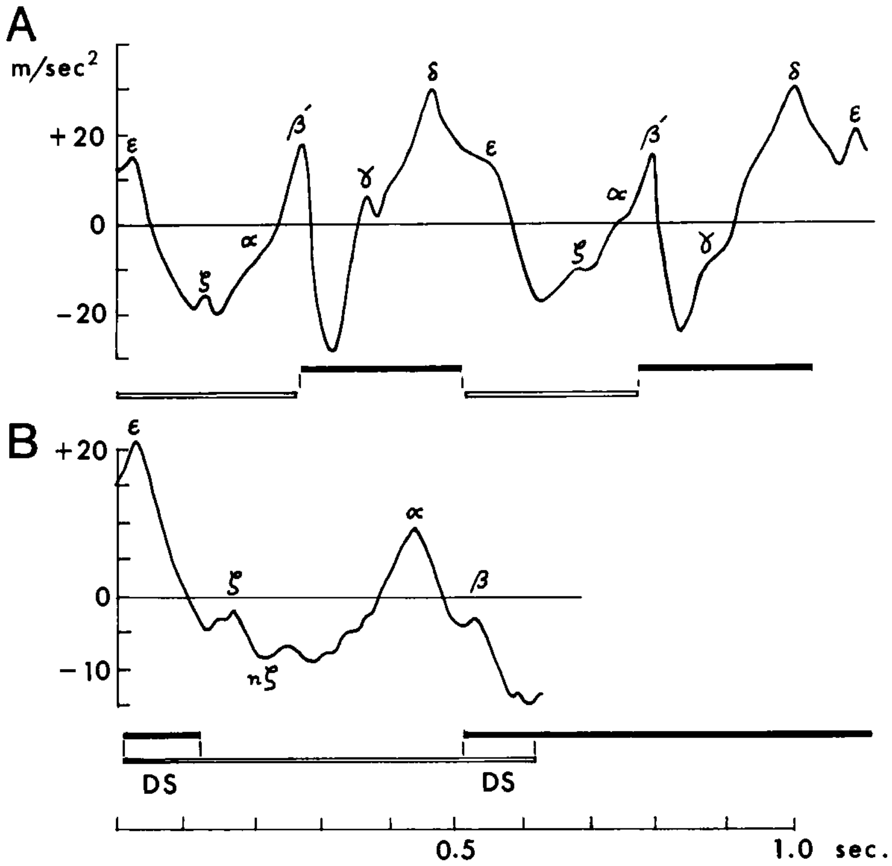

Fig. 1. Bernstein's longitudinal acceleration curves of the knee in walking; for toddler (A), and adult (B). Double support (DS) is absent for the child whose progression is half running.

fact that effector impulses gain more ready expression, with less peripheral interference in proximal segments. In contrast, to control the distal segments "the effector impulse either must be very strong or must coincide exactly with the moment when conditions in the distal system are particularly favourable for its appearance"; and then he says, "it is possible that this most opportune moment is a critical moment in the velocity of the limb when the inertial resistance is least perceptible". In other words, there will be a coordinative principle coupling placements or extension of the foot with a certain place in the phasic activity of the thigh and shin. This analysis applies to the placement of the feet in the substrate in agile walking and in running, a process which is in the adult more elaborate and exhibiting "a greater deftness, a higher degree of coordinational technique" as required for control of distal parts. As we shall see it is also vital to an understanding of grasping, catching and manipulating objects in the hand.

Bernstein points out that although children do not achieve adult efficiency in walking or running until past the age of ten, already at three or four they have excellent coordination and equilibrium, in that they can hop, pedal a tricycle, dance, etc. We must assume that the system coordinating the posture in relation to gravity and the supporting environment is present before biodynamic control is mature.

Important new information on the standing and walking of infants has come in recent years. Lee and Aronson (1974) proved that from the onset of standing the motor system is responsive to information from the surrounding visual framework about surfaces and edges. Slight movement of the walls and ceiling of a room suspended around an infant who has just learned to stand causes an immediate fall. Evidently the child begins standing and walking in a visual frame that the brain takes to be a fixed reference; displacement of this frame easily overrides the vestibular and mechano-receptive (somesthetic and proprioceptive) channels. Forssberg has observed standing and walking in infants with the aid of myograms and the Selspot system which tracks by television the positions of blinking infra red marker lights attached to limbs and computes displacements, velocities and accelerations directly, generating 'stick' figures to show positions of limb segments. He and Nashner (1982) found that automatic and rapid adjustments of standing to tilts of the support surface (somesthetic sensitivity of the foot and ankle) and movement of the visual surround were like those observed by Nashner for adults, but more varied, from 1½ years. Allowance must be made for the different dimensions of the body which put the child at a mechanical disadvantage. The infants also show automatic compensation for voluntary arm movements against an external load. Vestibular control could maintain an infant standing when support-surface information or visual inputs or both were eliminated (by unclamping the footplates or covering the eyes). Thus the basic control networks of the adult are functional, with about the same temporal efficiency, from the onset of standing. When somesthetic, vestibular and visual channels were put in conflict, however, children under 7½ years could not adapt to different contexts in which one or other channel had become unreliable. Thus below this critical age they became very unstable with conflicting sensory conditions, shifting weighting at random between the three channels. The authors believe that the development is due to the maturation of higher level neural systems that perform an "integrative" function, governing varied combinations of the automatic postural activities. Presumably this development is necessary to the achievement of efficient voluntary activity.

In a study of infant walking, Forssberg and Wallberg (1980) found that the highly irregular stepping of a newborn held over a moving belt may at times exhibit a considerable degree of coordination, but the steps support only about one third of the body weight at maximum thrust, lead to large vertical excursions of the body and are organized for support on the toes or digitigrade walking as in quadrupeds, not the specialized heel and toe (plantigrade) stepping that enables a human adult to balance the body efficiently over two feet. The leg of the newborn is kicked forward in an extreme forward swing as in Prussian marching and the foot raised high and then lowered with extension at the ankle to a support phase that can neither hold the body up nor propel it forward.

Developmental psychologists seeking evidence for theories of how voluntary action develops have been intrigued that newborn innate walking generally disappears after six weeks, when a supported infant is more likely to crouch or push out against the surface with both legs together. This has been taken as an example of the development of cortical efferent pathways of 'voluntary' control inhibiting inborn 'reflexes'. But the patterns of neonatal stepping are not reflexive and in some infants they do not disappear during the first year, their maintenance being promoted by artificial exercise. Towards the end of the first year as the child begins to crawl and stand, stepping when the body is held to assist balance become less exaggerated in flexion and gradually more regular. It supports the body at full extension and pushes it forward with less vertical oscillation than in the newborn. The foot is now placed with the heel striking first.

Comparison of neonatal walking with locomotor patterns of spinal cats indicates that the human pattern (which may be elicited within minutes of birth) is produced in rudimentary form by autonomous activity of the spinal "locomotor generator". Effective walking is 'learned' as supra spinal control circuits mature and establish influence over spinal circuits, a process which starts about two months after birth. One important consequence is that early ankle extension is suppressed. This and other changes in the timing of muscle actions converts digitigrade (primitive) walking to plantigrade (human) bipedal walking. But free unassisted walking occurs well before this matamorphosis is completed. Thus the control of equilibrium is achieved before the most efficient form of stepping develops (Forssberg & Wallberg, 1980; Forssberg, 1982).

Putting these results in relation to Bernstein's findings, one can see that effective proprioceptive management of plantigrade walking, with more subtle and regular modulation of both support and swing through, continues for some years after its inception. Modern analysis of the physiology of standing and locomotion in cats indicates that in a widespread pattern of developments in brain and stem and cortical tissues the cerebellum has a central place in this management of the most efficient proprioceptive assimilation (Forssberg, 1982).

We may conclude from the above studies that the fundamental neural program for standing and walking on two feet is present in a newborn and the spinal core of it is largely functional. Cerebral systems regulating posture and balance by mechano-receptive and exproprioceptive feedback are present in outline and they assist control of balance and posture from the earliest stages of actual standing and walking. Calibration of more efficient higher level controls takes a number of years, by a process which presumably involves active growth and differentiation of dendrites and synapses and maturation of axons in specific parts of the central nervous systems. Children achieve agile walking and running and learn special forms of progression in dancing, skiing and gymnastics by a process of selective reinforcement or retention of the most adaptive combinations.

In the course of these developments the structural basis for change in motor control is shown in both regressions in the overall efficiency of movement and exuberant flowering of feedback systems in active, varying and sometimes competitive interaction (Trevarthen, 1982). Every accurate developmental study of how movements gain guidance by specific control channels considered one at a time has revealed periods of sensitive and insensitive response to stimuli and periods of conservative or cautious exploitation of motor plans giving way to wasteful and experimental 'play' with control before a higher level of efficiency in control is consolidated. An example of change in sensitivity to feedback with maturation of a particular form of action comes from recent research on locomotion artificially assisted by means of wheeled 'crawlers' or 'walkers' before unaided crawling or walking is possible. Infants show variation in sensitivity to a visual drop (Rader, Bausano & Richards, 1980) and the evidence suggests that avoidance of the deep side of a visual cliff may occur for crawling before it does for walking; that is, the two forms of locomotion, quadripedal or bipedal are open to this form of visual guidance at different stages in the first year.

Now let us turn attention to the development of movements to reach and grasp or manipulate objects. These activities follow a developmental plan which has interesting points of resemblance to the maturation of walking. Again complex innate motor program undergoes systematic modification as sensory control processes are added. Even though the arm is a more complex kinematic system, with much greater freedom of movement than the legs, and dexterous use of the hands requires development of a much more elaborate cerebral control of the distal segments than is required for placement of the foot or clinging with the toes, object prehension attains a high level of proficiency with elaborate direction from the cerebral cortex before locomotion starts.

In 1967 and 1968 I made films at the Center for Cognitive Studies at Harvard of the reactions of infants from 1 week to 6 months of age to small objects that were suspended from a kind of fishing rod pivoted about the infant's head. From this a small brown furry toy was dangled about eye level and moved from side to side. I observed that not only were some babies one or two weeks of age capable of orienting towards and tracking the object with precisely coordinated rotations of head and eyes, even though their heads frequently wobbled considerably; they were also emitting small arm and hand movements towards the objects. Frame-by-frame analysis of films revealed that while the latter movements were unpredictable in occurrence and extent, they were certainly not random or reflex reactions. Observation of the coordination of arm and hand movements with small displacements of the object showed that they were definitely under some degree of visuo-spatial control. Nothing in the literature had led me to expect these 'pre-reaching' movements which I reported at a seminar at the Center. More detailed descriptions made later from films made in Edinburgh, with the aid of drawings made by projecting the film images on the underside of tracing paper, confirmed that voluntary prehension develops in human beings out of a form of visually age and directed towards an object moving slowly near the baby (Figure 2).

From the Harvard films of five subjects taken at weekly or biweekly intervals over the first five months I determined that the rather fluid total pattern of reach and grasp typical of newborns gave way to jerky, erratically aimed and fractional movements of a much more forceful kind as the infant became more robust in the second and third month. Ataxic swipes

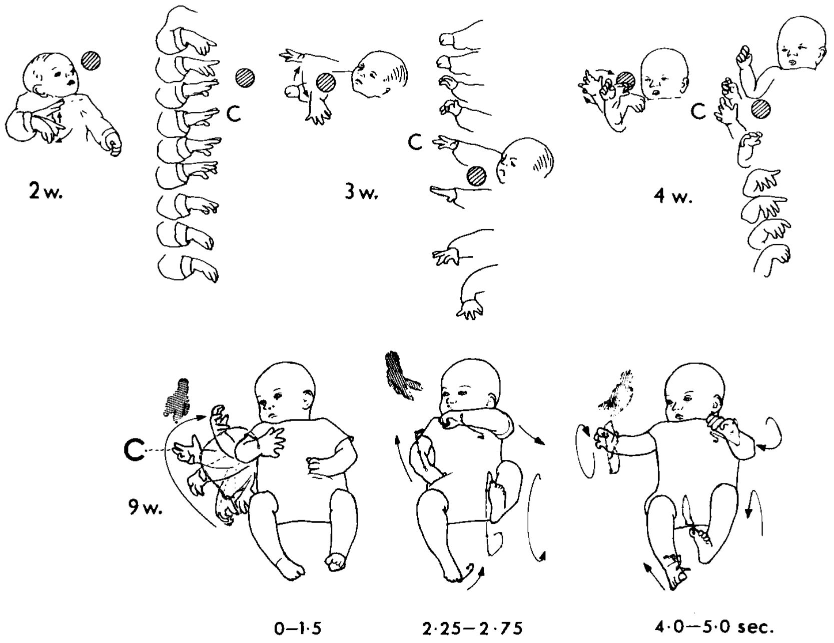

Fig. 2. Reach-and-grasp movements traced from films at 16 frames/second. Hand positions drawn at ¼ second intervals. Boy at 2, 3, 4 and 9 weeks.
Top: Newborn prereaching to a slowly displaced object varies from wrist extension to extension of the whole arm, but in all cases the fingers are maximally open at the climax of extension (C).
Below: At 2 months ataxic swipes are made without hand opening unless the infant is in a quiet state (left).

and grabs were then the commonest movements towards the toy (Trevarthen, 1974a, b; 1982). At the end of the fourth month a marked improvement in the steadiness and directedness of arm extensions occurred. Thereafter reaching and grasping followed the pattern of development described by other observers.

In previous work with split-brain animals I had seen that arm and hand movements of reaching to touch were under control of at least two complementary cerebral systems (Trevarthen, 1965, 1968b, 1975). Aimed arm extension, transporting the hand towards a visually located goal, could be directed by a system that was largely undivided after all the interhemispheric neocortical connections had been cut, but the mechanism for orientation and shaping of the fingers to make a discretely adapted touching or grasping movement was split into two parts, one serving each hand. Split-brain monkeys were able to catch rapidly moving objects efficiently with either hand when vision was restricted to one eye. This indicates that interception of the object was governed in them by an undivided or duplicate visumotor system different from the split one that controlled precise direction of dexterous manipulative movements - a result which gains interest in the light of von Hofsten's discovery, discussed below, that infants can catch objects efficiently before they have developed visually guided manipulation. I noted that while newborns were making integrated arm and hand movements of considerable delicacy in which proximal segments moved little, the 2- and 3-month olds were frequently responding to a suspended object, which they fixated and tracked avidly, with vigorous failing arm movements and clenched hands (Figure 3). Subsequently, as control of the extension of the upper arm improved so that a better aimed, slower and more regular reaching towards the object occurred, the hands again made guided grasping movements.

Studies by Kuypers and his colleagues (Kuypers, 1973) of the separate parallel motor systems of brain stem and spinal cord for musculature surrounding proximal and distal joints of the limbs led to a neat explanation of the effects of split-brain surgery on eye-hand coordination (Brinkman & Kuypers, 1973). It also offered the possibility of explaining developments in motor control during infancy in terms of successive waves of differentiation in a hierarchy of control systems. Proximal and distal segments of the forelimb, while designed to work concurrently in a cooperative manner, would be moved by systems with different types of reafferent sensory control and different phylogenetic histories. They could well have different epochs of maturation in the epigenesis of voluntary prehension, as indeed Kuypers had shown to be the case when he analyzed growth of cortico-spinal connections

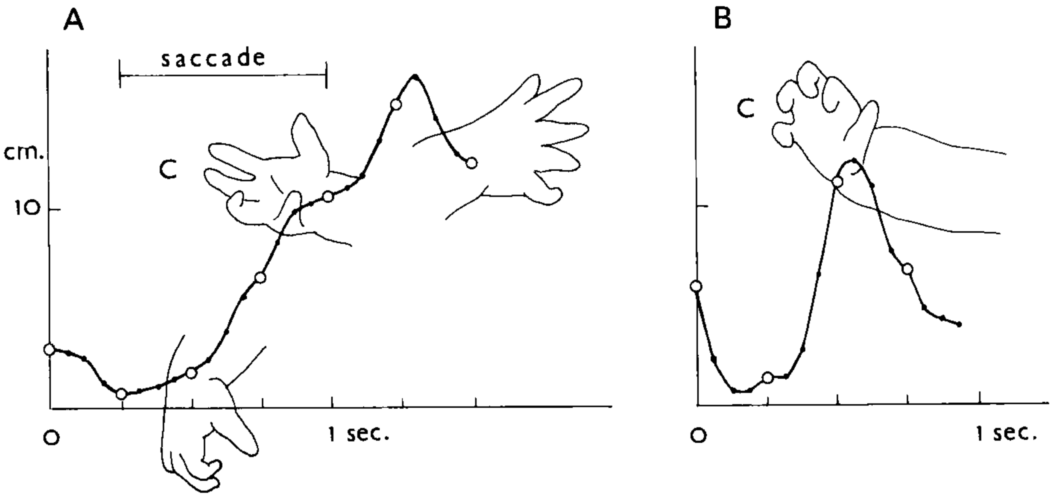

Fig. 3. Vertical displacements of wrist for 9-week-old shown in Figure 2, lower left and lower right. The slow reach, A, has a saccadic lift with slight deceleration at the midpoint. The hand is open at the brief climax (C). The swipe, B, is more accelerated and the hand is half-closed at the climax at which there is no arrest.

in young monkeys (Kuypers, 1962). The notion of assembly of a motor coordinating system by formation of transcortical connections between reflex arcs for different members and different special sense organs was replaced by one of progressive incorporation of feedback control loops for successive phases of action within an inherently programmed adaptive response system, or, to use Coghill's terms, by individuation of local reactions within a total pattern already given (Trevarthen, 1974a, 1978, 1979).

Since newborns could aim a reaching movement to something of a size that might be grasped it appeared that they could visually perceive the appropriate 'distal' object in the right part of the extracorporal space. I noted, however, that a perfect aimed 'pre-reaching' movement, commonly but not always coupled to a coordinated head and eyes orientation to the same locus on one side of the baby, was often produced by a quiet alert newborn when no object was present at that focal location. Therefore, although the form of movement was preadapted to grasp an object, its evocation was not dependent upon perception of a target. Its primary cause was a preformed neuronal network in which the appropriate patterns of excitation and transmission could arise either spontaneously or as a result of some unidentified extrafocal stimulation. (For example, the sound of movement of a person out of sight, or a change in the overall level of light).

In Figures 2, 3, 4 and 5 it may be seen that the patterning of prereaching with the baby seated upright is one which involves outward then inward rotation in the upper arm combined with weak extension or abduction then return, extension with supination then retraction with pronation in the forearm and extension then flexion of the wrist and fingers. When the infant was lying on its back, prereaching movements towards a suspended object appeared to be reprogrammed to take account of the changed pull of gravitation, which is evidence for rather complex postural controls. At the climax of the movement the forearm is balanced in a vertical position in both orientations of the trunk (Figure 4).

Patterns of finger extension and of apposition in flexion seem to vary widely in no obvious adaptation to the object's size and shape which is presumably poorly perceived, but frequently the hand shows signs of orienting with an outward or inward turning of the wrist. Therefore there was at least some organization of circuits in spinal motor centres for directing activity of the intricate muscles of forearm and hand that move the fingers. However, many hand movements appeared distorted by reflex grasping or avoidance responses. Both arm and hand were discoordinated when the infant was highly aroused or distressed, pre-reaching responses in orientation to a visual goal being clear only in reasonably calm and alert subjects.

Pre-reaching movements were too feeble and too stereotyped to achieve prehension with adequate guidance. They were also in prefunctional state with respect to body kinetics. Controlled arm extension, to allow progressive adjustment of grasp or prolonged handling of an object at a distance from the body or to track a displacing object and to transport an object once grasped, was beyong the newborn. The superordinate 'programs' for such accommodation of the movement developed in the next few months.

A preliminary kinetic analysis of pre-reaching movements exposes some interesting features of their genesis. Because the movement involves rotations uf upper arm and forearm about their long axes as well as extensions, retractions, abductions and adductions at shoulder and elbow, it can be only partly represented in one plane. Nevertheless vertical displacement of the wrist seen in the frontal plane gives a fair impression of the cycle which lasts about three seconds, reaching an apex in one second (Figure 5A). The movement oscillates and there is a preponderance

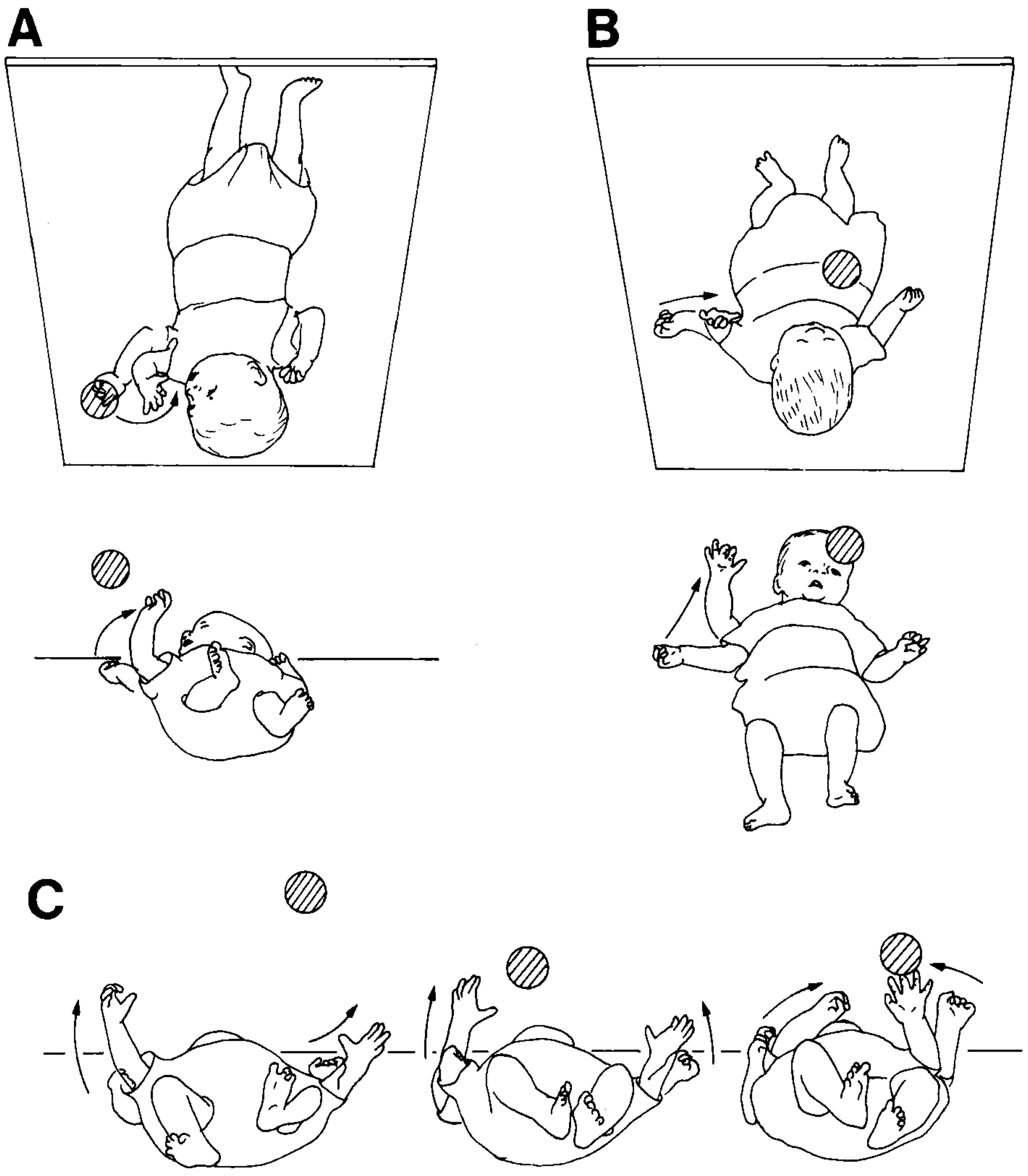

Fig. 4. Prereaching with different body positions, 2 to 3 weeks. A, lying on back; B, in chair with back inclined 15° from vertical. Mirror shows vertical downward views. C, successive attempts of a prone newborn to reach for a slowly displaced ball.

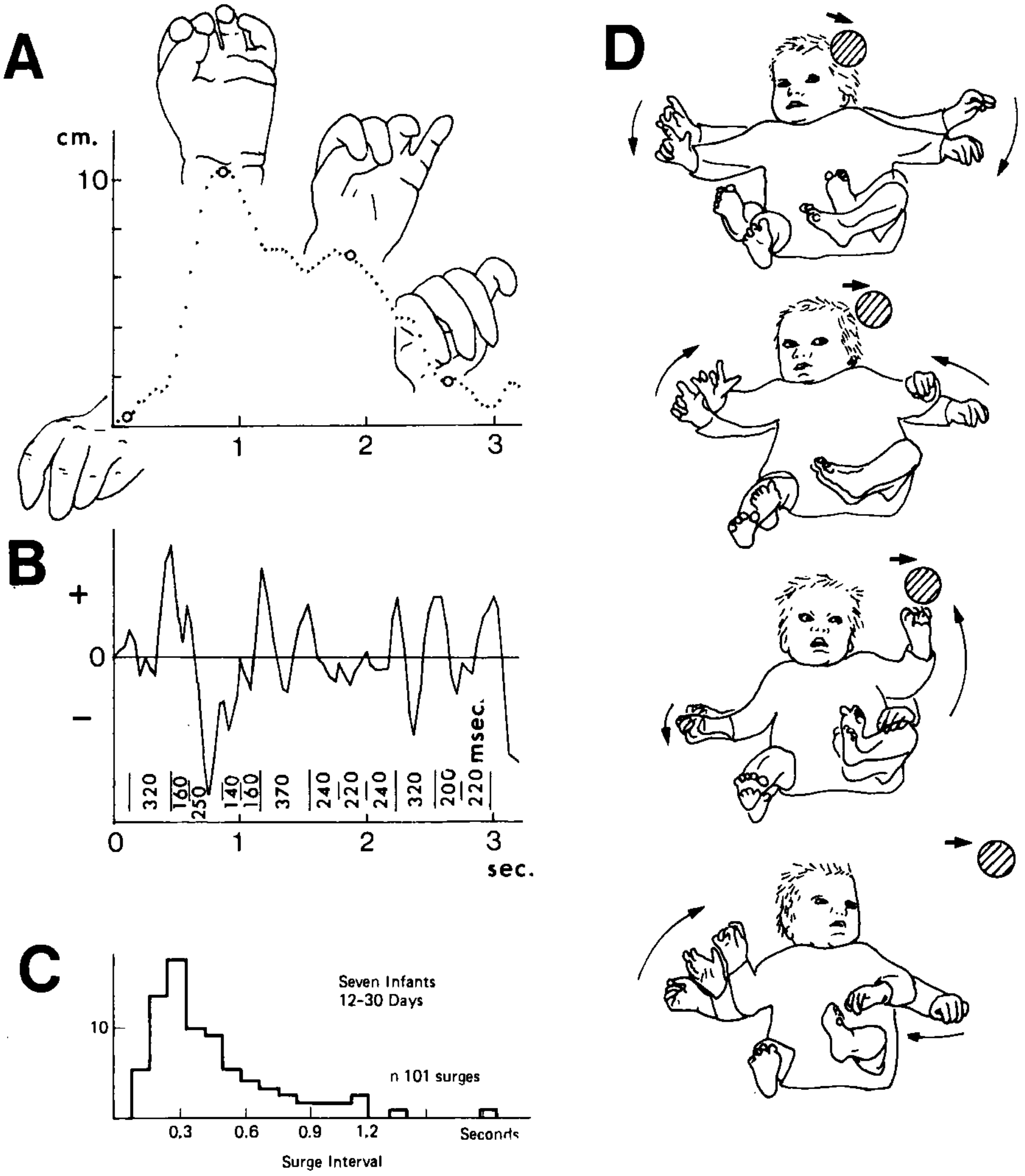

Fig. 5. Prereaching at 23 days after birth. Vertical displacement of the center of the wrist (A); acceleration (B) showing swinging or stepping. Hand part-open at climax of wrist displacement. C: Frequency histogram of intervals between lifts or wrist for 7 newborns shows a main period of approximately 300 milliseconds.
D: Successive postures while infant tracks an object moving slowly to her left.

of a regular stepping of motor activity at three lifts per second which is clearly revealed in an acceleration curve (Figure 5, B and C). The climax, when the wrist is at its apex of displacement and closest to the object, is usually achieved in two or three steps, and then the return to rest is slower. There is frequently a plateau or sustained elevation for about a second after the first drop from the climax.

Pre-reaching, since it does not involve guidance or redirection of movements during the cycle, can be described as 'ballistic' or 'open loop'. However, a succession of pre-reaching movements, possibly involving both hands, may show tracking in conjunction with whole body orientations (Figure 5D). Such oriented activities also involve the feet which move in quadripedal synchrony with the hands. The figures of hand and foot movements seem to be varied and complex, some resembling quadripedal walking, others more like climbing. It would be of interest to make an analysis of these movements in comparison with hand and foot placements of a person reaching while standing or walking, or a primate reaching to grasp while climbing and hanging on with the other limbs. I believe they show evidence of a very different organization from the crossed movements required when a standing quadruped extends a forelimb to touch an object on one side.

Opening of the hand and its aiming towards the target is tightly coordinated with the climax of wrist movement even in the earliest reaching (Figures 2 and 6). Thus the proximo-distal coordination essential to efficient reaching and grasping is provided for in the neural system that is formed before birth. Observations of Tryphena Humphrey suggest that this reach and grasp pattern is visible in movements of human fetuses at an age when it may be assumed that only spinal motor centres are functional (Humphrey, 1969). Humphrey (loc.cit) commented on the repetition of development of grasping movements, both fetal and postnatal stages showing a progress from closure of all fingers together without finger-thumb apposition to individual extension and retraction of index or little finger, and index-thumb apposition. Jeannerod's studies of adult movements to pick up objects show that the mature skill retains a temporal patterning of arm and hand movements which is independent of visual guidance (Jeannerod, 1982). This pattern is remarkably similar to the pre-reaching cycle illustrated in Figure 6.

When quiet, an infant of two- to three-months may perform a perfect and well-aimed reach and grasp, but the most common response is a jerky extension and retraction of the arm with considerable movement at the shoulder and

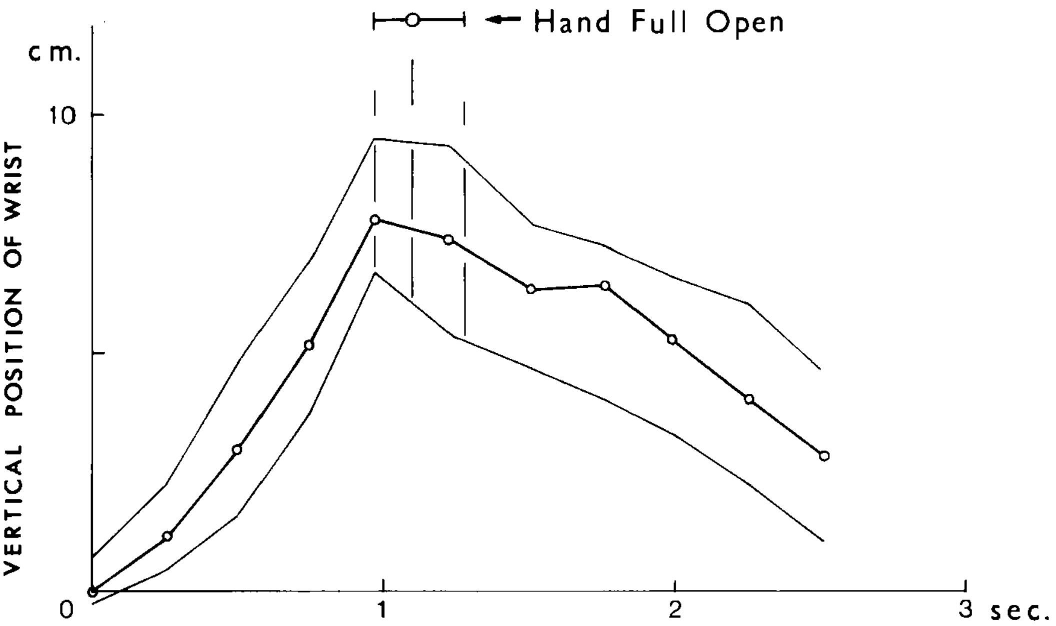

Fig. 6. Hand opening and arm extension in prereaching. Median height and interquartile range of center of wrist, timed from the start of displacement for 15 responses of 5 newborns. Maximum finger opening occurs at the climax of lift.

little or no orientation of the hand which usually remains fisted. The baby orients to objects and tracks them with improved oculomotor coordination, but the arms are held stiffly out from the body and they make erratic and poorly directed swipes. Both arms may make alternating jerks towards the object with fingers in flexion (Figure 2 and 3). Thus the reach and grasp pattern of pre-reaching is blocked or inhibited while the now visibly growing musculature of the shoulder and limb is responding with strong phasic contractions in which extensors and flexors are working in opposition. At four months there occurs a rapid improvement in proximal muscular coordination with a progressive diminution of the oscillations. By five months most infants are capable of well-directed if ataxic reaching movements in which the course of arm extension may be graded, redirected and stabilized for several seconds to permit grasping of a suspended object. Objects resting on a surface are not grasped so readily and reaching is less easily evoked by them at this age.

James Alt and I carried out an experiment at Harvard calculated to test the theory of Piaget that eye-hand coordination is acquired by infants, after they have 'learned' to aim their arms in different directions and to orient their eyes, by the infant shifting gaze back and forth between the hand and the object. Alternation of gaze is supposed to allow 'mutual assimilation' of spatial schemata that are initially separate for oculomotor and reaching movements. This theory was supported by White Castle and Held (1964) who observed hand regard with institutionalized infants whose reaching was retarded in an impoverished environment that lacked opportunities for the babies to use spontaneous reaching and grasping.

Alt devised a lightweight screen that could be attached to a headband on a baby. With the head of the infant facing forwards, this screen completely blocked the view of the arm and hand in either peripheral or central vision. With infants 16 to 20 weeks of age reaching movements were directed towards an object suspended in front of the infant and in no case did the infant make an attempt to see the arm either before attempting the reach or in the course of its execution. Figure 7 traced from a film taken at 16 frames/second shows one such response. It may be seen that the vertical component of wrist displacement manifests ballistic propulsion with steps separated by about 250 msec which are clearly seen in the acceleration curve. There appears to be an excessive lift of the hand which is corrected at 1.7 seconds after the record starts. The correction may have followed entry of the hand into the visual field, but it seems too soon. An alternative hypothesis is that the infant deprived of some advantage of peripheral vision of the approaching hand has wandered slightly off course and made a correction on the basis of feedback from the limb and an accurate localization of the object from visual fixation. By measuring reaching movements of infants fitted with prisms, von Hofsten has shown that a subject of about this age is capable of accurately determining the distance of an object by convergence of gaze (von Hofsten, 1977). Presumably visual direction is estimated with equal accuracy.

Von Hofsten has recently made accurate measures of the transportations of the wrist and orientation of the hand that babies make to intercept an attractive object that passes in an horizontal arc at about chest level (von Hofsten, 1980). He discovered that catching movements were easily elicited and that they were surprisingly efficient even for objects moving at a velocity of 30cm/second with infants who are just beginning to perform controlled reaching (18 weeks of age). Response to faster moving objects

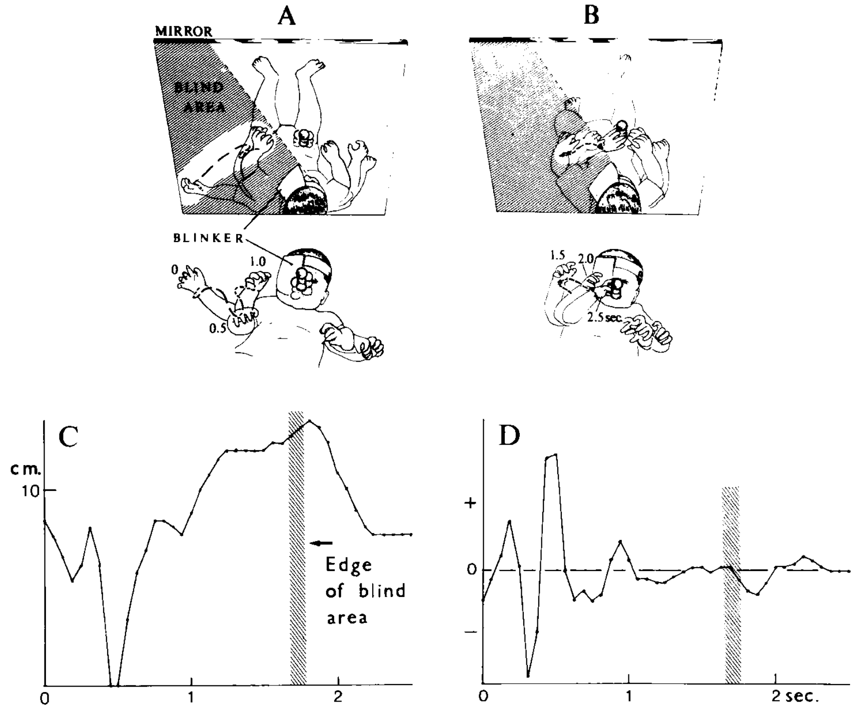

Fig. 7. Alt's experiment with screen preventing visual guidance of reaching arm. Subject 18 weeks. Center of wrist plotted from film at 16 frames/second. Possible visual correction of aim at last ½ second (B). Vertical displacement of wrist (C) and unsmoothed acceleration (D) show periodicity of lift, with correction for fall of arm at 0.5 seconds.

were quicker and more direct than those to slowly moving objects, the movement showing clear anticipation - that is, the trajectory of the hand was aimed ahead of the object from the start of the response, while the object was still out of reach, to intercept it accurately. First reaches were as accurate as those to subsequent presentations of the object and there was no sign of alternation of gaze between the object and the hand. Von Hofsten (1983) explains this performance by assuming that the infant makes a direct internal representation of the vector of displacement of the object from visual information, accurate for speed, direction and distance, and combines this with a vector of arm extensions that would project the hand at a definite rate towards the position of the object at the start of the reach. The resultant of these two vectors is then the path along which the wrist is actually carried. That is, the wrist is displaced both towards the object and with it.

Wrist displacement curves of von Hofsten's subjects confirm that there is an improvement in the central program that responds to reafferent (proprioceptive) information beyond four months. Initial reaches are ataxic with an oscillation of approximately 3/second. Later the movement shows two phases, an initial rapid extension lasting about ½ second and then a varied series of steps, more for a slower object, at about 4/second. Grasping and holding of the object (which has a very low inertia) occurs more frequently after six months in parallel with controlled reaching and these behaviours develop earlier in females, males catching up after six months.

Von Hofsten's observations of newborns (4-9 days of age) prove that extensions of the arm can be directed towards a visually fixated object whether there is a head rotation towards the object or not (von Hofsten, 1982). He confirmed that the hand moves in a succession of ballistic steps, reaches a point closer to the object at the climax if the object is being fixated and that it slows down at this point. He considers the movements to be part of a coordinated exploratory impulse that provides the substrate for later development of effective visuomotor coordination.

A film study of grasping objects from a surface in Gesell's laboratory by Halverson described improvement in orientation of the digits to obtain a precisely measured grip between thumb and fingers and a change from placing the whole palm over the object to orientation so that it came to lie between index finger and thumb (Halverson, 1943). Delicate manipulation of small objects under visual control occupies infants towards the end of the first year. Reflex studies by Twitchell (1965) chart an increasing sensitivity in the hand to contact with an object and differentiation of individual responses by the fingers. Furthermore, touch-stimulated responses develop in association with turning movements to correct for misreaching visually perceived (visual groping). This use of the distal component of the inherent reach-to-grasp program is refined after controlled arm extension has been achieved about six months.

The evidence from accurate observations of how walking and reaching and grasping develop which has been reviewed indicates that infants are born with a considerable part of the neural structures that will coordinate these functional patterns of muscle activity in adults. Responses newborns make to stimuli appear to arise as modifications of movement sequences that they can produce without stimulation. Developments in behaviour result, we find, from the acquisition of more efficient preparation in the innate motor coordinative structures for specific directions and magnitudes of stimulation. The process is at least partly a learning how to perceive conditions for specific plans for movement, and learning how to infer what conditions will be like in the future. Some of the afferent contribution to motor control and to learned improvement in control takes the form of inevitable reafference of feedback from immediate consequences arising in the body itself. Other forms of stimulation are dictated by events or objects outside the subject, who may act with selective orientation, approach, withdrawal or avoidance with respect to these events or objects. To accomplish such selective engagement with exterior phenomena the subject must possess a representation of a spatial field in which the locations of things may be specified with reference to the body and its movements.

The coordinations that neonates make spontaneously between movements of head and eyes, arms and hands, legs and feet when they orient to external events that they see or hear indicate that a spatial field exists at birth (Trevarthen, 1974b). However, a newborn infant can neither reach and grasp an object nor walk. For both kinds of action there is a systematic improvement in the uptake and prediction of information about the body itself (motor learning), and improvement in the use of perception to gain a useful description of the conditions, risks and opportunities for movement that are outside the body. This latter kind of development includes what psychologists call cognitive learning.

Although the products of cognitive development are mental representations that may exist in absence of movement to describe reality outside the body, the formation of these representations must be dependent upon biodynamic principles that are built into the nervous system and visible in the immature motor coordinations of infants. The anatomical and physiological specifications of the innate structures for coordination of movements may be perceived in the regular features of movements to which Bernstein's descriptive analysis draws our attention.

In both walking and running we find an orchestrated sequence of overlapping events at each joint such that, among other patterns, distal segments execute their greatest movements at a time of reduced proximal activity. The movements have a tendency to periodicity, being composed of surges - accelerations followed by decelerations - that reflect well-timed activity of antagonistic sets of muscles. Indeed the action may be described as composed of a succession of saccades, like those that couple conjugate rotations of the eyeballs, but with, in general, much lower maximum angular velocity (Figures 3 and 5, C). Oculomotor saccades occupy about 50 milliseconds, large displacements of the head or limbs take about 500 milliseconds (Figure 8). Major displacements are overlaid by small wavelets in the displacement curves of limb segments that suggest a regulated periodicity of saccades that serve to divide up or group episodes of engagement with sensory feedback. We have not looked at the periodic movements used in visual, olfactory, gustatory or tactual exploration, but we have seen periodicity in the output that will produce intermittent proprioceptive effects in the body. The tempo of this periodic output of motor activity in the course of complex behavioural figures of movement does not appear to be imposed on a passive nervous system by the inertial or pendular characteristics of the body and its parts, which are under continuous restraint by the tonic contractions in antagonistic sets of muscles. Saccadic periodicity expresses an intermittency in the central motivating system, one form of neural coding of time for behaviour.

The coupling of action in proximal and distal segments and the linking of special sensory systems (eyes, ears, hands, mouth) that use movement to extract and select information for perception are expressed in the shared beat of stepping of all the motor structures of the body. Further work is needed to determine how this coordinative integrity arises in the system that directs exploratory movements of different parts of the body and that couples explorations to performances which utilize the environment more deliberately. Available evidence would suggest that the anatomical structure for intermodal sensory equivalence and for equivalence of alternative orienting movements is functioning in newborns (Trevarthen, Murray & Hubley, 1981). Obviously their capacity to make performatory actions is very limited.

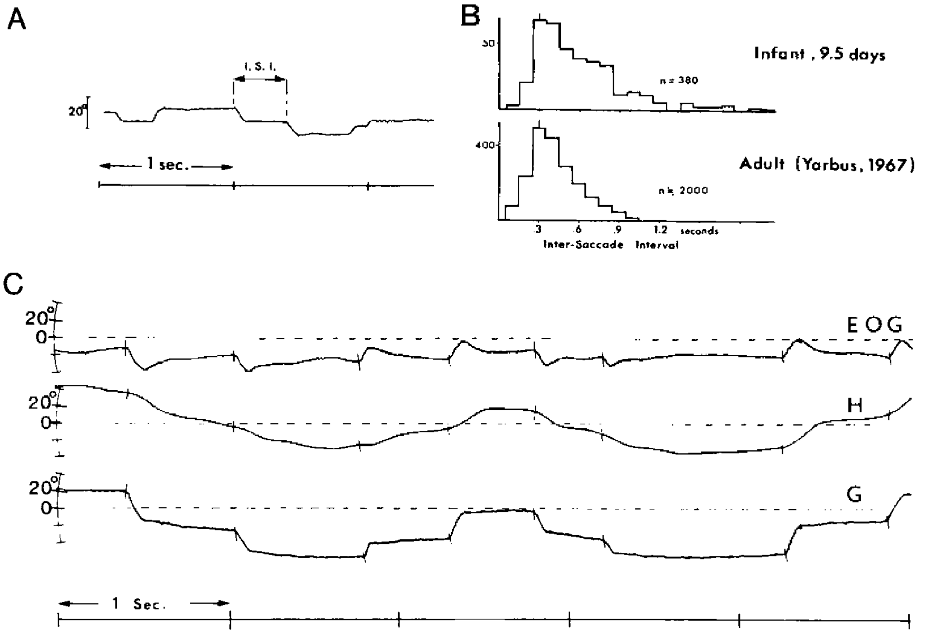

Fig. 8. A: Electrooculogram from 9.5 day old infant to show conjugate saccades of the eyes.
B: Histogram of intervals between saccades for same infant compared to that for adults measured by Yarbus (1967).
C: Infant, 11 weeks, tracking ping-pong ball with illuminated bulb at center that is oscillating in an arc concentric with infant's head. Tracking is saccadic. Eye rotations (EOG) add to coincident steps of the head (H) producing steps of gaze (G). Note: Drifts in gaze are an artifact reflecting mismatch in amplification of EOG and signal from potentiometer measuring head rotations. In reality back rotation of the eyes accurately compensate bead rotations.

Descriptive and experimental studies of the development of movements strongly support the idea that it is in the nature of motor coordination to 'explore' the mechanical periphery, to conduct a search for programs of activity that will exploit the potential of the body and of objects that come in contact with it, to generate forces that will contribute to adaptive actions and to efficiently subdue or avoid forces that might destroy coordination. Thus movements made by infants show up the strategy of the C.N.S. for achieving a more effective course of action; its caution, confidence, power and skill. These qualities of motivation appear in such features of the displacement curves as the number and acceleration of saccades, and the size of oscillations that arise from impulsive reactions of the muscles tightening or releasing to correct for errors of motor output (Bernstein's 'reactive' waves).

The scientific study of infant movements is just beginning and, so far, most attention has been given to their orienting movements and use of objects. Nevertheless, I believe enough has been described for us to entertain the following far-reaching hypothesis. I would propose that to understand the movements of infants fully we shall have to consider specialized forms of motivation that have evolved to show up steps of the planning for movement so other human beings can be affected. Certainly, expressive and communicative movements of adults capitalize on all the above described external signs of their brains' search for motor control, to generate messages about the intensity, confidence and curiosity behind what they are trying to do. Other subjects/agents, with appropriate perceptual and cognitive abilities, can respond to these messages. Thus is created the possibility of 'inter-subjective' engagement and cooperative action (Trevarthen, 1978, 1983b). There would seem to be a need for infants to possess at least the rudiments of this expressive and receptive ability, in order to obtain the human services they require from caretakers. They may also need to have it in order to develop more precise and effective forms of communication.

Figure 9 shows features of expressive manual gestures of young infants and their coordination with other expressive movements. These arm and hand movements have a close resemblance to those we saw in 'pre-reaching', but there are differences which appear to take account of the fact that such movements can have expressive value only if another person is playing attention to them.

Infants reach for, grasp and manipulate objects well before they crawl or walk. They gesticulate expressions of emotion and can regulate complex facial and vocal interactions with caretakers weeks before they manipulate

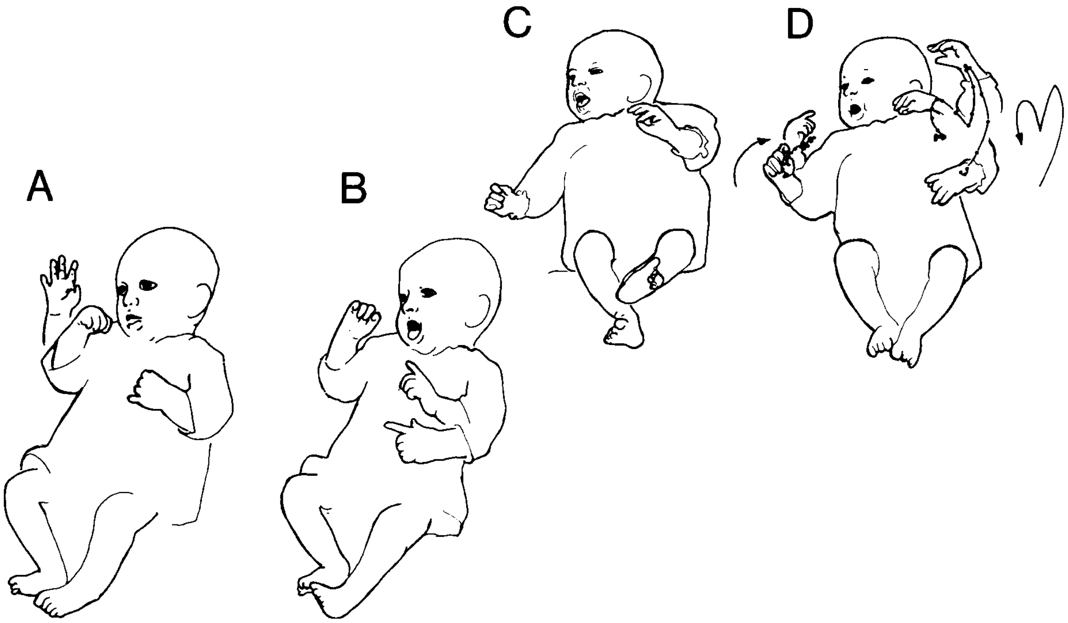

Fig. 9. Six-week-olds in face-to-face interaction with their mothers. Gestures accompany facial and vocal expression.
A: Infant waves right hand.
B: Two seconds later the right hand is closed and the baby extends and raises the left index finger, simultaneously opening mouth and extending tongue.
C: Infant raises left hand with fingers opposed and touches lower lip with tongue.
D: Same infant waves arms rhythmically while vocalising.

objects. Orienting and pre-reaching behaviours indicate that infants are born with biodynamic structures that embody a certain 'knowledge' or functional readiness for experience of objects, and the expressive behaviours are evidence of innate readiness for human communication. The order of development appears to be the reverse of that an empiricist would assume. Infants do not learn either to take possession of objects or to communicate by a process that builds links between sensory-motor arcs. Their brains contain a representation of an external reality and the people in it.

It is generally accepted that very young infants are highly sensitive to active stimuli - moving or changing visual effects, sounds of movement including gently ringing bells and speech, stroking and patting, rocking, etc. A baby is roused from sleep and calmed from agitation by human activity. How do they detect this activity in such diverse sensory forms? Close observation reveals that they attend particularly to events that specify nearby movements of persons, and that they are highly sensitive to the affective or motivational quality of behaviour. When briskly handled they become still and withdrawn, if surprised by a sudden vigorous action they cry, to gentle affectionate signs they attend quietly and with focalization. Mothers ministering to young infants closely observe all such indications of their state of alertness, contentment and interest. A mother coordinates her stimulations to relate intimately to what the baby does. Mothers also tend to employ repetitive movements grouped in clusters or bursts. The tempo of their gestures, vocalizations and shifts of posture becomes slowed, regular and accentuated (Figure 10A). The positive responses of the infants to this behaviour indicates that their mothers are unconsciously providing optimal communication. Microanalysis reveals that the movements of mother and infant interact with timing precise to a fraction of a second (Figure 11B). The infant's movements are similar in tempo to those of the mother (Figure 10A and C) and they may be very precisely reactive to and intercalated with the utterances of the mothers (Figure 11B).

It is significant that the tempo and organization of adult communication signals, especially those of baby talk, closely approaches the spontaneous tempo and organization of infantile movements, structural features which are discernible even when the infant is in a situation where there is no possibility of pacing by an outside source. Some years ago I proposed that this similarity of generative motor structure in infant and adult is responsible for furnishing the infant with a detector of human movement (Trevarthen, 1974c, 1978), a structure which resonates with the mother's

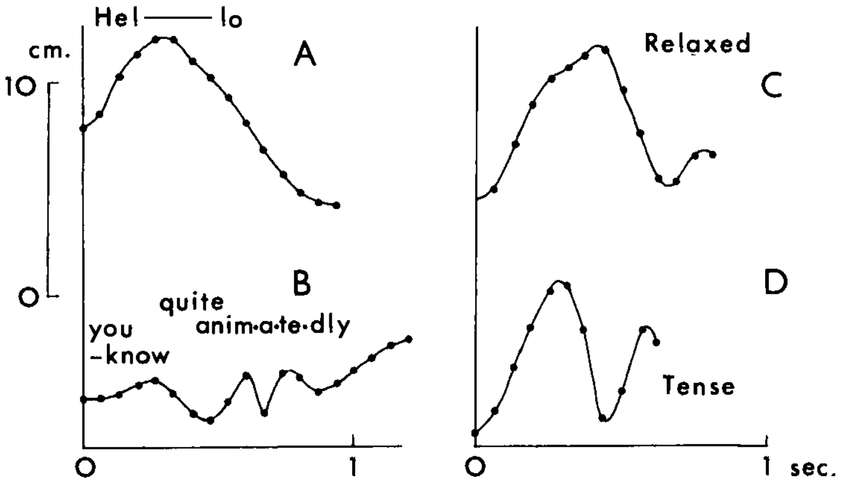

Fig. 10. Vertical displacements of tip of nose of mother coincide witb vocal emphasis.
A: Slow, regular displacement accompanying affectionate speech to a two-month-old.
B: Staccato burst of nods while talking to an adult.

Arm movements (vertical displacements of wrist) express level of motivation of her infant.
C: In animated, happy interaction with mother.
D: Angry trashing when ignored by mother, infant alternately tense and withdrawn or protesting (From Tatam, 1974).

patterned activity. Condon and Sander showed that indeed newborns have a high degree of self-synchrony between movements of parts of their bodies, and they claimed evidence that a newborn could become synchronized with the speech rhythms of an adult (Condon & Sander, 1974).

Other special features of human signals, such as the fundamental frequency of the voice or the colour of light reflected from the face, and the peculiar anatomy of expressive organs such as the face and hands must be preferentially discriminated as well. How else would a newborn show selective awareness of eyes, mouth and hands and their actions, and be able to imitate these actions (Maratos, 1973; Meltzoff & Moore, 1977; Field et al., 1982)? Nevertheless, the primary detector structure in the infant may be an 'ecphorator' for movement, as Bernstein called it, which has a measure of time built into it matching the time characteristics of the movements of an attentive and caring adult partner. This neural basis for empathic response would underlie imitation in both directions, and also turntaking and the more productive complementary forms of action in the two partners that appear in game playing and 'protoconversational' exchanges (Trevarthen, 1983a, b). In a recent experimental test, Kuhl and Meltzoff (1983) showed that five month olds could discriminate which movement of the mouth, seen in a pair of silent moving displays of a face pronouncing two different syllables, matched a speech sound presented on a loudspeaker. They also noted that the infants vocalized in an imitative manner in alternation with the sounds, and that the pattern of change of fundamental frequency in time of the infant sounds closely resembled that of the sound made by the adult. Figure 11B shows that already at six weeks, before voicing is controlled to produce pure sounds, an infant makes vocalizations with a segmentation close to the mother's utterances of baby talk. The mother modulates the pitch of her vocalizations to match the infant's expressions (Figure 11A and C).

While newborns a few minutes old may exhibit orienting and affective reactions that indicate a preference for human signals, a six-week-old is capable of focalizing on a mother's face and vocalizing in interaction with her (Figure 11). The improved orienting of the older baby guides the mother to sharpen up the forms of her baby talk. Delicately regulated engagements occur that depend on precise mutual tracking. If the engagements are perturbed by interruption or tampering with the feedback of expressive signals, the baby becomes distressed (Tatam, 1974; Murray, 1980, Trevarthen, Hubley & Murray, 1981).

In recent years much has been learned about the fundamental basis for human communication by detailed examination of the patterns of movement of mothers and infants while they interact. But this is only the beginning of an adequate analysis of the kinesics of mother-infant interaction. Well controlled 'protoconversational' engagements occur before reaching is effective. Indeed it appears as if the motor abilities related to expression of feelings about other persons matures earliest, next to the essential automatic activities of feeding, self-protection and crying for aid. Expressive actions that regulate interpersonal contact mature at least as quickly as the visual focalizing and tracking movements which undergo great improvement in the second and third months (Trevarthen, Murray & Hubley, 1981; Trevarthen, 1983a).

Before infants reach, their interactions with their mothers become more gamelike with an element of deliberate experimentation, positive and approach behaviours being mixed with negative and avoidant or defiant behaviour (Trevarthen, 1983a, b). A sense of humour emerges which is very sensitive

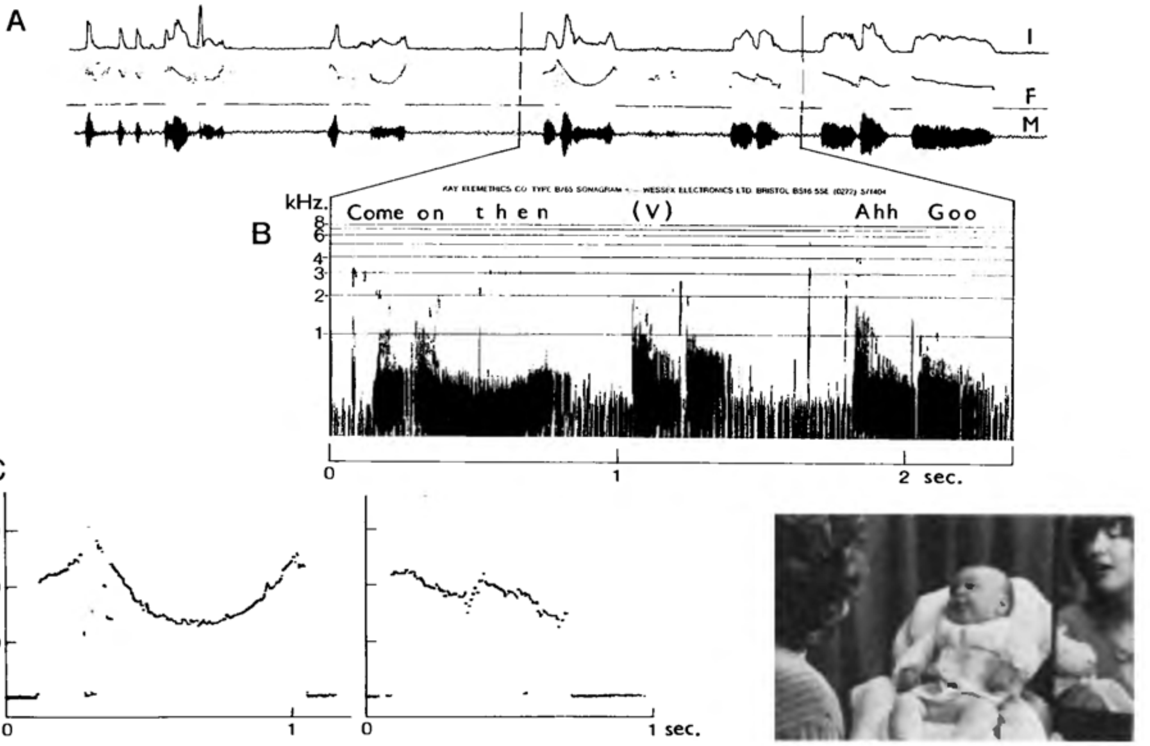

Fig. 11. A: Vocal interaction between mother and six-week-old. Mother makes three appeals for response with U-shaped intonation. After infant vocalizes a bi-syllable, V, she makes three imitative or sympathetic utterances with descending pitch. I= Sound intensity; F= Fundamental frequency; M= Microphone signal. B: Sonogram shows precise temporal intersynchrony of maternal and infant vocalizations. C: Enlarged plots of fundamental frequency for mother's utterances. Compare with head movements of Figure 10A. Photo shows infant at moment of vocalization, V.

to that special form of contingent and episodic activity from a partner called 'teasing' (Figure 12). Here we have evidence that the intersubjective meachanisms that are empathizing with or mirroring and reacting in reply to the signals of the partner undergo intrinsically regulated development. The infant experiments with reafference from the human partner. This important advance in communication occurs just before controlled arm extension develops. That is, before the infant begins to manipulate objects at four months of age (Trevarthen, 1983b). Play with objects develops later.

The expressive movements undergo rapid development in the second half of the first year. The infant progresses from babbling to protolanguage and may imitate a few words by one year. At the same time hand gestures become more distinct from movements of prehension. Hands and voice may be used by the baby to imitate a dancing or chanting game, such as 'clappa clappa handies' (Trevarthen, 1983b). Facial mimicry is also characteristic of humorous behaviour of infants over six months of age.

After nine months there is beginning to be an awareness in the infant of the purposes behind the actions of others and of symbolic or conventional forms of movement (Hubley & Trevarthen, 1979; Trevarthen, 1983b). Before the end of the first year a deaf child of deaf mute parents may begin to learn the arbitrary hand signs of American Sign Language (Bellugi, personal communication). The gestures of pointing, waving and signing exhibit lateral asymmetry from early in their development, which would appear to indicate control from lateralized regions of the cerebral cortex, tissues which achieve full maturity only years later (Trevarthen, 1983c). Obviously we have far to go before we can describe the natural stages by which expressive behaviours develop, but the outlines are already clear of an elaborate maturation before language is mastered.

Now that we possess more detailed descriptions of what infants can do, it is possible to put research into the development of coordinated behaviours on a more secure factual basis. Previously the concept of conditioning, which used a grossly oversimplified model of the cerebral mechanism for sensory-motor coordination, was supposed to explain the acquisition of all forms of adaptive response that required perception of external conditions. Social patterns of behaviour, in particular, were thought to be entirely learned. Although infants and children certainly do learn by imitation and this learning influences the form of their expressive movements, the structural foundations for the imitated movements cannot be learned. It is necessary to assume an innate structure that at least partly matches the structure of

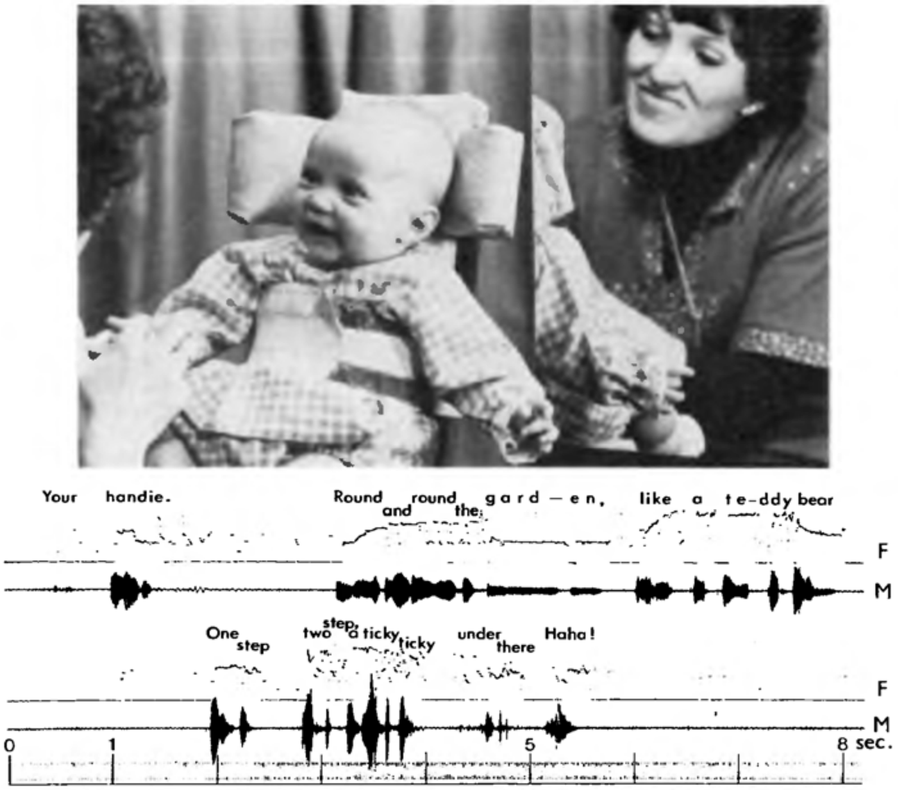

Fig. 12. Mother chants a nursery rhyme to an attentive 20-week-old, varying pitch and timing of her speech to control the emotion of her infant who collaborates enthusiastically.

the adult models to explain both imitation and the more complex reciprocal or complementary interactions which are characteristic of communication between child and adult from immediately after birth.

By following the microdescriptive method of Bernstein and by applying his kind of exact but imaginative theorizing all types of human activity may reveal their natural coordinative principles. These principles are the expression of structural features of neural systems that develop in the brain of an unborn child and that gain in adaptive power by systematic, directed exploitation of environmental conditions stage by stage through childhood. Thus learning itself becomes an inseparable part of the maturation of the 'biodynamic structures' that tie perception to action.

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CHAPTER IIIb

BIODYNAMICS OF LOCOMOTION, FORTY YEARS LATER

R.H. Rozendal

1. INTRODUCTION.

Urging physiologists to study human movement in a new way, Bernstein inadvertently became - by postulating a coordinational reflex - one of the founders of cybernetics. This reflex is, metaphorically speaking, "not an arc but a closed circle with functional synapses at both ends of the arcs" (p. 216). More interesting, within the framework of this paper, is his departure from classical physiological views by postulating rather structuralistic concepts of movements "not as chains of details but structures which are differentiated into details" (p.179). In 1940, he considered one of the most important aspects of his approach to be his observational and descriptive work carried out with maximal refinement on the locomotor process as if it were a very complex morphological object. An object which reacts, evolves and involutes. After a thorough description, he had hoped to decipher the dynamics of locomotor acts by means of more or less complex mathemetical and physiological tools, which would permit the revealment of the underlying central nervous processes (p.176).

In setting this aim, he claimed that locomotor acts as such deserved investigation. Spontaneous or intentional movements, he maintained, comprise a peripheral and a central cycle of interaction corresponding with biomechanical and informational processes. His work stands as an important contribution to biophysical research on movement. Therefore, according to Bernstein the programme of this research had to be twofold. In conformity with this view this paper will contain biomechanical as well as cybernetical considerations, as is quite customary in modern biophysical studies of movement. Both approaches - the biomechanical and the neurophysiological - were needed at the same time, according to Bernstein, as a one-to-one correspondence does not exist between the neuronally induced forces and the resulting movements. "Movements are not completely determined by effector processes", (p. 213). This is formulated in biophysical language at the bottom of page 167, metaphorically illustrated in

Fig. 14 and discussed by him in Section 5 of Chapter III under the terms functional non-univocality and indeterminacy.

In Section 3 of this paper this non-univocality will be discussed in the light of 40 years of research. This discussion will not be limited - as was Chapter III almost exclusively - to locomotion. Tentative conclusions will be drawn concerning the reformulation of the task of the coordinating nervous system in solving motor problems, starting from a biomechanical analysis of the motor processes.

By such an analysis, Bernstein tried to explain the task of the controlling (or coordinating) nervous system. By choosing locomotion, as an automated movement for which, from ancient times, man has learned to bring a lot of muscles into synergy, resulting in a movement of stable structure, Bernstein sketched a new important line of research. Next to locomotion he studied other skilled movements. Though he himself never used the word, the concept of optimalization is implicit in the choice of the movements studied, as well as in the more or less speculative considerations on motor coordination in Section 5 of Chapter III (Bernstein, 1967). Many researchers have taken the same classes of movements. Automated or - by training - highly skilled movements are made by numerous subjects in order to give the researchers a chance to "adopt constant criteria for the discrimination of the random from the regular" (p.171). Optimalized movements in sports and fast or ballistic movements of the extremities, especially the upper one, are studied most frequently next to walking and running.

General theories of walking and other forms of locomotion of idealized animals also depend on optimalizational principles e.g., the transformation of kinetic into potential energy or the storage and re-utilization of potential energy in elastic tissues appear to be some of the mechanisms used for optimalization of the energy cost of locomotion (Alexander, 1977; Cavagna, 1978).

Two points have to be made here. On the one hand it appears that the concept of optimalization has played a unifying role in the development of research on locomotion. It may be that the time has arrived for the various approaches to be reconciled and an attempt will be made in this paper to do so. On the other hand, such a reconciliation will be exclusively in the realm of a mechanistic approach to the problem of human locomotion. This is an inevitable corollary to the way in which the concept of optimalization has been worked out in both biomechanical and work physiological research since Bernstein's days.

In Section 4, amongst others, energetical optimalization of human locomotion will be discussed. This Section will further be characterized by a departure from one aspect Bernstein proposed to study: the neurophysiological or cybernetic aspect. It will be dedicated to progress in the biomechanics of locomotion in accordance with the preponderance Bernstein gave to the biomechanical analysis of locomotion in his Chapter III.

If one is headed to discriminate between "the random and the regular", to look at the generality and regularity and to the stabile and typical hallmarks of the processes, as Bernstein has put it (p.165 and 166), the finer mechanically reactive and especially the proprioceptive reactive (as Bernstein calls them, p. 209) phenomena might well be overlooked. In later cybernetical research, these phenomena and the reactions of the controlling Nervous System in adjusting to them are the key to a comprehension of the system. However, much of the biomechanical research on human locomotion has been confined to defining the regular and has not been directed to reactive details of the process. Only a few groups of researchers (e.g., Herman et al., 1976 and Grillner, 1975, 1979, 1981) have tried to study neurophysiological aspects of human locomotion. It is believed by the present author that a major breakthrough is to be expected within short time, as indicated in Section 4.

With respect to other movements than human locomotion the two-fold programme Bernstein proposed has been followed more directly. In his search for fast neurophysiological processes responsible for optimalization on this level, Bernstein postulated 'spontaneous pre-proprioceptive impulses' and 'reactive innervated impulses'. Furthermore he speculated on preparatory organization of the motor periphery including the motor neuron pool. His students have worked out this line of thought in a series of publications. The optimalization principle on the neurophysiological level in these publications is operationalized in the 'principle of least interaction' and in the concept of 'synergy' (Gelfand et al., 1971). Preparatory organization of the Central Nervous System as well as of the peripheral effectors is dealt with (Gurfinkel et al., 1971a; Feldman, 1980 ab). In current biophysical research, both play an important role.

Other lines of research dealing with neurophysiological problems are concentrated on the role of α-γ coupling (Granit, 1970; Matthews, 1972; Stein, 1974) and on changes in the reflexes during movement (for recent symposia on these topics, see Homma, 1976; Granit & Pompeiano, 1979). Here too, biophysical concepts enter: Houk (1979), taking the position that stiffness (the ratio of force change to length change) will be the leading variable. On the contrary Stein (1981) expressed "a hunch that with 10^{12} neurons and a considerable choice of motorneurons and peripheral receptors most if not all of the variables" (force, velocity, length, stiffness or viscosity) "can be used to control particular movements as required" (p. 211). The present essay will only deal with some of these approaches and only insofar as they are felt to be relevant for the present state of thinking on the problem of indeterminacy (or non-univocality) in Section 3. In this Section various attempts to analyze and explain the functional non-univocality from anatomical and neurophysiolocical sources are discussed. The possible biomechanical definition of the motor problem by recent modelling studies of human movement is sketched.

Bernstein's work up to 1940 gave rise to fruitful research and remainded valid in many respects notwithstanding new facts generated by people (often not aware of his work and that of his co-workers) up to this time. His contribution was remarkable, based as it was on methods lacking some modern tools now at our disposal. Foot reaction force measurement systems emerged in the years preceding his writing in 1940 (Asmussen, 1976), but he did not make use of it. Electromyography was used in human kinesiology only shortly after his 1940 writing (Inman et al., 1944). He had to content himself with rather primitive tools: from displacement time data differentiated twice, conclusions had to be drawn on forces, external or muscular. The question arises if we in our times do better? Other methodological questions, pertaining to generalization over various classes of movement, as well as on the validity of studies of walking on motor driven treadmills are discussed as well in Section 2.

This paper tries to react to some of the problems stated by Bernstein in 1940. Of necessity, it does not pretend to give a complete overview of the field of study and it lacks a thorough neurophysiological approach which lies outside the expertise of the author (1). In Section 5, the main stream of the essay is summarized.

2. METHODOLOGICAL DEVELOPMENTS IN BIODYNAMICS OF LOCOMOTION.

2.1. Introduction.

Research on human locomotion has from its birth been connected to instrumentation. It will be sufficient to recall that Muybridge succeeded in inventing a way to the orderly use of multiple cameras. Marey invented the movie film camera (Michaelis, 1955), which has led to a whole industry outside the realm of science. By calculating the trajectories of falling balls from the separate film frames, Braune and Fischer succeeded in a correct description of the time histories of the events studied and recorded by hand driven film cameras. Discussions about the correct use of the instruments and about new instruments form an important part of the debate in the scientific forum.

Bernstein developed a system for registration of the movement in study using photonics - as the modern glossary would describe it. He did not have at his disposal a method for direct registration of (foot)-reaction forces. He had to differentiate twice his photonic displacement time data in order to get information on accelerations, and muscular and external (reactive) forces. The question is, whether his procedure was reliable and moreover if the procedures currently in use are more dependable? Bernstein did not have electromyography - taken as a sign of neuromuscular activity - at his disposal. The question arises to what extent the use of electromyography has succeeded in throwing light on the problem of neural control of movement coordination and regulation since it was introduced in human kinesiology in 1944?

Bernstein recommended the study of human locomotion as a subject matter suitable for giving insight into the coordination and regulation of movement in general. Lines of research have started since on other modes of locomotion, other types or classes of movement. It is to be questioned if the generalizations derived from any class of movement are valid for all classes of movement. Information with respect to classes of movement pertaining to the involvement of the Central Nervous System are hardly more than provisional at this time. Generalization of the conclusions drawn from experiments on one class over various classes of movements is indeed questionable.

The validity of conclusions on research on human movement from laboratory experiments has been criticized, especially in the case where motor driven treadmills were used to study human locomotion. These problems will be discussed in the following subsections.

2.2. From guess to cost/profit analysis.

In biomechanics of human locomotion it is customary to differentiate twice displacement time data (from film or opto-electronic registrations) in order to extract information on accelerations and forces. This has a "very dubious theoretical basis" (Cavanagh, 1976). In an introductory paper to the session on "Instrumentation and Methodology" of the fifth International

Congress of Biomechanics Cavanagh (1976) stated:

> There are two basic questions that must be posed when displacement-time data from any source are to be used for subsequent numerical analysis. These are: 1) How fast should the sampling rate be? and 2) What techniques of smoothing and differentiation should be used?

He mentioned sampling rates found in the literature, varying from 16 frames per second in a study of squats to 190 frames per second used by Bernstein (2).

The rationale for the choice of sampling rates in registering the signal digitally, or in AD conversion of an analogous signal and in differentiating the signal is not always clearly stated in research reports. All signals are contaminated by measurement noise, mostly stochastic and not to be discerned from the signal proper in identical bandwidths. Reduction of this noise at the source is indicative (Lees, 1980). This problem of measurement accuracy remains difficult. Markers on the body for example may be a source of noise as an effect of the combination of the presence of high frequency impact situations and the existence of various time-histories of subcutaneous tissues over which the markers are placed.

In all registrations and processing, the resolution is of importance for accuracy. The resolution is the ratio of the dimensions of the total image and the incremental steps Δx in amplitude (distances, angles). Small resolution will result in low signal/noise ratio's. This will especially be the case if the movement is slow with respect to the total image dimension, as the mean noise will be constant and the signal will show lower increments or decrements in time (e.g., the vertical displacements in Bernstein's Fig. 5).

The sample frequency is the inverse of the Δ_t steps at which the signal is acquired. The desired minimal sample frequency is stated by the Shannon sampling theorem, by defining the Nyquist frequency as the frequency which is twice the highest frequency in the signal in which interest lies. When interest is confined only to the characteristics of a process in the frequency domain of signals of limited duration, a rule of thumb sampling frequency is advised of five times the highest frequency of the signal (Cavanagh, 1976; Lanshammar, 1982).

As all signals are contaminated by noise, and as the high frequencies of the signal and noise do not contain valuable information, filtering of the signal is recommended (Winter et al., 1974a; Lees, 1980), especially so, as differentiation acts as a high pass filter and amplifies the high frequencies (Lanshammar, 1982). This filtering has to be the first operation when it is not certain that frequencies higher than half the sample frequency do not occur in the signal. Otherwise the sampled signal will contain irretrievable faults, known as aliasing errors. Frequencies higher than half the sample frequency will be represented as lower frequencies in the sampled signal and lead to completely false results in subsequent differentiations (van Ingen Schenau, personal communication). Filtering will not, however, separate noise from the signal in the frequency domain in which interest lies.

In the time domain, various techniques of smoothing have been used in order to prevent the effect of differentiation of noise (Hatze, 1981a; Lees, 1980; Pezzack & Winter, 1977; Soudan & Dierkx, 1977, etc.). Moreover, in differentiating signals, which are stepwise quantified in amplitude and in time sampled, quantification errors are introduced, as the converted signal will have the form of a "staircase" instead of a "ramp". It is customary to denote these errors as quantification noise. These errors are in most cases not stochastic (Lanshammar, 1982). They may even be systematic, resulting in apparent reproducible variations in the first or second derivatives. The latter will become very misleading in higher frequencies, depending on the original ratio of signal amplitude and the increment Δ_x. Thus the effects of quantification noise will depend on the resolution. Quantification noise will also result in unacceptable signal/noise ratios (Lanshammar, 1982). Filtering and smoothing techniques have been used to overcome these difficulties.

Although Bernstein was aware of the importance of choosing an adequate sampling frequency, he apparently was not aware of the importance of sufficient resolution to avoid systematic quantification noise. Comparison of Bernstein's Fig. 22 with Figs. 1a en 1b will show differences in the vertical force summated for both legs during double support in human walking. This force was computed by Bernstein by differentiating twice displacement time data and it was measured directly by Yamashita and Katoh (1976) (Fig. 1b). The latter two measurements show one large vertical peak very unlike the "Vorderstosz" and "Hinterstosz" of Bernstein (3). The question then is if we measure and compute better than Bernstein did in his time? This question is addressed in what follows.

For a dependable choice of sample frequencies one has to know the highest frequency of interest in the movement in study. Cavanagh (1976) stated that such knowledge is scarce in the literature. Neilson (1972) studied - by means of goniometry - with a sampling rate of 20 Hz the maximal frequency of fast

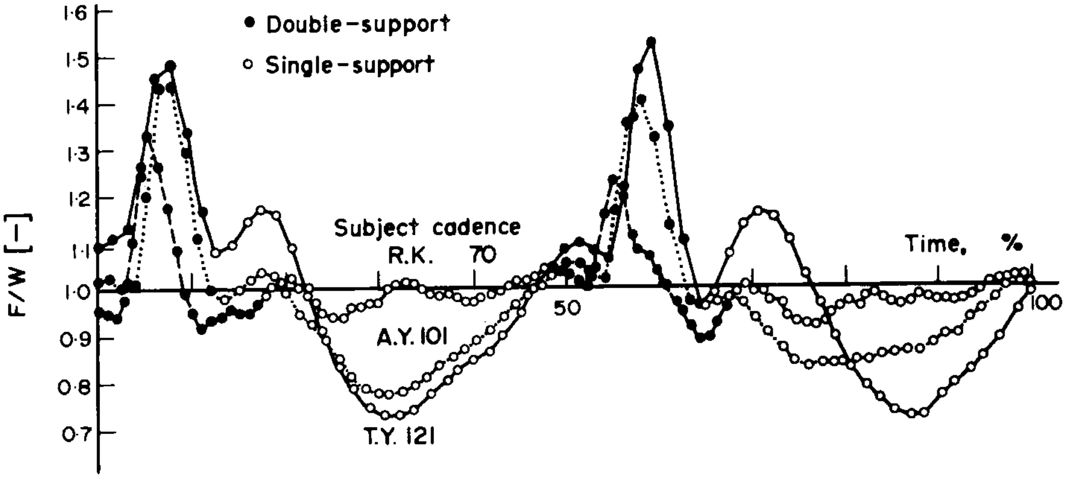

Fig. 1a. Vertical resultant forces in steady walking at different cadences and different subjects. Reproduced from Yamashita, T. and R. Katoh (1976) Fig. 4, Oxford, Pergamon Press, with permission of the publishers.

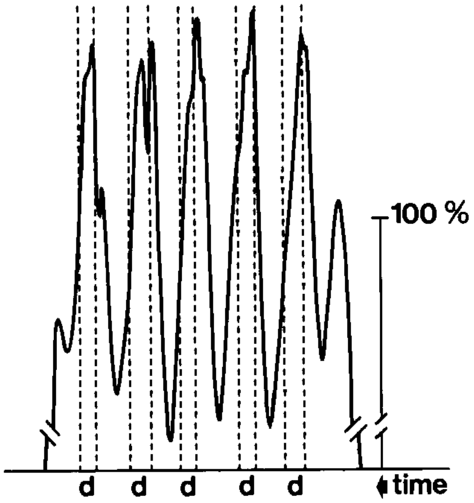

Fig. 1b. Vertical summated forces of a normal subject, registered by the University of Technology of Twente, on a 5 m long double force plate at the "Stadsmaten" Hospital, both in Enschede, Netherlands; mean walking speed km/h (personal communication).

oscillating elbow movements. Depending on the inertial mass of the underarm the movements occurred at a frequency of 4 - 6 Hz. Kinesthetic tracking movements were slower and did not reach 2 Hz. The maximal frequency of the signal was 5 Hz. The study of Gurfinkel et al. (1971b) mentioned maximal Fourier analyzed frequencies of wrist oscillations of 7 Hz without, and 10 Hz with, visual control. Njiokiktien (1970) as well as Thomas & Whitney (1959) reported on fore-aft and left-right oscillations in standing. The oscillations reported were 6 Hz and 15 Hz respectively. The maximal frequency of the signal was not analyzed.

Winter et al. (1974a) reported that in walking registered with a TV-system (60 Hz sampling frequency) 99.7 percent of the signal power of the signal on vertical displacement of the toe marker was contained in the harmonics below the 7th. This content was not speed dependent. They filtered their data twice with a filter with a cut-off frequency of 7 Hz, which resulted in a total cut-off at 4.5 Hz. Jacobs et al. (1972) analyzed vertical foot reaction forces (sample frequency 54 per stride) and concluded that the 7th harmonic and upwards contained only 10% of the fundamental harmonic (in walking this is the first). They did not state the frequency (4).

Smidt et al. (1977) concluded on the basis of accelographic data of the trunk (sample frequency 41 per stride) that most of the power was contained in the lower harmonics up to the tenth. A speed dependency of the distribution of the power over the even and odd harmonics was reported. Zarrugh and Radcliffe (1979) in a study of human walking concluded that "at high frequencies (above the fifteenth harmonic) the spectra resemble those of white noise". They considered "seven to twelve harmonics good compromise between information and noise for most gait variables, except ankle flexion which requires at least 20 harmonics".

So far in studies of human walking, a sampling frequency of 50 Hz seems quite reasonable. Capozzo et al. (1975) in stating the problem as a compromise "between accuracy and cost of data acquisition in terms of man/time necessary for the reading of the film", are of the opinion that even 20-30 frames per second will be generally sufficient. Lanshammar (1982) concluded that, in human walking, inclusion of the results of Zarrugh and Radcliffe (1979) frequencies of 5 - 16 Hz should be analyzed. The upper limit would demand a sampling frequency of 90 Hz for exceptional details. However, Simon et al. (1981), in reaction to Pezzack et al. (1977), as well as Hatze and Venter (1981) in reaction to Onyshko and Winter (1980) called attention to the fact that in foot reaction force studies high frequency components occur at heel strike in walking. Such frequencies appear in bare foot walking up to 75 Hz, and are liable to be dampened by shoewear to about 50 Hz (Simon et al., 1981). From the clinical viewpoint such impact waves will be of importance (Voloshin et al., 1981). In running, higher impact frequencies would be expected, but adequate shoewear may lessen the problem. Indeed Cavanagh and Lafortune (1980) measured peaks at heel strike in running with special shoewear, showing a rise time of 23 ms to 2.2 times body weight, resulting in frequencies of only about 10 Hz.

The conclusion must be, that the choice of the sampling frequency is dependent on the (details of the) movement in question. In human movement studies, sampling frequencies of 50 Hz will be sufficient in most cases (Lees, 1980). Filtering of data on a cut-off frequency based on the significant signal frequency, with a filter of known (and to be reported) characteristics is necessary (Pezzack et al., 1977). If the frequency of the motion in question is not known, a preliminary analysis is needed. The decisions on the cut-off frequency and sampling rate are arbitrary but may be reasonably founded and documented, on a cost/profit basis.

For the smoothing of amplitude-time data in order to reduce noise (including quantification noise) different techniques are advocated. Winter et al. (1974a) filtered their data twice with a 2nd order Butterworth filter having a cut-off frequency at 7 Hz. Moving average techniques were used by Smith (quoted by Lees, 1980). Pezzack et al. (1977) compared finite difference, Chebyshev least squares polynomials, and digital filtering followed by a first order finite difference technique, and found the latter procedure most promising for the analysis of a simple instrumented movement. Soudan & Dierkx (1979) used spline functions, while Hatze (1981) criticized such usage. In the use of cubic spline functions the smoothing factor has to be guessed by trial and error. In fact, when using spline functions, the shape of the derivatives is defined beforehand in deciding to fit the data with a known function. This should not be advised when one is interested in the actual shape of derivatives.

In the use of higher order polynomials or Fourier approximation, arbitrary decisions play also a role in the choice of the order of the approximation. Hatze (1981) proposed an algorithm which overcomes these difficulties. This algorithm appears to be much faster in terms of computation time than the cubic spline method. However, the method is only applicable if sufficient data points are available. This sufficient number depends on the character of the movement under study, meaning that if doubt arises as to the smoothness of the movement, or if the smooth phases are of a short duration, a large number of data points (from a high resolution data acquisition system) is advised.

With the accumulation of a body of knowledge on the biodynamics of human movement, such choices will be based more and more on rational cost/profit analyses. In reporting, the pertinent data on the techniques used should be given, in order to enable the reader to form an opinion about this choice. By such means the study of the biodynamics of locomotion will at least become of age. Workers will refrain from being content either with the a posteriori reasonable looking outcomes of first and second order derivatives, or with very time and money consuming procedures.

It is concluded that given the restricted resolution of most movement registration systems, the calculation of the first and particularly of the second order derivatives from sampled positions is still a problem as it was in Bernstein's days. Only the time needed to analyse the subsequent frames by the introduction of modern motion-analyzing equipment and opto-electronic systems has since improved. Some of the latter systems have also very satisfactory resolutions.

In choosing a method to acquire acceleration data direct measurement of acceleration or forces should be considered. Smooth acceleration curves derived by differentiating twice displacement time data are still to be mistrusted. The smoothing and differentiating filters used may have had a too low cut-off frequency. These frequencies should be reported.

Though Bernstein used presumably adequate shutter frequencies, he apparently was not aware of the importance of adequate resolution in his locomotion studies. So he presumably introduced systematic quantification errors. Therefore the vast amount of facts Bernstein and his pupils Popova, Saltzgeber and Spielberg accumulated, did not warrant the theory built on it. Had they had electromyography at their disposal, their work would have been easier while more directly carried out on muscular activity.Nevertheless, Bernstein's genius in thinking about the various consequences on the neurophysiological level lead to a very successful theory: it has provoked many followers to fruitful research and conclusions.

2.3. Kinesiological electromyography.

Bernstein was undoubtedly right in criticizing "the concept in elementary textbooks that excitation of the flexor muscles results in flexion and that stimulation of the extensor muscles results in extension of the joints they control" (p. 174). This point of view was remarkable in an area without electromyography (EMG) as a powerful tool in human kinesiological research.

Important moments in the development of this tool were its introduction in human kinesiology in 1944 by Inman et al., the early attempts at quantification (Inman et al., 1952) and, important for this discussion, the paper of Ralston et al. (1976). In this latter paper as well as in other papers quoted by Inman et al. (1981, p. 101), it was noted that a time lag of about 30 ms exists between the onset of the EMG-signal and development of force. This time lag is possibly caused by biomechanical processes (stretching series elastic components) as well as phenomena of the excitation-contraction coupling (5). As acceleration is not infinite a pure mechanical cause of this time lag is present too. The lag between the end of the EMG-signal and force production is ten times longer, due to the relaxation of the chemical contractile machinery. Both time lags may hamper the validity of the use of EMG as a tool in description of mechanical 'actual muscle functions' (muscle functions within the mechanical context of the movement in study).

The interplay of the muscle forces or moments and inertial or gravitational moments is difficult to elucidate. In spite of this and of the cautious remarks of Bernstein quoted above, researchers have tried to ascribe 'actual muscle functions' (in terms of moments or torque) to muscles electromyographically active during movements. In generalized human walking such descriptions are beautifully illustrated by Inman et al. in their highly instructive 1981 monograph.

For a good understanding of 'actual muscle functions' it is of importance to know whether or not an active muscle shortens (concentric contraction) lengthens (excentric contraction) or does not change its length. Better still, estimations of the rate of change of muscle length should be related to quantified and normalized EMG data (Grieve et al., 1978), even if it is recognized that muscle length is not identical to muscle fibre length.

The orientation of muscle fibres will in many cases not be parallel to the working line of the muscle. This will have a definite influence on possible muscle function as revealed by the length-force relation (Woittiez et al., 1983b). They showed that the length-force relation (often misnamed as length-tension relation) depends on the architecture of the muscle in question. Contrary to the theories of Benninghoff and Rollhäuser (1952) and of Gans & Bock (1965) who thought that the angle between muscle fibres and tendons might be decisive, the length of the muscle fibres relative to the muscle belly length appears to predict length-force relationships for muscles of different architecture of different species (rat, frog) very well (Woittiez et al., 1983a). Therefore, attempts towards quantification of muscle function in dynamic situations following the approach of Grieve et al. (1978) are recommended. They related EMG activity to length changes in the muscle in question and succeeded in stating qualitatively the muscle function.

Hof & van den Berg (1977, 1981a-d) succeeded in quantifying the EMG-force relationship in a dynamic situation. A linear relationship between force of the triceps surae muscle and EMG was established. The search for such relationships has not been as successful as it was in static conditions (e.g., Bigland & Lippold, 1954; Rau & Vredenbregt, 1973). This has hampered the use of EMG in explaining the force histories of movements. Operationally explored relationships are therefore of importance. In the study of Hof & van den Berg (1977, 1981a-d) a salient point was that the EMG signal was used as an input to their muscle model and not as a direct equivalent of the force output of the muscle. In the muscle model not only force, but also velocity was included by the incorporation of the Hill model (6).

Formulating muscular function, relative to the function of other structures delivering moments and forces about the joints as well, demands specific assumptions about the other structures. Quanbury et al. (1975), Robertson & Winter (1980), Winter et al. (1974b, 1976ab), Winter & Robertson (1978) computed power transmission by joint forces, muscles and other moments, between segments of the body in walking. Working with some crude assumptions (e.g., only movement in the saggittal plane was studied), they concluded that the joint forces are as important as the muscle forces. Correlation with EMG studies (like the one of Dubo et al., 1976) should render a better understanding of muscle function in walking. Such a study rendering views on muscle function in relation to other force and moment transmissions at the joint, is still awaited. The issue is, that at present it is not possible to ascribe unequivocally force and moment transmission to the muscles and joint structures. Even if Pedotti (1977) was right in his statement that antagonistic muscle activity in walking is quite scarce, such activity appears to be of importance in many other movements (van Ingen Schenau et al., 1983), as well as in walking especially of bi-articular muscles in the thigh (Elftman, 1966). Modelling of joint motion and defining muscle function on the assumption of reciprocal activity of antogonistic muscles (Morrisson, 1970ab) may be proven to be an invalid procedure, as Lombard & Abbot (1907) foresaw (7).

From these examples it may be concluded that the understanding of the biodynamics of human locomotion in terms of actual muscle functions as revealed by EMG is as yet rather primitive. This is possibly the reason why in human walking the EMG signal is scarcely used as a sign of the activity of the nervous system in coordinating and regulating the movement as Bernstein would have wished. Exceptions to this rule exist : the work of Herman et al. (1976), Craig et al. (1976) and Cook & Cozzens (1976) to which reference will be made in Section 4 and the work of Pedotti (1977) which has already been mentioned.

In seemingly less complex motions, in human subjects, the use of EMG as an estimate of neural activity has been abundant (e.g., Feldman, 1980ab; Gurfinkel et al., 1971; Hallet et al., 1975; Wadman, 1979; Wadman et al., 1979, 1980ab; as well as many studies of neurophysiological aspects of human movement).

In the study of neural regulation of walking in cats and other animals EMG has been used successfully (e.g., Grillner, 1975, 1979, 1981; Ivanova, 1973).

It is felt that in the near future other concepts of muscle function than that of delivering of force or moments will become important. The thought of Elftman (1966) about the role of biarticular muscles of the thigh as energy savers in human walking is such a concept. These muscles appear to transmit power from one segment to another and vice versa without a considerable change in length. The work of Winter and his associates on power flow between body segments in walking in conjunction with the method of Grieve et al. (1978) relating EMG to length-changes in the muscle in question would render a confirmation of the existence of such energy-saving mechanisms.

The stimulating thought of the existence of a "stretch-shortening cycle" (Asmussen & Bonde-Petersen, 1974) in rather fast movements or in counter-movements involves much debate and research (van Ingen Schenau et al., 1983). Its possible importance in repetitive or cyclic movements was already stressed by Elftman (1966). Both, the "stretch-shortening cycle" as well as the energy conserving function of bi-articular muscles are involved in the explanations of energetical optimalization of walking (Cavagna, 1978, see also sub-section 4.3.3).

Reciprocal innervation of monarticular antagonists might be another way to save energy (Pedotti, 1977). In running and hopping storage of elastic energy in muscles and in tendons also saves energy (Alexander, 1977, 1980; Cavagna, 1978). Specialization of the muscle fibres into fast, slow and various subtypes for different functional use is expected to be of importance in the human subject as has been shown in various animals (Goldspink, 1977; Pool,1980).

EMG studies will help in the future to elucidate such mechanical and physiological processes. These processes form an important part of the degrees of freedom, which Bernstein postulated to exist (see Section 3). It is felt that much of the research on human kinesiological EMG has been used to better define these degrees of freedom rather than to throw light on the regulative activity of the Central Nervous System in coordinating human locomotion. It is also felt that this has been a necessary condition for tackling the problems of coordination, Bernstein tried to tackle with the indirect methods at his disposal (8).

2.4. Generalization problems.

Bernstein thought that studying human locomotion and other highly skilled movements in order to elucidate their coordination and regulation was imperative because these movements are automated and "have generality". A large number of subjects "have all mastered for example the act of walking incomparably better than any of their individual professional skills" (p. 172).A large body of knowledge on this automatic cyclic movement, walking and running has been gathered since then.

Fast repetitive cyclic arm motions have also been studied (Feldman, 1980ab). Fast as well as slow arm movements have been the object of study by many workers (e.g., van Dijk, 1979; Feldman, 1979, 1980ab; Wadman et al., 1980). In some instances fast and slow movements have been compared with each other (e.g., Hallet et al., 1975).

Standing, or posture, has been examined intensively by members of the International Society of Posturography (Bles, 1977, 1978ab; Bonnet et al., 1976; Gurfinkel, 1973ab, 1974, 1976, 1979abcd; Nashner & Woollacot, 1979). Standing may be also considered as an automatic movement. Other quasi-static movements, like aiming have been investigated (Gurfinkel et al., 1971). Most of these studies have been undertaken with the ultimate goal Bernstein posed: the elucidation of the role of the Central Nervous System in the coordination and regulation of movement.

The rationale for the choice of these classes of movement appears to lie in the contention that they are optimalized (e.g., by learning) and therefore repeatable within narrow limits.

In biomechanics, most of the above-mentioned classes of movement have also been studied. Much research has been carried out into sport movements. Some of the research projects on sport movements have been successful in developing biomechanical models for each cycle of movement, in stating a general power balance for the particular sporting achievement as a whole and in defining efficiency terms on a biomechanical and work physiological level (van Ingen Schenau & Bakker, 1980; van Ingen Schenau, 1982; van Ingen Schenau & de Groot, 1983). The generality of these models is apparent from the fact that their essential elements are applicable to speed skating, cycling (van Ingen Schenau, 1981) as well as to swimming (Toussaint et al., 1983). Automated-learned- fast arm movements, automatic movements, like standing and walking as well as sportive movements, especially those of champions on national and international levels, will be optimalized. Subjects in the study of sport movements are willing and able to reach a fairly constant level of attainment allowing critical investigation of the model parameters (9).

Optimalization in the other direction, in order to instruct the performers how to perform better on the basis of the input of performance parameters in optimizing models has become a topic for research in recent years (Hatze, 1976, 1981b). This would imply that starting from biomechanics an insight can be reached into the coordination and regulation of such movements (see e.g., Hatze, 1980a).

Irrespective of the question whether the classes of movement in study are 'real life' or not, they are often denoted with terms like volitional, goal directed, intended, or, automatic, automated. Such terms imply or assume a varying but ill defined involvement of the Central Nervous System (CNS). It might even be said that a large part of the research programme is directed to solve the question of the nature of the involvement of the CNS, so for the time being these definitions have to be operational or provisional. In the theories of Bernstein's Russian associates and pupils, concepts have been formulated like synergies - being "those classes of movement which have similar kinematic characteristics, coinciding active muscle groups and conductive types of afferentation" (Gelfand et al., 1971, p. 331). The concept of synergy is related to the concept of least interaction at the level of the CNS, implying that on the basis of a small number of leading parameters and the determination of the basic afferentation necessary for the realisation of the movement, the task of the CNS is held to be quite simple.

Some of the synergies, like the one for walking might even be innate (Willemse, 1961; Andre-Thomas & Autgaarden, 1966; Bressan & Woollacot, 1982) needing only simple activating signals from higher centres as Grillner and associates (1975, 1979, 1981) and Shik et al. (1968) have shown for cats and dogs (Shik & Orlovsky, 1965). Such relatively simple explanations for the control of movements may also appear valid for human walking. But it is to be questioned if such explanations can be generalized to some or all other classes of movements studied?

Some examples will clarify this point. Firstly, in the author's department a series of experiments was undertaken to ascertain if the relative number of muscle spindles in a muscle would be correlated with the time of mastering single motor unit control. This appeared true (van Ravensberg et al., 1978). But learning motor unit control also appeared possible in muscles like mylohyoideus, for which it is reported, they do not contain muscle spindles (Kleppe et al., 1982). It was even shown that the recruitment order of single motor units could deviate from the size principle (Millner Brown et al., 1973; Bakker de et al., in press). In biceps brachii muscle, a multi-functional muscle (elbow flexion, supination of the forearm), single motor unit activity was registered in different tasks. Units with a high force threshold for flexion tended to show a low threshold for supination and vice versa (Ter Haar Romeny, et al., 1983). When high forces are asked for, both classes of motor units may be used. These facts strongly suggest that motor units are recruited selectively. The input of the motor neuron pool from higher centres differs for each task.

The second point relates to the sensory input. Sittig (personal communication) carried out experiments on slow arm motion. The subjects could not see the position of their arm. They could only see target lamps. In the starting position, indicated by a light, their biceps brachii tendon was subjected to vibration. As a consequence the arm drifted from this position. The subjects were unaware of this drift. They were asked to flex their arm in order to reach an end position indicated by a second lamp either under the condition of continuous vibration or without further vibration. In both conditions the end position was reached correctly. In performing these experiments as well as in carrying out single motor unit control tasks, the subjects do not know which information source they use in performing.

In the experiments in the author's laboratory on single motor unit control some subjects were not successful in learning the task while others were successful in a short time. Interestingly, trained musicians performed better than non-musicians (van Ravensberg, et al., 1978). In these experiments, too, subjects were not aware which information source they used. Apparently the contention of Stein (1981) that a rather large number of control parameters can be used by the CNS, is valid for the movements in these experiments. The CNS appears to be more versatile than was thought. The use of control parameters in question might possibly be trained as appeared the case for the musicians.

Thus, in setting up experiments to disclose functions that will be attributed to one of the sensory elements in muscle, it should be borne in mind that other elements exist and may have a function. By choosing false or prejudiced experimental designs the experimenter might succeed in either begging the question or in harvesting irrelevant answers.

The results of the experiments mentioned in the preceding paragraphs should not be generalized to other movements, studied in other experiments, e.g., they do not imply that the size principle (Milner-Brown et al., 1973) will not be valid. This principle will be valid in many circumstances as well as in many classes of movement. The question arises: in which classes of movement? Are the movements in the above mentioned experiments to be classified volitional, intentional or conscious? Certainly, some of them have a certain artificiality. Does the label artificial help any further? It seems to the present author that as long as we do not know how to classify movements in terms of involvement of the sensory and motor parts of the Nervous System, some caution is required in generalizing from experiments on provisionally classified movements to other classes of movement. Like Bernstein, in choosing a certain class of movements, skilled or automated - whatever that may say in terms of involvement of the Nervous System - many researchers tend to generalize their findings to other classes of movement. As long as we do not know how to classify movements in terms of involvement of the Nervous System, such generalizations have to be considered with caution.

2.5. Validity of experiments on walking on motor driven treadmills.

The question posed in the preceding Section pertains to generalization. In this Section a similar problem will be discussed: are the findings of walking experiments performed under laboratory conditions on a motor driven treadmill valid for free walking? This is a problem of ecological validity.

If walking on a motor driven treadmill is not ecologically valid, the vast research efforts reviewed by Grillner (1975, 1979, 1981) on step generators of automated walking movements would not be valid. Moreover, the optimalization of energy expenditure in human walking studied by Molen et al. (1972b) and Zarrugh et al. (1974) would not be valid, notwithstanding the firm grounds these researchers found in the work of Atzler and Herbst (1928) who eventually succeeded in inducing their subjects to walk overground, but in circles.

The problem is reviewed by van Ingen Schenau (1980) who appears to be the first to give a fundamental explanation of the biomechanics of treadmill walking. Using a reference frame (x-y-z-coordinates) moving with the belt he demonstrated that a mechanical difference between overground and treadmill walking does not exist even if the treadmill is set at a slope. A rigorous, constant speed of the belt - not influenced by the variable inertia due to the movements of the subjects - is a necessary condition. Under this condition the Galilean invariance "indicates that the laws of Newtonian mechanics are identical in all reference frames which move with uniform speed with respect to one another". Van Ingen Schenau (1980) concludes:

> The only other possible cause for differences in treadmill and overground locomotion is the difference in visual and, to lesser extent, auditory information. The visual information will be particularly important in maintaining equilibrium and stability during locomotion. In overground locomotion the surroundings move with respect to the subject which is not the case in treadmill locomotion. This could cause a difference in regulation of the movement pattern resulting in differences in the kinematics and/or energy consumption. In studies concerning the regulation of human posture, the relative importance of visual information in this regulation has been shown (Bles et al., 1977; Brandt et al., 1976). The differences found will probably be diminished if the information from the surroundings during treadmill locomotion could be aligned with the information in overground locomotion. It would be interesting to develop an experimental set up in which the subjects get visual information comparable to that in overground locomotion. A treadmill can prove an extremely stressful environment for a subject. Experiments performed in our laboratory with 9 - 16 year old boys showed that most of these children were rather frightened to walk on the treadmill used. This treadmill had a width of 1 m and was elevated 1 m above the surrounding floor. Nine out of ten subjects showed much longer double support phases when walking on this treadmill compared to overground walking (mean difference: 15%). This fear for walking on a treadmill will probably diminish when the treadmill is built in such a way that the belt seems an integral part of the floor.

We will end this Chapter in a prospective way. Walking on a motor-driven treadmill will constitute an expedient experimental set up. It will enable experimenters to study the influence of varying - by experimental manipulation - perceptive input. In this way human walking on a motor driven treadmill will be used to study the biodynamics of human locomotion, with the Bernsteinian goal: the elucidation of the regulation and coordination in motor acts, just like the work of Grillner (1975, 1979, 1981) for cats. Two methodical considerations are to be made. The motor driven treadmills used in such studies are to be in conformity to the rigorous requirements van Ingen Schenau (1980) stated. The registration of the movements has to be undertaken with a data acquisition system of sufficient resolving power.

3. FUNCTIONAL NON-UNIVOCALITY.

As movements are - as Bernstein stressed - "non completely determined by effector processes" (p.213), this indeterminacy or non-univocality has been the central core in the argument for the need for (proprioceptive) afferentation in Bernstein's theory. He postulated "more or less complicated auxiliary systems which ensure constant and complete control by the centre" (p. 213)(10).

In this Section some of the many lines of research directed to elucidate this non-univocality will be discussed. Bernstein (1940) stated:

> The deeper the functional gap with the absence of univocality between the centre and the periphery, the more complex and unstable is the real relationship between impulses and movements; the greater (in mathematical language) are the number of degrees of freedom of the motor periphery relative to the central effector, the more complex and delicate must be the organizational control to which we have referred. This organizational control is motor coordination.
>
> In this context the idea of coordination is in the closest relationship to the idea of functional non-univocality of the connections between the motor centre and the periphery, between impulses and effects. The more our knowledge of the forms and types of these non-univocalities increases, the deeper becomes our understanding of the coordination of movements.
>
> At present a whole series of sources of this indeterminacy are known (p. 105).

He discerned anatomical, mechanical and (neuro-)physiological sources of indeterminacy. On pages 105 and 106 examples have been cited by him. Some of these sources would be formulated in contemporary language as non-linearities of the functioning musculoskeletal system. For example the length-force relation of muscles (Ramsey & Street, 1940; Ralston et al., 1947) and the force velocity curve (Wilkie, 1950) are non-linear.

Some authors have tried to explain some non-linearities away by the assumption that lever arm values increase while according to the length-force relation, force decreases (Inman et al., 1981) resulting in quasi-linear torque values. Constant, or nearly constant lever arms, however, which are assumed to exist with respect to the extensors of the arms and legs by Inman et al. (1981) would lead to a problem of non-linearity for the controller which has to master the length-force relation as well as the force-velocity relation in effecting movements. Possibly, the problem for the controller in solving the motion problem, will be less in submaximal contractions. However, factual knowledge of length-force relations in non-isometric submaximal functioning of muscles is very scarce, and speculative considerations are abundant notwithstanding the elegant synthesis of Bahler et al. (1968).

The function of the control system itself, consisting of very non-linear devices - neurons - has been analyzed successfully in terms of linear system theory (van der Gon, 1983). Assuming parallel signal transmission and signal processing and a very simple processing device, van der Gon succeeded in modelling linear movement control by a system consisting of non-linear working units. Parallel transmission from higher centres has also been assumed in the theories of the descending α-γ co-activation (Merton, 1953). A second parallel signal-transmission originated from the various receptors in the motor system (11). The processing of both streams of impulses results in motor unit activation. Linearity is reached by the assumption of differences in size or input impedance of the motor neuron pool units, meaning the size principle (Milner Brown et al., 1973; Hatze, 1980a; Henneman, 1981). Van der Gon corroborates his theory by experiments of Vinken et al. (1983) and Ter Haar Romeny et al. (1983), carried out on simple arm movements.

The view of Houk (1975, 1979) on the functioning of muscle receptors in the signal processing might be appreciated as an endeavour to simplify muscle control. Parallel processing of Golgi tendon organ signals end muscle spindle signals could lead to the processing of quasi-linear muscle stiffness (force change over length change) in the regulation of simple movements like elbow flexion or extension. Parallel descending influences are hypothesized by Feldman (1974, 1976, 1980ab) in postulating a superposition of tonic and phasic co-activation of antagonistic muscle pairs. The first activation would lead to a pre-set stiffness of the effector system (invariant characteristic)(12).

The concept of invariant characteristics as well as manipulation of sensory input were used by van Beekum (1979), Juta et al. (1979) and Vincken (1982, 1983) in experiments on control of arm posture and movements. The ecological validity (in the sense Gibson (1979) uses this concept) of such experiments is strongly doubted by Reed (1982) in implying that such postures and movements are highly "artificial". In most of such experiments, the influence of gravity and of other sensory systems than the system the experimenter wants to study, is excluded by the experimental design. Conclusions drawn might not be valid for spontaneous movements, as in the production of these movements the influence of gravity and many sensory systems may play an important and decisive role in solving the motor problem ('spontaneous movements' and 'movement problem' are the terms Bernstein used). Van der Gon (1983) remarked that modelling the nervous system while taking in account all the existing differences between the elements of the system, would ask for a model as complex as the system itself. "For that reason one often starts with idealized elements with a kind of average properties". The same holds true for the effector system and its motor and sensory elements.

In such relatively simple experimental designs, it was thought that at least in the first 100 msec of fast movements open loop control exists: the motor program as revealed by EMG will not be disturbed by suddenly blocking the movement (Wadman et al., 1979, 1980ab). However, when the disturbance consists of displacement of the target position (a light), very fast modulation of the EMG takes place, suggesting a closed loop and the existence of some kind of efference copy in the nervous system (Gielen et al., 1983). Reed's (1982) opinion that "a functional movement is always under mixed control" appears to be corroborated by these experiments on simple "artificial" movements. Reed (1982) reformulated Bernstein's concept of functional non-univocality:

> Bernstein suggests that the animal is in continual <u>dis-equilibrium</u> with its environment, requiring that it not <u>react</u> to stimuli, but rather that it <u>act</u> all the time and that it constantly <u>evaluate</u> its actions with respect to ever changing current conditions, while at the same time <u>modulate</u> its activities so as to meet its needs and goals within the environment (p. 108).

In fact, Reed's argument demands experimental designs on more complex and ecological valid movements in which actively exploring perceptual systems play an important role (13).

The Russian studies on the regulation of posture led to the conclusion that another simplifying concept in motor control i.e. the use of the stretch reflex is of no importance in the control of stance (14). It is in all probability blocked in favour of a postural (reflex) control mechanism of a superior kind (Eklund, 1973, quoted by Bonnet et al., 1976). In slow, whole body sways, as well as in fast imposed disturbances the EMG activity of the leg muscles appears not to be related to change in leg muscle length. A central programme of postural fixation was postulated by Gurfinkel and his co-workers. This programme would secure stability by regulating muscle function. Short latency responses (45-55 msec) could not possibly result from receptors in the foot. They suggested that these responses were mediated by afferentiation from distant muscles and joints in the legs and the trunk. Gelfand et al. (1971) proposed "the compensation of respirating disturbances of the erect posture of man as an example of the organization of inter-articular interaction" (p. 373).

Indeed the coordination of posture as well as other movements is highly complicated as Zalkind (1973) indicated:

> It is well established now, that performance of locomotor response is usually accompanied by changes in the activity of a lot of muscles, sometimes rather distant from the region of the main response (so called postural reaction)(p. 19).

If the various afferentations which have been proven to influence movement tasks are taken into account (15) the chances for ambiguity of experimental designs bearing on complex movements is apparent. Unequivocality will only be brought about by rigorous experimental manipulations.

Gelfand et al. (1971) coined the concept 'functional synergy' defined as:

> ...those classes of movements which have similar kinematic characteristics, coinciding active muscle groups and conductive types of afferentation (p. 331).

Cyclic locomotive synergies (walking, running, swimming, etc.), synergies of throwing, blowing, jumping "and a certain (small) number of others" (p. 311) have been discerned. When the concept of synergy for the explanation of the regulation of all classes of movements is used, the existence of non-univocality as defined by Bernstein is denied. This remains the case when basic synergies are introduced which form "a dictionary of movements" (p. 332) By using such concepts a one to one correspondence between CNS activity and effector activity is assumed (16).

It seems to the present author, that in some movements synergies may exist such as Nashner et al. (1979), Nashner and Woollacot (1979) claimed for posture e.g. a sway and a suspensory synergy. Such synergies have to be adaptive in character in order to enable the animal to react to various disturbances. The adaptation will have a reflex-like character in one class of movement - like standing - or in for example some details of a structured movement in the Bernsteinian sense (p. 69) (Forssberg et al., 1975; Gurfinkel & Latash, 1979a). In walking and running for example the adaptation to speed will be under the influence of higher centers (Forssberg, 1983). In other classes of movement these reactive adaptations may even have the character of a conscious activity of higher CNS centers and may even be connected with anticipation.

The principle of least interaction is another attempt to simplify the task for the nervous system. This concept implies that on the basis of a small number of leading effector parameters and the determination of the basic afferentiation necessary for the realization of the movement, the higher levels of the CNS are able to control movement in a simple way (Gelfand et al., 1971). In this way these levels would be freed to be occupied by others tasks. If this concept is to be successful, it must be applicable to those classes of movement provisionally defined as 'automatic'.

Indeed, on the basis of experimental evidence, step generators at the level of the spinal cord are postulated for automated movements such as walking and running of cats. The generators, working with varying intensity, produce variations predominantly in the stance phase of the limbs (the phase in which the feet are in contact with the ground) or - better still - in the extension phase (during which extensor muscles are active, a phase which is slightly out of phase with the stance phase proper). These variations result in a shorter duration and faster contractions of the muscles with the increase of speed. The intensities of the generator's activities are regulated from supraspinal structures (Grillner, 1975, 1979, 1981), but their intra-limb and inter-limb coordinating functions appear to be organized at the spinal level (Forssberg et al., 1980ab).

The integrated actions of muscles in the flexion phase and in the extension phase could be regarded as two synergies. This idea is supported from pathology; e.g., in hemiplegia where the extensor synergy has in most cases a higher tone than the flexor synergy (Brunnström, 1970; Fugl-Meyer, 1980) in standing as well as in walking. This would indicate some similarity between the generating neuronal subsystems. Such a similarity was also proposed by Nashner (1979). In pathologic cases, e.g., hemiplegia, the adaptive character of the synergy is decreased.

According to Kornhuber (1974) other non-automatic movements may be regulated as follows. In rapid movements cerebellar preprogramming regulates the spinal activity

> with regard to timing and duration of activity. The basal ganglia serve as a ramp generator for slow voluntary smooth movements of different speeds. For those movements that need sophisticated analysis of tactile objects, the output patterns of cerebellum and basal ganglia are further processed in the motor cortex (p. 267).

Even when it is granted that parts of these latter theories are speculative and at least rather imprecise in defining the classes of movement to which they pertain, it is nowadays possible to formulate the problem to be solved by these coordinating functions (generators, cerebellum, basal ganglia and cortex) at least on a theoretical level.

If it is supposed that the CNS has a certain "knowledge" of the peculiarities of the system it has to coordinate (i.e. the sensori-motor system) then a part of the non-univocalities would be explained away. Recent studies on non-linear properties of muscle activation show that the CNS seems to be capable of coping with the non-linearities of muscles by innervational impulses with varying time intervals (Stein et al., 1981). Energetically optimalized muscle contraction could be predicted by a relatively simple model. This was experimentally verified by Hatze & Buys, 1977 (cited by Hatze, 1980a). Hatze (1980a) proposed another "teleological adaptation operating in the neuromuscular control system". He modelled a logarithmic relation between the number of activated motor units and the relative cross-sectional areas occupied by these units. This model predicted the data of Milner Brown et al. (1973) on the recruitment order of motor units. Hatze concluded that in many non-maximal tetanic muscle contractions, a "principle of maximum grading sensitivity, the realization of which is the motor size unit law" (p. 34) can be postulated.

By such teleological reasoning it might be supposed - rightly - that the neuromuscular system is organized in such a way that the problems of non-univocality from both anatomical and (neuro)-physiological sources are solved. There still remains the problem of non-univocality from the mechanical sources enumerated by Bernstein on p. 214. The CNS, in coordinating movements has to solve these problems. On the basis of modern research it is now possible to formulate these problems very well. Computer programs of a multilinked model of hominoids consisting of 17 links exist (Hatze, 1977a, 1980b, 1981b). The model represents 42 three-dimensional degrees of freedom roughly in conformity with the major determinants of human walking (Saunders et al., 1953; Inman et al., 1981, see Fig. 2). Gravitational and inertial moments are included. The model stimulates and optimizes movements. It is able to analyze real life movements when fed with data from a Selspot movement registration system. As the model also contains 46 muscle groups, it appears possible to formulate the problem to be solved by the CNS in coordinating movements, by muscle activation at least in the saggittal plane (17). As the model contains general myo-cybernetic control sub-models, predicting motor unit recruitment and stimulation rates (Hatze, 1977b, 1978) and accounting for the non-linearities of the length-force and force-velocity characteristics of muscle, the model should be able in the near future to pinpoint the task of the neural controller in the optimal execution of movements such as walking, jumping etc. In conclusion, by the use of the extensive models as developed by Hatze it will be possible to formulate the complex task of the neural control system with respect to the mechanical sources of non-univocality in solving a motor problem.

These models have been developed using the principles of optimalized actions of the CNS interacting with the non-linear peripheral musculoskeletal system. These principles may only be valid for fully learned, automated or in any way optimalized movements. In new movements, the non-linear feedback, to which the CNS is fully accustomed is somehow manipulated by higher centres. The CNS has a knowledge of the effector system and its relation to the environment which largely surpasses our experimental descriptive and analytic understanding.

The existence of non-equivocality has a profound influence on the usage of different experimental designs and on generalization of the conclusions so derived.

4. LOCOMOTION STUDIES.

4.1. Introduction.

Locomotion, especially human walking, has been studied since the work of Braune & Fischer, Marey & Muybridge at the end of the 19th century. On the West-European subcontinent, in the United Kingdom and the U.S.A. research on this topic nearly came to a standstill in the early decades of this century. The revitalization of research on human locomotion in the U.S.A. and especially in California after World War II (Inman et al., 1981, preface) stemmed from the urgent need of providing war-veterans with adequate protheses. Elsewhere in the U.S.A. researchers as Steindler & Drillis, exiled from pre-war Germany, had already transplanted the German interest in the functional anatomy of the muskolo-skeletal system (Fick, 1904-1911; Spalteholz, 1933; Braus, 1921; Benninghoff, 1938) to the U.S.A. (Steindler, 1935, 1955; Drillis, 1958). It is left to historians of science to place these developments in true perspective (18).

For such an historian it would appear that contrary to the trend elsewhere, in Russia an investigator - Bernstein - engaged in industrial ergonomic research, got an assignment to do an analysis of walking in order to solve some problems in the design of footbridges (see Bernstein, p. 171, footnote). He used photonic techniques. He concluded that the importance of the subject merited a special branch of physiology. This led to the monograph currently being reviewed. For the pupils of Bernstein, as well as for the numerous students of human locomotion elsewhere, the primary goals of the study were directed to specific applications of the knowledge gained by their work. These primary goals were surpassed by an ultimate goal developed in the publications contained in Bernstein's (1967) book. The ultimate goal of the developed programme was formulated by Bernstein in 1940 and earlier. It has been pursued by other workers as well, even while carrying out contract research directed to short term goals (e.g., the evaluation of presthetics and orthotics, Klopsteg & Wilson, 1954) or optimalization of sportive movements for coaching directives (e.g. de Groot et al., 1983). The scientific forum, in organizing itself in bodies such as the International Society of Biomechanics and its various branches, moved towards an independent research tradition aiming at high levels of generalizability. It is to be hoped that the scientific community will take the responsibility for this relative independency, as indeed did Bernstein.

In this Section attention is first paid to the phenomenalistic description of human gait and some issues in this type of work are taken up. Secondly the energetic optimalization of different modes of human walking is seen as a topic of much interest. Explanations for energetic optimalization are given. Walking and running are the two modes of human gait. It is well known that competition walking will cost more per metre distance traversed than running, so that in running other optimalizational principles must be assumed. The study of the transition of walking into running is recommended. General theories on stiff-legged (one linked) walks of idealized animals and man have been developed. An attempt is made to accommodate the knowledge on energy cost of human walking with such a theory.

Thirdly some attention is given to modelling of gait with n-linked models. Lastly some remarks are made on clinical applications.

4.2. Phenomenalistic description.

4.2.1. General overview

The important work on gait of the researchers in California over a period of 35 years has been reviewed recently by Inman et al. (1981). This beautifully produced book contains a well illustrated description of generalized human walking.

In generalized human walking seven major determinants or elements are considered by this group to be essential. The first three are pelvic rotation about a vertical axis, pelvic list (adduction in the hip joint of the stance leg) and flexion of the knee in stance. These elements result, amongst others, in a decrease in the vertical displacement of the body's center of mass (see Fig. 2).

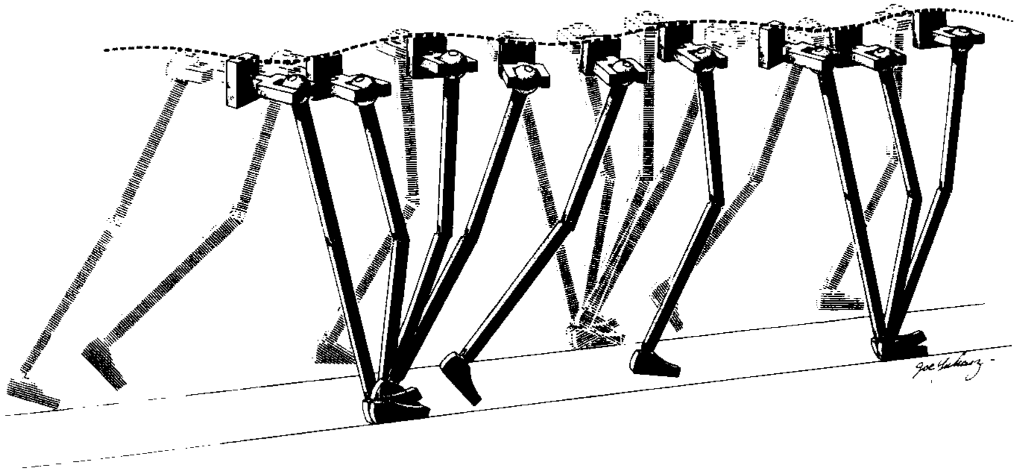

Fig. 2. Sinusoidal pathway of the center of mass under the influence of the major elements of walking (see text). Reproduced from Inman et al. (1981), Baltimore, Williams & Wilkins. The figure was published originally as illustration of Saunders J.B. de C.M., V.T. Inman and H.D. Eberhart (1953). The major determinants in normal and pathological gait, Journal of Bone and Joint Surgery 35A, 543, who gave also permission.

The ankle joint, of which the stiffness is regulated by flexor and extensor muscles, smoothes the path of the upper end of the leg. Supinatory and pronatory movements in the foot have a further, rather small, smoothing effect. Knee flexion in the first half of the stance phase and plantar flexion in the ankle, as well as knee flexion starting at the end of stance, also make a contribution to the smoothness of the pathway of the body's centre of mass. The movements in the joints mentioned are regulated by activity of muscles or by their compliance in active or passive states.

Lateral displacement of the body is related to the transverse distance of the feet when they are in contact with the floor. Rotations of the upper trunk segment, the shoulders, in counterphase with pelvic rotations, as well as the swing of the arms in counterphase to the swing of the lower limbs smoothes the forward accelerations of the centre of mass.

Most of the details, as well as the above-mentioned elements, of the complex walking process are dependent on the mean walking velocity. For that reason, a description of generalized walking will be preponderantly of didactic value. It renders no quantitative information. It gives an overview of "the genenal structure" but will not give information on "the finer details in which this structure is able to differentiate" in the meaning of Bernstein's views on locomotion. In order to analyze such details, laboratory experiments have been undertaken in which environmental variables are well under control and instrumentation is possible. The use of such an analytical approach poses the question of validity. It is well known that subjects are apt to alter their way of walking when they are conscious of being observed. Drillis (1958), Finley & Cody (1970) and Molen et al. (1972a) covertly observed walking pedestrians in order to obtain valid standards.

Differences in mean velocity, stride frequency as well as in step length in different locations occurred. A sex difference was noted: women taking more and smaller steps while maintaining lower speeds than men. The ratio step length/step frequency appeared to remain fairly constant for both sexes and over the range of speeds the subjects were found to walk. This ratio was different for both sexes.

Comparison of data of Molen et al. (1972a) with data from Du Chatinier et al. (1970), the latter pertaining to a constrained walk in which speed was imposed, showed that the subjects in this situation tended to a higher frequency resulting in a significant lower ratio. In contrast, Herman et al. (1976) reported, for a "preferred" walk under laboratory conditions, a lower frequency than the pedestrians covertly observed by Finley & Cody (1970).

The differences reported are quite small and will not contaminate general theories.

Such differences may be of importance when one wants to study the finer details which possibly occur in walking on motor driven treadmills (see subsection 2.5). Herman et al. (1976) pointed to the possibility that at very low speeds a relatively longer time is spent in double support in order to attain maximal stability. In constrained walks, according to Du Chatinier et al. (1970) the same might be true. A higher step frequency resulting in a longer total double support duration was observed in untrained adults walking on a motor driven treadmill compared with track walking. Under both these conditions, the step frequency appeared to be higher than in a covertly observed outdoor walk (unpublished observation). The same was reported for children by van Ingen Schenau (1980). However, such "artificialities" were not seen after careful procedures during which, the subjects became accustomed to the experimental conditions. Carefully designed instructions to subjects can solve the problem of validity, as was confirmed by Herman et al. (1976).

Most authors working like Herman et al. (1976) with a range of speeds, or like Murray (1967) and Murray et al. (1964, 1966, 1970) with clusters of free and fast speeds, do find simple linear relations between the various components such as stride frequency, step length, duration of stance and swing phases on the one hand and walking speed on the other. Free chosen speeds, as well as speeds imposed by instruction, as far as this is measured by these variables, do not differ in structure (Herman et al. 1976). From such data, it appears that at free speeds step length between subjects tends to vary more than step frequency. In various speeds, the ratio step length/ step frequency remains fairly constant over a range of speeds of 0.7 - 2.0 meter per second (Herman et al., 1976). The same was found by Decan (1965) after normalization for body height and by Inman et al. (1981). They concluded however that body height could not be the only factor in deciding this spatio-temporal dimension of walking (19).

Step length and step frequency vary linearly with speed, resulting in an inverse relationship between speed and cycle duration. Stance time (i.e. the duration of the stance phase) varies strongly and linearly with mean walking velocity or cycle duration. Swing time varies also with speed but considerably less so (Murray et al., 1966; Herman et al., 1976, see Fig. 3).

The above-mentioned relations have as a consequence that the double support time shortens with higher speeds. The duration of the double support time versus cycle time decreases exponentially with speed. At very low speeds

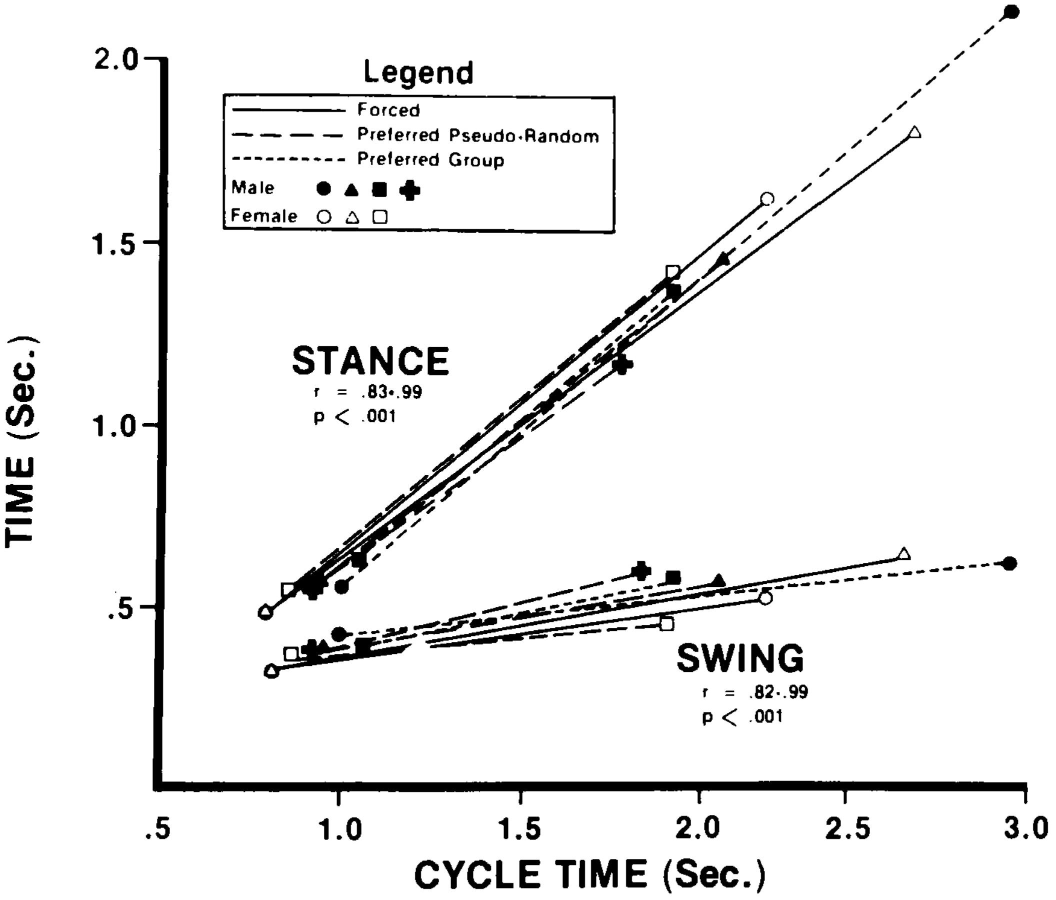

Fig. 3. Sample regression lines for the relationship between stance and swing periods and cycle (= stride) time period, during various instructed and preferred walks in both sexes. Note that the time interval between values of stance and swing times at any cycle time is indicative of the double support period. Reproduced from Herman et al. In: Herman, R.M., S. Grillner, P.S.G. Stein & D.G. Stuart (1976) New York, Plenum Publishing Corporation, with permission of authors and publishers.

the double support time will take 50% of the cycle duration (20). At speeds of about 2m/sec the duration of the double support phase will reach its minimal value. Instead of walking, running without double support, may be the choice of locomotion (Grillner et al., 1979).

Herman et al. (1976) noted further a strong covariance between times of maximal joint excursions in both phases of gait and cycle time in walking at normal speeds. Further analysis of the joint excursions suggested an "unifying principle of joint coupling" (p. 34).

Herman et al. (1976) appear to have been the first and only researchers who have statistically analyzed various details of the human walking process over a wide range of speeds, including speeds like those seen in pathology of walking. They correlated joint excursions, EMG data and temporal factors of gait. The data were related to concepts of Bernstein and his coworkers as well as to the work of Grillner and associates. It is felt that in the near future the details of the research of Herman and associates will be of importance, when the work of Grillner and associates is expanded to EMG of human walking.

Herman and his associates related EMG to speed. At very low speeds the EMG patterns varied very widely, whereas at preferred speeds the variability of the magnitude and duration of the discharges of the muscles in study was four to sixfold the variability of the magnitude and duration of joint excursions. Nevertheless EMG bursts could be related to the time histories of gait parameters. Any measure of EMG "intensity" increased with speed.

4.2.2. Problems in phenomenalistic descriptions of human gait

Three issues are of importance in the descriptive research on human walking and running.

First, Inman et al. (1981) called attention to the fact that the literature does not show good agreement on the relative duration of the double support phase with respect to cycle duration. They cited studies of Contini et al. (1964), Finley et al. (1975), Grieve & Gear (1966), Murray et al. (1966). They commented that the accuracy of measurement and differences in measurement procedures may be the cause of the disagreement (21). Drillis (1958) reported a variation in the ratio of swing time/stance time of 0.5 to 0.8 with speed, indicating a variation of 0.25 to 0.1 (22) for the ratio of double support time versus stance time for the range of speeds studied. This would mean that the ratio of double support time to cycle time is not invariant with speed of walking. Thus, the contention that double support time decreases exponentially with speed, is quite right (Herman et al., 1976, see Fig. 3).

The second issue, is a consequence of the first one: as the relationships between stance, swing and double support periods and the cycle time vary differently with speed, normalization of time-dependent data over speed may be an unjustified procedure. In the double support phase the kinematic chain of the legs and the pelvis is closed. A straightforward interpretation of EMG data in connection with twice differentiated displacement-time data is not possible without measurement of foot-reaction forces. Interpretation of such data on the supposition that they pertain to swing phase, while in reality double support existed, will lead to ambiguous results (23).

The third issue is related to the reasons why the results of many reports on EMG during walking are not reviewed in this paper. It is quite customary to relate EMG data to the timed phases of gait: stance, swing and double support phase as percentages of gait cycle time. EMG data are utilized in this way to explain muscle functions during gait in terms of forces, generating accelerations etc.. Such research will not

> provide a more reliable comprehensive basis for a subsequent broad extension of investigation into the genesis and pathology of locomotion (Bernstein, p. 171).

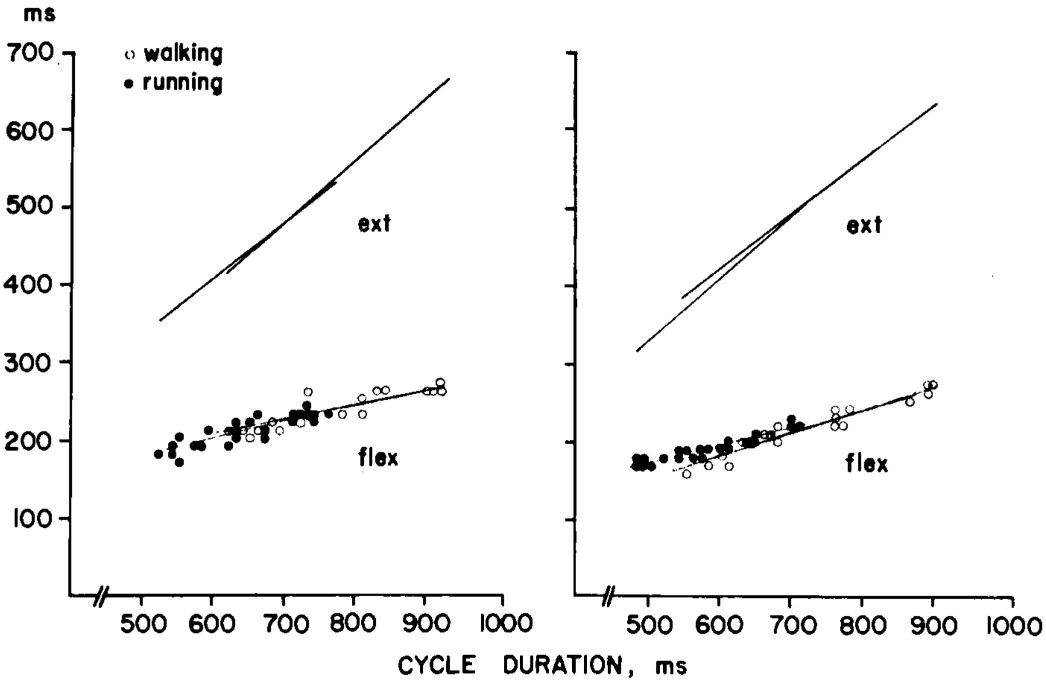

Figure 4a

Figure 4b

Fig. 4. Duration of the flexion and extension phases versus step cycle duration. The lines for the flexion phases (flex) are drawn for walking (open circles) and running (filled circles) separately. For the extension phases only the calculated lines are drawn. In A the general picture is illustrated, in B one of two subjects out of 8 subjects showing a significant difference ($p < 0.05$) between the slopes for walking and running is illustrated. Reproduced from Grillner, S., J. Halbertsma, J. Nilsson and A. Thorstensson (1979), Amsterdam, Elsevier Biomedical Press, with permission of authors and publishers.

Herman et al. (1976) clearly and explicitly carried out their research with this goal in mind. Neural and biomechanical aspects of control were discussed by these authors. They related EMG data to joint excursion data. Relating EMG data to time factors of gait will not be the best choice of methods. Grillner et al. (1979) stressed this issue. They described the relationship of flexion phase and extension phase to cycle duration (and therefore speed) in walking and running (see Fig. 4).

They defined flexion phase T_{FL} as the interval between the onset of flexion in the knee joint (during stance phase) and the onset of the subsequent extension (in mid or end of swing). T_{EXT} or extension phase was defined as the rest of the cycle. The relationships of these phases to cycle duration are linear. This can be expressed as $T_{FL} = aT_{EXT}+b$ in which a varies from 0.14 to 0.34 and b from -0.02 to 16 sec. In 6 of the 8 subjects the coefficients for walking and running did not differ significantly and overlapped in the region of cycle durations found in the transition of walking into running. In this region subjects are able to choose for one of the two modes of gait.

It seems to the present author that the existence of rigid statistical connections between EMG and time and space factors of gait - such as stance and swing phase - is no proof of their existence as control elements. The duration and the intensity of muscle contractions during flexion and extension phases, as defined by Grillner and associates and not during the stance and swing times, are better candidates. Observations of EMG in these firstly named phases of human walking should be a critical experiment for this issue. Such observations are to be expected within a short time (Halbertsma, 1982, personal communication).

In a lot of the earlier work on human gait it was not known how to relate effects (step length, frequency, stance time, swing time) to causes (neuronal activity). Thus the description of these effects was only used as a reference frame for the description of kinematic phenomena (as joint excursions) or for the analysis of the mechanical functions of muscles. It did not serve the goal Bernstein posed for such descriptions: analysis of coordination and regulation of movement by neuronal activity.

4.3. Energetic optimalization of gait.

4.3.1. Introduction

Energy expenditure expressed in units per kg body mass per unit of time is a measure of the physiological load on the body's cardio-respiratory system. Energy cost expressed in units per kg body mass per unit of distance traversed is a measure of energetic cost over profit.

In walking, the physiological load will not be a constraining factor. Nevertheless the cost/profit ratio will be optimalized. This mode of locomotion can be maintained over long periods. In gait at higher speeds, the rate of expenditure (per unit of time) will rise. Thus a mode of locomotion should exist in which the cost per distance traversed will be such as to postpone maximal use of all cardio-respiratory reserves: running.

The maximal speed attained in running is said to be 16 km/h in untrained and 22 km/h in trained subjects. Running at moderate speeds can be kept up for considerable times and distances. In competitional walking or race walking the maximal speed is said to be 15 km/h. This mode of walking can be kept up too for considerable times and distances. In both strategies - running and competitional walking - the possibility exists that anaerobic work will curtail the overt use of chemical energy. The latter is customarily estimated from measurement of 0_2-consumption, while the former is only to be estimated by blood-gas analysis.

This Section will go into some aspects of energetic optimalization of gait. Biomechanical and physiological mechanisms and processes are to be explored. Next to the regulative and coordinational actions of the nervous system these mechanisms and processes are believed to be decisive for the optimal solutions of selection of speed, step frequency and length in walking. In the selection of modes of gait at speeds far lower and higher than the optimal speed, energetic aspects may also be decisive.

General theories on locomotion of animals (and men) also pertain to optimalizational principles. At least two modes of gait, walking and running, are discerned (next to galloping and jumping). They are in use by a wide variety of terrestrial animals. The first one - walking - is characterized by the use of legs which are thought to be rather stiff: a stiff-legged gait. In running the flexion of knee and stiffle joint, counteracted by stretched muscles or tendons, is characterized as a compliant mode of gait. Both modes of gait are studied in terms of efficiency (Cavagna, 1978) and compared with swimming and flying (Alexander & Goldspink, 1977). Rather low efficiencies appear to exist in terrestrial stiff-legged walking modes. The efficiency terms are formulated as a ratio between work done to reach potential and energetic levels of energy of the body's centre of gravity of the total potential and kinetic energy levels (Cavagna, 1978; Winter, 1979) on the one hand and energy cost on the other hand.

Various energy saving mechanisms are proposed in the so called stiff-legged walk of human beings. In the compliant running gait the very compliancy is thought to lead to a lower energy cost.

Running differs from walking in the absence of a double support phase in the former. This phenomenon is connected with the compliant and stiff character of the contact with the ground in these modes of gait. Other modes (hopping, galloping) will differ in another way in this respect. The general theory of Alexander & Jayes (1978ab, 1980) and of Alexander (1980) tries to correlate contact characteristics and potential and kinetic energy characteristics of such modes of gait. In this Section an attempt will be made to reconcile the various approaches to human gait.

4.3.2. Energy expenditure rate and cost of human gait

Energy expenditure expressed in units per kg body mass per unit of time was shown by Ralston (1958) to depend on the speed of walking in a quadratic fashion of format $\dot{E}=b+mv^2$, where $\dot{E}$ is the energy expenditure rate (expressed as cal/min) per kilogram body weight, v is speed in m/min and b and m are constants. The expression is valid for a range of speeds from 50m/min up to 100m/min.

Using weighted (24) data from various investigators Zarrugh et al.(1974) and Inman et al. (1981) combined estimations for b and m:

$$\dot{E} = 32 + 0{,}0050v^2 \qquad (1)$$

This equation is illustrated in Fig. 5 (lower dashed line).

As many of the implications and intricacies of such work have recently been reviewed by Inman et al. (1981), it is not necessary to go into detail here: differences in experimental conditions, in sex, the invalid convention of some investigators in subtracting the expenditure in standing from that in walking etc., will not be dealt with. Instead the focus will be on problems of optimalization.

Zarrugh et al. (1974) deduced from their own experimental data and data from Molen et al. (1972) a general hyperbolic function which takes into account the step frequency n in steps/min and the step length s in metres:

$$\dot{E}_w = \frac{\dot{E}_o}{\left(1 - \frac{s^2}{s_u^2}\right)\left(1 - \frac{n^2}{n_u^2}\right)} \qquad (2)$$

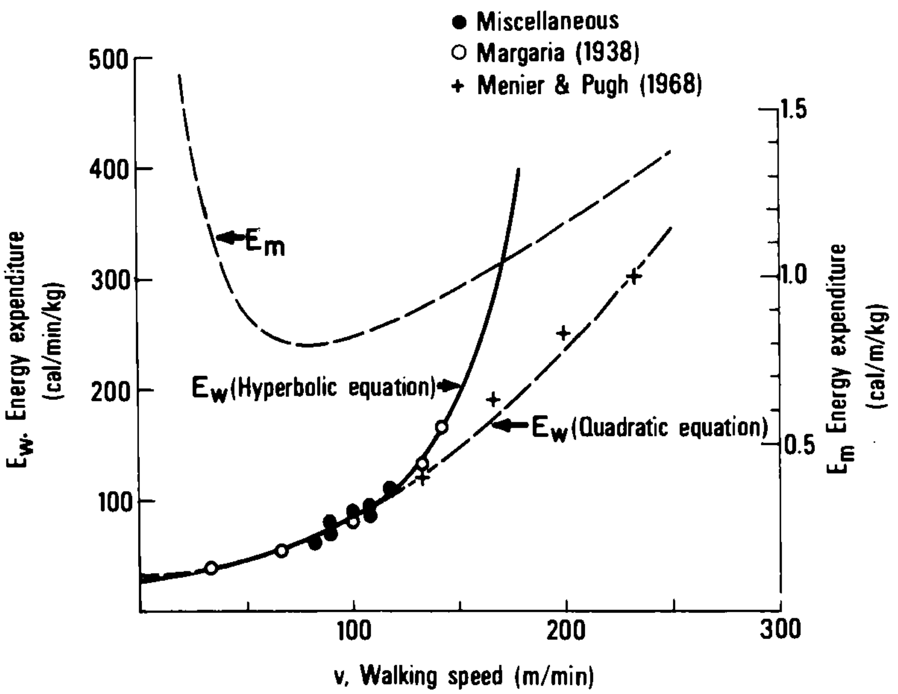

Fig. 5. Energy rate, bottom lines referring to the equations in the text, and energy cost, top line, idem. Reproduced from Inman, V.T., H.J. Ralston & F. Todd (1981) Baltimore, The Williams and Wilkins Company, with permission of the publishers.

where $\dot{E}_o = \dot{E}_w$ when $n=s=o$, n_u and s_u are upper limits of n (step frequency) and s (step length) as $\dot{E}_w$ approaches infinity. The rationale for such an approach was put forward by Molen et al. (1972b). In equating $\dot{E}$ with speed only, the free choice of any combination of step frequency and step length at a given velocity is not accounted for.

It was observed by Molen et al. (1972a) that walking pedestrians choose such combinations of step length and frequency that a mean ratio of step length/step frequency of 0.0070 results for males and 0.0060 for females. In their study on energy expenditure in walking on a motor driven treadmill, ratio's of 0.0072 and 0.0056 were found (Molen & Rozendal, 1967). From other studies it was apparent that this ratio differed significantly between sexes (Du Chatinier et al., 1970). It was thought that human subjects walking at a range of speeds would choose an optimal combination of step length and frequency, resulting in a minimal energy expenditure cost per unit task or metre distance travelled. This appeared to be true as was graphically shown by Molen et al. (1972b) and mathemetically modelled by Zarrugh et al. (1974). (See Figs. 6 and 7).

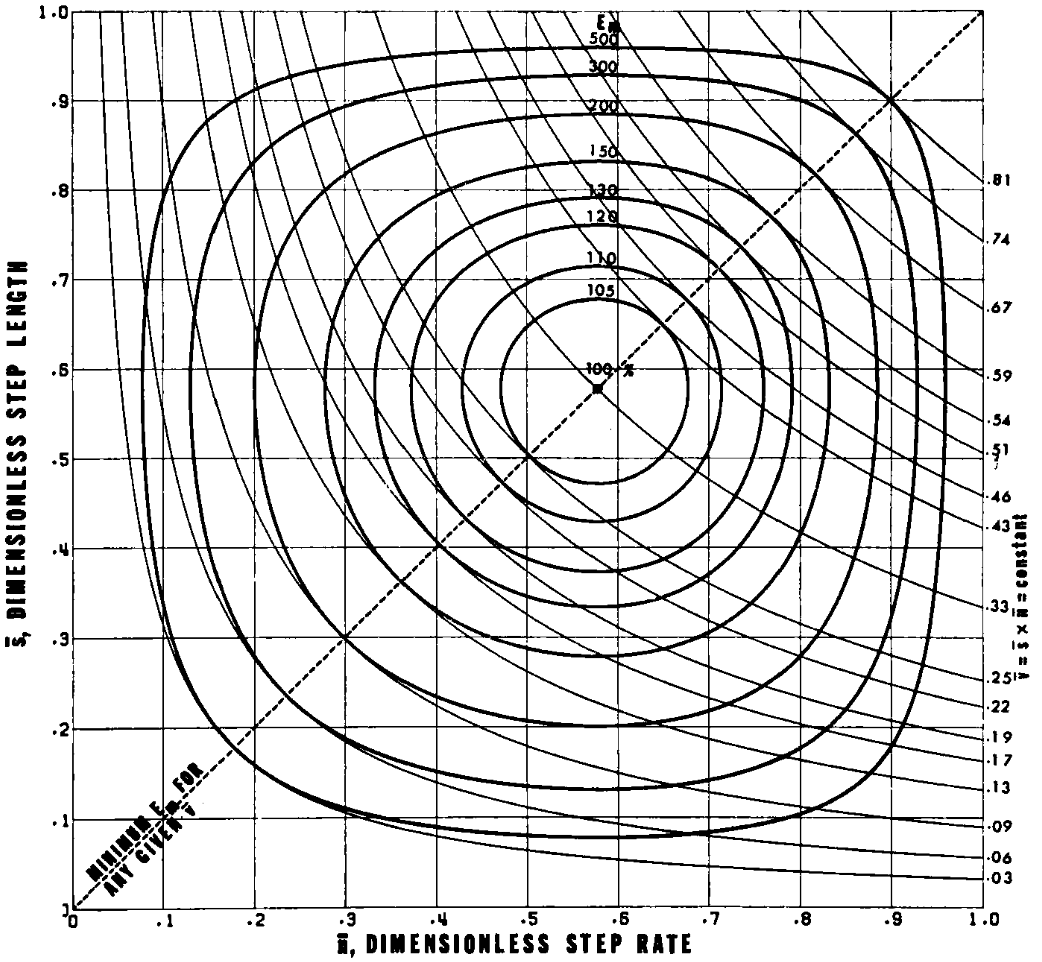

Fig. 6. General model of the dependence of the energy cost per metre in human walking, on the choice of step length and step frequency (here depicted in dimensionless formats). Hyperbolic functions of $\bar{v} = \bar{s} \times \bar{n} =$ constant. Ellipsoid functions denote percentages of energy cost per meter of the minimum (100%). Tangencies of both functions indicate combinations of $\bar{s}$ and $\bar{n}$, optimal with respect to energy cost. Reproduced from Zarrugh, M.Y., F.N. Todd & H.J. Ralston (1974) Berlin, Springer, with permission of the authors and publishers.

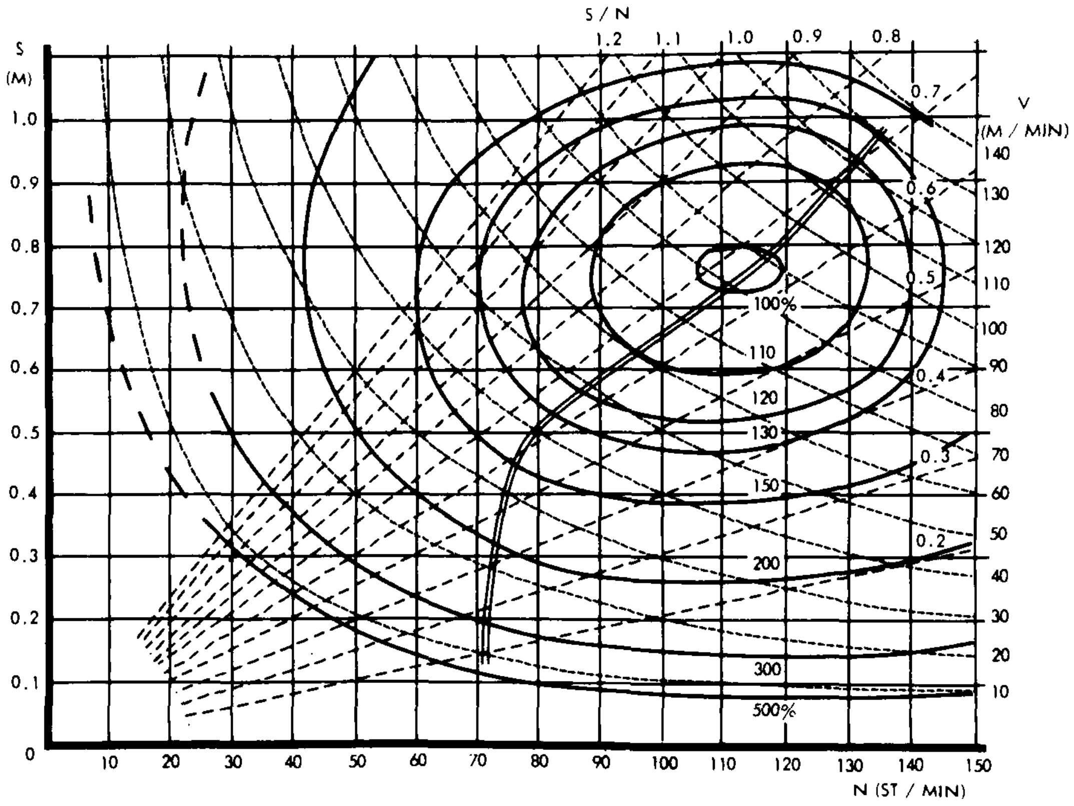

Fig. 7. Graphic representation of energy cost levels (ellipses) expressed as in Fig. 6 in various combinations of *s* and *n* as experimentally determined by Molen, N.H., R.H. Rozendal and W. Boon (1972). Hyperbolae: levels of *v*. Optimal combinations of *s* and *n* are depicted by a double line.

From this model it appears that an energetically optimal speed exists between 4 and 6 km/h at which cost per metre is minimal. At higher and lower speeds of walking the minimal cost is attained by specific combinations of *s* and *n*. Over a range of speeds this ratio remains fairly constant. The difference in this ratio between the sexes was shown to depend, at least for a significant part on differences in leg length (Van Baak, 1979). At very low speeds the ratio is lower too. Otherwise stated: lowering speed at optimal cost levels will be reached by lowering step length while retaining a fairly constant step frequency (Molen et al., 1972b). This latter detail is not taken up in the model of Zarrugh et al. (1974).

Inman et al. (1981) in reviewing this work stated:

> Equation (2) will be valid for all kinds of walking with spontaneously chosen or intended combinations of step frequency and step length. In natural walking, where the subject adopts his own natural cadence for a particular speed it can be shown that equation (2) reduces to:
>
> $$\dot{E}_w = \frac{\dot{E}_o}{(1 - v/v_u)^2} \qquad (3)$$
>
> where v is speed and v_u is upper limit of v, equal to $n_u s_u$ (p. 66).

Equation (3) is illustrated in Fig. 5 (solid line).

$\dot{E}_o$ has an average value of 28 cal/min/kg and $\dot{v}_u$ = 240m/min, as was shown experimentally by Ralston (1958) for $\dot{E}_o$, and as was mathematically deduced by Zarrugh et al. (1974) from data of the authors, of Atzler and Herbst (1928) and of Molen et al. (1972b) with regard to maximal speed. This latter figure seems quite unrealistic for natural walking, since Inman et al. (1981) state 145 m/min as the top natural speed. A speed of 240 m/min will be reached in race walking.

Up to 100 m/min, equations (2) and (3) predict virtually the same values for energy expenditure rates. At speeds of 120 m/min the hyperbolic equation (3) predicts higher expenditures than the quadratic (1). About this speed (of 2 m/sec) the subject has a choice. Heightening of s will change his walking movement into a run, while heightening of n will result in competition walking (race walking). Indeed, Hogberg (1952) found that in the course of a run at between 10 and 12 km/h, increments in speed are predominantly caused by lengthening of s.

Margaria (1963) and Menier & Pugh (1968) found that in speeds over 8 km/h the energy rate in running is lower than that in race walking. Inman et al. (1981) commented that to their surprise the quadratic equation (1) predicted the energy cost of the competition walking Olympic subjects of Menier & Pugh (1968) well within 10%. Data of Margaria (1938) on walking and running fitted the hyperbolic function (3). The latter fact would suggest that running will demand a higher expenditure rate than race walking at the same speed, a suggestion which is quite contrary to naive expectancy. Indeed, Van Baak (1979) confirmed that the energy rate in walking will be slightly higher than that in running in the same subjects at the same speeds at 8 km/h and higher.

As the equations of Van Baak (1979) for walking and running were valid for 4.2 up to 8.6 km/h and 7 - 16 km/h respectively, the range of common speeds was not very extensive. Extrapolation of the quadratic equation of Van Baak for walking renders over-estimations of the expenditure rates of the Olympic competitional walkers analyzed by Menier & Pugh (1968). The subjects of Van Baak (1979) were not trained. The issue of the training status of subjects is of importance in the problem of the predictive power of energy expenditure rate equations to (sub-)maximal speeds. In the transitional zone, in which subjects can choose - irrespective of their special training - for running or walking, cardio-respiratory functioning will not prove to be the constraint. Do the models on energy cost per metre predict better?

The graphic model of Molen at al. (1972b) as well as the mathematical model of Zarrugh et al. (1974) predict that either choice (for increasing exclusively or mainly *s* or *n*) would give an increase of energy cost relative to holding s/n constant. The only difference in what the two models would predict is that this increase would be slightly larger in the case of Zarrugh's model. Cavagna (1978) in a review of the work carried out by himself and his associates, as well as Van Baak (1979) state that the cost per metre in running remains fairly constant over a wide range of speeds, whereas in walking, in the transitional range of speeds, the cost will rise steeply.

The conclusion must be, that models like those of Molen & Zerrugh do not correctly predict the cost per distance traversed in running. Mechanisms reducing the cost in running do however exist. At high speeds, Van Baak (1979) showed that the cost diminished. This could possibly be related to anaerobic work. At intermediate speeds, other mechanisms will, in later sections of this essay, be proposed.

4.3.3. Biomechanical explanation of optimal speed in human walking

In walking, the external positive work of the muscles - as defined by Cavagna (1978) - is quite small. The changes in potential and kinetic energy of the body's centre of mass are 180° out of phase while opposite in direction. It is assumed that muscles convert potential in kinetic energy and vice versa. This mechanism is not possible in running as the kinetic and potential energy changes are nearly in phase.

For walking Cavagna (1978) defined:

$$\text{percent recovery} = \frac{W_f + W_v - W_{ext}}{W_f + W_v} \times 100$$

where W_f is work brought about by the forward speed changes, W_v is work done against gravity (potential and vertical kinetic energetic components) and W_{ext} is the external work. This percentage recovery is maximal at the optimal speed, when the external work as well as the cost per metre is minimal

(Cavagna et al., 1976). Cavagna (1978) offers the following explanation:

> The exchange between gravitational potential energy and kinetic energy take place in the best way at intermediate speeds of walking, leading to a reduction of the external work done per unit distance: this in turn leads to a reduction of the cost of walking per unit distance. Is this the only explanation for the minimum cost of walking at 4 - 5 km/h? As mentioned above, the metabolic energy expenditure in locomotion is not only the result of the mechanical energy changes of the centre of mass (W_{ext}) but also of the kinetic energy changes of the limbs relative to the centre of mass (W_{int}). The efficiency with which the muscles perform mechanical work done ($W_m = W_{ext} + W_{int}$) and the energy expended. This ratio was measured as a function of speed and attained a maximum at intermediate speeds of walking (Cavagna & Kaneko, 1977).

Thus a mechanical and a metabolic factor (greater efficiency of muscular contraction) are introduced by Cavagna (1978).

Winter (1979) proposed a more rigorous definition of efficiency. Internal work, in his definition, comprises all the changes in potential and kinetic energy components. After computing the total energy of the body's centre of mass, he compared this with the sum of the segment energies. He expressed the differences as percentages of the sum of segment energies approach. The differences between various test runs varied from + 3.2 to - 41.7% with a mean of - 16% (± 10.6 SD). These differences do not appear to be dependent on step frequency and consequently on speed of walking. The refinement proposed by Winter (1979) will affect efficiencies as computed by Cavagna & Kaneko (1977), but will not affect their conclusion with respect to the dependency on speed of the efficiency.

Zarrugh (1981) pointed to the fact that the work rate of head, arms and trunk (HAT) has a minimum at the speed and step frequency found in the optimum. However, most explanatory remarks in the literature focus on processes in the lower limbs.

For an explanation of the maximal efficiency at the optimal speed, as well as for the choice of the constant ratio s/h at higher and lower speeds pendulous (Herman et al., 1976) or quasipendulous (Grieve & Gear, 1966) motions of the legs are proposed. In such pendulous movements the interchange of potential and kinetic energy would not call for muscular intervention (Cavagna, 1978) or only for a minimal muscular intervention. Elftman (1966) proposed that the stretch shortening cycle in non-articular and even in bi-articular muscles could save energy. Herman et al. (1976) as well as Pedotti (1977) called attention to the fact that activation of antagonists about most joints does not occur in walking.

Bernstein (1976, p. 106) believed that the functional non-univocality resulted, in the multi-linked pendulous legs, in too many degrees of freedom, which had to be mastered by neuro-muscular activity in a very complicated way. More recent explanations of the energetic optimalization of walking confirm such a belief.

4.3.4. The transition of walking into running

In the range of speeds at which transition of walking into running customarily takes place, cardio-respiratory function is not decisive. This transition has not as yet been studied very extensively.

It was experimentally studied by Grillner et al. (1979). They stated that this transition occurred at about 2m/sec or 8 km/h, suggesting a rather wide range of speeds. Herman et al. (1976) indicated that this transition has to occur at the cycle duration in which stance and swing time (extrapolated) would comprise the whole cycle time (25). According to these authors, cycle frequency will then be 1.7 cycles (strides) per second ± 0.1 (s.d.). This will result in 204 steps/min, seemingly an unrealistic value in reality to be reached in competition walking.

Grillner et al. (1979) indicated stride cycle durations of 0.75 - 0.60 sec for the transitional zone between speeds of 1.8 and 4.0 m/sec (see Fig. 8). Walking with the latter velocity will be competition walking and will reach the maximal speed Zarrug et al. (1974) predicted.

Alexander & Jayes (1978b) stated that at a velocity of 180 m/min the change cannot be postponed except in race walking, while Inman et al. (1980) stated a speed of 145 m/min as the upper limit of natural walking. At 150 m/min or 2.5 m/sec the subjects of Grillner et al. (1979) displayed a stride cycle duration of about 0.75 sec. This would result in 1.33 cycles per second or 159 steps per minute with a step length of 0.94 m and a step length/step frequency ratio of about 0.0060.

In the transitional range of speeds, the flexion and extension phases, as well as the cycle duration (and stride frequency) and consequently the stride length are equal in walking and running at the same speeds. Support phase duration falls in running, the cycle is completed with a floating phase. Support length, an estimate of the distance traversed during support also falls, while the floating phase accounts for a relatively longer distance

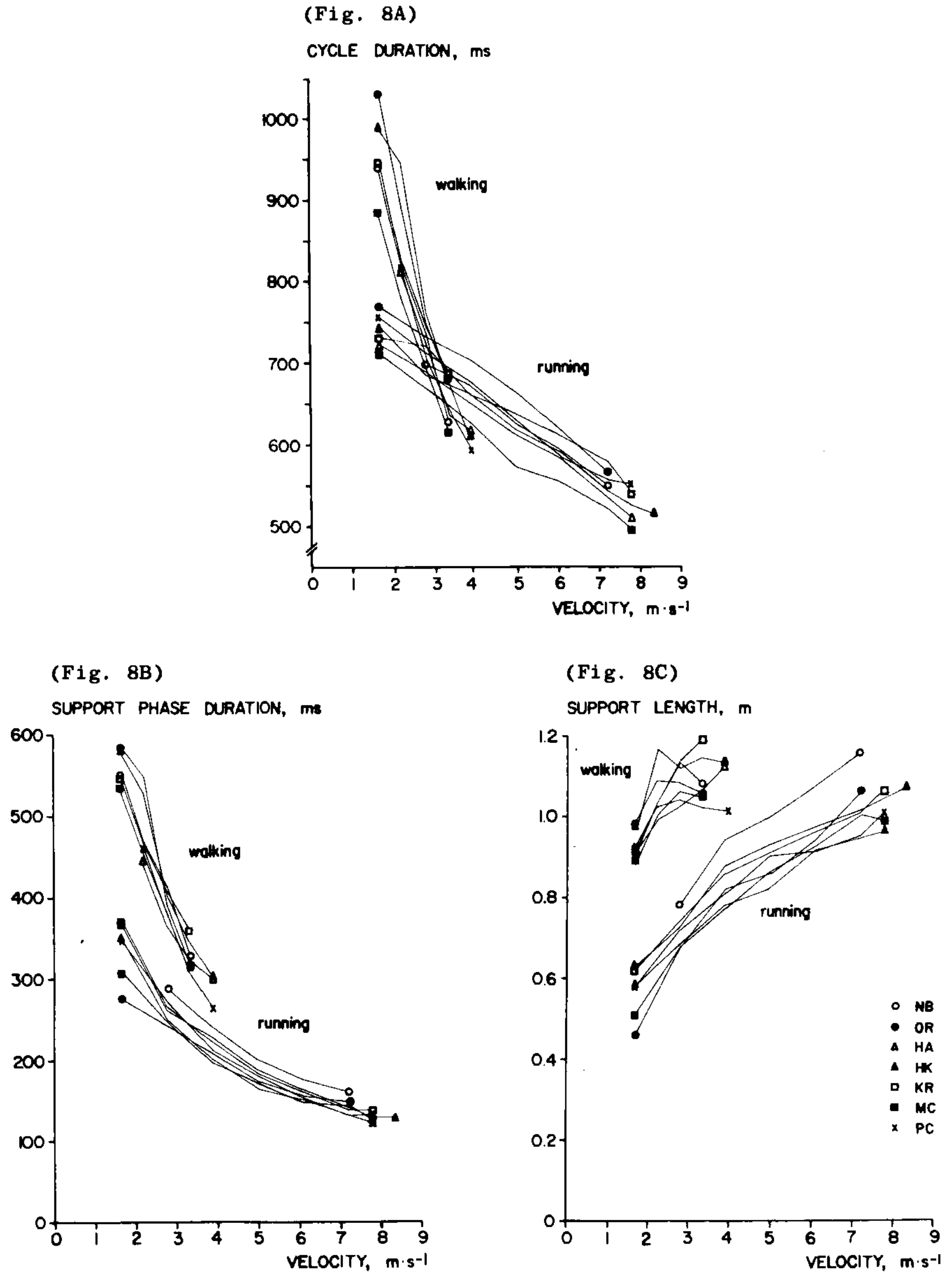

Fig. 8. Cycle duration, support phase duration and support length (derived from support phase duration and $\bar{v}$) versus velocity of walking and running for different subjects. Each data point is the mean of 10 successive measurements. Reproduced from Grillner, S., J. Halbertsma, J. Nillson and A. Thorstensson (1979) Amsterdam, Elsevier Biomedical Press, with permission of the authors and publishers.

traversed. The amplitude of the knee flexion during the flexion phase is smaller in running than in walking at speeds below 2 m/sec. In running a range of speeds from 2m/sec to 8 m/sec the knee flexion amplitude increases strongly. At 8 m/sec the support length will attain the same maximal value of 1.1 m as is reached in race walking.

The cycle duration in race walking falls steeply, while the cycle duration in running does not. The implication is that, indeed, in running, different velocities are mainly reached by alterations in stride length and duration of the floating phase. In running, it is possible to reach shorter flexion and extension phases (Fig. 4) with higher amplitudes of knee flexion. Grillner et al. (1979) commented that lower torques in the hip joint would result from the higher amplitude of knee flexion. In running, the constraint of the maximal support length is also postponed to higher speeds. Lastly, in walking, the legs in the double support phase work against each other doing positive and negative work (Alexander & Jayes, 1978b). This will not be the case in running.

4.3.5. Energy cost and generalized theories on gait

As has been mentioned already, walking is characterized by Cavagna (1978) and by Alexander & Jayes (1978ab, 1980) as a stiff-legged walk. Apart from the two modes of gait - walking and running - numerous others may exist, especially in tetrapods (Hildebrand, 1976), but Alexander & Jayes (1978ab, 1980) maintain that a quadruped can be treated as two bipeds walking one behind the other. Notwithstanding the remarks of Grillner et al. (1979) that the amplitude of the step in quadrupeds is constrained by their very quadrupedy, the contention of Alexander and Jayes is taken for granted here, as well as their contention that only a few modes of gait exist if they are described in terms of time histories of the levels of potential and kinetic energies of the body's centre of mass with respect to the stride cycle.

In this respect human walking and running are different. In walking, the centre of gravity will rise upwards in midstance, when it passes over the rather extended leg, while in double support it will be at its lowest position. This has consequences for the level of potential energy. In one complete cycle (step from left through right mid-stance to left mid-stance) the centre of gravity will complete one sinusoid curve of which the frequency is dependent on mean velocity and the amplitude is dependent on the length of the legs. This is called by Alexander & Jayes (1978ab, 1980) a stiff-legged walk. In human running, the legs in stance phase bend considerably more and it is called a compliant walk. This compliancy is considered to be the cause of the fact that the centre of gravity is lowest at mid-stance and highest in the floating phase, resulting in a complete-stride trajectory which is in opposition to that produced by walking.

In paragraph 4.3.3. it has been mentioned that, in walking, the level of kinetic energy of the body's centre of mass is opposite to that of potential energy. The total level of energy is flattened out. In running, both levels add up (see Fig. 9a, I (a) and IV respectively). From an energetic viewpoint both modes of walking are radically different in their demands on the cardio-respiratory system.

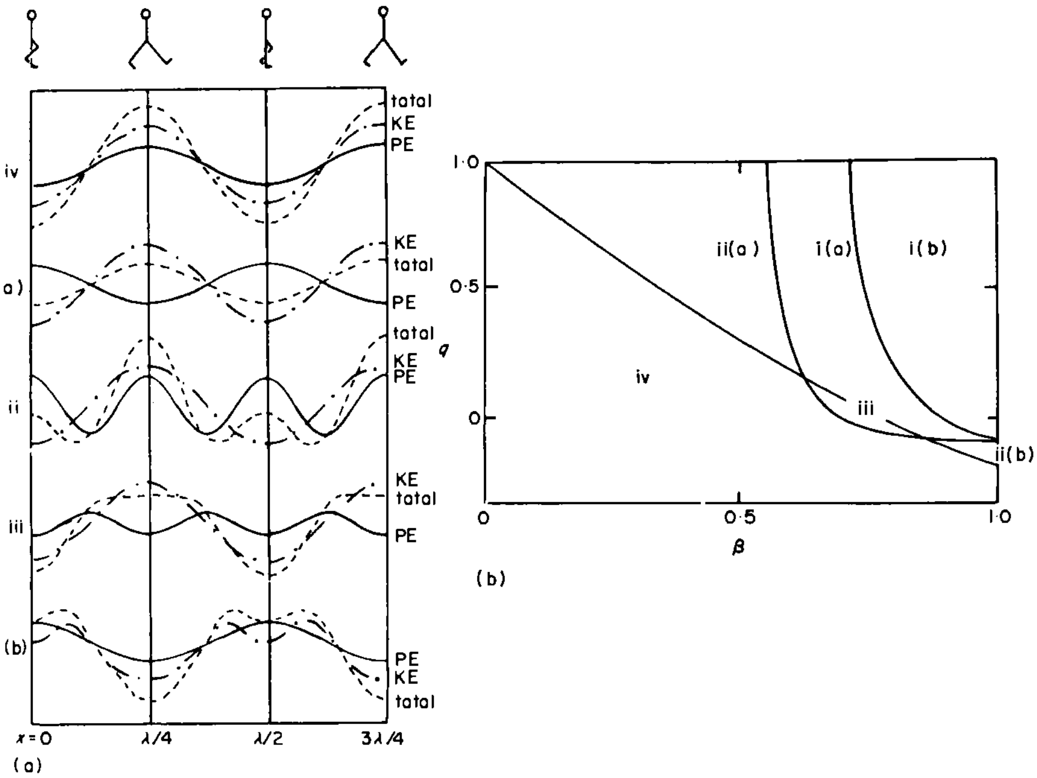

Fig. 9. a) Schematic graphs of total, kinetic and potential energy levels (ordinate) against the stride as distance traversed (abscissae) for a stiff-legged biped. b) A graph of q against β, showing the values which give rise to each of the possibilities illustrated in a). For explanation see text. Reproduced from Alexander, R.M.N. & A.S. Jayes (1978) London, The Zoological Society of London, with permission of the authors and publishers.

Alexander & Jayes (1978ab, 1980) modelled these demands in relation to β - *q* diagrams (see Fig. 9b). By *q* is meant the negative of the ratio of the first and third arguments of the Fourier transformed vertical reaction forces. This ratio is speed-dependent. In the fastest walking mode q would be 1.0 and in running -0.33 (26). The definition of β is: the fraction of the duration of the stride for which any particular foot is on the ground. As has already been mentioned, the extreme value of β in walking is 0.5, lower values resulting in running, without double support. In practice, at β values of about 0.6 walking will transform into running. As the authors assume very low fluctuations of speed within the stride, β approximately equals support length (distance travelled during support) divided by stride length. By this assumption, the model takes into account some of the effects of the major elements of gait (Saunders et al., 1953) e.g. smoothing the curve of kinetic energy, while the fluctuations of kinetic and potential energy in the vertical direction are over-estimated by the assumption of stiff legs. This will result in an over-estimation of the fluctuation of total potential energy in excess of the assessment of Cavagna (1978).

In a further extension of the model Alexander (1980) commented that in the model of Alexander & Jayes (1978) possibly another over-estimation of the total work done had been introduced. They reasoned that in the double support phase the legs were doing work against each other (positive and negative, both demanding energy consumption). It has been argued by Cavagna (cited by Alexander, 1980) that at the beginning of the double support phase the reaction force on the forward leg is in line with the axes of the ankle, knee and hip joints, not exerting any moment with respect to these joints.

> Since its length is (for the time being) constant and no moment is acting about any moving joint in it.

Cavagna concludes this leg is doing no work, positive or negative. Indeed Winter (1979) showed that in human walking the level of energy in both legs is rather low during double support, while lower still in the forward (stance) leg. The energy level in the future swing leg rises steeply, to become maximal in mid-swing.

The above mentioned refinement of the model of Alexander and coworkers must affect the predicted energetic demands and, therefore, the β - q conditions for reaching optimal levels of energetic demands, as it has been shown (Alexander, 1980). However, the supposition of a stiff-legged walk will over-estimate potential energy fluctuations. In the stance phase the knee will be flexed, slightly with low and more clearly with higher speeds. Knee flexion in stance is correlated with mean walking speed as well as with a deeper valley between the two peaks of the vertical foot reaction forces in human walking (van Ingen Schenau, personal communication). Alexander & Jayes (1980) also showed the same dependence of q on mean walking speed. If this dependence is indeed brought about by knee flexion in the stance leg, a further refinement of the model is called for.

Nevertheless the model Alexander and his co-workers proposed appears to be very powerful in predicting the energy demands in various modes of walking of different animals as well as of humans. The model does not predict the demands of running very well. According to the authors it was not developed with this aim. Measurements of human walking on force plates confirmed the assumptions about the speed dependency of q. Humans walking at various speeds appeared to lie in zone Ia of Fig. 9b, while race walking at high values of q and $\beta > 0.5$ will occur in zone IIa. Of this mode of walking, the authors state that it is a mode most animals avoid with the significant exception of athletes in races "in which the rules of the game override considerations of economy and energy" (Alexander & Jayes, 1978a, p. 39).

They calculated the total positive work performed by idealized bipeds per step, $\dot{E}$, defined as 32 $k/mg\lambda^2$, where k is a constant (for human walking k is about 1.7) with the dimension of length, m is body mass, g is gravitational acceleration and λ is stride length. As

$$kF_x = utF_y$$

where u is mean velocity during the stride, $\dot{F}_x$ and F_y are the horizontal and vertical sagittal forces, and t is time, in $\dot{E}$ the potential and kinetic energy were represented. Levels of $\dot{E}$ were calculated for different speeds of walking: 72 m/min, 102 m/min and 163 m/min respectively (see Fig. 10a, 10b and 10c respectively). At 72 m/min the minimal energy level is found at $\beta = 0.6$ and $q = 0.4$. At 102 m/min it is found at nearly the same duty factor and $q = 0.5$, meaning that the vertical reaction force has a deeper valley between the two principal thrusts.

Both optima for these velocities lie within the 110% $\dot{E}_m$ zones of the models of Molen et al. (1972b) and Zarrugh et al. (1974). At the competitional walking speed of 163 m/min the minimal energy requirement would be reached at a duty factor β of 0.6 and q values about 1.0. For this velocity the models of Molen & Zarrugh and associates predict $\dot{E}_m$ at a level of 150% of the absolute optimum. Alexander & Jayes show a value of total positive work per step which is 1.5 times the values at the other two velocities (27).

Alexander (1980) corrected the above mentioned over-estimation of the energetic demand (or power output, as it has been called by him) by calculating

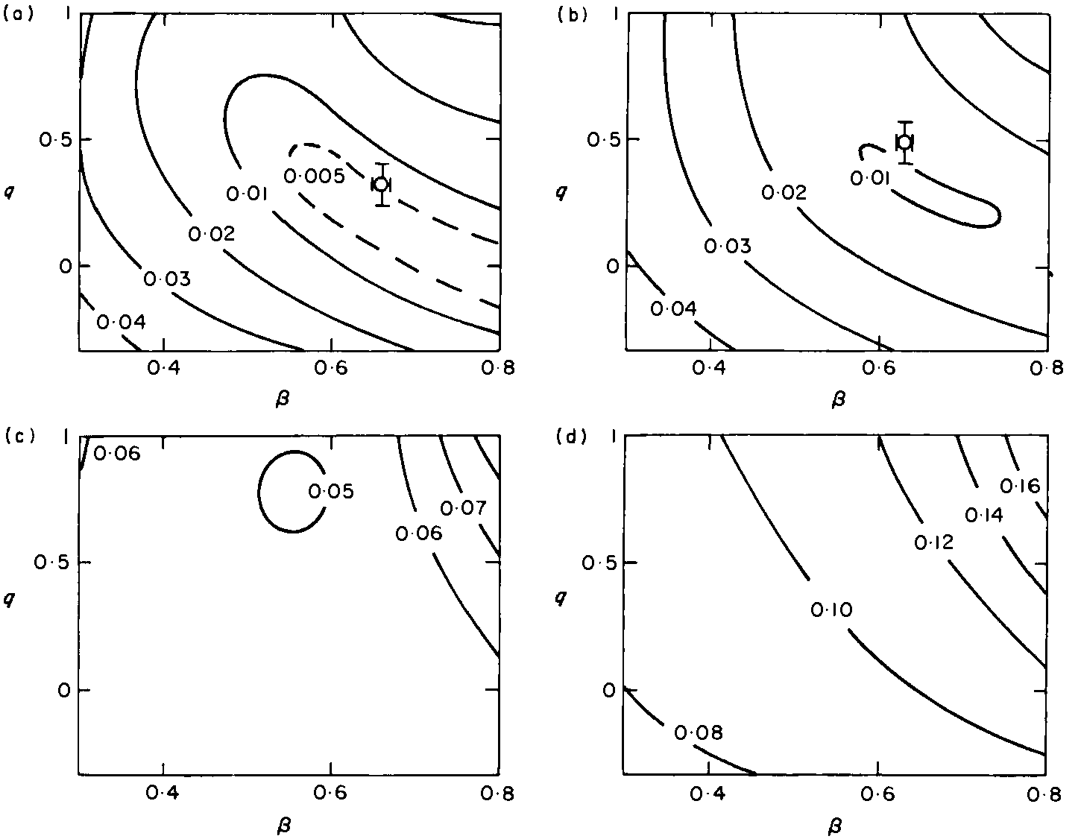

Fig. 10. Total positive work levels performed by the idealized biped, calculated from the model for about a) 72 m/min, b) 102 m/min and c) 163 m/min of human walking. Optima in dashed lines. Reproduced in adapted form, from Alexander, R.McN. and A.S. Jayes (1978) London, The Zoological Society of London, with permission of the authors and publisher.

the work of each leg separately. Graphs on speeds of about 50 m/min, 102 m/min, 204 m/min and 290 m/min were produced, showing energetic optima (see Fig. 11). The correction seems to result in a wider variation of the duty factor β and values for *q* over the range of speeds studied, in accordance with the data of Alexander & Jayes (1980) - with respect to the dependence of *q* on speed - and of Herman et al. (1976) on the dependence of stance, swing and double support duration on speed.

These facts indicate that the predictive power of this model of idealized gait modes of idealized animals seems rather rigorous. But if the finer details are considered, some issues need to be taken up (28).

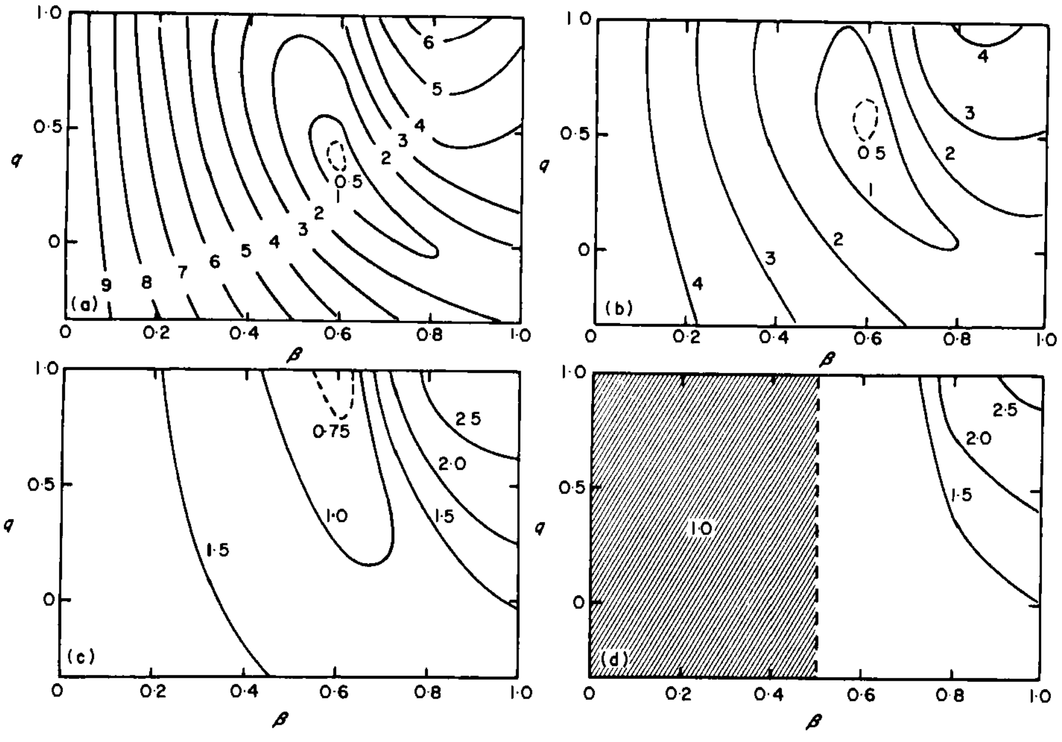

Fig. 11. Graphs of power output, based on energy demands and a physiological efficiency term, and expressed as $U/mg^2/f\eta$), where U is metabolic power, m is body mass, g is gravitational acceleration, f is stride frequency and η is efficiency expressed as $1/\eta$ units of metabolic energy. Note that the values of work levels in these graphs must be multiplied by 16 gk/u^2 to obtain values comparable to those in Fig. 10. In the multiplier mentioned, k is a constant and u is the mean velocity during a stride. For human subjects the mean value of $k = 1.7\ h$, where h is hip height. The velocities are about 50 m/min (a), 102 m/min (b), 204 m/min and 289 m/min. The latter speed will only be attained by running. Reproduced from Alexander, R. McN. (1980), London, The Zoological Society of London, with permission of the author and publisher.

It has already been concluded that the models of Molen & Zarrugh and associates do not predict the cost of running. Assuming the optimizational principles of these models, Inman et al. (1981) calculated that $\dot{E}$ per stride renders quite unrealistic optimal stride frequencies. Stride frequency is related to the relative durations of time factors (swing, stance and double support durations) and thus to the duty factor.

As Inman et al. (1981) showed that the energy rate of competition walkers was predicted very well by their compounded energy rate equation (1), the contention of Alexander and Jayes that competition walkers do not walk in an energetically optimal way needs to be refined.

The model of Alexander and Jayes possibly mis-estimates the required energy per step as the corrections proposed by Winter (1979) are not fully accounted for in the refinement Alexander (1980) proposed. The measurements of Alexander and Jayes (1978ab, 1980) on human subjects showed indeed that parameters of human walking had to be placed in the β-q diagram of Fig. 9b in the border zone between (Ia) and II(a).

Walking in zone II(a) results in variations in the total energy fluctuations depicted in Fig. 9a II. Comparison with Fig. 12 (taken from Winter, 1979) shows some interesting similarities. This calls for further refinement of the model of Alexander and Jayes, for example a two-linked model, with the possibility of knee-flexion during stance as a reaction to the impact at heel contact.

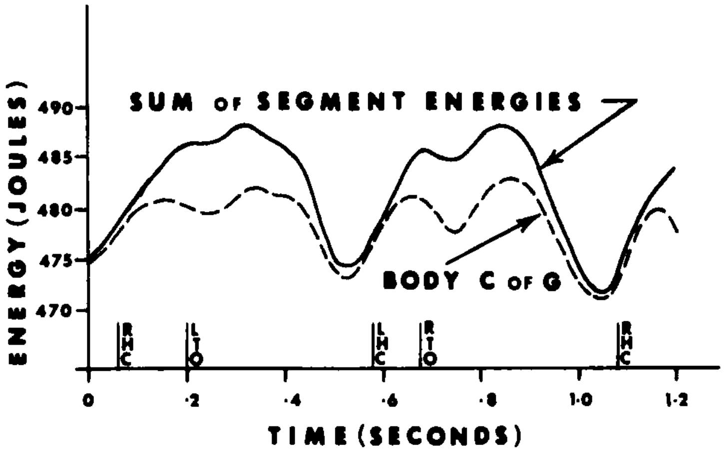

Fig. 12. Comparison of the results of two methods to calculate the total body energy during the walking stride. Dashed line: bodies centre of gravity approach is unable to detect the energy changes from reciprocal movements of different segments. Therefore the solid line resulting of the sum of segment energies approach will be higher during most of the walking cycle. Both approaches show peaks at double support and mid swing which do not resemble the total energy levels of Fig. 10aIa any better than those of Fig. 10a II. Reproduced from Winter, D.A. (1979) Rockville Pike: The American Physiological Society, with permission of the author and the publisher.

The calculations of Alexander and Jayes pertain essentially to energy demands, while the models of Molen and Zarrugh and associates pertain to energy consumption or cost. Cavagna (1978) showed that the efficiency - i.e. the relation between physical demand and physiological cost - varies with speed. Thus a straightforward correlation of the model of energy demand and energy cost at different speeds is not feasible. Future work, in which the energy demand is predicted according to the proposals of Winter (1979) and restating the efficiency terms, will be of value for the solution of these problems. Indeed, Alexander (1980) introduced an efficiency term. He indicated that work theories (energetic cost) and energy theories (energetic demand resulting from fluctuations in the various forms of mechanical energy) will only predict equal power requirements, if the work theories take into account "the possibility that different muscles in the same leg may do positive and negative work simultaneously". He further commented that in human walking the minima or optima might be "moved much by taking into account of leg mass". It must be concluded that future work will clarify these issues. In such work the work theories and energy theories will serve for each other as instruments for validation.

The model of Alexander and Jayes was not meant to and did not predict exactly the energy levels required by running. Alexander (1977) strongly advocated the storage of elastic energy as an energy saving mechanism in running. As in running, as well as in walking at moderate speeds, the cardio-respiratory functioning is not the constraint, studies on the cost per metre will be more instructive than studies of energy rates.

The transition of walking into running is governed by other constraints than the energy rate, it is dependent for example on leg length.

In general, research on human movement has been fostered and has flourished from studies on movements in extreme or constrained conditions such as sport activities. Race walking and the transition of walking into running are tasks many subjects are able to perform optimally. They are recommended for further study in order to reconciliate the work of the authors cited.

The format of this recommendation is felt to be completely Bernsteinian.

4.4. Modelling of gait

In the literature on walking and running, the reader is confronted with mathematical modelling of these processes. Such research is aimed at an understanding of the indeterminacy discussed in Section 3 of this essay as well as other aims. McGhee (1981), one of the leading investigators in this field, stated:

> While such models evidently represent a gross simplification of true biological complexity, many of the fundamental characteristics of human motion are nevertheless explainable by such an approach and there exists a substantial potential for further application of this methodology both in clinical medicine and in biomechanics of sports.

The methodology of modelling is at the present time dictated by the emerging theory of multi-jointed computer-controlled robotic systems. It is believed that from this connection new insights into neural control will be obtained. The models are n-linked rigid bodies with simple hinge joints or joints with three or six degrees of freedom. Torque generators act at each joint, representing muscles (intersegmental moments). Joint forces (intersegmental forces) may be taken into account (Robertson & Winter, 1980). Next to the assumption of rigidity of the body segments, for some calculations additional assumptions have to be made (e.g. for the calculation of power flow fluctuations: pure rotation in joints, constant moment of inertia, absence of friction, and joint forces acting through the centre of rotation, were assumed by Quanbury et al., 1975). Generally, the admissibility of such assumptions and other simplifications, depends on the goals of the investigation in question. In Section 4.3 the power is shown of a very simple model in the hands of an ingenious investigator: Alexander used a quite simple model and by simplifying assumptions about the frequency domain of his measured data, succeeded in formulating a very general theory. By contrast, the work of Hatze cited in Section 3 of this paper is very complicated with respect both to the model proposed (17-linked) as well as to the mathematical procedures involved. His investigation is aimed amongst others at understanding optimalization in locomotion processes of individuals.

Other constraints appear more serious. For example, the complexity of the conventional mathematics applied to rigid body mechanics. Equations especially tailored for use with general computers may solve this problem (McGhee, 1981).

Models are used for motion analysis problems or conversely for motion simulation problems. The various methods (free body analysis), generalized coordinates and free body equations with hard constraints) were discussed recently by McGhee (1981). Applications by the same author to robot locomotion are to be found in his article in Herman et al. (1976). A difficulty peculiar to all modelling - and already mentioned by Bernstein (1967, p. 8 and subsequent pages) - is the introduction of body parameters into the model. Work on this problem is still needed and is in progress, as shown elsewhere in this volume (Woltring, this volume).

Most work on gait appears to have been done on five to seven-linked models. The interested reader will find numerous reports in the Journal of Biomechanics as well as in Biofysika (Biophysics). The question of the choice of the number of links will also depend on the goal of the study. A five link-model, excluding the feet, but including HAT, will possibly be sufficient for investigators interested in the general characteristics of energy states of the body's centre of gravity. But, if a more detailed account of this history is wanted, provision for the feet has to be made (Winter, 1979). If interest lies in the exact measurement of step length, a massless pelvic segment between the three-linked legs is needed (McGhee, 1981). HAT, and one-linked arms are minimum requirements if interest lies in the time histories of kinetic rotational and translational histories. This will result in a nine- or ten-linked model.

Most of these models are two dimensional, medio-lateral movements, forces and moments are assumed or proven to be of minor importance. It seems to the present author that the validity of such simplifications requires to be substantiated from more advanced work. This will be especially true for applications on abnormal gaits in clinical practice. In future the work of Aleshinsky and Zatsiorsky (1978), Orin et al. (1979) and others on three-dimensional models will prove to be of utmost importance, as will the model of Hatze (1977ab, 1978, 1980b and 1981b) mentioned in Section 3.

4.5. Clinical applications

A large number of investigations have been set up to evaluate such movements as the gait of patients. Such evaluations should help in diagnosis as well as in documentation of progress in therapy. It has been recommended that gait or human movement laboratories would fulfil a role in rehabilitation clinics and other centres, not unlike the role of laboratories in other branches of medicine. In retrospect, it is contended by the present author, that much of this work has been of tremendous value in so far as it was carried out primarily with the goal of understanding human movement. The monograph of Inman et al. (1980) will stand as a hallmark and proof of this contention.

A critical remark will be made here. Some of the complexly instrumented laboratories were developed with the intention of doing only clinical work. When the laboratory has been completed and instrumentation is in working order, one or more publications on the methods in use will be produced. These papers often illustrate a difference in one or more of the parameters measured between normal subjects and patients. The first group of subjects sometimes is hastily put together, while the latter group is inhomogeneous. In this way the "proven" difference is equivocal.

When the measured parameter cannot be interpreted in terms of functioning of the moving subject, such an approach will lead nowhere. Most measured parameters are interrelated in a complex way (e.g. see the discussion on double support duration in Section 4.2 of this paper) or are at least velocity-dependent. Velocity-dependent parameters will appear to be abnormal in most patients, while they are moving with (extreme) low velocities (see, for example, Jacobs et al. 1972). Again the work of Herman et al. (1976) has to be cited, as they tried to sample data of normal subjects walking at extremely low speeds - as seen in pathological states. Such work is regrettably scarce. If normative data are not available about the parameter used, detailed research on dependence of the parameter in question from for example velocity, step length, body characteristics etc., is indicated. This is a costly and cumbersome procedure, but it is a necessary one. Contini's (Contini et al., 1965, p. 430) remarks still hold:

> We still do not know enough about locomotion, particularly pathological locomotion. For the present, we suggest that, in providing an amputee with a prosthesis, his gait pattern should approach the "normal". We are not really sure, however, that the normal walking pattern for an amputee is the "normal" of the non-amputee which we try to achieve. We certainly do not know the normal walking patterns for those suffering from other motor disabilities. We have made some progress, but there is much research left for those who will follow.

5. CONCLUDING REMARKS

In Bernstein's days it was not possible to acquire high resolution displacement time data, nor did he have a system for direct measurement of forces or accelerations at his disposal. The facts upon which Bernstein formulated his theory were insufficient: quantification noise of low resolution displacement data, differentiated twice, leads to very misleading results. Nevertheless his theory has been very successful and is still of major importance today.

Resolution is still a problem. Filtering techniques are recommended to cut-off unavoidable noise at frequencies higher than those in which interest lies. In reporting, the characteristics of the filtering technique ought to be mentioned. Filtering and smoothing techniques to lessen the unwanted effects of processing of noise within the frequencies of interest are recommended. The choise of these techniques, as well as of the sampling frequency, can be rationalized on a cost/profit basis.

Kinesiological EMG has been used infrequently for the goals Bernstein would have recommended: description and analysis of the neuronal regulation of muscluar activity in walking. When the work of Grillner and associates is extended to human walking and running a major breakthrough can be expected.

The use of motordriven treadmills in the study of walking and running is, from a mechanical point of view, a valid approach, provided the velocity of the band is not influenced by the varying load. However, the possibility remains that e.g. visual information will influence some details of walking and running. Research in the influence of sensory information on the kinematics of these movements is recommended.

Generalisation from one provisionally defined class of movement to another needs caution. This is the more so, as movements are not carried out under conditions of a one to one relationship between neuronal activity and effector processes. This indeterminacy or non-univocality is felt to be the core of Bernstein's theory. Many sensory systems will be actively used by the organism in order to keep itself informed about the ongoing disequilibrium with the environment, on which it acts rather than reacts.

Experimental designs in which the information is diminished by manipulation to only one or some of the sensory systems, will render simple models. These models will render successful analyses, leading to linear system control theories. On the other hand such theories will lack a firm basis for generalization while not based on ecological valid experiments. Alternatively, experiments designed to include the functioning of more sensory systems and/or complex movements, are endangered by ambiguity of explanation, due to functional non-univocality.

The mechanical sources of indeterminacy in the relationship between the organism and its effector system on the one hand and the environment on the other, are nowadays described with the help of complex models, including muscle control, for optimalized movements. Teleologically speaking the CNS "knows" the effects of the other sources of indeterminacy quite well and acts on the basis of this "knowledge". Its function is optimalized in this sense.

Bernstein's Russian colleagues proposed "synergies" and the "principle of least interaction" as working hypotheses for the explanation of non-univocality in the resolution of motor problems. It is concluded that the concept of synergy is in conflict with the Bernsteinean view on indeterminacy in a lot of classes of movements. Synergies are only to be postulated if they are adaptive in character. The principle of least interaction may be active in automated movements, like walking; in which generators at spinal levels are influenced by relatively simple descending signals. Within the effector processes, adaptive flexor and extensor synergies, known from pathology, may act. These may be concordant with synergies postulated for the maintenance of posture.

For other classes of movement, other controlling mechanisms from higher levels are postulated by neurophysiologists.

Human gait has been studied very extensively. Nevertheless, in simple descriptive and analytical studies problems existed which are nowadays capable of solution. The quite customary procedure of describing phenomena like EMG in the time course of normalized strides leads to ambiguous interpretations as the double support time varies exponentially and the stance and swing time vary directly (but differently) with speed.

Walking is optimalized energetically in terms of cost per distance traversed. In running, the cost per metre will be nearly constant for a large range of speeds and this cost will be lower in running than in competition walking.

The optimal cost at speeds of about 5 km/h in walking is tentatively explained on various levels: e.g. spatio-temporal factors, the stretch shortening cycle, the use of specialized muscle fibres. In running, storage of elastic energy is said to be another energy-saving mechanism.

However, human running, and the transition from walking into running has not been studied very well. As the study of human movement under various constraints (e.g. sport or optimalized movements) has been shown to be a successful approach this kind of study is to be recommended.

General theories on idealized biped locomotion leading to formulation of the energetic demands are to be reconciliated with the experimentally defined energetic cost. This reconciliation appears to be quite possible if the models of idealized animals become more complex: two- or n-linked models and/or refinement of the demands from kinetic energy are needed.

In general, modelling of gait has been rather successful especially if a sufficient number of links is incorporated into the model. Such modelling is not limited to walking, but pertains to all kinds of specialized movements, including those from sport.

Clinical application will benefit from studies of the factors which influence the variation of the generalized or idealized movements. In human walking, many parameters are speed dependent. Unless data on very low speeds have been gathered from normal subjects for comparison, measurement of a parameter of unknown significance is quite useless, as most patients walk with abnormally slow speeds. Parameters to be used to validate pathology, have to be explainable in terms of their functional significance.

Notes

(1) Recent reviews giving a more complete overview of the various parts of the programme Bernstein proposed are e.g. Alexander & Goldspink (1977), Gelfand et al. (1971), Getz (1980), Grillner (1975, 1979, 1981), Gydikov et al. (1973), Granit & Pompeiano (1979), Herman et al. (1976), Homma (1976), Inman et al. (1981), Schmitt & Worden (1974), Szentagothai & Arbib (1974) and Talbot & Humphrey (1979).

(2) It remains to the present author unclear which shutter frequency was used by Bernstein in his studies of walking and running. Though being aware of the importance of this point Bernstein nevertheless does not mention the exact data in the discussion of his results in Chapter III of his 1967 book. One of his co-workers, in a study of physiological tremor stated that a frequency of AD conversion of 50 Hz was adequate. In a footnote it was said that later on this rate was increased to 125 Hz (Gurfinkel et al., 1971).

(3) As quantification errors are not random, a signal sampled with high shutter frequency after having been differentiated twice will show rather regular "accelerations".

(4) In walking with a stepcycle of about 0.6 sec (moderate speeds), the basic frequency will be 1.7 Hz. This would result in a highest frequency of importance of about 10 Hz.

(5) For a recent study using a calcium mediated activation model, coupled to a force generating model, simulated and experimentally confirmed, consult Wallinga-de Jonge et al. (1980, 1981).

(6) The Hill model states the force-velocity dependence (Hill, 1970).

(7) Lombards paradox states that two antagonistic bi-articular muscles, contracting at the same time will extend two joints, like the hip and knee joint, providing the moment arms on the extension sides surpass the moment arms on the flexion sides (Rasch & Burke, 1967).

(8) For a general discussion on EMG, the reader should consult Basmajian (1978). For an account of various methodological points of the use of EMG in kinesiology, especially quantification of the signal, reading of Grieve (1975) is recommended. Recently a report of the 'International Society for Electrophysiological Kinesiology Ad Hoc Committee on Units, Terms and Standards in the Reporting of EMG Research' became available. It may be requested from Dr. R. Lehr, School of Medicine, Southern Illinois University, Carbondale, Ill. 62901, U.S.A. Some of the recommendations of this report and further requirements of design and reporting EMG research in human kinesiology were discussed by Rozendal & Meijer (1982).

(9) Or, in Bernstein's words: formulating the regular in distinction from the random (see p.171).

(10) Modern views on Bernstein's thoughts (Reed, 1982) hold that as a consequence of this, descending (effector) pathways should not be called motoric, as they also control afferentiation. The latter should, in a similar manner, not be called "sensory". With respect to the first mentioned concept reference should be made to Wall (1967, 1978): descending pathways serving in gate control. This concept was more or less foreseen by Bernstein (1967, p. 219) in his remarks pertaining to effector pulses arriving but not passing through, as well as to "preparatory organization of the motor periphery in order to guarantee

optimal selection of conductivity".

Reed (1982) calls attention to the -explicitly speculative - paper of Wall (1970) in which it is suggested that the role of some movements is to gather information. The present author has for a long time cherished the thought that postural sway serves as a method of gaining information about one's own posture. But the possible existence of such mechanisms does not call for the abandoning of the concept of sensory input in analytical studies. Nor do Bernstein's views, as expressed in Chapter III of the 1967 book, corroborate Reed's (1982) view on this latter point. The α - γ co-activation, influencing the sensory input from the muscle spindles does not render the whole input non-sensory in character.

(11) These are not restricted to the muscle spindles (Houk, 1976, 1979) and possibly not to these and the Golgi tendo organs (see Section 2.4 of this essay) see further Stein (1981, p. 205) for some arguments against a one-parameter sensory input in the control of movement. For an example of a one-parameter approach, see Van Dijk (1978).

(12) Bernstein's (p. 219) remarks on tonus as a condition of readiness are to be understood as generalizations of this special case.

(13) Reed (1982) argues further that action systems, not motor systems would be the elements for such a study. This discussion will be left to other reactors with more competence in this area.

(14) Elner et al. (1972, 1976); Litvintsev (1973); Bonnet et al. (1976); Gurfinkel (1973); Gurfinkel et al. (1974, 1976, 1979abcd). This is not to say that in other movements or other experimental designs stretch-reflex mechanisms do not play a role (Gottlieb et al., 1970).

(15) E.g. the visual system in arm-hand movements (Herman et al., 1981); the visual system in standing (Gurfinkel et al., 1976), as well as various kineasthetic sources in standing (Gurfinkel et al., 1979abcd); the visual system in standing (Amblard & Cremieux, 1976; Berthoz et al., 1979; Galyan, 1978; Galoyan et al., 1976; Zikmund & Balla, 1973); the visual, vestibular and proprioceptive systems in standing (Kapteyn, 1973; Bles, 1979); the proprioceptive system of the trunk in standing (Gelfand et al., 1971).

(16) The quoted definition of synergies by Gelfand et al. (1971) is conflicting with the proposition of Bernstein (1967, p. 179) on the reactivity of live movements.

(17) The model as yet has to be extended for three-dimensional analysis (Hatze, 1981b).

(18) It is felt by the present author that between both World Wars in the United Kingdom mainly phisiologists were interested in the locomotor system. What would we do without the work of Hill, revisited by himself in 1970? (Hill, 1970).

(19) See also Van Baak (1979).

(20) E.g. Herman (1976) 0.9 cycles (strides)/sec.

(21) E.g. in a cycle of 0.7 sec, double support times of 50 ms or less were reported (Murray et al., 1966), suggesting that an accuracy of the measurement of 10 ms or less is desirable.

(22) It is to be noted that Herman et al. (1976) asked their subjects to walk at very slow speeds too, in conformity with speeds only seen in gross pathology, resulting in a variation of the double support time of 0.5 to 0.1.

(23) Onyshko & Winter (1980) introduced a constraint in their model in order to draw conclusions about the joint forces and moments in the double support phase, without measuring foot reaction forces.

(24) The weighting was according to numbers of subjects of investigations of Bobbert, 1960; Corcoran & Brengelman, 1970; Cotes & Meade, 1960; Molen & Rozendal, 1967 and Zarrugh et al., 1974, totalling up to 57 adult normal males and 29 normal adult females.

(25) This would theoretically occur at 0.5 cycle time (stride time) and 0.25 stance time and result in a duty factor β of 0.5. The duty factor β is defined by McGhee (1968), as quoted by Alexander & Jayes (1978a). It is the ratio of stance time to cycle time.

(26) Only 5 arguments of the Fourier series were studied for the vertical and only three for the fore-aft forces.

(27) Note that comparison between cost per distance and rate per stride at the same speed in walking and running is admissible: in Fig. 8a is shown that at the same speeds in walking and running the cycle durations are quite similar, meaning that stride lengths are also quite similar.

(28) First, some details pertaining to Fig. 10 are taken up. The speed of 72 m/min would result from 105 steps of 68 cm length. From 105 steps or 52.5 cycles/min a cycle time of 1.12 sec and a stance time of 0.73 sec can be deduced (Fig. 3) resulting in $\beta = 0.65$. The speed of 102 m/min would result from 122 steps of 82 cm length and $\beta = 0.66$.

$\dot{E}$ per meter would result in 1.5 times the optimum of Alexander & Jayes at a speed of 72 m/min, and in 1.22 times this optimum at a speed of 102 m/min. In the model of Molen et al. (1972) both speeds would result in $\dot{E}$ per meter of 110% of the absolute optimum.

The speed of 163 m/min would be attained with about 150 steps of 110 cm length per minute. $\dot{E}$ per meter would be (0.9 x 1.5=) 1.35 times the optimal values at the other two speeds. Indeed, 1.35 x 110% is about 150% the model of Molen et al. (1972) indicated.

In the hyperbolic equation (3) 57 cal/kg/min is predicted for the lowest speed, 77.7 cal/kg/min for the speed of 102 m/min and 310 cal/kg/min for the race walk. This would result in 0.19 cal/kg/step, 0.75 cal/kg/step and about 2 cal/kg/step respectively. Per metre 0.8, 0.7 and about 2 cal/kag respectively are predicted, 0.8 being the absolute optimum (see Fig. 5, upper dashed line). The quadratic equation predicts, for 163 m/min, an energy rate of 164 cal/kg/min, resulting in about 1.1 cal/kg/step and in about 1 cal/kg/m. This latter value appears too low, while the earlier value, predicted from the hyperbolic equation (3), namely 2 cal/kg/m, appears too high, if the models of Alexander & Jayes (1978b) and of Molen et al. (1972) are invoked.

A further distinction is the above mentioned difference in the energy rates per step predicted from the equations (2) and (3) for the speeds of 72 and 102 m/min and the calculated energy fluctuations in the model of Alexander & Jayes (1978). The latter predicts similar optima per step at both speeds, as does the model of Molen et al. (1972) for cost per metre, while Inman et al. (1981) predict different expenditures per step for each of these speeds.

With respect to the data of Fig. 11c and 11b the quadratic equation (1) predicts 44 cal/kg/min for 102 m/min and 240 cal/kg/min for 204 m/min. The latter is about five times higher than the first. The hyperbolic equation predicts for 204 m/min 200 cal/kg/min, which is less than three times the predicted value for 102 m/min: 77.7 cal/kg/min.

The conclusion must be that further experimental studies have to be carried out on the energy expenditure in race walking and running, on the hypothesis that an energetic optimum for each speed will be found to lie along a hyperbola going through the zone 0.01 in Fig. 11b and the zone 0.05 in Fig. 11b. This would imply that even competition-walkers will optimalize their mode of walking on the basis of energetic demands as well as cardio-respiratory constraints. They will do this by training, of course.

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SECTION 4

Chapter IV **SOME EMERGENT PROBLEMS OF THE REGULATION OF MOTOR ACTS**
N. Bernstein

Chapter IVa AN ECOLOGICAL APPROACH TO PERCEPTION AND ACTION
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CHAPTER IV

SOME EMERGENT PROBLEMS OF THE REGULATION OF MOTOR ACTS

N. Bernstein

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The period of struggle towards the recognition of the biological importance, the reality and the generality of the principle of cyclical regulation of life processes is now behind us. As often happens during periods of scientific quarrying, glimpses of the new ideas and general forecasts may now be noted in the work of the older classical physiologists (Bell, Sechenov, Jackson), and of those nearer to our own epoch (Ukhtomskii, Wagner). The present author gave an account of the application of these concepts to the problem of the co-ordination of movements in a report in 1929 in which this principle was given a general mechanical foundation (15), and in 1935 he reduced it in general terms to differential equations (see Chapter II). The debate, in these initial stages, was conducted sharply, but now seems to be over. Now we are living through a period of the extensive development of this principle and of its elaboration and cultivation in heuristic models and in automatic processes in the most varied fields of practical application. Clearly, now is the time to look forward, to attempt to formulate some of the new problems which occur in this area, and to direct attention to some insufficiently illuminated and evaluated aspects of the circular principle of control. The most suitable data to be discussed are from the area of the physiology of motor acts.

I

The motor activity of organisms is of enormous biological significance - it is practically the only way in which the organism not only interacts with the surrounding environment, but also actively operates on this environment, altering it with respect to particular results. The theoretical lag observed in this area in comparison with the physiology of receptors or of internal processes is therefore very puzzling. So is the neglect which is obvious from the sections on movement in physiological textbooks which vary up to now from zero to some few pages. It is necessary to indicate briefly how great is the resulting loss sustained by physiology as a whole.

If movements are classified from the point of view of their biological significance to the organism making them, it is clear that on the first level of significance we have acts which solve one or another particular *motor problem* which the organism encounters. Leaving the analysis of this expression aside for the moment, we may note that *meaningful* problems which can be solved by motor action arise, as a rule, out of the external environment. This at once draws a distinction between meaningful actions and the range of independent movements which are not concerned with overcoming external forces, or a large number of momentary single-phased movements such as withdrawal of the paws. It is already apparent from this that laboratory physiology which, with very minor exceptions excludes from the experimental chamber all movements except reflexes to pain, defensive reflexes (primarily the scratching reflex*), diminishes its resources of explanation not only quantatively but also qualitatively, and as we now see not only in relation to narrowly motor problems.

First, if in the case of relatively independent movements (pointing, drawing lines in the air, etc.), some knowledge of mechanics and biomechanics is necessary in order to demonstrate the incontrovertible necessity for circular sensory regulation, for acts of movement which are concerned with the surmounting of *external forces* this necessity is evident from the outset. In all sorts of motor problems in locomotion (especially complicated ones - running over uneven ground, jumping on to an elevation, swimming through waves, and many other examples), in fighting with other animals, in industrial processes carried on by human beings, the overcoming of *independent* forces is always a prerequisite for solution. These forces are consequently not foreseeable, and because of this they cannot be overcome by any sort of stereotyped movements directed solely from within. A careless consideration of these processes of active interaction with independent surroundings (apparently the limitation of explanation to "atomic movements" alone seemed quite justified to the atomist-mechanists of the last century, who regarded a whole as the sum of its part and nothing more) led to the situation that the principles of sensory-feedback

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*The reaction designated as orientation reflex has only been included in this category for terminological reasons, and as far as we know has never been employed for the direct investigation of reflex action.

connections, which could have been examined and established for the case of motor control a hundred years ago, remained obscure until the present.

For many years the concept of the open reflex *arc* has been a universal guiding principle in physiology. We cannot exclude the possibility that in such elementary processes as the salivation reflex, or in such abrupt actions of secondary biological significance as withdrawal from a painful stimulus, etc., the arc really does not form *a reflex ring* characteristic for the scheme of the control process, partly because of the short duration of the act, and partly because of its extreme simplicity. However, it is both possible and probable that because of these characteristics of brief duration and extreme simplicity the cyclical nature even of these processes has so far escaped attention and record. (For the process of salivation this is already almost beyond doubt). However this may be, it seems very likely that a reflex visualized on the model of an *arc* is only a rudiment, or a very special case, of a physiological reaction*.

It remains to describe yet another loss which physiology has sustained from the replacement of real acts of movement solving objective problems as they arise, by fragments of movements of an almost artificial character. This particular loss has so far not been sufficiently emphasized; it has not merely impoverished our knowledge in the area of *receptor* physiology, but contains the root of important methodological errors.

It must be noted that in the detection of *releaser signals* which are incorporated in the operation of one or another reflex arc - the only role in which they were studied by physiologists of classical schools - the afferent systems, at least in the higher animals and in man, function in essentially and qualitatively different ways than in the role of tracking and corrective mechanisms which they play in the fulfilment of motor acts. This difference becomes clear if, once again considering the matter from the point of view of biological significance, we turn our attention to those properties in the receptor organs which had to be developed by natural selection. In order to act as a *signal-releasing system* the receptor must necessarily have a high degree of sensitivity, that is thresholds which are as low as possible, both for the detection of the absolute strength of a signal, and for discrimination between signals. The most significant

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* I would not even exclude the possibility that the first reflex in the form of an arc appeared in the world in the same place as the first "elementary sensation" - both of them in a laboratory experiment, or in the minds of experimenters.

biologically are the *distance receptors*, smell, hearing (even of ultra-high frequencies) and vision in various orders of rank for various types of animals. In order to discriminate meaningful signals further from the chaotic background of "noise" it is necessary to develop an entirely analytic or *analysational* function for the corresponding apparatus in the central nervous system. (It is quite natural that I.P. Pavlov, in greatly extending our knowledge of the *signal-releasing* functions of the receptors, should have termed them *analysers* only in his latest years complementing this with the term synthesis). Finally, the most important mechanism in this signal-releasing role, as Sechenov hypothesized in advance, has been clearly revealed experimentally by investigators abroad (who were stimulated by the practical problems of observation in war) to be the processes of active systematic *search* ("scanning") or examination by telereceptors of every diapason of function. This is an entirely active process, and the effector side of the organism is here employed in a manner completely analogous to that which is later explained to underly afferentation in the control of movements. I must, however, note at once that this last process has nothing in common with the processes of the evocation of organized effector acts for the integral active perception of objects in the external world, as we shall discuss later.

Once a meaningful motor act has been set in motion by any given sensory signal, the demands of biological expediency on the afferent function are quite different. It was these demands which led to the formation of the processes of annular *sensory correction* during phylogenesis. We may consider the formulation of the motor problem, and the perception of the object in the external world with which it is concerned as having their necessary prerequisites in *maximally full and objective* perception both of the object and of each successive phase and detail of the corresponding movement which is directed towards the solution of the particular problem. The first attribute of the receptors which may be described in the context of this role is that of completeness, or synthesis. This has been adequately described and studied by both psychologists and neuro-physiologists under the headings of sensory synthesis or the sensory fields. Among these fields we have, for instance, the body scheme, the spatio-motor field, the synthesis of objective or qualitative (topological) space and so on. The author has attempted to give a thorough account of the roles of these fields in a book on the structure of movements (23). It is here only necessary to recall that: (1) in this functional area the synthesization of the operation of the receptors is not manifest theoretically (as was the case above) but can be directly followed in the norm and in pathology and (2) for each of such sensory syntheses effecting the sequential direction of motor acts, the structural scheme of connections between the activities of various proprio-tango- and telereceptors has its own specific quantitative and qualitative properties.

In this case the confluence of elementary information flowing from the peripheral receptors to the central synthesizing apparatus is so deep and stable that it is in general practically impossible to distinguish by introspection. All, or almost all, types of receptors take part in the activity described above (possibly with the exception of taste alone), but to significantly different degrees. On the first level there is the general system of the proprioceptors in the narrow sense; further there is the co-function of all tango- and telereceptors, organized on the basis of all previous practical attempts in order to fulfil the role of functional proprioception. Other purely physiological aspects of the operation of the receptors in the circular function - parameters of adaptation, "comparison thresholds", periodicity of function, etc. - will be discussed in the second part of this chapter.

The second determining sign of the receptors as participants in the annular co-ordinational process - *objectivity* - is of such great significance that it will be necessary to deal with it more thoroughly.

It has been possible to analyse receptors in terms of an open arc only in their role as signals (releasers or inhibitors). It was this role which led to the use of the term *signal system* for the entire complex of organs of perception in the central nervous system. Where perceptual organs operate in this way, objective accurate information is not, in general, required of afferent function.

The reflex system will operate adequately if to each answering effector there is attached its own (a) constant and (b) correctly recognized releaser signal-code. The composition of this code may be entirely arbitrary, without introducing interference into the functional system if these two conditions alone are observed. This indifference on the part of the central nervous system to the meaningful content of a signal is not a strange, purely biological phenomenon but part of the very nature of the process of signal apprehension. This is shown by the conduction of signals, with perfect control of the necessary connecting and switching in remote-control apparatus. It is possible to construct two identical automata - aircraftprojectiles, motor boats, etc. - with identical motors, wheels, schemes and radio relays, etc., and to organize the situation without any structural differences so that in answer to the radio codes A, B, C, D, etc. the first system responds with the reactions *1*, *2*, *3*, *4*, and the second with the reactions *4*, *2*, *1*, *3*, or whatever is wanted.

The characteristics of operation of the receptor system are quite different, in respect of its control-co-ordination functioning, in the process of solution of a motor problem. Here the degree of *objective reality* of the information is a decisive prerequisite for the success or failure of the action to be performed. During the entire course of phylogenesis of living organisms natural selection inexorably sifted out those individuals in which the receptors controlling motor activity operated like a curved mirror. Over the course of ontogenesis each encounter of a particular individual with the surrounding environment, with conditions requiring the solution of a motor problem, results in a development (sometimes a very valuable one), in its nervous system of increasingly reliable and accurate *objective representations* of the external world, both in terms of the perception and comprehension involved in meeting the situation, and in terms of projecting and controlling the realization of the movements adequate to this situation. Each meaningful motor directive demands not an arbitrarily coded, but an objective, quantitatively and qualitatively reliable representation of the surrounding environment in the brain. Such an action is also an active implement for the correct cognition of the surrounding world. The achievement or failure of a solution to every active motor problem encountered during life leads to a progressive filtering and cross-indexing of the evidence in the sensory syntheses mentioned above and in their components*. This also leads to knowledge through action and *revision through practice* which is the cornerstone of the entire dialectical-materialistic theory of knowledge, and in the cases selected here serves as a sort of biological context for Lenin's theory of reflection**.

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* The indubitable fact of the existence in the human central nervous system of *some* qualitatively different sensory syntheses does not contradict the point made on the objectivity of the representation in the brain. The latter finds an adequate explanation in the physiology of the co-ordination of movements.

** "Domination over nature appearing in human practice is the result of objectively accurate representation in the head of the human being of the phenomena and processes of nature and this is evidence of the fact that this reflection (within the limits which practice demonstrates for us) is objective, absolute external truth". V.I. Lenin, *Materialism and Empiriocriticism.*

The comparison made in the last few pages between the two types of theory of functioning of perceptual systems of the organism which are as yet unequal in terms of their scientific pedigrees and in the extent to which they have been worked out, allows us to illuminate some new aspects of the mechanism of operation of classic signal processes of elicitation, or of differential inhibition, of the reflex reaction.

A long time before telemechanics confirmed the essential principal of *conditionality*† of releasing or switching codes, the same fact was established for biological material by the famous discovery of I.P. Pavlov. The fact that any perceptible stimulus may equally easily be converted into a releaser signal for one or another organic reflex appears to be remarkably universal in biological material. As subsequent studies by Pavlov's school have shown (Speranskii, Bykov), in the entire complex of physiological function, apparently down to the deepest least accessible processes such as hormonal processes or the processes of cell metabolism, there is no single process which may not be connected, and in principle by the same method, to any releaser stimulus. This remarkable indifference of the nervous system to the nature and content of releaser stimuli was noted by I.P. Pavlov at the very beginning of his investigations into the range of phenomena which he discovered. Even the term employed for the stimuli which he grafted on to the trunks of the older organic reflexes - *conditioned* reflexes - is evidence of this. The terminology suggested by V.M. Bekhterev, "combinational" stimuli and reflexes, is less deep in relation to the internal significance of the phenomena, but is nevertheless quite adequate to describe the system of such mechanisms as have been explained up to the present time.

Two conditions are always necessary for the conversion of any given supraliminal agent into a conditioned releasing stimulus for any given organic reflex; (1) the coincidence or combination of this agent with the realization of the given reflex within the limits of a short time interval and (2) - an auxiliary condition - a given number of repetitions of this conjunction. The former of these conditions directly relates the phenomenon selected to the *cycle of association by contiguity*, as it were, characterized

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†Conditionality in the plan under discussion does not *demand* objectivity and does not contradict it. The comparison and delimitation of the signal-releaser and corrective functions of the receptors undertaken in this paper is probably sharper and more "either/or" than is the case in physiological reality where, doubtless, both forms of function may be superimposed upon each other from time to time and may interpenetrate.

by an indifference to the meaningful content of the associated items or receptions. It is interesting to note that for the conversion of indifferent stimuli into conditioned releasers their juxtaposition with the *effector* and not with the *afferent* component of the natural reflex is necessary. This last component is employed in the typical conditioning experiment only as a means of establishing and working out the effector arc. This is proved, for example, by the existence of so called conditioned reflexes of the second order, when the indifferent stimulus possesses releaser properties for a given reflex in spite of the fact that the effector part of the latter is set into action, not by an unconditioned by by a conditioned stimulus of the first order, which was earlier worked into the reflex. Another proof of the above may be seen in the fact that in the methods employed in animal training the reward which reinforces the unconditioned afferent impulses of feeding in the animal is given *after* the correct performance of the required activity in response to the corresponding conditioned command and is not, in this case, the unconditioned releaser stimulus for the trained activity. This detail, which has been underestimated in the past, attracts out attention in the present context because it seems that the formation of associative links in the brain between *afferent processes* and the *effector* portion of the reflex can be explained only if this effector realization of the reflex is reflected (again by means of ring feedback) in advance in the central nervous system, and can there already undergo association with the afferent processes of conditioned stimulation. We may find here yet another confirmation of the fact that "return-afferent" or feedback acts exist as direct components of the process in classical reflex arcs, and only escape observation for the time being.

The second condition of formation of conditioned connections is that termed auxiliary above. It would indeed be difficult to explain why a given number of repeated associations is required otherwise than because of the necessity for the experimental subject to discriminate the new reception introduced from the whole chaos of external stimulation bombarding him. The number of repetitions must be adequate to determine the *non-randomness* of the juxtaposition in time of the intero- or proprioception of the realized reflex with just one particular element out of the totality of extero-reception. In this sense - in relation to the necessary and sufficient number of repetitions - a stimulus which is indifferent in terms of its meaningful content may prove more difficult to discriminate and require more trials to attract the interest and attention ("orientational reaction") of the subject. The older, naively materialistic conception of gradually "beaten" tracks or synaptic barriers in the central nervous system may already be considered to be relegated to the archives of science*.

We must here mention a fact which remains obscure even in the light of new thinking in the physiology of regulation. The structure of almost all conditioned associations which have been set up is such that a new *conditioned afferent* signal is grafted on to an organic *unconditioned effector arc*. The variation, both in unconditioned effector processes and in the afferent "call-signs" which may be attached to them is almost infinite; but almost no single case is known in which we observe the *reverse* structure of a conditioned link, where a new conditioned reflex termination is attached to an unconditioned afferent arc. Cases of this reverse type were to some extent observed in the work of Eroofeeva at the beginning of this century, but I.P. Pavlov himself, in his "Lectures on the Operation of the Major Hemispheres", accompanied their description with a whole series of limitations and reservations. However this structural paradox may be explained in the future, it is clear that the inertia of actually existing effector semi-arcs makes it extremely difficult to employ their structural mechanism for the learning of unknown movements, for the establishment and learning of habits of movement and new skills, etc.

An examination of the question of signal codes and their conjoint roles in the aspect of regulatorary physiology may, it seems to us, throw a new light on the problem of the so-called *second signal system* (I.P. Pavlov). It is clear from the above analysis that the variability in possible conditioned signal codes is not in any way restricted, and that even speech phonemes, which do not in any way illustrate *in this respect* their role as members of any particular class, require, like all signal stimuli, only to be perceived and distinguished from each other.

Nobody has applied the concepts of a second signal system, or of architectonic fields homologous to Wernicke's field in man, to dogs, bears, sea-lions or cats; though all these animals may be trained to set up

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* If any given indifferent reception is repeatedly coincident in time with some unconditioned process, for example interoreception of salivation, etc., then the so-called probability *a posteriori* that this coincidence is not accidental rises very rapidly, and after ten associations is already very little different from unity. For the formation of a connection it is, however, necessary that both the indifferent stimulus itself, and the fact of the constant coincidence of both stimuli attract attention, that is to say, stimulate the processes of the active reception by the subject.

linkages and differentiations in response to verbal signals (though they are not even all higher mammals), as readily as to other forms of stimulation. These phonemic signal codes, which are not in any way different from other forms of codes, might have been the genetic embryos of phoneme signs in primitive man - a type of rudimentary imperative from which verb forms gradually evolved[*].

On the other hand, the *denominational* elements in speech out of which human beings have formed the category of proper names never had, and logically never could have a signal function in the sense described above. For this reason the treatment of the second signal system as a system of verbal representation of *things* (in general, the primary receptions of external objects representing in this sense the aggregate of the elements of the first signal system), which presents itself clearly in the list of names employed by experimenters using the so-called speech-movement method, is the result of a deep and mistaken confusion between two sharply distinct physiological functions and speech categories. *Words as signals* do not form any special type of system, and in the role of releaser phonemes are quite accessible to many animals which are still very far indeed from the function of speech. *Words and speech as reflections of the external world* in their static (proper names) and dynamic action and interaction with the subject (verbs, judgements) do indeed form a system which is attainable by, and characteristic of man alone; but to call *speech* which has attained this degree of meaningfulness and development a *signal system* is to confuse it with one of its most inessential and rudimentary manifestations[**].

The idea of a second signal system is doubtless one of the consequences of the methodological confusions described above, and due to the fact that physiologists recognized only one signal-releaser role of the receptor apparatus and undervalued its most important biological and social functions,

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[*] I must here make the following points: firstly, I do not include in the above any attempt to define the *chronological order* in which verbs and nominative categories may have evolved in primitive man, and secondly, that I agree entirely with well known philologists as to the phenomenon of the *secondary* employment of nominative elements with an exhortatory signal meaning in primitive speech.

[**] We must add to the above that the construction of robot automata which are able to *understand* speech is a quite hopeless problem for modern technology. On the other hand, robots which can react differentially to a few different speech phoneme signs which are given to them may already be built without any difficulty in principle.

that is, the cognition of the surrounding world through action and the regulation of action within it. The sign of equality placed between the ideas of reception and signal obliged the investigators of this earlier period to refer even perceived words to the category of signals; meanwhile, it has not been possible to circumvent the striking independence of speech as a specific inherent form of *homo sapien's* symbolic representation of the perceptual world and of himself in it. The tolerance extended to atomism, as mentioned above, has allowed easy bypassing of the *structuring* of speech (which makes it not a collection of words, but an instrument of thought) and its treatment as a sum of speech signals mainly concrete-objective in content.

Russian physiology contrived to avoid another, much more important, gnosiological error perpetrated by many thinkers in the Western world. This is also entirely a result of considering only a single aspect of receptor function: from the fact that it is clearly possible to reconcile the perfect operation of reflex functions with the complete arbitrariness of their sensory codes it is very easy to slide from the position of the recognition of the symbolic nature of all reception in general, and of the conditionality of the picture of the world in the brain and the psyche, to the concept of the un-knowability of objective reality and similar idealistic conceptions which have been disproved by authentic science long ago.

II

We may now attempt to make our analysis of the mechanisms of motor co-ordination in the higher organisms more precise having two problems in view: (1) to extract from this analysis the maximum information available at the present time as to the general laws governing mechanisms of control; and (2) to attempt to discover what constitutes those motor peculiarities of the higher animals, and of man in particular, which sharply and quantitatively distinguish their operation and resources from all that we might expect from the techniques of automatic processes of the present day, and perhaps of the near future. In the present analysis we shall have to touch upon many points which have already been thoroughly analysed in their time (23, 29); in order to avoid irrelevant repetition I shall dwell on them as briefly as possible in the present report, merely pointing out logical lines of analysis, leaving the reader interested in a more detailed exposition to turn to the works referred to. It will here be best to attempt to complete and extend the questions we have touched upon, which mainly concern the basic principal mechanisms of co-ordination and control, touching in the process upon errors which have now become apparent.

The first clear biomechanical distinction between the motor apparatus in man and the higher animals and any artificial self-controlling devices, as I have repeatedly emphasized, lies in the enormous number (which often reaches three figures) of *degrees of freedom* which it can attain, both in respect to the kinematics of the multiple linkages of its freely jointed kinematic chains, and to the elasticity due to the resilience of their connections - the muscles. Because of this there is no direct relationship between the degree of activity of muscles, their tensions, their lengths, or the speed of change in length. To explain how the control of a movement is complicated by each additional degree of freedom we give the following two examples.

A ship on the surface of the sea has three degrees of freedom (if we ignore rocking movements) though, in practice, control of *one* degree of freedom only - that of the direction or *course* is enough as on the surface of the sea, if the ship deviates somewhat from its course it is not necessary to return to the old *track* in order to establish its former direction, but quite sufficient to follow a path parallel to it, a couple of cables to one side or the other. This problem is easily and adequately solved by an autopilot with a compass. Let us now, however, consider an automobile which must travel along a *road of limited width* automatically dealing with all the curves and bends which it encounters. Here the direction of the car depends in practice upon *two degrees of freedom* of the car's mobility. This analysis shows that irrespective of the means by which the machine receives information on the course of the road (relative, for example, to the centre line), whether this is perceived by photo-, electro- or mechanicoreceptors, etc., the block diagram of the apparatus which guides the car along a winding road keeping it close to the centre line must incorporate: (1) a receptor of the distance from the line, and its sign; (2) a receptor of the angle between the axis of the machine and the line, and its sign; (3) a receptor of the effective curvature of the road; (3) a summing and analysing comparator system; and (5) a system of regulation to suppress incidental swing of the machine to one or the other side of the course. This great increase in complexity is a result of the problem of automatization with only one more degree of freedom. As far as we know no automat of a similar type has yet been constructed anywhere. It is useful to point out that the enormous difficulties of construction are not in any way connected to problems of signalling or to the construction of receptors of the types mentioned; the technical knowledge for receptors of all these types exists at present. The point of the difficulty lies in the organization of the *central recording* of information originally obtained from photo-elements or magnetic relays in the form of the nature, intensity, and succession of impulses controlling the servomechanics of the steering apparatus.

I draw my second example, for comparison, from the field of normal human motor co-ordination where all afferent organs function normally and the only unusual conditions are those of the motor problem. Fasten the handle end of a ski-stick in front of the buckle of a subject's belt. Attach a weight of 1-2 kg to the far end and on the right and left sides of the wheel attach a length of rubber tubing long enough to allow the ends to be held in the subject's left and right hands. Instruct the subject, turning the stick point forwards, to stand before a vertical board on which a large circle, square or other simple figure has been drawn, and to try, manipulating the ski-stick only by pulling on the rubber tubing, to follow the contours of the figure with the point of the ski-stick. The stick here represents one segment of an extremity with two degrees of freedom; the tubing is analogous to two antagonistic muscles introducing a further two degrees of freedom into the system. This experiment (which is very useful for demonstrations in an auditorium) makes clear to all who attempt it just how difficult and complicated it is to control systems which require the co-ordination of four degrees of freedom, even when under the control of a human being in possession of his full complement of receptors, but without motor practice with this task, who has been dealing with his bone-muscle motor apparatus from the first weeks of his life.

The definition of co-ordination which I have given in previous accounts still appears to me to be comprehensive and accurate: *The co-ordination of a movement is the process of mastering redundant degrees of freedom of the moving organ, in other words its conversion to a controllable system.* More briefly, co-ordination is the *organization of the control* of the motor apparatus. In the basic definition I have deliberately not discussed the reinforcement, inhibition, etc., of the redundant degrees of freedom, but their mastery. This is because (as extensive work on children, sportsmen, and also hemiparetic subjects and amputees (9, 14, 21, 65) has shown) fixation eliminating the redundant degrees of freedom mentioned above is employed only as the most primitive and inconvenient method, and then only at the beginning of the mastery of the motor skill, being later displaced by more flexible, expedient and economic methods of overcoming this redundancy through the *organization* of the process as a whole. The importance of the role played by the organization of the control of interactions, even in the simple case of the control of only two degrees of freedom, is already apparent from our first example of the automatic control of a car along a road. It follows, from the definition above, that co-ordination cannot be regarded as some sort of independent activity - as a particular act directed at the external world. It is better to regard it as a means of ensuring responsiveness and flexibility of execution in the motor system; it may be regarded as a type of *motor servo-mechanism*.

In studies on the structure of movements I have thoroughly considered the reasons which emphasize the biodynamic necessity for the organization of the mechanisms of motor co-ordination on the ring principle. I have also described some aspects, revealed by observation, of those physiological processes of interaction in control which effect the co-ordinational guidance of a movement through the mediation of sensory syntheses of various levels of structural complexity. We saw how important, among the unpredictable and almost independent forces which must be perceived and overcome, is the part played by *reactive forces* which occur together with external forces, which are inevitably produced in movements of the multi-linked kinematic chains of the motor organs, and which are complicated in exponential progression by each additional link in a series of joints and by each new degree of freedom of movement. We shall not discuss this purely biodynamic side of the problem any further at this point (see Chapter III).

We now turn to a question which has been left obscure in the studies above but which has become increasingly pressing in the context of contemporary developments in physiological thought. If motor co-ordination is a system of mechanisms ensuring the *control* of the motor apparatus and permitting its rich and complex flexibility to be utilized to the full, what can we say at the present time about the means and mechanisms of this *control* of motor acts? How may the regularities we now observe in this control be employed in the interests of applied cybernetics, and which aspects or properties of these regularities can be isolated as most specific to the nervous systems of the higher animals and of man, so that we may more precisely illuminate the gap which still qualitatively divides (and clearly will divide for some time to come) such processes as may be attained by automata from those which are realized in the motor acts of the life processes of highly developed organisms?

We must first briefly deal with some problems of terminology and attempt to systematize the principal types of self-regulating systems which are known at the present time (from here on we shall for brevity indicate this term by its first letters S.S.) and list the problems and topics in which we are interested.

All systems which are self-regulating for any given parameter, constant or variable, must incorporate the following elements as minimum requirements:

(1) *effector* (motor) activity, which is to be regulated along the given parameter;
(2) *a control element*, which conveys to the system in one way or another the *required value* of the parameter which is to be regulated;
(3) *a receptor* which perceives the *factual* course of the *value* of the parameter and signals it by some means to
(4) *a comparator device*, which perceives the discrepancy between the *factual* and *required* values with its magnitude and sign;
(5) *an apparatus* which encodes the data provided by the comparator device into correctional impulses which are transmitted by feedback linkages to
(6) *a regulator* which controls the function of the *effector* along the given parameter.

In this way the entire system displays a closed circle of interaction, the general scheme of which is given in Fig. 31. Between the elements which have been enumerated there are frequently included auxiliary devices of secondary importance such as amplifiers, relays, servomotors, etc.

The short terms used by German authors for values of the parameter to be regulated are very convenient and we also find it expedient to use them. The *required value* will in future be designated as *Sw* (from the German *Sollwert*), the *factual value as Iw (Istwert)*; the *discrepancy* between these which is perceived by element 4, or more exactly the excess or difference of *Iw* over *Sw* $(Iw - Sw)$, will be designated by the symbol Δw.

As an example given by Wiener (77) from an idea by his partner Rosenblueth the co-ordinational control of the act of seizing a visible object from a table top may be regarded as a constant process of estimation of the rate of diminution of that section of the path over which the hand must still travel to meet the object under consideration. We have every justification to designate the position of the object as *Sw*, the current position of the hand as Iw and the regularly diminishing distance between them as the variable Δw $(Iw - Sw)$. I must explain that both in the explanation above and henceforward, I shall regard the co-ordinational process in terms of *micro-intervals* of its track and of time, basing this treatment on data which have been accumulated over years of work by my colleagues and myself. For this reason I shall regard in this respect the *continuous planned path or process of movement of an organ* as the variable Sw and the factual variable coordinates of the latter as Iw. In the present context Δw will be the threshold values of deviations which are more or less

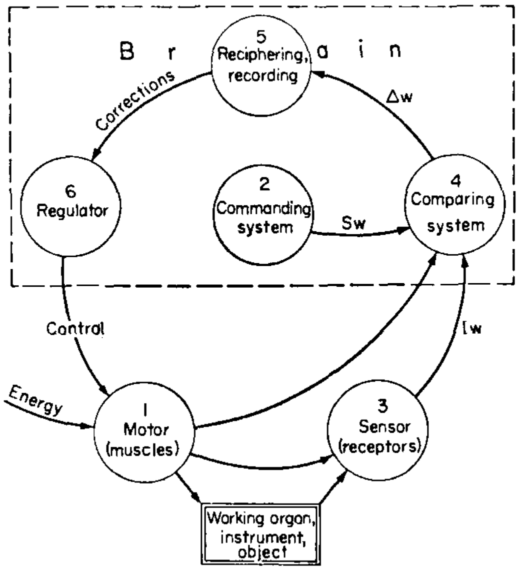

Fig. 31. The simplest possible block diagram of an apparatus for the control of movements.

accurately corrected *during the course of the movement*, as an example of which we may take the deviations of lines drawn by hand with a pencil or the point of a planimeter from a ruled line which a subject is set to follow. Consequently, in our sense Δw is not a systematically diminishing macro-distance, but oscillates in value, increasing and diminishing with the attainment of small values of variable sign and direction.

The central command post of the entire S.S. ring system is naturally its control element 2. Depending on the character of the Sw given by this element all conceivable types of S.S. may be divided into two large classes: S.S. with fixed constant values of Sw (so-called stabilizing systems) and S.S. with values of Sw which vary for one reason or other (tracking systems). The law of the course of the change of the given Sw may be called the *program* of the functioning of the S.S. Changes between successive stages in the realization of a program may be stepwise or continuous, and may, in various cases, be functions of time, of the path of action, of the working-point of the motor effector or of some intermediate resultant stage of operation, etc. In the most complex and flexible systems even the programs may be interchangeable.

Stabilizing systems are most primitive in their function and least interesting from our point of view although their analogues may be found in the operation of reflex-ring regulation among physiological systems. There are numerous examples of such systems in technology, beginning with Watt's centrifugal speed stabilizer for steam engines. A biological example is the pressure-receptor system of stabilization of arterial pressure which has been subjected to a thorough experimental examination from this point of view by Wagner (75). The motor apparatus of the organism, in all its functions and in principle of an S.S. system of the tracking type with a continuous program of changes for successive regulations of *Sw* in each case.

All elements of the most simple scheme of circular control incorporated in our list and in our plan (see Fig. 31) must also necessarily occur in organic regulational systems in one form or another, most especially in systems of motor control. Our knowledge of these structural elements in the living motor apparatus is very uneven. We still know nothing whatever of the physiological properties, or even the neural substrates, of elements 5 and 6. The moving elements of 1, the motor effectors of our movements - the skeletal muscles - belong, on the other hand, to a category of objects which have been investigated by physiologists and biophysicists very thoroughly and in great detail. The operation of element 3 of the schema - the receptor complex - has been thoroughly studied, but only from one point of view - as was shown in the first part of this report - and the aspect of its operation now under consideration still provides a great number of topics for investigation. I shall attempt to discuss here what can at present be said, both factually and hypothetically (with ordered degrees of probability), about the physiological structure of elements, 2, 4 and 3 of the scheme for the control of motor acts; I shall also incidentally discuss as they arise problems which we have already encountered, though we are still very far from being able to solve them. It will be appropriate to begin this review with the "command point" of the scheme - with the control element 2.

Every intelligent purposeful movement is made as an answer to a motor problem, and is determined - directly or indirectly - by the situation as a whole. Inasmuch as a motor act by the individual (an animal or human being) incorporates a solution to this problem, there is a basis of some program or other which will be realized by the control element. What is it that represents such a program of control for a movement, and by what is this program controlled in its turn?

In the monograph on the structure of movements (23) I discussed in detail *how* sensory corrections arise and *how* they affect the course of a movement. The topic raised here concerns a different problem which was almost ignored by the book referred to; *what it is precisely* that is corrected, and *what* controls the process and the basis of such corrections.

Observation of the simplest movements, such as those in the category of isolated movements (drawing straight lines through the air, indicating a point, etc.), may give the impression that the guiding principle underlying the program of changes in *Sw*, by means of which corrections to the movements are realized, is the geometrical form of such movements: the observation of linearity if it is necessary to describe a straight line, the observation of direction when indicating a particular point, etc. In coming to this conclusion, however, we fall into the error of mistaking the particular for the general. In the forms of movement described above it is indeed true that correction is effected by means of a geometrical image, but only because this is precisely the point of the problem. In the second of the examples which we have given (i.e. that of pointing), the geometrical guiding element of the movement has shrunk to a single point in the visual field. It is quite sufficient to be acquainted with cyclogrametric records of the movements of pointing with a finger at an object, carried out with optimal skill and accuracy, to become convinced that *N* successive gestures by the same subject are made through *N* non-coincident trajectories which only gather, as at a focus, in the vicinity of the same required point which is being indicated*. This means that the geometrical principle of correction is restricted, as far as possible, to the determination of the minimum extent of a movement which is absolutely necessary, and gives way in the remaining parts of the movement to some other guiding principles. The reliability and speed of their courses and their faultless coincidence on the target provides evidence that such principles are certainly incorporated in each micro-element of the gesture of pointing. (Compare this movement with one of a patient with ataxia!).

The error of mistaking the particular for the general becomes obvious

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* Cf., also, Fig. 21.

as soon as we turn from movements which are geometrical in respect to their goal to motor acts of another type. If we consider relatively simple specialized motor acts which are repeated many times and so become, as the term is, automatized, we may be sure that the attainment of their motor goal (locomotion in sport, industrial processes, etc.) is adequately achieved much earlier than the automatization of the movement and its stabilization to a significant degree of geometrical consistency. In many cases the goal of the movement is attained at the very first attempt. It follows that the kinematic motor composition of an act is by no means a universal invariant which guarantees the success of the action to be fulfilled. If we turn from the simplest and most repetitive actions to more complex purposeful movements, which are frequently multiphasic and conditioned by the need to overcome variable external conditions and resistances, such broad variations in the motor composition of movements become a universal rule.

If the program of a motor act is discussed, as a whole, in macroscopic terms we cannot discover any other determining factor than the image or representation of the result of the action (final or intermediate) in terms of which this action is directed and which leads towards the comprehension of the corresponding motor problem. In particular, how and by what physiological means the image of the envisaged or required effect of the movement may function as a guiding principle of the motor structure of an act, and as a program for the direction of the effector element, is a problem to which there are as yet no signs of any concrete or determinate answers. Whatever forms of the motor activity of higher organisms we consider, from elementary movements to multiphasic industrial processes, writing, articulation, etc., analysis suggests no other guiding constant than the form and sense of the motor problem and the dominance of the required result of its solution, which determine, from step to step, now the fixation and now the reconstruction of the course of the program as well as the realization of the sensory correction.

The fact that I have referred, as the characteristic guiding principle of the motor act, to the understanding or the representation of the result of an action (which is a topic for psychological investigation), and that I have emphasized that we do not yet understand the physiological mechanisms at its basis, does not mean that we can never understand the essentials of these mechanisms, or that we must not give them our attention. We are already in a position to consider the undivided psychophysiological unity of the processes of the planning and co-ordination of movements, and to apply a determinate terminology to the psychological aspects of the necessary control factor, while as physiologists - lagging behind psychology in the forefront of the study of movements (as we said above) - we are unable to analyse the physiological aspects to these processes. However, *ignoramus* does not signify *ignorabimus*; and the very title of the present chapter emphasizes that its purposes were rather to raise and formulate unsolved subsidiary questions than to answer those already propounded.

In Chapter 8 of the book referred to in ref. 23 there was a thorough discussion of how, and under the operation of what causes, the motor structure of movements which are repeatedly carried out may be formed and stabilized during the development of so-called motor habits through practice. As a brief extract we may here emphasize that even in the case of such uniformly repetitive acts the variability in the motor picture and in the range of initial conditions may at first be very great, and a more or less fixed program develops depending on the extent to which a motion is practised, and by no means at the first attempt. The process of practice towards the achievement of new motor habits essentially consists in the gradual success of a search for optimal motor solutions to the appropriate problems. Because of this, practice, when properly undertaken, does not consist in repeating the *means of solution* of a motor problem time after time, but in the *process of solving* this problem again and again by techniques which we changed and perfected from repetition to repetition. It is already apparent here that, in many cases, "practice is a particular type of repetition without repetition" and that motor training, if this position is ignored, is merely mechanical repetition by rote, a method which has been discredited in pedagogy for some time[*].

It is possible to be a little more concrete in one's statements in regard to the *microstructure* of the control of continuously flowing motor processes. In whatever form the general guiding directives of the anticipated solution are decoded into the concrete, detailed *Sw* elements of direction,

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[*] In exercises in sports and gymnastics, the motor structure (referred to as style) is incorporated as an integral part in the meaningful aspect of the given problem. For this reason it is one of the primary objects of the trainer to achieve as determinate a formulation and as rapid a stabilization of the motor structure as is possible for his pupil, but this does not in any way contradict the position maintained above as to the correct definition of practice.

forces, velocities, etc., of each limitingly small (more exactly, threshold value) section of the movement, it is beyond dispute that in the subordinate sections of the effector complex there exists a corresponding version of *Sw* broken up into details in this way. It must be observed that the impingement of each current proprioception (in the broad or in the functional sense of the expression) upon successive momentary directing values of *Sw* provides at least three different types of information, all equally important for control. Firstly, one or another degree of discrepancy between *Iw* and *Sw* (Δw) determines, on passage through the ring system, the necessary correctional impulses; this part of the process will be more thoroughly studied in discussions of the "comparator element" (No. 4 in our list). Secondly, the information which is provided by the receptors as to the particular point of succession attained by the realization of the motor act incorporates the excitatory impulse for the transfer or switching of *Sw* to the next micro-element of the program in the train; this aspect of function is mainly reminiscent of what Anokhin (4, 5) has termed sanctional afferentation. There is, finally, a third side to the process of reception - apparently one of those phenomena which may be adapted to perceptual models only with extreme difficulty. In every act of movement related to the overcoming of independent and variable external forces the organism continually encounters complications which are irregular, and in most cases unpredictable, which disrupt the course of the movement from paths which are set up in advance by programs. It is impossible, or at least extremely difficult, to master these by means of correctional impulses integrated with the initial program of the movement, whatever this may be. In these cases the receptor information acts as the stimulus for the adaptive restructuring of the program itself "in progress". These may range from minor purely technical alterations in the trajectories of the movement to other adjacent paths, to qualitative reorganizations of programs which involve changes in the very nomenclature of successive elements and stages of the motor act, and eventually amount to the adoption of new strategies of solution. Such terminations and reconstructions of programs as a result of receptor information occur much more often than one would expect, because they often involve only the lower levels of co-ordination and do not draw on the assistance of conscious attention (anyone who has walked, if only once in his life, on a surface that is not as level as a parquet floor will agree with this).

In the monograph on the structure of movements (23) it has been thoroughly demonstrated how numerous are the forms and ranges of correctional processes which, in the organization and mastery of a motor act, may be found to be distributed between interacting background levels of co-ordinational control. As was said in this book, what we describe as the *automatization* of a motor act is the constantly maintained transmission of numerous technical (background) corrections to the lower co-ordinational systems, the sensory syntheses of which are most adequately organized for corrections of the given type and quality. The general law, to which we know of practically no exception, that all component processes in control corrections disappear from the field of consciousness, besides being related to the guiding level connected to the very sense of the motor task, also provides us with evidence of the apportionment of correction in terms of levels which we call automatization. It is useful to emphasize at this point that in higher organisms (and in man in particular) there exists a rich and multisided sensorily equipped hierarchical system of co-ordinational levels involved in circular control both for the realization and for the momentary meaningful restructuring of various motor programs. This is clearly a consequence of the enormous number of degrees of freedom of the motor apparatus referred to earlier (which can only be controlled by a system as complex as we find here). This is also the underlying biological mechanism which permitted organisms having such a powerful central apparatus for motor control to develop their organs of movement during phylogenesis without being limited by the number of kinematic and dynamic degrees of freedom involved.

We must now turn to element 4 of the scheme given in Fig. 31. This element - the comparator device (as we have conditionally termed it) - is simultaneously a most interesting and puzzling physiological object, although the time is already quite ripe to begin its systematical investigation.

The ring principle of regulation is necessary in any mechanism which undertakes a comparison of the current values of Iw and Sw, just as it is in all artificially constructed S.S. Some such systems transmit an estimate of their mutual discrepancy (Δw) to the subsequent stages of the regulating systems. This (Δw) is basic to the process of transmission of effector correctional impulses to the periphery. If there were no such functional system in the brain it would be impossible, acting on the basis of the Iw receptions alone, to provide for any sort of independently acting correction. Here we at once encounter a completely distinct process, by means of which we compare and perceive not differences between two simultaneous or successive receptions, (as, for example, in the case of measurements of the thresholds of differentiation for any given receptor), but between *current perceptions*, and the representation, in some form or other, of the *internally controlled element* in the nervous system (we do not yet precisely know whether this is a representation, an engram, etc.). The value of Δw is obtained as a result of this comparison. Because of this process we may discuss a separate threshold, the comparison threshold, as we may term it; in the most elementary cases this threshold is obvious, and easily accessible to measurement. So, for example, we have the threshold of the visual-vestibular correctional reaction at the beginning of the tilt of a bicycle from the vertical; the threshold which characterizes the beginning of the corrective movement of a pencil on a deviation from an imaginary straight line which has to be drawn between two points on a sheet of paper; the threshold for the control of the voice which may be obtained from the voice oscillograms of students learning to sing and attempting to hold the voice to a note of determined frequency, etc. More interesting and typical features of the process under discussion wil be given below.

One of the most important elements of control over the motor processes is the reception of currently occurring variations in Iw for *velocity*. The tachometry of artificial S.S. may be carried out on various principles which, however, always make use of some physical value directly measured by a particular apparatus, and directly related to velocity. (As an example we have the amount of friction on an armature of a spring controlled by the intensity of a magnetic field, etc.). It is essential for our purpose to recognize that receptors which are directly and immediately sensitive to changes in velocity do not exist in the organisms which we are considering. This problem, however, is solved in the central nervous system in some quite special manner, and clearly either by the same sort of comparator mechanism, or by an extremely close homolog to it, the reception of the momentary position of a moving organ being compared *with the trace* of the same reception of the momentary position at a preceding interval of time Δt. The value of Δt may be estimated (for the purposes of an orientational approach) as being of the order of 0.07-0.12 sec, as I shall try to demonstrate below.

If we consider the course of synthetic receptor processes of the most various types, then the phenomenon of *fresh traces* (as we have conditionally termed it), to which we have referred above, appears to be remarkably universal and of fundamental significance. In the visual perception of movements it would be impossible to perceive *not only the velocity but also the direction* of a given movement if the process of perception were not based on ceaseless *comparison* of current receptions with the fresh traces of immediately preceding ones. When we perceive a melody or a word aurally, we do not merely register the separate successive elements - the sounds - but also the time course of a melodic line, or the time picture of the phonemes together with their tempo. We can discriminate qualitatively between rising and falling sequences of tones, between the phoneme *Va* and the phoneme *Av*, etc. If I perceive, with my eyes closed, that a line is being traced with a pointer on my skin, I do not perceive merely the location, but also the *direction* of succession and the *velocity* of the movement of the pointer as two qualitatively distinct *properties*. These are perceived as being in some way primary. They appear to be primary or primitive to such an extent that they are in all respects qualitatively similar to raw sensations. These traces retain their active form only for a minute fraction of a second, so that fresh traces are sharply distinct from the usual phenomena of *memory* - which is the means of long term retention of centrally coded phenomena.

In a large number of cases the control of a movement requires the continuous perception not only of current *values* of this difference (Δw) but also of the *velocities* with which these differences increase or diminish. As Wagner (75) has correctly observed, often (for example in cases of small but rapidly increasing values of Iw), control is exercised precisely by means of perception of the velocity of change of Δw, because it is useful to react sensitively to the very beginning of the development of an adverse deviation even earlier than the absolute threshold value of this deviation may be recognized and responded to. The indisputable fact that our sensory synthesis can also respond differentially to various rates of change of Δw is evidence that in the process of comparison under discussion the phenomenon of fresh traces must operate. The process of comparison is not, in this case, that of Iw and Sw, but of the fresh trace of their difference (Δw) occurring a fraction of a second earlier in time with a current value perceived at a given moment. In mathematical terms this is the process of the perception of the derivative $d(\Delta w)/dt$.

There is no doubt that the processes of perception of velocities and directions, the processes of comparison of Iw and Δw with their fresh traces for all dimensions of reception, etc., cannot in fact proceed *continuously*, but most rather do so in terms of differential intervals of time dt, there being some lower limit to the interval of time Δt which we may consequently regard as a threshold value. At the basis of these processes we find values of a particular type of thresholds, thresholds for *time*, which are clearly in very close physiological relationship both to the thresholds characterizing the speed of the psychomotor reaction and to physiological parameters such as lability, refractoriness, constants of adaptation, etc., which are clearly in need of immediate and intensive investigation. There is no doubt that psychologists specializing in the organs of sensation will already be in a position to criticize and to add material which is important to the argument in favour of the idea of fresh traces given above for the elucidation of the present problems*.

I should like to put forward the following notes towards a working hypothesis. In the thirties of the present century, M.N. Livanov had already found that the amplitudes of the peaks of the β waves on electroencephalograms vary considerably in magnitude from the peaks and troughs of the α waves, appearing, as it were, to be modulated by the latter. This fact may be taken as evidence of some sort of periodicity in the variations of excitability of the *cortical elements observed in the* α *rhythm*. Gray-Walter (47) noted that the lower threshold limit for the fusion frequency for flashes, cinema pictures, etc., in the ocular apparatus closely coincides with the frequency of the α rhythm, and even varies in parallel to the latter in individuals. It also does not seem to be a coincidence that the lower limit of the fusion frequency for hearing at which the specific sensory properties of a *sound* can be discriminated lies at about the same range of frequencies. There are, moreover, some unpublished orientational investigations by V.S. Gurfinkel on holding and movement in the unloaded hand (cf., also, (46)) and also a series of cyclogrametric observations by L.B. Chkhaidze on the rhythms of the acceleration impulses in the foot of a cyclist**. In both these cases the alternation of correctional impulses

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* In particular there arises the natural problem of the relationship which the mechanism of fresh traces bears to psychological mechanisms in the more general problems of engrammatization and of memory. Recent data suggest with ever increasing persuasiveness the paramount importance of the complex and many-sided nature of the biological processes organizing the reception and transmission of information. Further investigation shows how distinct are the phenomena of fresh traces from other earlier investigated forms of the function of retention of impressions, and of what the anatomic-physiological substrates of such impressions, etc., may be.

** I must express my debt to V.S. Gurfinkel and L.V. Chkhaidze for these personal communications.

is in complete mutual synchrony, and falls within the limits of the same frequency band as the α rhythm, i.e. 8-14 c/s. Is there no reason to suppose that this frequency marks the appearance of rhythmic oscillations in the excitability of all, or of the main alements of the reflex S.S. of our motor apparatus, in which a mutual synchronization through rhythm is doubtless necessary? We might also see in this light the ordering of the sensory and co-ordinational processes in terms of threshold values of the intervals of time Δt separating the moments of greatest refractoriness from the moments of maximal lability during which a momentary impression Iw is held in the form of a fresh trace until a subsequent rise of excitability. The distribution of the α rhythm over the entire surface of the cortex, its particular dominance in the receptor zones, and their synchronism with it over the whole of this range may also be taken as evidence in favour of this hypothesis. We may then describe the α rhythm as the pacemaker mechanism which gives to co-ordinational processes their determinate time parameter - their type of Sw of time, and intervals of Δt - as a regulating internal physiological pendulum appearing in these processes, such as that which British physiologists describe as a pacemaker. It must naturally be emphasized that whether this pacemaker is related to the α rhythm or not it has great physiological importance. There is an urgent need for quantitative investigation and for the determination of its relationships with such psychophysiological indicators as simple reaction time, the personal equation, etc.

It remains for me breifly to describe one more characteristic of the co-ordinational process which is closely related to the phenomenon of fresh traces and to the parameter Δt.

Situations are encountered in the processes of motor control in which great, sometimes decisive, importance attaches to correction of an advance or anticipatory character. This is particularly the case where, during the course of any given segment of a movement, retrospective control becomes practically impossible. There is a whole class of such motor acts (so-called ballistic movements) whose existence is only made possible by means of this type of anticipation: throwing at a target (throwing stones, spears, all possible ball games, etc.) jumping across a ditch or a high obstacle, a sweeping blow with a heavy hammer, etc. We must also note the existence of analogous anticipation in a number of similar motor acts, where it necessarily co-exists with corrections of a usual type; these are *movements which forestall others*, similar to those made by hounds following a wild animal and making rushes which are directed, not towards the momentarily visibly position of the quarry, but across, towards an anticipated or extrapolated point of intersection with its trajectory. There are many examples of this sort of thing - cathing a moving object with one's hand, passing a ball to a running team-mate, interposing a racket across the path of a moving ball or spheroid as in the game of table-tennis, and many others. Mittelstaedt (58) proposes that these two types of correction be distinguished from each other, and that they should be regarded as two equally important classes which he terms *Regelung* and *Steuerung*. Another category is more important in the present context.

The existence of correction of the anticipatory type, and the fact that we encounter it much more frequently than appears at first sight, directs our attention to the importance of anticipation in realizing any type of goal-directed motor act. Programming, as has been demonstrated above, is determined by the apprehension of motor problems as they arise, and represents an anticipation both of the result which is determined by its solution, and of such motor techniques as are necessary for its attainment (the latter if only in the most general terms). Many psychophysiological processes are entirely based on a similar "sight into the future". These have been termed "sets" and only in recent years has the term come to attain its full significance. Just as an analysis of the operation of the "setting or commanding complex" 3 reveals an hierarchical range (of levels of operation) beginning with the organization of the program of the motor act as a whole, and extending down to the level of the most detailed "micro-*Sw*" from moment to moment, or from Δt to Δt, we cannot now avoid the conclusion that in order to guarantee the completion of the micro-elements of the program and to undertake the direction of the motor process the successively emitted *Sw* must always *precede the actual movements*, preceding them if only by the threshold interval of time Δt. This is, however, enough to ensure that the equilibrium destroyed in this way (between the attained *Iw* and the future intended *Sw*) guarantees the dynamics of striving towards the end result. So, speaking semi-figuratively, the current micro-regulation of movements shuttles constantly between the present moment t and the limits of the interval from $t - \Delta t$ ("fresh traces") to $t + \Delta t$ (the anticipatory *Sw*).

I have brought together in these pages a number of problems related to the regulation of the life activity of the higher organisms, prompted by the analysis of motor acts. In the following chapter, I shall consider critically such current or imminent problems as the function of the coding organs, the interdependence between discrete and wave-like processes in the central nervous system, and, finally, some new prospects in the direction of the application of mathematics to the physiology of the activity of the nervous system.

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CHAPTER IVa

AN ECOLOGICAL APPROACH TO PERCEPTION AND ACTION

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1.0 INTRODUCTION

In his chapter on "Some emergent problems of the regulation of motor acts" Bernstein identifies four major problems:

(1) If perceiving were not a matter of being accurately aware of the objective facts of the environment and of one's actions, then the reliable control of activity would not be possible. However, the orthodox theory of receptor processes implies an arbitrary relation between these processes and the circumstances - environment and action - to which they nominally refer. This theory is inadequate to explain the everyday achievements of animal activity. What is needed is a theory that accounts for how perceiving keeps an animal in contact with the reality that bears on the successful conduct of its actions.

(2) Patently, animal activity is an instance of self-regulation, but what kind of self-regulation? Is it of the type conventionally expressed by self-regulating artifacts or do the regularities of animal activity follow from principles that are, as yet, unique to natural systems?

(3) Neither the geometry nor the kinematics of movement can serve, in the general case, as the determinant of the composition of an act. An action is what it is by virtue of its intention, that is, the motor problem (a needed change in the relation of the animal and its environment) toward which the action is directed as a solution. How are we to understand an intention as (a) the principle guiding the overall formation of an act and (b) the influence dominating the selection of its details?

(4) Clearly the control of activity is more than a retrospective matter. In the most general of cases, control must be prospective. For example, in basketball, one exerts forces against the ground of a specific magnitude so as to cause the hands to be at a specific height at a specific time to intercept a thrown ball. What is the basis of this anticipatory capability

that makes possible the realization of any goal-directed activity?

Problems (1) and (4) are discussed in Section 2.0 and problems (2) and (3) are discussed in Section 3.0.

2.0 ON THE OBJECTIVITY AND ACCURACY OF PERCEIVING

For any animal, activity takes place with respect to surfaces. For terrestrial animals, the most important surface is the ground. The ground is not even. Neither is it geometrically and materially uniform from place to place. There are gradual and sharp changes in the ground level. There are cracks and gaps. Liquid and solid areas are interspersed. Further, the ground surface is cluttered with closed, substantial surfaces. Some of these are attached, others are movable and some move under their own power. The clutter varies greatly in size. But for any terrestrial animal there are always closed, substantial surfaces both smaller and larger than its size. Some of the ground's clutter are barriers to locomotion but invariably there are gaps large enough to permit passage and barriers small enough to be hurdled or climbed. Locomoting from place to place, finding paths through the clutter, is necessary given the uneven distribution of the resources on which the persistence of the animal depends.

As Bernstein remarks, the meaningful problems that activity solves arise out of the layout of surfaces surrounding the animal, the environment. A few such meaningful problems are depicted in Figure 1. Awareness of the "problems" and awareness of the activities that do or do not solve them is the role of perceiving. It is obvious to Bernstein that perceiving (both the layout of surfaces and activities with respect to the layout) must be "objective" and "accurate". If perceiving fell short of these requirements - if it were, on the contrary, "subjective" and "inaccurate" - then meaningful, adaptive activity would not be possible. Bernstein writes (p.346): "We may consider the formulation of the motor problem, and the perception of the object in the external world with which it is concerned as having their necessary prerequisites in <u>maximally full and objective</u> perception both of the object and of each successive phase and detail of the corresponding movement which is directed towards the solution of the particular problem". What Bernstein says seems straightforward enough: perceiving must keep an animal in contact with its surroundings and with its behavior. It will be argued, however,

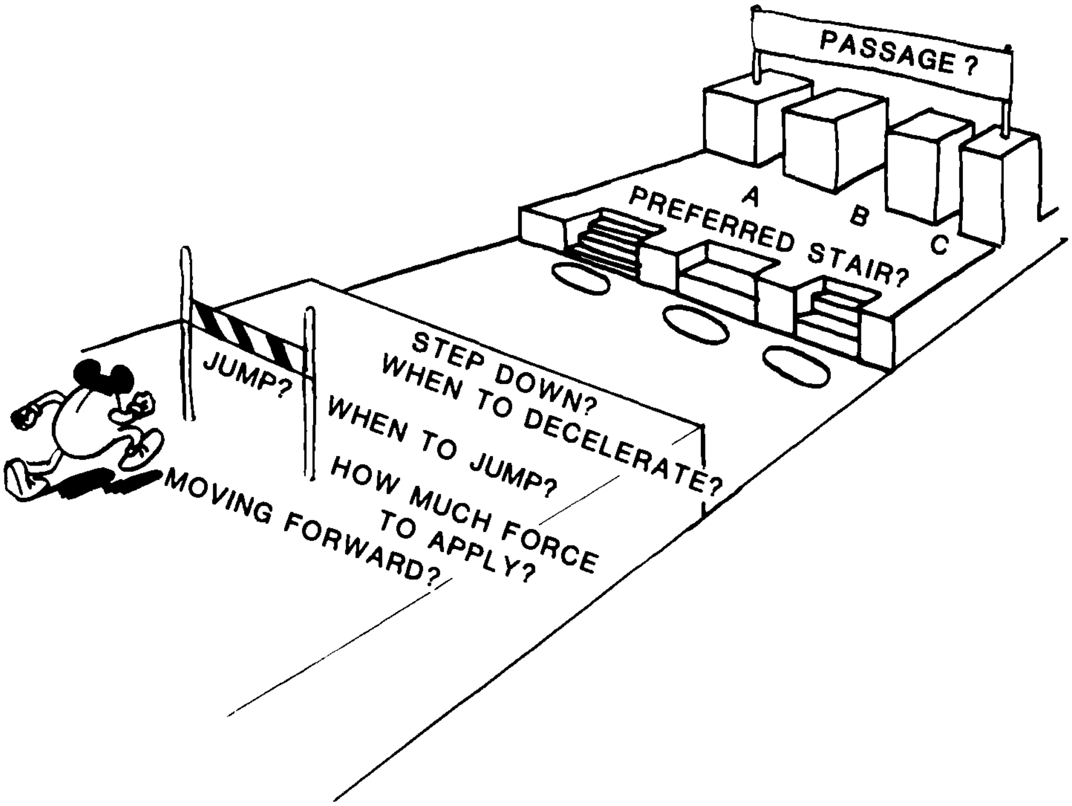

Fig. 1. A small sample of the meaningful problems that the surrounding layout of surfaces poses for a locomoting animal.

that a number of fairly radical steps have to be taken to insure that the theory of perception that we develop as scientists can live up to the natural demands placed on perception by normal activity in cluttered surroundings.

Clearly, Bernstein believes that the role of "afferentation" in the <u>guidance of activity</u> is the significant role, even though afferentation as a <u>trigger of reflexes</u> has a better scientific pedigree and is better understood by physiologists. The triggering role of afference assumed prominence because of the tendency to focus research on artificial movements - discrete responses made to momentary and punctate stimuli - rather than on activities resolving environmentally defined problems. Bernstein considered this triggering role of afference, developed as it was in the context of the reflex arc, to be overvalued and pointed out two unwelcome consequences of this overvaluation.

First, it established a bias to equate receptor processes in general with signals that release or inhibit reactions. Bernstein reminds us that this equation leads to the unacceptable interpretation of the receptor processes accompanying linguistic events as just triggers - the so-called second-signalling system. Closer to the present concerns, he points out that the emphasis on the afferent triggering of reactions obscured the fact that afference modulates ongoing movements. Bernstein saw a distinction between the traditional physiology of reaction and the physiology he wished to promote - a physiology of activity (Gelfand, Gurfinkel, Fomin & Tsetlin, 1971; Reed, 1982a). In this respect (and others) he was of kindred spirit with Gibson (1966, 1979, 1982) in rejecting the classical view of action as (merely) responses triggered by signals emanating from either outside the body or inside the brain. However, it should be noted that Bernstein labored under a major terminological manifestation of the classical view, namely, the correspondencies of the terms "sensory" and "afferent", "motor" and "efferent". This was unfortunate given that he rejected the conceptual identity that these correspondences implied.

As Gibson (1966, 1979, 1982) and Reed (1982a) have ably argued, the psychological concepts of sensory and motor cannot be equated, respectively, with the anatomical structures termed afferent and efferent. The anatomical definition of sensory system (as receptor elements, cortex, and the afferent pathways that mediate them) fails to accommodate the adjusting, optimizing steering and symmetricalizing of sense organs - that is, their purposive activity (Gibson, 1966). Bernstein recognized this inadequacy. In referring to the systematic searching by sense organs, he wrote (p.346): "This is an entirely active process, and the effector side of the organism is here employed in a manner completely analogous to that which is later explained to underlie afferentation in the control of movements". The anatomical definition of motor system (as cortex, motoneurons, and the efferent pathways that mediate them) fails to accommodate the dynamic responsiveness of effector organs to changes in the external force field brought about by changes in the orientation of effectors to the surround and to the body - that is, their contextual sensitivity (Turvey, Shaw & Mace, 1978). More than anybody before him, Bernstein sought to substitute the analysis of action in terms of efferent commands from cortex to motoneurons by an analysis of action as the selective use of information about the environment and about one's movements to selectively modulate one's movements with respect to the environment (cf. Gibson, 1979).

Second, the tradition of regarding afferents as triggering signals enforced, in Bernstein's view, a general attitude toward afference as arbitrarily related to the environmental conditions that cause it. All that is required for the successful initiation of a reflex is afferentation that is constant and recognizable by the effector apparatus. The proximal cause of a reaction need bear no necessary relation to the distal cause. As Bernstein sees it, this idea of an arbitrary connection between afferent states of affairs and environmental states of affairs is pernicious. If the afferent (or sensory) codes are arbitrary (as is claimed by Müller's Doctrine of Specific Nerve Energies and its successors) and if what the animal perceives is based on these codes, then what is there to guarantee that the animal's perception is objective and accurate? The depth of Bernstein's concern is expressed in this quotation (p. 359): "...from the fact that it is clearly possible to reconcile the perfect operation of reflex functions with the complete arbitrariness of their sensory codes it is very easy to slide from the position of the recognition of the symbolic nature of all reception in general, and of the conditionality of the picture of the world in the brain and the psyche, to the concept of unknowability of objective reality and similar idealistic conceptions..."

2.1 THE CARTESIAN PROGRAM

The orthodox and very popular representational/computational approach to mind (see Chomsky, 1980; Fodor, 1975; Pylyshyn, 1980) is consistent with the arbitrary coding theme that Bernstein believes (incorrectly, as we will claim below) to be rooted in the reflex philosophy and methodology. The representational/computational view abides by a "formality condition" - the explicit understanding that mental operations are formal, symbol manipulations performed on formal, symbol structures (Fodor, 1980). To a computer (and, by analogy, to a brain) it is immaterial whether its internal codes refer to this or that fact; how the signals are formatted and how they relate consistently among themselves by rule are what matters, not their meaningful content. We raise the spectre of the formality condition for two reasons. One reason is that Bernstein, despite his dislike of this condition in the guise that was familiar to him, invokes a mechanism for the control of activity that is continuous with the representational/computational thesis and, therefore, with the formality condition. Bernstein suggests that an ordered sequence of set points - representations of required values - governs the flow of afference and efference within the acting animal. The ordered sequence is a program prescribing the general form of the activity; it is a representation of the activity for the effector organs (cf. Cummins, 1977; Shaw, Turvey & Mace, 1982).

The other reason is that the formality condition is clearly tied to the historical tradition that began with the Cartesian Doctrine of Corporeal Ideas. It is this tradition that encourages the arbitrary coding interpretation of afference, not the reflex arc methodology which is itself a restatement of the Cartesian doctrine (Reed, 1982b). Descartes' doctrine, stated very generally, is that all awarenesses are awarenesses of states of the body or, as we would be more prone to say today, states of the brain. In contemporary thought, it is said that direct access to environmental and behavioral states of affairs is limited to the physical (or bodily) outputs of transducers which are linked not to the environmental and behavioral states but to the basic energy variables (e.g., intensity and wavelengths of light) (e.g., Boynton, 1975; Fodor & Pylyshyn, 1981). The question that this doctrine poses has been at the base of almost all theories in psychology, viz.: How can the environment be known objectively and accurately and acted upon successfully when the ideas one has about such things are based on awarenesses merely of brain states? Descartes had an answer to the subsidiary question of how primary objective qualities might be derived from secondary subjective qualities and it has been a persistent ingredient in almost all subsequent theorizing. He assumed an act of understanding that passed judgment on what environmental things might have caused the brain state; in his best known example, he assumed a rule-governed, quasi-mathematical process of inference from the states of the eye muscles and the visual nerves to the distance of an environmental object.

We can now focus sharply on the full implications of Bernstein's innocent claim that the coordination of an animal and its environment must be based on objective and accurate facts. Because of the pervasiveness of the Cartesian doctrine in physiology, psychology and cognitive science (see Reed, 1982b; Shaw, Turvey & Mace, 1982), it is generally accepted that an animal's awareness of its activities and of the surface layout to which they refer is not direct but mediated. Descartes had proposed rules, inferences and judgments to get to these objective facts of activity and environment from the directly given, subjective brain states. To Descartes' list of cognitive or epistemic mediators, later theorists have added representations, schemas, programs, models, organizing principles, meanings, concepts and the like. Whether dressed in its traditional or modern garb, the Cartesian program for explaining how felicitous activity is achieved in a cluttered environment faces a profound predicament. There is nothing in this explanation to guarantee that the proposed inferential operations performed on the brain states will yield conclusions that are objective and accurate rather than fatuous. In responding to John Locke's version of the Cartesian program, Berkeley thought a guarantee was unwarranted and emphasized the phenomenalism (that there are only phenomenal objects such as ideas) implicit in the Cartesian program. Hume thought a guarantee was unlikely to be forthcoming and emphasized the skepticism (that there may well be an environment and activities oriented to it, but no one can be sure of their existence) implicit in the Cartesian program. It is to thoughts such as those expressed by Berkeley and Hume that Bernstein refers when he remarks on "...the concept of unknowability of objective reality and similar idealistic conceptions..." Bernstein (p. 353) goes on to say (too cavalierly, in our opinion) that such thoughts "...have been disproved by authentic science long ago." As scientists committed to an objective reality, we must claim that it is knowable by animals, more or less. However, a scientific account of perception that is consistent with this realist posture has been thwarted, in our view, by the almost universal acceptance of the Cartesian program. As long as the Cartesian program is the accepted strategy for explaining the coordination of an animal and its environment - as long as the awareness of surface layout and action is claimed to be cognitively mediated - then the thoughts of Berkeley and Hume cannot be dismissed cavalierly and the predicament identified above remains firmly entrenched in psychological and physiological theory.

Accurate, objective conclusions might be assured if the inferential operations (and the various cognitive entities such as representations, etc.) were tightly constrained by reality. But the Cartesian program denies an animal direct contact with reality; to reiterate, only brain states are directly contactable. The problem for the Cartesian program, therefore, is how to get the reality that bears on the felicitous control of activity into the mind or nervous system of the animal. There are several responses to this problem. The most popular response is that a model of reality is constructed by a process of justifying inferences in the course of either evolution or ontogeny, or both (Bernstein advances a solution of this type). We will briefly summarize some of the reasons that render this response (scientifically) unacceptable (see Shaw, Turvey & Mace, 1982; Turvey, Shaw, Reed & Mace, 1981, for a fuller discussion).

All forms of non-demonstrative inference proposed by inductive logicians - enumerative inference, eliminative inference, and abductive inference - can be expressed as a confirmatory relation between evidence and hypothesis. The conditions of adequacy for confirmation vary among the forms of inference (see Smokler, 1968) but this is immaterial to the points we wish to make, viz., that the very notion of inference requires (1) the ability to project relevant hypotheses and (2) the availability of predicates in which to frame evidence statements <u>and</u> hypotheses. To clarify, the notion of a basic set of hypotheses is explicit in eliminative and abductive inference and implied in enumerative inference. For example, one version of abduction (Hanson, 1958, p. 72) goes as follows:

> Some surprising phenomenon P is observed.
> P would be explicable as a matter of course if H were true.
> Hence, there is reason to think that H is true.

If a model of reality were derived from inference, then it would have to be supposed that appropriate hypotheses - hypotheses that were generalizations about environmental states of affairs - were already at the disposal of the animal. What is their origin? Surely the answer cannot be "inference" for that would precipitate a vicious regress. But if the answer is not "inference" then the only option for the Cartesian program is that the origin of the hypotheses is both extra-physical and extra-conceptual. These are mutually exclusive categories.

The same conclusion follows from the point about the availability of two kinds of predicates, those for framing evidence statements and those for framing hypotheses. The predicates in an evidence statement <u>stand for energy variables</u> and, by argument, have their origin in physical processes. But for any form of inference there must be available, concurrently, predicates in which to couch hypotheses and these must be predicates that <u>stand for environmental properties</u> (such as an obstacle to locomotion). The origin of these environment-referential predicates cannot be inferential otherwise the argument is regressive; and it cannot be physical (law-based) because that option is denied the Cartesian program, by definition.

The general conclusion to be drawn is that a reliance on inference takes out a loan of intelligence that science can never repay: The Cartesian program is not a scientifically tractable program and, a fortiori, is a program for perception that science would be ill-advised to pursue.

2.2 GIBSON'S ECOLOGICAL PROGRAM

We believe that the Cartesian program must be abandoned if a scientifically acceptable account is to be provided of the perceptual objectivity that Bernstein regards as the sine qua non of action. To ease the break with tradition, it may help to remember that Descartes built his perceptual theory around thought, not action. Gibson's (1979) is an approach to perceiving that takes the control of activity as its central concern. In this approach the Cartesian doctrine of corporeal ideas is rejected together with the many perplexities that it entails. Rather than founding perceptual theory on brain states that are related tenuously to the environments and activities of animals, Gibson founds perceptual theory on structured energy distributions that are lawfully related to the environments and actions of animals. Rather than asking how accurate objective inferences from brain states to the facts of environments and actions are made, Gibson asks how information specific to the facts of environments and actions is detected. Rather than assuming that the conventional variables of physics provide the only legitimate basis for describing the environment, Gibson advances the idea that the environment can be legitimately described in terms that are referential of the activity capabilities of animals.

It will not be possible for us to do complete justice to Gibson's perceptual theory in these pages (see Gibson, 1979; Michaels & Carello, 1981; Reed & Jones, 1982; Turvey et al., 1981; Turvey & Carello, 1981). We will restrict ourselves, therefore, to those Gibsonian concepts that we take to be most central to the control of activity - the concepts of information and affordance. And we will restrict ourselves to the perceptual system of greatest relevance - the visual perceptual system.

Information is optical structure generated in a lawful way by environmental structure (for example, surface layout) and by the movements of the animal, both the movements of its body parts relative to its body and the movements of its body as a unit relative to the environment. This optical structure does not resemble the sources that generate it, but is specific to those sources in the sense that it is nomically (lawfully) dependent on them. The claim is that there are laws at the ecological scale that relate optical structure to properties of the environment and action (Gibson, 1979; Turvey et al., 1981).

This treatment of information and the notion of ecological laws rests on an optical analysis that departs from the classical geometric ray optics and the more contemporary physical optics. Though some have argued to the contrary (e.g., Boynton, 1975; Johansson, 1970), neither of these analyses is sufficient to capture the richness of light's structure subsequent to multiple reflections from surfaces of varying inclination and substance and undergoing various types of change. Gibson's push has been for a theory of optics that can do justice to ambient light as a basis for the control of activity. Given that activity is at the ecological scale of animals and their environments, Gibson termed the sought-after optical theory ecological optics. The limitations of conventional optical analyses recognized by Gibson (1961, 1979) are echoed by illumination engineers (e.g., Gershun, 1939; Moon, 1961; Moon & Spencer, 1981) whose goals are much more modest than Gibson's. In the subsection that follows we consider the activity-relevant questions raised by Figure 1 in terms of Gibson's ecological optics.

2.2.1 HOW DOES THE ANIMAL KNOW THAT IT IS MOVING FORWARD?

Forward rectilinear motion of a point of observation relative to the surroundings will lawfully generate an expanding optical flow pattern globally defined over the entire optic array to the point of observation. (A locally defined expansion pattern, kinematically discontinuous at its borders with the optical structure in the large, would be lawfully determined by a part of the surround moving relative to the point of observation. In natural circumstances there can be no ambiguity, contrary to the standard claim (von Holst & Mittelstaedt, 1950) about what is moving - the animal or part of its environment (Turvey, 1979). As noted above, the lawfulness of optical structure at the ecological scale is the basis for its functioning as information for the control of activity: If A lawfully generates B, then B specifies A. Lishman and Lee (1973) have shown that humans walking voluntarily forward will report that they are walking backwards when exposed to the global optical transformation that is lawfully generated by backward locomotion (and which, therefore, specifies that the walker is moving backward). Further, when flying insects are exposed to global optical transformations that are the lawful consequences of forces that produce rotation, vertical displacement, and yaw, they respond with the appropriate counteracting forces (Srinivasan, 1977); Turvey & Remez, 1979).

2.2.2 HOW DOES THE ANIMAL KNOW FROM WHERE TO JUMP (TO ACCOMMODATE AN UPCOMING BARRIER) AND HOW DOES IT KNOW WHETHER ITS DECELERATION IS ADEQUATE (TO ACCOMMODATE ITS LOCOMOTION TO AN UPCOMING BRINK IN THE GROUND)?

The answers to both of these questions depend, in the Gibsonian perspective, on information about the imminence of contact (with barrier or brink). Lee (1974, 1976, 1980) and others (e.g., Koenderink & Van Doorn, 1981) have identified an optical variable, symbolized as $T(t)$ by Lee (1976), that is equal to the inverse of the rate of dilation of a bounded region of optical structure. $T(t)$ is lawfully generated by the approach at constant velocity of a point of observation to a substantial surface in the frontal plane, or vice versa; it specifies the time at which the point of observation will make contact with the surface.

Obviously, the existence of an optical variable specifying time-to-contact bears directly on the question posed by Bernstein (Question (4) above) of how control can be prospective. Any answer to the question of prospective control is constrained by the requirements that (1) causes precede effects and (2) causes be actual rather than possible states of affairs. An event at a later time cannot cause an action at an earlier time and only actual events can be causal. In the Cartesian program, the bases of prospective control are representations; actual mental states existing in the present (rather than future, possible states of the animal-environment system) are the causes of activity. The logical format of these representations in the case of controlled collisions must be that of a counter-factual, roughly of the form "if I don't change what I am doing and the conditions continue to be as they are, then X is likely to occur". The basis of prospective control in the Gibsonian program is exemplified by the time-to-contact variable, viz., there is information in the present optical structure (e.g., the value of $T(t)$ at t_i) specific to what will occur if the present conditions continue (e.g., collision at time t_j). To draw the contrast sharply, in the Gibsonian program the basis for prospective control is sought in laws at the ecological scale (that relate present optical properties to upcoming properties of the animal-environment system); in the Cartesian program the basis is sought in inferential processes (that relate the semantically neutral outputs of transducers to a counter-factual representation). Reiterating the arguments raised above, the Cartesian solution to the problem of prospective control begs the interesting questions; for example, how does the animal construct just that counterfactual representation that is right for the current situation?

Let us look at an example of the use of the time-to-contact variable. The gannet, a large seabird that feeds on fish, hovers about thirty meters above the water. On sighting a prey, it dives down first with its wings partly spread for steering and then with its wings folded so that it enters the water vigorously but cleanly. It may hit the water at speeds approaching 25 ms^{-1} (or 55 miles h^{-1}). The action problem for the gannet is to retract its wings soon enough to avoid fracturing them but not so soon as to hinder the accuracy of its dive. Given that the gannet dives from varying heights, at varying initial speeds, and in varying wind conditions, how does it properly control its entry? Lee (1980) and Lee and Reddish (1981) have concluded that wing retraction is initiated when the time-to-contact variable reaches a certain margin value. (Because the animal is accelerating in the dive, the same margin value of $T(t)$ will be associated with different actual times-to-contact. The birds are seen to fold their wings a longer time before contact the higher the starting point of the dive).

There is reason to believe that the time-to-contact variable is the basis of prospective control in a number of related circumstances. Data on the kinematics of catching a ball (Sharp & Whiting, 1974, 1975), hitting a baseball (Hubbard & Seng, 1954), infants' reaching for a moving object (Van Hofsten & Lindhagen, 1979; Van Hofsten, 1983), stepping down (Friedman, Wannstedt & Herman, 1976) and falling on one's hands against an inclined board (Dietz & Noth, 1978) are amenable to such an analysis (see Fitch, Tuller & Turvey, 1982; Fitch & Turvey, 1978; Lee, 1980). The last situation is depicted in Figure 2. The triceps brachii muscles are shown to tense in preparation for an upcoming collision in which the arms must absorb the momentum. With the eyes closed, the electromyographic index of the initiation of muscle tension is tied to the start of falling; with the eyes open and with different falling distances the index occurs at varying times after the start of falling but at an approximately constant time prior to contact.

We should remark that the fact of a simple, single optical property specifying the imminence of contact has implications for another of Bernstein's concerns, namely, how an animal can adjust its behavior to the velocity of things. Bernstein pursues a conventional argument that velocity is arrived at by a process of comparing the present location of a thing with the memory trace of an immediately preceding location and dividing the deduced distance traveled by an internally determined estimate of elapsed

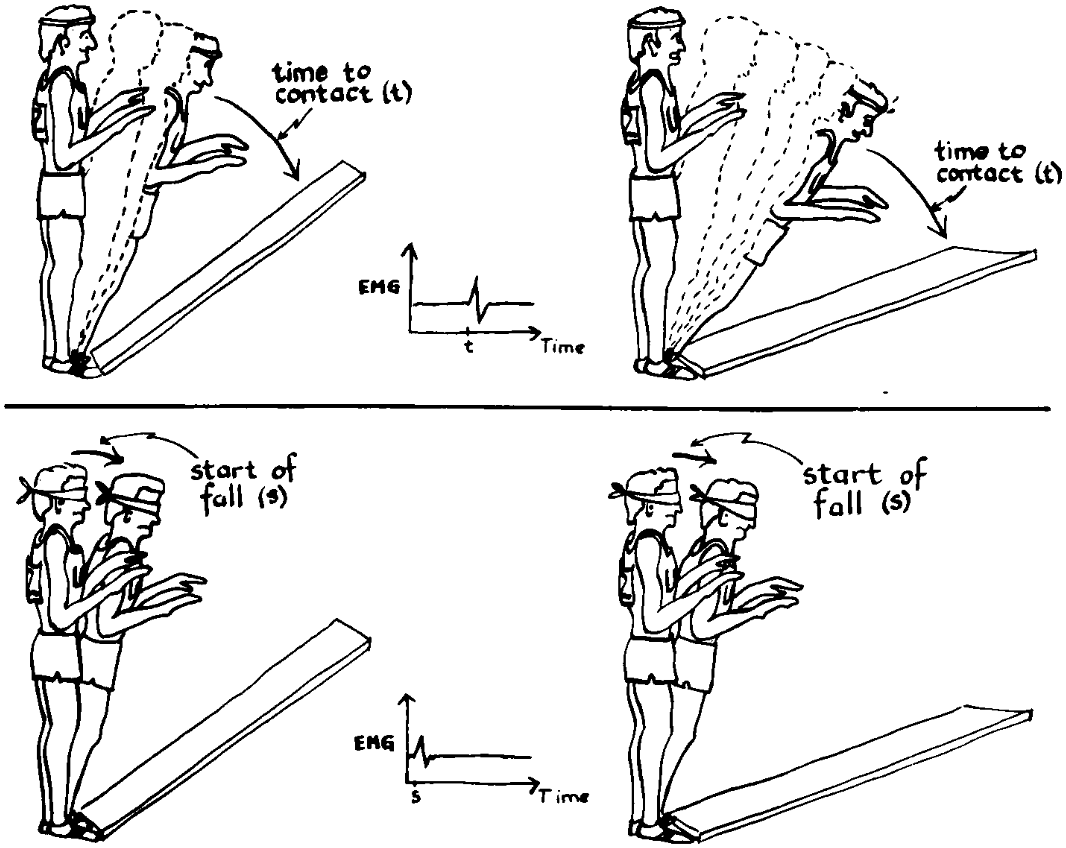

Fig. 2. With the eyes open and with different falling distances the initiation of tension in the triceps brachii muscles occurs at varying times after the start of falling but at an approximately constant time prior to contact (Above). With the eyes closed initiation of muscle tension is tied to the start of falling (Below). (From Fitch, H.L., Turvey, M.T. & Tuller, B. The Bernstein perspective: III. Tuning of coordinative structures with special reference to perception. In, J.A.S. Kelso (Ed.), Human motor behavior. Hillsdale, N.J.: Erlbaum, 1982).

time. The inadequacies of this kind of explanation have been discussed in detail (Gibson, 1979; Turvey, 1977). Here we wish to comment only on the questionable strategy of analyzing higher-order activity-relevant variables in terms of the putatively more basic variables of displacement and time. Inertial guidance systems are based on Newton's laws of inertia and gravity. These systems detect accelerative forces. They determine velocity and distance indirectly through the single and double integration, respectively, of the accelerative forces. In like fashion, adherents to the Gibsonian program (Lee, 1980; Runeson, 1977; Turvey & Shaw, 1978) argue that the imminence of collision is not inferred from a preliminary determination of speed of approach and distance from surface; rather, the basis for an animal's knowing when a surface will be contacted is the detection of $T(t)$ as such. The point is that to understand how perception controls activity we must be willing (i) to question the primary reality status of the basic variables of physics; (ii) to look for variables (observables, quantities) at the ecological scale that uniquely specify the relation of animal to environment; and (iii) to consider hard- or soft-molded processes that detect these ecological variables (rather than knowledge-based procedures that construct representations of them from conventional physical variables).

So, how does the animal know from where to leap? The answer, to be blunt, is that it does not need to know the proper place; rather, it needs to know the proper time. The former depends on the speed, the latter does not. Evidently, as anticipated, the successful leaping of a barrier depends on the time-to-contact variable. It also depends on body-scaled information, but we will have more to say about that below. And how does the animal know whether it is braking sufficiently? An animal's deceleration is adequate if and only if the distance it will take the animal to stop is less than or equal to its current distance from the brink (Lee, 1980). Adequacy of braking is specified by whether the rate of change of $T(t)$ equals or exceeds a critical value (Lee, 1976; 1980). A related observation is that flies begin to decelerate prior to contact with a surface at a critical value of $T(t)^{-1}$ (Wagner, 1982).

2.2.3 HOW DOES THE ANIMAL KNOW THAT THE BARRIER IS JUMPABLE AND THAT THE BRINK IS A STEP-DOWN PLACE (RATHER THAN A FALLING-OFF PLACE)?

Knowing that something is in the class of jumpable objects and that some other thing is in the class of step-down places would be treated in the Cartesian program as the imposition of subjective, meaningful categories on an objective, meaningless surround. Conventionally, it would be said that the animal has concepts of such things and debate would focus on how such mental entities could be established. Careful analysis would reveal that, given the departure point of the Cartesian program, empirical contributions to such concepts would have to be secondary to the rational contribution (Fodor, 1975). In sharp contrast, the Gibsonian program seeks to uncover a natural, lawful basis for knowing what activity (or activities) a situation offers. Consider a brink in the surface that happens to be a step-down place for a given animal rather than a place where it would have to jump down or climb down or steer away from. To begin with, the property of the brink as a step-down place for the animal cannot be captured in the scales and stardard units of physics. These scales and units are intended to be "fully objective", that is, observer- or user-independent. They are extrinsic measures, in that the standards on which they are based are divorced from and external to the situations to which they are applied. To capture a step-down place for a given animal requires intrinsic measures, those whose standards are to be found in the situation of animal and brink. In Figure 3 the separation of surfaces (R) must somehow be expressed in units of the animal. Leg length is obviously significant but scaling surface magnitudes in terms of the unit 'eye-height' is probably a better move (cf. Lee, 1974, 1980). A lawful allometric relation (Huxley, 1932; Gunther, 1975; Rosen, 1968) is to be expected between eye height (E) and leg length (L): $L = aE^b$, where a and b are constants. (Eye height will, of course, vary

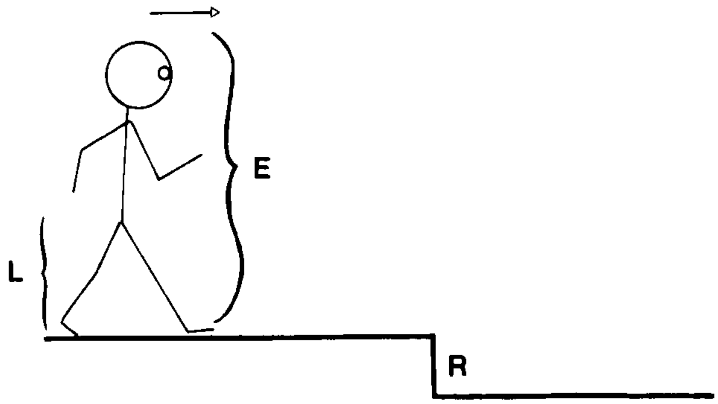

Fig. 3. Approaching a brink of a surface. E is eye height, L is leg length and R is the surface separation.

with the animal's posture but our intent here is to convey the style of the analysis rather than its full detail). If the separation of surfaces (R) at a brink is below some critical number, nE (or is less than or within a tolerance range nE + S), then the separation is a step-down place; above this critical number (or range) it is a place that requires some locomotory strategy other than stepping down. Noting that E is unity, there is a dimensionless quantity that marks the boundary between the activities stepping down vs. jumping down, for example - that a brink offers an animal. Now the question becomes whether or not there is an optical property specific to this dimensionless quantity.

First, a point of observation moving toward a brink in a surface (where one surface partially occludes another) will lawfully generate an optical flow pattern in which there is a discontinuity, viz., a horizontal contour above which optical structure magnifies and gains and below which optical structure magnifies but does not gain. The non-gain and gain of structure are specific, respectively, to the occluding surface currently supporting the animal and the occluded surface to which it is heading. Second, from Figure 4 (after Warren, 1982) it can be seen that the separation (R) of the occluding and occluded surfaces can be expressed in units of the height of the point of observation E and in terms of the ratio of the rate of

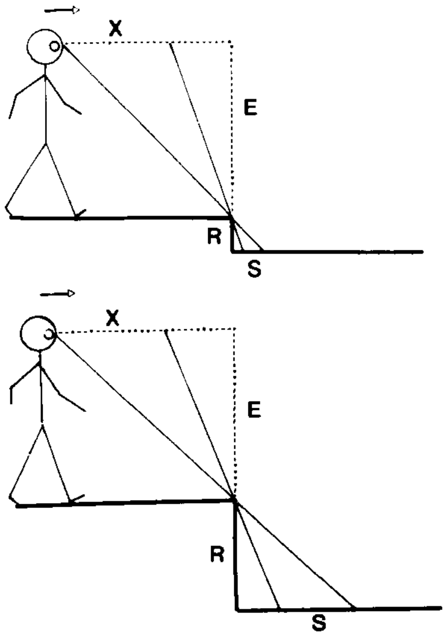

Fig. 4. In approaching a brink of a surface the ratio of (dX/dt)/(dS/dt) depends on the surface separation R relative to the eye height E.

displacement of the point of observation (dx/dt) to the rate of gain of structure (ds/dt). Letting $t_1 - t_0$ be the time of a step (x), for the same stepping rate the gain of structure (s) is greater the greater the separation of surfaces (R). Although the example is crudely developed, it makes an in principle argument that there will be a dimensionless quantity of optical flow, such as (dx/dt)/(ds/dt), which is specific to the vertical separation of surfaces at a brink, scaled in units of the observer. A critical value or range of this optical flow property will specify the boundary between places the animal can step down from - those that can be accommodated by limb extension - and places requiring a different manoeuver.

Dimensionless numbers play a significant role in many branches of physics. The commonly used numbers, referred to as principal π-numbers by Schuring (1980) (of which the Reynolds, Raleigh, Mach, Prandtl, and Froude are prime examples), are built from laws. Thus, the Reynolds number which applies to fluids, is built from Newton's law of inertia and the law for shear stress of a Newtonian fluid. The two laws are cast as dimensionless ratios or numbers (e.g., $\pi = F/ma$), and these two numbers in ratio give the Reynolds number. At a critical value of the Reynolds number, the inertial forces (favoring turbulence) dominate the viscous forces (favoring laminar flow) and there is a shift from the one kind of flow to the other. Generally speaking, the major dimensionless numbers in physics mark off, at critical values, a change in the relation of forces from a balance between them to a dominance by one of them and, thereby, mark off distinct physical states. In like fashion, it seems that the dimensionless numbers built from purely optical variables mark off, at critical values, distinct states. They are not physical states associated with distinct forms of energy absorption, however, but specificational states (see Kugler, Turvey, Carello & Shaw, in press). Thus, in the example just given, the dimensionless quantity (dx/dt)/(ds/dt) specifies step-downable when it is below a critical value and non-step-downable when it is above that critical value.

We wish to underscore with two well developed examples the potential significance of dimensionless quantities to law-based explanations of the control of activity. When $dT(t)/dt \geq -0.5$, it specifies that the point of observation will stop prior to contact with an upcoming substantial surface if current conditions persist, whereas when $dT(t)/dt < -0.5$ it specifies that there will be a collision between the point of observation and the surface if the current conditions persist. This critical value of the rate of change of the time-to-contact variable is an invariant optical quantity:

Whether the animal is approaching a surface or being approached by a surface, the quantity -0.5 marks off two distinct specificational states concerning the collisional consequences of the animal's current activity.

The second example returns the focus of this subsection to the perception of the kind of activity that an arrangement of surfaces affords an animal. Warren (1982) investigated the perception of stairways that varied in riser heights in terms of two questions: (1) Could a person perceive whether a stairway was climbable in the normal fashion (a question of the critical riser height)? and (2) Could a person perceive how costly, in metabolic terms, a stairway would be to climb (a question of the optimal riser height)? A preliminary analysis of the biomechanics of stepping up revealed that the riser height (R) beyond which normal stair climbing would be impossible was a constant proportion of leg length (L), viz., .88L, or R/L (a dimensionless quantity) = .88. Subjects, who differed markedly in height (1.63m vs. 1.93m), saw photographs of stairways with risers that ranged between .51m and 1.02m and were asked to judge the climbableness of each stairway. Although the riser height that distinguished the stairways judged to be climbable from the stairways judged to be nonclimbable differed between the two groups of subjects when measured in meters, it did not differ when measured in leg length. For both groups of subjects R/L = .88, that is, the critical riser value that had been determined from biomechanical considerations. With respect to the optimal riser height, the metabolic cost of climbing at 50 steps/min on an adjustable, motor-driven stairmill was evaluated at riser heights varying from .13m to .25m for short (1.63m) and tall (1.93m) subjects. The minimum energy expenditure per vertical meter (cal/kg-m), indicating optimal riser height, occurred at a riser height of R = .26L. In two visual tasks, a forced choice task and a rating task, the stairways were pitted against each other in pairs. The tasks revealed that the preferred riser height (the stairway that was seen to be the one that could be climbed most comfortably) differed between the two groups of subjects when measured in meters but it did not differ when measured in leg length. The preferred or optimal value for both groups was .25L in the forced choice task and .24L in the rating task, very close to the optimal value of .26L determined by metabolic measurement.

2.2.4 AFFORDANCES

In Gibson's (1979) terminology, step-down places, falling-off places, climbable-places, collide-withable surfaces, travel-throughable openings and so on (Figure 1) are affordances. That is to say, they are properties of the environment taken with reference to the animal. An affordance is an invariant arrangement of surface/substance properties that permits a given animal a particular activity. It is a real property - one might even say a physical property - but one that is defined at the ecological scale of animals and their niches. By the laws of ecological optics, the light structured by an affordance will be specific to the affordance - as the above examples suggest. The optical property specific to an affordance is like the time-to-contact property: It is not decomposable into optical variables of a putatively more basic type. Consequently, it is claimed, the perceiving of an affordance is based on detecting the optical property that specifies the affordance. In the Gibsonian program, perceiving an affordance is not mediated by computational/representational processes. It is said to be direct, and understanding how this can be - understanding the physical processes at the ecological scale that make possible the direct perception of the reality that bears on the control of activity - is what the Gibsonian program is fundamentally about (Section 3.2).

3.0 PRINCIPLES OF SELF-REGULATION

It is fair to say that working under the Cartesian program one is inclined to explain regularity (of activity) by reference to intelligent regulators. In the Cartesian view of things, it is an act of the intellect that interprets the outputs of sensory transducers and puts them to use with respect to externally oriented desires. Intelligence in its various manifestations (e.g., judging, comprehending, decision making, comparing, projecting and evaluating hypotheses, recognizing, reconsidering, commanding, and so on) is at the core of the Cartesian explanation of the control and coordination of movement. For Descartes himself the intellect was equated with the soul - or as Ryle (1949) liked to say, disdainfully, "the ghost in the machine".

The contemporary student of movement who chides all 'little man in the brain' explanations of control may, however, be firm in his belief that the concepts he borrows from cybernetics and formal machine theory are acceptable explanatory tools. Personally, we think such convictions are suspect. Concepts such as set-points, programs, and so on are superficially attractive in that they refer to material things that perform the role historically ascribed to homonculi. Under closer scrutiny, such concepts are revealed to be the products of an intelligent act performed by a being with foreknowledge of the regularity to be achieved. The concepts of cybernetics and formal machine theory are seductive because they facilitate the simulation of 'regularities' but they are not, we believe, in the best interests of explanatory science. First, these concepts necessarily assume intelligence and rationality - assumptions that were, after all, the reason for science's original and persistent displeasure with Descartes' homunculus. Second,their promise is limited, at best, to describing and, perhaps, to predicting regularities. But explaining, in the sense of identifying the lawful basis for behavior, is ineffably beyond their reach.

At one time, Bernstein was enthusiastic about the relevance of cybernetical and formal machine analogues to the physiology of activity. He later became much more circumspect with regard to their appropriateness. Cybernetical notions figure prominently in his discussion of "Some Emergent Problems of the Regulation of Motor Acts" (as we will underscore in the subsections that follow). But in later chapters he questions the propriety of cybernetics for biology and physiology and intimates that "the "honeymoon" between these two sciences" (p. 542) may be over (also pp.546 - 547). In Section 3.1 we critically evaluate the cybernetical treatment of Bernstein's regulatory notion of circular causality and in Section 3.2 we outline the physical conditions for that principle. Our belief, consonant with Bernstein's later impressions, is that the physiology of activity would fare better married to a physics that addresses the ecological scale and its natural regularities than to a formal theory of the regulation of artifacts.

3.1 THE RING PRINCIPLE (CYBERNETICALLY INTERPRETED)

Bernstein is convinced, and properly so, that self-regulation is based on circular causality - the "ring principle" as he terms it. He embraces the familiar interpretation of this principle, the one advanced by cybernetics: a referent signal or set point mediates signals fed forward to and fed back from a device or process (generically referred to as 'the plant'). For the conduct of an activity a single referent - and, a fortiori, a single ring - will rarely be sufficient. Bernstein assumes an ordered sequence of referent signals. Insofar as a referent signal must predate the afferent and efferent flow that it mediates, so the order of the referent signals must largely be ascribed prefatory to the activity. In

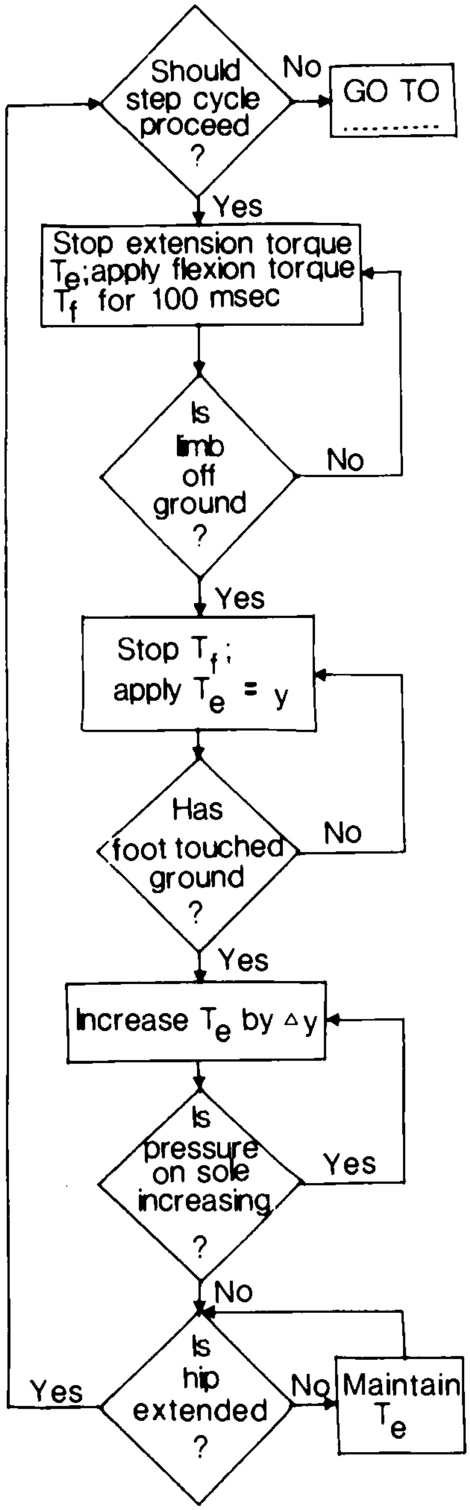

Fig. 5. A program formulation of a locomotory step cycle (Adapted from MacKay, W.A. The motor program: Back to the computer. Trends in Neuroscience, 1980, 3, 97-100).

brief, Bernstein's proposal for self-regulation is the popular notion of a program. MacKay (1980) identifies the kinds of detail one might expect to find in a program for a step cycle of locomotion (Figure 5).

In addition to identifying (1) the general a priori prescriptive nature of the program, this example illustrates nicely that a program is (2) an orderly sequence of preferred quantities, (3) an orderly sequence of commands (to the skeletomuscular machinery that realizes these quantities), and (4) an orderly sequence of symbol strings (the representational format for the quantities and the commands). It also illustrates a more profound feature of the program conception: (5) that rate-dependent processes - the irreversible thermodynamics and the mechanics of the skeletomuscular system - are coupled to and constrained by rate-independent structures - the symbol strings.

The centrality of the ring principle to self-regulation cannot be doubted. (The reciprocity of locomotion and global optical transformations described in Section 2.0 is one example of the principle's ubiquitous application). What can be doubted is whether the properties identified in (1) through (5) above are necessarily entailed by the principle.

3.1.2 THE CONCEPT OF THE REFERENT SIGNAL

The sollwerts (required values, set points) that have been used frequently to 'explain' the stabilities of vegetative processes (thermoregulation respiration, feeding, drinking, etc.) are more fictitious than real (e.g., Friedman & Stricker, 1976; Iberall, Weinberg & Schindler, 1971; Mitchell, Snellen & Atkins, 1970; Werner, 1977). The observed stable quantities of vegetative processes (e.g., human body temperature of 37 degrees centigrade) are not prescribed values or goals playing a causal role. They are, more accurately, resultant quantities, indexing a stable relation between independent processes (force systems) defined over the same state variables (Iberall, 1978; Kugler, Kelso & Turvey, 1980, 1982; Yates, 1982b). As we like to put it, these so-called sollwerts are not a priori prescriptions for the system but a posteriori facts of the system's processes.

The experiments of Zavelishin and Tenenbaum (1968) are illuminating in this regard. They focussed on two respiratory variables - the resistance r of the air to inspiration and the duration d of inspiration. The function f relating d to r was identified. A function F relating r to d was imposed (Figure 6). Circular causality was thereby established.

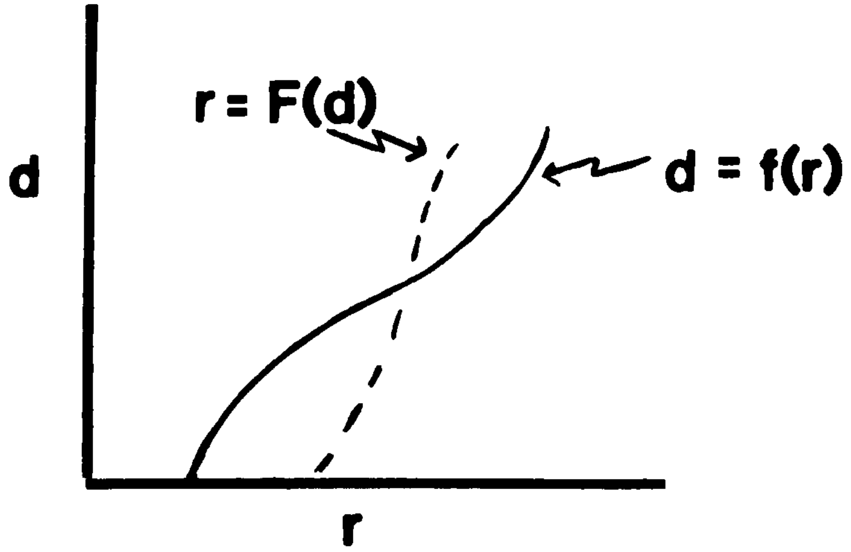

Fig. 6. Circular causality defined over the respiratory variables r (resistance to inspiration) and d (duration of inspiration).

The value of d at which the respiratory system settled down was that value mutual to the two functions. If F was chosen to intersect f at more than one value of d then the system would settle at one of the mutual points or oscillate between them depending on the actual details. Figures similar to Figure 6 are to be seen in Mitchell et al. (1970) and Werner (1978) with reference to temperature regulation, and in Guyton (1981) and Yates (1982a) with reference to the pressure-flow relationships for blood circulation.

Each of the aforementioned instances of the ring principle or circular causality involves two distinct pathways of influence between two variables, x and y. The system in question must satisfy two independent causal laws, one linking x to y and one linking y to x. The real equivalent of the point of intersection of the two functions in the x by y coordinate space is the equilibrium operating point of the system - the only point that satisfies both causal laws. The equilibrium point is not frozen; it can be shifted by changing the system parameters (see Guyton, 1981; Mitchell et al., 1970; Werner, 1977; Zavelishin & Tenenbaum, 1968). In sum, each of these instances of circular causality exemplifies a stable equilibrium state that is achieved without the processes of measuring the istwert of the bounded variable, comparing it to the sollwert, and amplifying the difference to bring about an action that reduces this difference. The processes of measurement, feedback amplification, and comparison that Bernstein takes to be the minimal requirements of self-regulation are not to be found.

3.1.3 INTENSIONAL DESCRIPTIONS AND TELEOLOGICAL EXPLANATION

How general is an interpretation of self-regulation that does not implicate the conventional, intelligence-based, mediating mechanism of cybernetics? Intuitively, a notion such as a referent signal or program, and the related processes of feedback and the like seem to be called for whenever we construct a description of a system (S) such as:

'S prefers (wants, desires, seeks, etc.) G,'

where G can be the value of a property of S, a property of a thing in S's environment, an orientation of S to the layout of the environment, and so on. A statement of the above kind is called an intensional context or description. Basically, it involves borrowing the property of one thing, G, to build a property of another thing, S, viz. 'prefers G'. What is the status of the borrowed property G?

Orthodoxy invariably interprets intensional description as license to ascribe concepts: To predicate of S the property 'prefers (wants, desires, seeks, etc.) G' is to ascribe to S the concept of G. Similarly, in matters of perception, to say that 'S can perceive step-down places' (Section 2.2.3) is to say, by the orthodox interpretation, that S has a concept of step-down places. What is it about intensional description that invites conceptual ascription? Why should the convenience of describing a property of a thing S in terms of a thing G be translated into the claim that S possesses or embodies in some form the thing G? Empirical considerations reveal that the intensional context 'biological system S prefers a body temperature of 37 degrees centigrade' does not mean that the end-state of 37 degrees is encoded in the system's central nervous system and, relatedly, does not identify a relation between S and a central nervous system representation of the quantity 37 degrees centigrade. The lesson of this example is twofold: First, intensional description does not mandate conceptual ascription; and second, intensional may simply be a way of referring indirectly to lawful processes. It seems, therefore, that intensional description will invite conceptual ascription to the degree that a lawful basis for a given regularity is unexplored or indiscernible (Turvey et al., 1981).

Let us turn away from vegetative processes, such as temperature regulation, to the more general case. Consider the following example of a goal-directed activity, to be designated L. A swiftly flying bird suddenly changes its posture, spreads its wings, flaps them briefly, glides, flaps its wings a little more, and alights gently on a branch. A teleological description (Woodfield, 1976) of L reads:

> 'S did B in order to do G',

where S refers to the bird, B to the behavior and G to the goal of alighting on the branch with a minimal transfer of momentum to the bird's interior (Kugler, Turvey, Carello & Shaw, in press). This teleological description of L can be expanded (after Woodfield, 1976) to make the implied internal conditions transparent, albeit in "mentalese" (Fodor, 1975):

> 'S did B because S (i) wanted to do G and (ii) believed that B would lead to G'.

Now we have a teleological explanation of L.

It is very important to distinguish a ring principle (or circular causality) explanation of L from a teleological explanation of L. The ring principle takes G for granted and explains how S gets to G. By definition, a ring principle explanation consists not so much of a single sentence of the type 'S did B (at a particular time, in a particular way, etc.) because ...' but consists, rather, of a set of sentences describing cycles of acting-perceiving-change (in the animal-environment relation). The ring principle explanation of L would be in terms of the reciprocity of the bird's approach and optical flow, with particular emphasis on the decelerative forces supplied by the bird with respect to maintaining the optical flow property dT(t)/dt within the range specifying a "soft" contact (see Section 2.2.2). The teleological explanation of L also takes G for granted, but it explains why S does B. Thus, the two explanations are complementary (Woodfield, 1976). If the processes governed by the ring principle are viewed as dynamical, then the states of S ('wanted', 'believed') are tantamount to field boundary conditions on dynamical processes. The form of these particular boundary conditions is that of non-holonomic constraints (about which more will be said below).

Clearly, the intention (i) in the teleological explanation of L above is Bernstein's 'image of achievement' (Pribram, 1971) that constrains the variations in the content of the belief (ii) until G is done. Recall that, for Bernstein, where actions are planned they are planned in terms of biological consequences (that is, in terms of how an activity will change the animal-environment relation) and not in terms of the pattern of bodily movement (Problem 3 of the introduction). But can the state picked out by the phrase 'wanted to do G' be interpreted as an 'image' in the sense of an actually existing mental or neural representation of G? Woodfield (1976) cautions thusly:

> It is tempting to think of a goal as a concrete future event, and to think of the present desire as involving a conception of that future event, with the conception of the goal being in some sense logically or ontologically derivative from the goal itself. But this is the wrong way round. A goal just is the intentional object of the relevant kind of conception (p. 205).

Let us see what a Gibsonian analysis of G looks like. The goal G in the goal-directed activity L involves two aspects: one, a surface X that can support S and two, a soft, feet-first collision with X. The former aspect defines an affordance, and under the Gibsonian program an affordance is optically specified. That is, the light structured by a branch is specific to the support property of that surface layout vis-a-vis the bird's proportions. The latter aspect, that of the soft collision, is specified by $dT(t)/dt \geq -0.5$. The two aspects of the intention 'wanted to do G' in L might be interpreted in the Gibsonian program as follows: 'wanted to do G' is a matter of having detected information that continuously specified a surface of support and having detected information that continuously specified the intensity of an up-coming collision with that surface, on the occasion of a certain metabolic condition of S.

One should be circumspect about the generalizability of an analysis of the preceding type. Intentionality is a large issue and the reader's favorite example of intentional behavior is probably much more elaborate than L. However, states of affairs such as L are common; they comprise the larger part of an animal's daily directed activities. And insofar as the Gibsonian program can anchor teleological explanations of goal-directed activities such as L in natural laws at the ecological scale, it promises a natural basis for intentionality. Be that as it may, the comparison between the Cartesian and Gibsonian programs on the subject of intentional objects (the goals of goal-directed activities) is sharply drawn. Under the Cartesian program, intentional objects are represented in an internal medium; under the Gibsonian program, intentional objects are lawfully specified by structured energy distributions (Turvey et al., 1981).

3.1.4 COMMANDS AS INFORMATION IN THE INDICATIONAL SENSE

Any disquiet with the concepts of internally encoded required values or intentional objects as representations, extends to the concept of 'commands'. Is circular causality in general, and the perception-action ring in particular, mediated by commands? Although it has been a commonplace to say that the brain commands the body,this way of talking has been subject to little scrutiny. As Reed (1981) has observed, there is an entire theory of action wrapped up in the notion of central nervous system commands and much conceptual effort will be required to unravel it. We will give some hints of what is involved. To anticipate, issues raised in the preceding subsections will make a repeat appearance but in a subtly different form.

The control of activity is founded on information, as both Bernstein and Gibson have sought to understand. "Commands" are a kind of information which can be termed indicational because their role is to indicate an action to be performed (Reed, 1981), much as a stop sign on the highway indicates the action of arresting the forward motion of a car and a directional sign on the highway indicates which turn to make. Indicational information is incomplete. To be commanded to stop one's car is not to be told the details of how to do so. Obviously, the informational basis for controlling activity is not exhausted by information in the indicational sense. To stop the car requires information about when to begin decelerating and information about when the deceleration is sufficient and so forth. This sense of information was discussed in Section 2.2 and in the immediately preceding section. Consonant with the terminology of these earlier sections, we will refer to this sense of information as specificational. The important point to be made is that an indicated act cannot be performed without information in the specificational sense. On generalizing, this point reads: The indicational sense of information is always predicated on the specificational sense of information.

Holding this dependency in abeyance for the present, let us focus on the commonalities between commands - as sources of information in the indicational sense- and rules. Neither commands nor rules can determine an action, both commands and rules can be violated or ignored, both commands and rules can enter into conflict (creating demands for impossible outcomes), and both commands and rules require an explicit act of comprehension for their functioning (Reed, 1981). For these reasons, a lawful determinate account of the control and coordination of activity cannot be founded on the notion of commands or information in the indicational sense. A further undesirable feature is that the criticisms that apply to a body-states or sensation-based theory of perception (see Section 2.0) apply to a command-based theory of action: There is no rational explanation of the genesis of the knowledge that forms and interprets commands. A command-based theory of action looks like another unrepayable loan of intelligence.

The lawful basis of optical structure relevant to activity's control was labored in Section 2.2 in order to make the notion of specification transparent. Where information in the indicational sense is close to the concept of rule, information in the specificational sense is cognate with law. Laws are determinate, non-negotiable (they can never be violated or ignored), harmonious (they can never give rise to impossibilities), and they do not depend on explicit knowledge for their functioning. In the cybernetical interpretation, the ring-principle is mediated by indicators (commands). But it is apparent that this need not be so, for the same reason that mediation by referent signals need not be so. It is an unmediated, law-based interpretation of the ring principle (rather than a mediated, rule-based interpretation) that is the focus of the Gibsonian program (see Section 3.2). A lawful account of the control and coordination of activity cannot be founded on information in the indicational sense but it could be founded on information in the specificational sense.

3.1.5 SYMBOLIC AND DYNAMICAL MODES

The contrast of indicational information and specificational information parallels that of discrete symbol strings and continuous dynamical processes or, equivalently, rate-independent structures and rate-dependent processes. These contrasts are said by Pattee (1973, 1977, 1979) to identify a Complementarity Principle that is the hallmark of living systems. Living systems are seen to execute in two modes, the symbolic and the dynamic, which are incompatible and irreducible. Consequently, understanding biological, physiological and psychological phenomena is said by Pattee to rest with the elaboration of this complementarity. The computational/representational approach to these phenomena that is championed by the Cartesian program is flawed - in Pattee's view - because it attempts to explain only through the discrete symbolic mode. Similarly, in his view, an approach that seeks to explain such phenomena using only (sic) the laws of dynamics will also prove inadequate. By Pattee's reasoning, both modes must be given full recognition; the phenomena in question are the result of the coordination between the two modes. Stated more sharply, complementarity is advanced as a principle that calls for simultaneous use of formally incompatible descriptive modes in the explanation of the characteristic phenomena of living systems (Pattee, 1982).

There is, however, an asymmetry between the two modes that has to be appreciated. Nature uses the symbolic mode - nonholonomic (nonintegrable) constraints - sparingly. Dynamics are used to the fullest, wherever and whenever, to achieve characteristic biological effects. Symbol strings are used, now and then, to direct dynamical processes and to keep down their complexity - in other words, to trim the dynamical degrees of freedom. In Figure 5, which depicts a prototypical program formulation of activity, the opposite strategy is at work. Very many nonholonomic constraints are exploited to achieve ('to explain') the kinetic and kinematic regularities of a locomotory step cycle. The question of how the dynamics - properly construed for the biological scale in terms of the conjunction of statistical mechanics and irreversible thermodynamics (Iberall, 1977; Prigogine, 1980; Soodak & Iberall, 1978) - might fashion the phenomenon is not addressed, nor is the question of how the symbol strings interface with the dynamics. Pattee's analysis is an important one for those students of movement who would pursue the Cartesian program with its emphasis on the symbolic mode: Only in the working out of the physics of a regularity can one identify the nature and type of symbol strings (nonholonomic constraints) needed to complete the explanation. To begin with the symbolic mode, and to adhere strictly to it, invites an account that will be plagued by arbitrariness (as, surely, is the account of a step-cycle represented by Figure 5). To begin with the dynamical mode, and to pursue it earnestly, promises an account that will be principled.

There is, however, a deep problem with Pattee's Complementarity Principle. For Pattee, the discrete symbol strings function as information in the indicational sense. The proposed complementarity, therefore, is one of indicational information and dynamics. The problem with endorsing a view of indicational information and dynamics as formally incompatible is that it rules out any explanation of the origin of indicational information. We and others have recorded our disquiet with the Complementarity Principle for just this reason (Carello et al., 1983; Kugler, Kelso & Turvey, 1982).
One suspects that for the consistency of physical theory, information in the indicational sense should be lawfully derivable from dynamics (or information

in the specificational sense).

3.2 THE RING PRINCIPLE (PHYSICALLY INTERPRETED)

In this final section we provide an overview of the physical foundations of Bernstein's ring principle. Whereas the cybernetical interpretation of the ring principle is consistent with the Cartesian program, the interpretation evolving in physical biology is consistent with the Gibsonian program.

3.2.1 OPEN SYSTEMS AND THE ROLE OF CAUSAL DYNAMICS

According to classical physics, living systems are continuously struggling against the laws of physics. Within the last few decades, however, it has become increasingly apparent that those physical systems that are open to the flow of energy and matter into and out of their operational components behave in a manner which suggests the behavior of living things and suggests a dramatically different view of causal dynamics (see Yates, 1982a,b, for a review). Whereas the behavior of an isolated physical system is strictly determined by the system's initial and boundary conditions, systems open to the flow of energy and matter can evolve internal constraints which 'free' the system's dynamics from its initial conditions. The arising of the new internal constraints serves to limit the trajectories of the internal components, thereby reducing the system's internal degrees of freedom. As these constraints arise, new spatio/temporal orderings are created and the system derives new ways of doing business with its surroundings (that is, new ways of transacting energy).

While living systems can be viewed as following from the laws of physics, one distinguishing characteristic that emerges in systems of this order of complexity is the ability to internally time-delay energy flows. This is accomplished through the maintenance of internal potentials from which the system can periodically draw energy so as to produce a generalized external work cycle. This self-contained source of potential energy (usually in chemical form) allows the system to be characterized as self-sustaining. The ability to be self-sustaining means that the system's behavior is no longer governed strictly by minimum energy trajectories or external work cycles defined on external gradient fields. The possibility now arises that a self-sustained system can temporarily depart from the constraints defined by the external potential minimums. Departures from and returns to minimum regions defined in the external potential field require some form of sensitivity to the gradients; and this, in turn, requires some form of self-sustaining system. The ability to selectively discriminate low order potential gradients (Frohlich, 1974; Volkenstein & Chernavskii, 1978) and the ability to form an autonomous, persistent self-sustaining system (Iberall, 1973, 1977) are fundamental characteristics of living systems.

3.2.2 DETERMINANT TRAJECTORIES IN PARTICLE/FIELD SYSTEMS

Particle physics (classical, quantum and relativistic) studies the trajectories of particles to infer the dispositions of potential fields. The assumption underlying the above strategy is that variations in the observed force field are strictly a function of the particle's position in the field. The above assumption rests on two requirements: (i) that external potentials remain constant (in both space and time), and (ii) that the particle has no internal means for introducing or absorbing forces (which could contribute to a trajectory's departing from the minimum regions defined by the external potential field). Particles satisfying these two requirements have their trajectories completely determined by the form of the external potential field: The minimum regions identify geometrical singularities in a topological field. The particle system is completely determined by and causally dependent on the topological form of the external potential field.

3.2.3 SELF-SERVING SYSTEMS AND CIRCULAR CAUSALITY

If, however, the particle system of interest has an internal means for generating and dissipating forces of a magnitude comparable to the external forces - that is, the system is self-serving - then the behavior of the particle need not be completely determined by the topological form of the external potential field. The particle has available internal potentials that can generate and absorb forces that, when combined with the forces generated by the external potential field, can yield equilibrium states that are not strictly defined by the topological singularities of the external potential field. The behavior of this class of particle system can be said to be *nondeterminant* with respect to its relationship with the external potential field (cf. Kugler, Kelso & Turvey, 1982; Kugler, Turvey & Shaw, 1982). While the particle's equilibrium states are no longer determinately specified by the state of the external potential field, the particle is still, nonetheless, causally coupled to the forces generated by the external potential field. That is to say, changes in the forces generated by the external potential field will require compensatory changes in the particle's internally generated forces if an equilibrium state is to be maintained invariant - the external and internal force systems are causally linked in a *circular causality* with respect to invariant equilibrium states.

The physical concept of circular causality (cf. Iberall, 1977) is meant to identify the lawful nature of the coupling that links the exterior potential field (and its associated force field) with the interior potential field (and its associated force field). Self-serving systems and their associated equilibrium states are lawfully coupled to the external potential field through circular causality; they are systems whose interior potential fields play an active role in fashioning final equilibrium states.

Self-serving particle systems are characterized by low energy couplings that relate the particle's position to its external field. The low energy coupling is defined *relative* to the external work cycle generated by the particle. The coupling defines a ratio of the forces generated by the external work cycle in proportion to the forces generated by the external potential field. A dimensionless number can be used to qualitatively distinguish the nature of the coupling:

$$\text{Pi} = \frac{\text{(forces generated by the external work cycle)}}{\text{(forces generated by the external potential field)}}$$

$\text{Pi} \leq 1 =$ high energy coupling

$\text{Pi} > 1 =$ low energy coupling.

A high energy coupling ($\text{pi} \leq 1$) defines a coupling in which the particle's external work cycle is insufficient to resist the external field's potential gradients. If, however, an external work cycle is generated that resists the external field's potential gradients ($\text{Pi} > 1$), and contributes actively in the organization of equilibrium states, then the coupling can be considered to be of a low energy nature. The low energy coupling realized by a self-serving system forms a lawful basis from which a generalized theory of information can be derived.

3.2.4 INFORMATION AND THE ECOLOGICAL APPROACH TO PERCEPTION AND ACTION

Central to the Gibsonian program is the claim that information must refer to physical states of affairs that are specific and meaningful to the control and coordination requirements of activity (Turvey & Carello, 1981).

Following Gibson (1950, 1966, 1979) the above requirements for information are to be found in the qualitative properties captured in the structured patterns of energy distributions coupling an animal to its environment (see Section 2.2). These patterns (1) carry, in their topological form, properties that are specific to components of change and components of persistence in the animal-environment relation; (2) are meaningful (i.e. they define gradient values) with respect to the animal's internal potentials; and (3) are lawfully determined by the environment and by the animal's movements relative to the environment. According to the Gibsonian program, information is a physical variable that defines a coupling that is specific and meaningful with respect to the changing geometry of the econiche (defined by the animal/environment qua particle/field system totality). The energy patterns coupling the animal (internal potential field) and environment (external potential field) are continuously scaled to the changing parameters and dimensionality of the system (cf. Kugler et al., 1980). The information carried in the evolving geometry of structured energy distribution is information about the animal dynamics (internal potential layout) relative to the environmental dynamics (external potential layout). This concept of information is consistent with Thom's view of information as geometric form (cf. Kugler, Kelso & Turvey, 1982):

> ...any geometric form whatsoever can be the carrier of information, and in the set of geometric forms carrying information of the same type the topological complexity of the form is the qualitative scalar of the information (Thom, 1975, p. 145).

Information as a geometry of form (defined over potential fields) arises as an a posteriori fact of the system. The information can be carried in the form of geometric manifolds which are created, sustained and dissolved within a large variety of physical flow fields. The flow fields can be assembled out of mechanical, chemical or electro-magnetic constraints.

3.2.5 ON THE DETERMINANT NATURE OF INFORMATION AND THE NON-DETERMINANT NATURE OF BEHAVIOR

The goal of physics for the twentieth century has been to understand the nature of the energy states exhibited by particles at all scales of magnitude. The foundation of physics rests on the commitment (explicit or implicit) to natural laws, that is, the commitment to a natural continuity is energy states reducible to symmetry statements (equations) defined on conservations. The strategy for defining natural laws rests on the identification of trajectories assumed by particles. While this strategy has valid application for simple particle systems (non-self-serving systems), its application toward explicating the natural laws governing the energy states of self-serving systems must be seriously questioned. The behavior of a self-serving system is not strictly determined by the energy states of the external potential fields. As noted, the energy states of the internal potential field play an active role in the determination of the observed trajectory. While the behavior (observed trajectory) of a self-serving system has a nondeterminant status, the informational states defining the low energy coupling that relates the external potential field to the internal potential field has a determinant status. The information states are invariant (i.e., stable and reproducible) in the strictest sense of lawful determinism. A physical analysis of the behavior (observed trajectories) of self-serving systems must entail an inquiry into the low energy informational states that lawfully couple an animal (complex self-serving particle) to its environment (external potential field). (For an example of a physical analysis of the role of low energy couplings, see Kugler, Turvey, Carello & Shaw, in press). It can be argued that the goal of a physics befitting Bernstein's physiology of activity is that of identifying the laws that create, sustain and dissolve low energy informational states.

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CHAPTER IVb

SOME COMPUTATIONAL SOLUTIONS TO BERNSTEIN'S PROBLEMS

G. Hinton

INTRODUCTION

Bernstein's great strength was his appreciation of the immense complexity of the apparently simple task of motor control. He lived at a time when movements were seen as "responses", and their structure was generally regarded as a minor matter, of much less interest than their variable relationship to the "stimuli" that elicited them. Against this background, he insisted on analyzing the difficulties inherent in real motor acts. He understood the profound problems caused by reactive forces, surplus degrees of freedom, the unpredictability of external forces, and the need to use sensory feedback to modulate the effects of central commands. One may quibble with some of his solutions, but his understanding of the major problems was far ahead of his time.

In this commentary, I shall focus on three issues that were of major concern to Bernstein. First, what can we infer about the code that the brain uses to communicate with the periphery, and what does that tell us about how the computation is organized? Second, if the brain knew just what movements it wanted the body to make, could it figure out what to tell the muscles in order to make it happen? Third, how is it possible to coordinate a system with so many degrees of freedom that interact in such complex ways? How does the brain make sensible choices among the myriad possibilities for movement that the body offers?

Experiments on people, animals, and robots, and a better understanding of the computational problems have led to interesting theoretical advances in the last few years. I shall concentrate on explaining some of these developments and showing how they provide solutions to some of Bernstein's problems. To simplify matters, I assume a particular task - rapid reaching movements to a visually specified point without visual feedback from the movement. This task is complex enough to contain many of the problems that interested Bernstein but simple enough to be tractable. I shall have little further to say about the many important problems that the task avoids. These include:

1. *Impedance control*: By setting the stiffness of the muscles appropriately, it is possible to control the mechanical response of the body to externally applied forces (Benati et. al., 1980b; Hogan, 1980).
2. *Manipulation planning*: Skilled manipulation involves figuring out what forces, impulses, or movements to apply to objects and how to use the arms and fingers to do it (Mason, 1981; Lozano-Perez, 1979).
3. *Dynamic refinement of motor plans*: Planning is just one component of manipulation. Another crucial aspect is the use of tactile feedback to dynamically refine under-specified aspects of the plan as it is being performed. Bernstein emphasises that this type of close integration of movements and feedback is a central feature of motor control.
4. *Locomotion*: The maintenance of dynamic balance while walking or running is an area where rapid progress is currently being made in the computational theory (Raibert & Sutherland, 1983).

1. THE EQUIVOCAL EFFECTS OF CENTRAL COMMANDS

Suppose you want your finger to trace out a geometrical form in space. To do this you must activate the muscles in your arm so that they generate the necessary torques at the joints (though this may not be the best way to think about it). As Bernstein points out, generating the right torques is harder than it seems for several reasons:

1. The force exerted by a muscle depends not only on its level of activation, but also on its current length and the rate at which it is changing. So neural impulses to the muscles cannot unequivocally specify the forces to be exerted.

2. The magnitude and axis of the torque created by a given force in a muscle depends on the geometrical configuration of the muscle about the joint, and this varies as the joint angle changes.

3. It is hard to know what torques *should* be exerted to achieve a desired effect on a joint angle. The effect of a torque depends on the angular inertia about the joint, and in a moving chain of linkages like the arm, the angular inertia about each joint depends on the angles and *angular velocities* of all the joints. Also torques applied at one joint produce angular accelerations at *other* joints.

4. Even if the torques could be computed centrally and applied unequivocally by the muscles, the problem would still not be solved because small variations in the static or dynamic configuration of the body cause the same torques to produce different effects, and the central apparatus cannot know what the exact state of the body will be when its torque signals arrive there.

Between them, these arguments are certainly compelling evidence against the idea that the central apparatus simply sends out the right force signals to the muscles. They suggest that some more subtle code is used and that some peripheral computation is involved. This is an important discovery. Bernstein, however, seems to draw a further conclusion which is not justified and which hinges on a false assumption and a mathematical error. He appears to believe that the central apparatus is not concerned with torques and that it somehow deals with a different kind of representation that only gets turned into torques at the periphery. I think he would have been firmly opposed to the idea that the required torques are computed centrally and are then encoded in some other form for transmission to the muscles. I also think that this may be what actually happens.

Bernstein's false assumption is never explicitly stated, but is clearly evident in his discussion of the six properties required of every self-regulating system. He assumes that the detection of error and the corrective feedback signal are both neural events. He does not seem to have realized that the complex properties of muscles, which make them respond equivocally to neural impulses, are not a *problem* that makes motor control more difficult but an evolutionary adaptation that makes motor control easier. These special properties allow muscles themselves to act as very fast, negative feedback devices without any neural detection of the error (as will be explained in the next section).

Naturally, neural feedback is very important in real motor acts and the existence of other types of feedback does not mean that we can ignore the complex problem of how neural feedback is integrated into the action. However, physical feedback effects must be properly understood before it is possible to analyze the role played by neural feedback loops in real motor acts, even if these neural loops are very fast (Adams, 1976). I therefore describe in some detail how the physical properties of muscles can be used to simplify motor control.

I shall show that muscles can provide almost instantaneous feedback effects *provided* that the signals sent to the muscles use an indirect encoding of the required forces. Thus the fact that the signals do not directly encode torques (or rather the muscular forces needed to generate them) may not be because the forces are too hard to compute or because evolution could not produce muscles that responded unequivocally to force signals, but because the centrally computed forces need to be encoded in a special way to cash in on a neat trick for getting instant feedback.

When this trick was first discovered it was initially interpreted as a way of avoiding force computations altogether. The feedback provided by the muscles was seen as a way of moving the limbs around without any explicit force computations, but there is now convincing evidence against this interpretation (Hogan, 1982; Bizzi, Accornero, Chapple and Hogan, 1982), and it seems probable that the properties of muscles allow them to produce a very rapid feedback effect, but the forces still need to be precomputed. In general, it is always better to combine a precomputed feedforward component with a feedback loop than to rely on feedback alone. Feedback can only produce forces when there are errors, so big forces require big errors (Horn, 1978). By using feedforward to generate some of the required force, the errors can be reduced to those needed to produce the difference between the precomputed force and the force that is actually needed.

Bernstein's mathematical error is his claim that reactive interactions make the computation of torques exponentially hard. This claim underpins his view that the central apparatus does not explicitly compute torques. He is just plain wrong about the difficulty of the computation. It is not trivial, but it is now known to be quite easy. It has linear rather than exponential complexity. (This is discussed in more detail in section 2). Bernstein can hardly be faulted for his mistake. The easy way to do the computation has only been discovered in the last few years, and even this algorithm is quite complex when compared with the kinds of computation people were willing to attribute to the brain before we had modern computers.

Bernstein's overestimation of the difficulties created by reactive interactions, and his failure to realize *why* the signals to muscles did not directly represent forces appear to have led him to reject the idea of a full internal model of the dynamics of the body, and I think this was a serious mistake.

WHY LENGTH-TENSION FUNCTIONS MAKE GOOD CONTROL SIGNALS

Although it is intellectually easier to imagine that central signals specify forces or torques, it is actually much easier to control a system by sending out signals that specify *functions*. The α-motoneuron inputs to a muscle specify not a force, but a function that relates the length and the rate of change of length of the muscle to its tension, and hence to the force and torque that it generates (Asratyan and Feldman, 1965). From a design standpoint, the use of "length-tension" functions seems like an unnecessarily roundabout way of getting the muscles to generate the right forces. Why didn't we just evolve muscles that unequivocally generate a required force when told to do so?

To understand why it is better to specify length-tension functions than forces, consider the following simple problem: Suppose we have a system with just one joint, and we wish to change the joint angle according to some internal schedule which specifies what the joint angle should be at each instant. If there is an internal model of the dynamics of the physical system, it is possible to compute what torque is required at each moment in order to follow the required trajectory. If there is a torque motor at the joint, all we need to do is to tell it the computed torque. This open-loop scheme is simple but it is not robust. If the internal model is imperfect in any way, the computed torques will not be quite right, and the joint angle will diverge from its desired value. We could add a mechanism that sensed these deviations and modified the torque signals appropriately, but this mechanism would be subject to delays due to the slowness of neural transmission.

Now consider the alternative scheme using "angle-torque" functions. The internal model is used to compute the required torque, as before, but then a further computation takes place. The required torque and the desired joint angle are used to select an angle-torque function which will yield that torque at that angle. Then this function is sent down to an angle-torque motor which combines the angle-torque function with the current joint angle to generate the actual torque. Clearly, if the internal model is correct, this scheme will generate the internally computed torque. But why bother to turn the torque into a function and then convert it back again in the motor? The reason is that this scheme makes the system robust against errors in the internal model.

Suppose the internal model underestimates the physical mass that is being moved. In the simple open-loop scheme, the torque motor will always generate torques that are too small. In the angle-torque scheme, the internally computed torques will also be too small, but when these are combined with the current angle to select an appropriate angle-torque function, the *desired* value of the angle will be used. When the function is turned back into a torque by the angle-torque motor, the *actual* angle is used. So the torque actually generated differs from the computed torque, and it differs in an interesting way. If the physical system lags behind the internal model, the angle-torque function will generate a larger actual torque than was intended. So by using angle-torque functions we get an automatic feedback effect. The discrepancy between the internally desired joint angle and the actual joint angle gets converted into an increment in the actual torque. If the angle-torque function is linear, the mathematics is easy. The torque, τ, can be expressed as:

$$\tau = s(\alpha - \alpha_0) \tag{1}$$

where α_0 is the angle at which this function would yield zero torque, α is the joint angle, and s is the stiffness. (The equation assumes that angles are measured in the opposite direction to torques). The two parameters which can be varied to generate the different functions within this class are α_0 and s. At any particular moment, they must be chosen to yield a particular function which gives the appropriate torque at the appropriate angle. In other words, the function must be selected so that:

$$\tau_c = s(\alpha_c - \alpha_0)$$

where τ_c is the internally computed torque and α_c is the internal specification of the required angle. The *actual* torque generated is given by:

$$\begin{aligned} \tau_a &= s(\alpha_a - \alpha_0) && \text{(2a)} \\ &= s(\alpha_c - \alpha_0) - s(\alpha_c - \alpha_a) \\ &= \tau_c - s.e && \text{(2b)} \end{aligned}$$

where α_a is the actual angle and e is the difference between the actual and intended angles. Thus, by encoding the required torques into angle-torque functions we get an automatic negative feedback term in addition to the computed torque. Moreover, this feedback term does not require any neural sensing of the positional error. It is just a property of the way the angle-torque motor behaves. If the functions are non-linear, the mathematics is more complex, but the actual torque can still be characterized as the computed torque plus a negative feedback term that depends on the positional error.

In equation 1 there are two quite different parameters, α_0 and s which determine an angle-torque function. At first sight this seems redundant, because a single free parameter, α_0, would allow a function to be chosen that would satisfy equation 1 (i.e. would generate the desired torque at the desired angle). However, equation 2 shows why it is advantageous to be able to vary s as well as α_0 when choosing an angle-torque function. The value of stiffness, s, determines the size of the feedback term for any given positional error. If the internal model is fairly accurate, the discrepancy between τ_a and τ_c will be small and so the positional error, e, will not need to grow very big before the feedback term s.e is sufficient to compensate for the difference between τ_a and τ_c. If the internal model is inaccurate a larger value of s.e will be needed. If s was a constant, a bigger feedback term would require a bigger error to generate it. If, however, the internal model is *known* to be inaccurate a larger value of s can be used so that the necessary feedback is generated without requiring large positional errors. This use of high stiffness to overcome the inadequacies of a poor internal model fits in well with Bernstein's observation that when first performing a new task people's movements are very stiff but that they later become more relaxed as they master the task.

Even if there is no reliable information about the inaccuracy of the internal model it is still possible to dynamically optimize the stiffness parameter so that the errors are kept within reasonable bounds with as little stiffness as possible. Houk (1979) has suggested that neural detection of the error in position may be used to control the stiffness. This rather indirect type of feedback has a major advantage over the more conventional idea of using the neurally detected error to directly generate a corrective force. Neural detection and transmission are slow, and by the time an error has been detected and fed back as a correction, the system may have changed enough so that the original error is no longer what needs to be corrected. Indeed, the current error may be in the opposite direction so that the correction only makes things worse (this is the classic cause of oscillations in systems with delayed feedback). By modulating the stiffness of the angle-torque function and leaving the actual feedback to the muscles themselves, these problems are avoided because the feedback is all in the physics so there is very little delay.

There are no angle-torque motors in the body, but by using two or more opposing muscles about each joint, it is easy to implement angle-torque functions provided the muscles exhibit the linear equivalent - length-tension functions. In the simplest case, the relationship between the length-tension functions of two opposing muscles and the angle-torque function they implement is easy to state. Simple length-tension functions have the form:

$$t = s(l - r)$$

where t is the tension, s is the stiffness, l is the length and r is the resting length. If the leverage of the muscle about the joint remains constant, this can be rewritten in angular terms as:

$$\tau = s(\alpha - \rho)$$

where α is the joint angle for which the muscle has lengt l and ρ is the angle for which it has its resting length. The net torque generated by two opposing muscles is therefore:

$$\begin{aligned}\tau_{net} &= \tau_1 - \tau_2 \\ &= s_1(\alpha - \rho_1) + s_2(\alpha - \rho_2) \\ &= (s_1 + s_2)(\alpha - (s_1\rho_1 + s_2\rho_2)/(s_1 + s_2))\end{aligned}$$

This has the same form as equation 2a with s equal to the sum of s_1 and s_2 and α_0 equal to the weighted average of ρ_1 and ρ_2, using s_1 and s_2 as weighting coefficients.

More complex cases in which there are many muscles acting about a joint, non-linear length-tension functions, and varying leverages of a muscle about a joint are trickier, but they retain an important modularity. The required length-tension functions can be computed from the required angle-torque function, and so this function is all that needs to be communicated. Decisions about how to implement it can be made locally.

A further complication is created by the fact that many muscles act about two different joints. This means that the length of the muscle and hence the torque it exerts at any given level of innervation depends on both joint angles. This allows the torque exerted at one joint to be contingent upon the current angle of a neighboring joint, and Hogan (1980) has shown how this property can be very useful for giving the body the right motor impedances.

VISCOSITY PROVIDES A VELOCITY FEEDBACK TERM

The idea that the physics can be used to provide feedback effects is not restricted to positional feedback terms. In many circumstances, it is also helpful to have negative feedback that is determined by the difference between the desired and actual *velocities*.

So far, we have ignored the fact that the force generated by a muscle depends on its rate of contraction. Again, this initially seems to be a complicating factor but actually makes control easier by providing a velocity feedback term. The simple one-joint model illustrates this effect if there is viscosity at the joint (or in the motor). If the internal model of the dynamics of the system includes an accurate representation of this viscosity, it is possible to allow for it in selecting the appropriate angle-torque function. The computed torque then has two components, one to accelerate the system and one to overcome viscous drag. The angle-torque function must be selected to provide the sum of these two torques. Now, suppose the physical system is actually going more slowly than the internal model. There will be less viscous drag than predicted, and so some of the torque that was destined to overcome viscous drag will actually be left over and will go into accelerating the system. This gives a velocity feedback effect. The discrepancy between the actual angular velocity and the intended one gets turned into a torque that reduces this discrepancy. Moreover, no neural comparison of the intended and actual velocity is required. The feedback is generated by the physics.

In the body, there is little viscosity at the joints themselves, but the viscosity in the muscles gives an equivalent effect. From a design standpoint, the viscosity of muscles initially looks like a mistake and one is tempted to see it as a regretable imperfection of the biological hardware. It is true that it consumes energy and it is true that it slightly complicates internal models of the dynamics, but the almost instantaneous velocity feedback term that it provides may make it worth while.

2. COMPUTING TORQUES

An important ingredient of Bernstein's argument for the necessity of co-ordinative structures is his claim that computing torques is extremely difficult because of the reactive forces. This helps support his view that higher levels of the motor system are not directly concerned with torques. The idea is that higher levels use some other code, and the reactive forces are dealt with lower down in the system. This might be feasible if reactive interactions are handled by local feedback mechanisms, but if feedforward components are involved then they are much harder to compute locally because they necessarily involve non-local reactive interactions. Bernstein himself points out the importance of feedforward control towards the end of chapter 4, but he gives no hint as to how it is to be achieved.

Bernstein claims that reactive interactions between different joints in a chain of segments have exponential complexity. In other words, for each extra mechanical degree of freedom, the magnitude of the computation required to figure out the reactive forces is *multiplied* by a constant factor. If this factor is large the problem rapidly becomes intractable. Actually, far from being exponential, the problem is linear. For each new degree of mechanical freedom, a constant amount is *added* to the computation. Bernstein can hardly be blamed for not realizing this, since it was only recently discovered. Over the last few years the complexity of the best known algorithm has fallen from order(N^4)(Uicker, 1965) to order(N)(Luh, Walker and Paul, 1980). This makes it far more plausible that the brain could actually compute the torques required to follow a desired trajectory.

The mathematics used in computing the torques is fairly complicated (Luh et. al., 1980), but the physics underlying it is relatively straightforward. We first convert all the information about the desired trajectory into a single global non-accelerating frame of reference. Then we simply solve the equations of motion for each segment in turn starting with the most distal one. The desired linear and angular accelerations of this segment relative to the global frame are known and they allow us to compute the forces and torques acting at the joint between this segment and the penultimate one. Once these forces and torques are known, we can solve for the unknown forces and torques at the proximal end of the penultimate segment, and so on.

Luh et. al. work with the general case in which sliding joints are allowed as well as rotating ones. If there are only rotational joints, we can further simplify their method by only solving for the torques. Even though the joints are moving through space, at any instant a joint is at a point in space and the angular momentum of the whole of the system distal to the joint about that point in space can only be affected by the torque applied at the joint and by externally applied or gravitational torques. If these latter are known, the torque applied at the joint can be determined from the rate of change of angular momentum of the distal system about the point where the joint now is. Linear forces acting through the joint cannot affect the angular momentum about it, and neither can torques acting at other joints. The rate of change of angular momentum is simply the sum of the rates of change for each of the segments that is distal to the joint. This means that it is possible to solve for the torque at one joint without first solving for the torques at all more distal joints.

CAN THE BRAIN DO ARITHMETIC

The discovery of an algorithm of linear complexity for computing torques is an important advance for robotics, but it is not necessarily relevant to the issue of whether the brain can do the computation. Computers do arithmetic very well. They represent numbers precisely enough so that the progressive accumulation of rounding errors during a long computation does not swamp the answer, and they perform each operation so fast that they can perform long sequences in a fraction of a second. Brains seems to work quite differently (Von Neumann, 1958). They have neither the speed nor the accuracy for the kind of computation discussed above. But they do have billions of processors each connected to thousands of others. If the computation can be decomposed into many pieces that can be performed in parallel, and if each piece has little sequential depth, both the speed and the accuracy problems can be solved. It may be possible to decompose the torque computation into separate pieces that can be performed in parallel for each joint, at the cost of a certain amount of duplication. It would be necessary, for example, to perform separate additions, at each joint, of the rates of change of angular momentum of the distal segments. This inevitably involves adding together the same numbers several different times, and increases the computational complexity from order(N) to order(N^2). However, the increased number of operations is not large, and may well be worth the time saved by the parallelism. The brain has lots of processors, but not much time. Benati et. al. (1980a) present a way of decomposing a similar computation into pieces each of which can be done in parallel by a hardware module that might correspond to a group of neurons.

It is important to remember that the internal computation of the torques does not need to be nearly as accurate as performance measures might suggest. If angle-torque functions are used, the "instant feedback" effect will take care of minor errors. Even rather inaccurate feedforward computations of the torques yield much better performance than feedback alone, so a sloppy internal model of the dynamics is much better than none at all.

A RECONSIDERATION OF THE EQUIVOCAL EFFECTS OF CENTRAL COMMANDS

Given the preceding discussion of the merits of angle-torque functions and the feasibility of computing torques ahead of time, it is easy to see the flaws in the arguments that suggested that torques are not computed centrally. The equivocal response of muscles to neural signals is just right for implementing angle-torque functions, and the optimal choice of these functions requires precomputation of the required torques. The need for feedback is satisfied by the physics of the system, but this does not mean that the torque computations are left entirely to the periphery, because the advantages of feedforward control over pure feedback require precomputation of torques. Finally, the idea that reactive interactions lead to exponential complexity in the computation of the torques is a plausible conclusion, but it is false.

The use of angle-torque functions is a particular example of a much more general principle. If a central controller does not know the precise state of the system it is controlling, or the precise forces that will be encountered, it can either wait for sensory feedback before it sends down commands for action, or it can send down contingency plans and leave the periphery to decide which contingency applies. Angle-torque functions are just contingency plans that have a very compact form. Even though he does not seem to have realized that muscles themselves can decode contingency plans, Bernstein was well aware of the advantages of the contingency plan approach as a way of organizing the interactions between different levels of control in the motor system.

3. THE DEGREES OF FREEDOM PROBLEM

Bernstein, was committed to achieving a mechanistic understanding of human motor control and he realized that the kinds of mechanism available at the time were totally inadequate. He was therefore forced (in the true spirit of articial intelligence) to speculate about possible mechanisms that might be adequate for the task. Unfortunately, he did not have computers to simulate these mechanisms and so they remained vague in many respects.

Bernstein decided that the existence of surplus degrees of freedom in the motor system posed a major problem for any theory of motor control. There are more joints in the human body than seem to be necessary for any one task, and each joint is typically affected by more muscles than seem to be necessary. This creates two kinds of problem. First, it is hard to decide exactly what to do because there are more degrees of freedom in the way the body moves than there are constraints in a typical "motor problem" so the motor problem cannot uniquely specify its solution. Second, even if we could decide exactly what to do with each joint and each muscle, there are still so many of them to be coordinated that the task is liable to swamp any "central executive", especially if modifications are required to cope with unpredictable external forces or the effects of errors in the internal model.

Bernstein proposed that the motor system handles these complexities by using hierarchical coordinative structures. He does not give a very clear statement of the idea anywhere in this book, but it appears to be an example of a way of handling complexity that should be familiar to all computer programmers. Instead of trying to bridge the gap between the "motor problem" and the neural impulses to the muscles in a single span, the gap is progressively narrowed by using a hierarchy of schemas. At the highest level there are schemas that translate the motor problem into terms that are more suitable for the next level down, and so on all the way down to the muscles. At each level, the schemas mediate between the task requirements passed down by the higher level schemas, and the possibilities that are made available by the lower level schemas, given the current state of the motor apparatus. The main advantage of the hierarchical approach is that the higher levels do not need to be concerned with low-level details. The myriad degrees of freedom provided by the individual muscles do not need to be explicitly considered, much as a general does not need to explicitly consider each soldier's actions in planning a battle.

The idea of hierarchical coordinative structures is very attractive, but it is under-specified and there are several different ways to make it more precise. One interpretation is in terms of a qualitative hierarchy in which each level deals with a different type of entity. Examples of possible types are hand-positions, joint-angles, torques, and muscle activations. A different interpretation of coordinative structures is discussed and criticized later.

A FOUR-LEVEL HIERARCHY

Hierarchical decomposition is an excellent way to deal with complexity when the task can be naturally divided into a number of levels, and decisions can be made at one level with little or no consideration of the level below. Saltzman (1979) has discussed possible levels at length, and makes finer distinctions than the ones which follow. Four good candidates for natural levels in motor control are:

1. *The level of the motor problem.* The planning activities that govern deliberate behavior may give rise to a sequence of relatively well-specified and self-contained motor problems such as "reach out to object A" or "grasp object A". This would allow the planning routines to ignore the details of the movements (though they would have to be sure that they were feasible).

2. *The level of movements of the body.* It may be possible, in solving a given motor problem, to decide how the body should move without explicitly considering the torques required to implement the movements. Similarly, decisions can be made about how the end-effectors should respond to external forces (Mason 1981) without considering the angle-torque functions at the joints that are needed to implement the desired motor impedance of the end effectors.

3. *The level of torques.* The torques required to cause a desired movement can be computed without explicitly considering how the muscles are going to implement these torques. Similarly, the angle-torque functions required to implement the desired impedances of the end-effectors can be computed without considering how these functions are to be implemented.

4. *The level of muscle innervation.* This is the bottom level of the hierarchy, and motor control is considerably simplified if the required muscle properties are only computed after the required torques or angle-torque functions have been decided.

In this modular decomposition, as in most modular schemes, there are various intrusions of lower level constraints on higher level modules. Certain movements, for example, are impossible because the muscles are not strong enough. More importantly, the efficiency or accuracy of certain movements depends on the magnitude or rate of change of magnitude of the muscle forces required for the movement. So the choice of a good movement appears to depend on considerations that are two levels down in the hierarchy. However, the modularity of the levels can probably be saved by incorporating, at each level, heuristics for making choices that will be easy of efficient to implement at the next level down. The heuristics contain *implicit* knowledge about the level below, but they are phrased in the terms of the level they are at, and are thus easy and fast to apply at this level. A concrete example of such heuristics is given below.

In chapter 4, Bernstein argues against the idea that a motor problem like reaching to a particular point in space is solved by computing a spatial trajectory. If he is right, the separation between levels 2 and 3 above may well be wrong, but this argument appears to be flawed. It rests on the observation that there is considerable variation in the trajectory used from trial to trial, but very little variation in the endpoint. This observation certainly rules out any model in which the knowledge of where to reach is encoded as a spatial trajectory, but is does not rule out models in which the spatial trajectory is computed afresh on each trial. Minor variations in the rules used for forming the trajectory could easily give rise to a family of slightly different trajectories that all had the same endpoint. (Slight variations in the coefficients in the model of reaching described below would have just this effect).

Minor variations in the spatial trajectory from trial to trial, would be very helpful in discovering a trajectory that not only solved the problem, but did so with the least effort or greatest accuracy. They would allow changes in the trajectory formation rules to be correlated with measures of performance like energy expenditure, jerk, or accuracy. In this way it would be possible to progressively improve the trajectory formation rules so that they gave rise to trajectories that had good dynamic properties, even though the formation rules did not mention torques or forces explicitly. This is an example of the idea of using heuristics at one level that *implicitly* contain knowledge about lower-level considerations such as torques and forces.

The idea of qualitatively different levels is only one of the ways in which hierarchical structure can be applied to motor control, and although Bernstein clearly distinguished some of these qualitative levels, his notion of coordinative structures seems to have been far richer. Several people, influenced by Bernstein, have proposed a view of coordinative structures which emphasizes the role of higher level schemas in constraining the possibilities at the next level down (Greene, 1972; Turvey, Shaw, and Mace, 1978).

Underlying this view is the belief that surplus degrees of freedom make motor control *harder*, and that they therefore need to be removed by imposing extra constraints. This may be true if control is performed by a sequential central executive with limited resources, but there are parallel, distributed forms of computation which are not hampered by surplus degrees of freedom. Indeed, if the right kind of computation is used, extra degrees of freedom make motor control *easier*. The next section briefly described an iterative distributed computation that coordinates many degrees of freedom in satisfying two goals simultaneously. A simplified task has been used to illustrate the style of computation, but the same style would work for more complex, three-dimensional tasks with more degrees of freedom.

4. A DISTRIBUTED PROGRAM THAT REACHES AND BALANCES

Consider a two-dimensional puppet composed of six segments as shown in Figure 1. The foot always remains fixed and is regarded as the proximal end of the puppet. The joints can all move between limits that are roughly appropriate for a person, and each segment has a roughly appropriate mass. Given a starting configuration and a desired position for the distal end of the lower arm (the tip), the task is to compute a set of joint angles that puts the tip at the goal position, puts the center of gravity vertically above the foot, and is as similar as possible to the initial configuration.

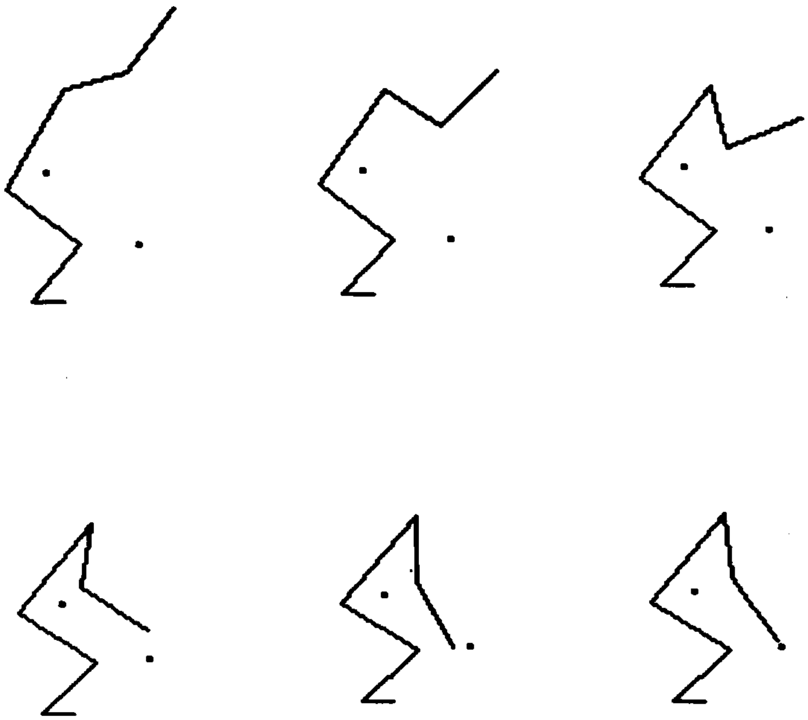

Fig. 1. This shows a sequence of configurations generated by the iterative algorithm. One of the black dots represents the center of gravity of the whole stick-figure, and the other represents the goal to be reached. The configuration is shown on every second iteration. The reason for the overshoot is that in addition to the computed joint increment, half of the previous increment is also added. This smoothes out oscillations and thus allows bigger co-efficients to be used without causing divergent oscillations. Extra processors which control several joints at once were used in this example. These extra processors are described later in the text.

REACHING ALONE

To begin with, let us ignore balance and just consider the problem of getting the tip to the goal. A simple physical analogy suggests a way of organizing the computation. Suppose we took a real pin-jointed puppet and connected the tip to the goal with a rubber band. The tip would move towards the goal and the surplus degrees of freedom would not cause any difficulties. Moreover, the method would work perfectly well if some of the joint angles were temporarily frozen.

It is relatively easy to *approximately* simulate the physics using a distributed computation in which there is one processor for each joint. The rubber band (the "desire vector") generates a torque about each joint that is given by:

$$T_j = d.r_j$$

where d is the magnitude of the desire vector and r_j is the perpendicular distance from the j'th joint to the line along which the desire vector acts. The joint angle is then incremented by a small amount that is proportional to the torque. To ensure that light segments move more easily than heavy ones, the increment , ΔA_j, is also proportional to the moment of inertia, I_j, of the distal portion of the system about the j'th joint:

$$\Delta A_j = k_r.T_j.I_j \qquad (3)$$

where k_r is a constant that determines the size of the increments used in the reaching computation.

On each iteration, three types of computation occur in sequence:

1. The processor for each joint computes a new joint angle. To do this it must have access to the global desire vector, the line along which the vector acts, the position of the joint in space, and the moment of inertia of the distal portion of the system. The new joint angles can all be computed in parallel.

2. Given the new joint angles, the new positions of the joints in space can be computed by starting at the foot and following the chain of segment-lengths and joint-angles. Once the position of the tip is known the new desire vector can be computed.

3. Given the new joint positions, the new centers of gravity and hence the new angular inertias of the distal portions about each joint can be computed by starting at the tip and adding in the segments one at a time.

Provided k_r is sufficiently small, the combined effect on the desire vector of all the separate joint increments will be approximately the same as the sum of the effects of each increment by itself. All of the interactions (i.e. cases in which changes in one joint angle alter the way in which the desire vector is affected by changes in another joint angle) are mediated by the process of updating the positions of the joints in space, because the angle at one joint determines the positions of more distal joints and hence it determines the magnitude of the torque exerted by the desire vector about those joints. (The torque is a measure of the amount the desire vector would be changed by incrementing the joint angle).

This simulation glosses over all the complexities of the dynamics of a real physical system. It is actually a faithful simulation of a bizarre system in which inertial forces are negligible compared with viscous ones, but the viscosity at a joint is always set equal to the angular inertia about that joint.

BALANCING ALONE

If we ignore the reaching problem and just try to find a balanced configuration in which the center of gravity of the whole system is above the foot, a very similar computational scheme can be used. Instead of a global desire vector, there is a global measure of the extent to which the system is out of balance. This is simply the horizontal distance of the center of gravity from a vertical line through the center of the foot. Each joint angle can be incremented, in parallel, so as to reduce this quantity and then the new joint positions and new centers of gravity can be computed just as in the reaching algorithm.

Each joint controls the position of the center of gravity of the segments that are distal to it. The extent to which an angular increment to the j'th joint restores balance is determined by the mass, M_j, of the distal portion and also by the horizontal distance that its center of gravity moves. The horizontal distance moved is proportional to the vertical distance, V_j, between the joint and the center of gravity. So to help restore balance, the angular increment at the j'th joint is given by:

$$\Delta A_j = k_b . V_j . M_j / I_j \qquad (4)$$

where k_b is a constant and I_j is its moment of inertia of the distal portion about the j'th joint.

COMBINING REACHING WITH BALANCING

To find a configuration that satisfies both the reaching and balancing goals simultaneously, the increments required for reaching and the increments required for balancing can simply be added together on each iteration. The relative values of k_r and k_b determine the relative importance of satisfying the two goals. If these values are set appropriately, joints near the foot are primarily influenced by the balancing goal because they control a large distal mass (M_j in equation 4), whereas joints near the tip are mainly affected by the reaching goal. Figure 1 shows a typical sequence of configurations generated by this simple parallel algorithm.

INADEQUACIES OF THE MODEL

There are many criticisms of this simple computational model:

1. It gets stuck at local optimum, and so it can fail to find a suitable final configuration even though one exists. This is an important limitation of this kind of iterative parallel computation and it suggests that any computation of this type would need to be combined with more qualitative, schematic knowledge which could specify, very approximately, which region should be searched in the space of possible configurations. The use of schematic knowledge resembles table look-up, but it differs because the iterative computation used to compute the precise details of the final configuration is more powerful than simple linear interpolation between table entries.

2. It does not take obstacles into consideration. Obstacle avoidance requires an understanding of the space occupied by the body, and it also requires a more global view of the trajectory. The kind of myopic computation in which the current configuration is gradually changed into the desired one cannot deal with obstacles properly because it cannot see far enough into the future to avoid cul de sacs. An altogether different style of parallel computation may be required to avoid obstacles.

3. It is not parallel enough. Steps 2 and 3 in the computation seem to be inherently sequential. Actually, at the cost of some extra computation, they can be made parallel. If, for example, the orientations of the segments relative to the global frame of reference are stored and updated at the same time as the joint angles, it is possible to compute the new position of a joint in space simply by adding together the vectors for all the more proximal segments. This addition can be performed in parallel. The extra cost is that the same vectors get added several different times in computing the positions of different joints, and when a joint processor decides to update its joint angle it must also increment the global orientations of all the segments distal to it. This destroys the purely local communication structure in which each joint-processor only communicates with the processors for adjacent joints.

4. It takes a large number of iterations in certain situations. A simple example is shown in Figure 2a. The problem is that the line of action of the desire vector passes almost exactly through the shoulder and elbow joint. This means that the torques about these joints are very small so the joint angles are only changed very slightly on each iteration. It is obvious to us that the way to reach the goal is to shorten the arm by flexing it, but there is no explicit representation of the length of the arm in the parallel computation and so this insight cannot be incorporated. The next section shows how the introduction of higher order variables that coordinate the activities of several neighboring joints can overcome this problem and can dramatically reduce the number of iterations required.

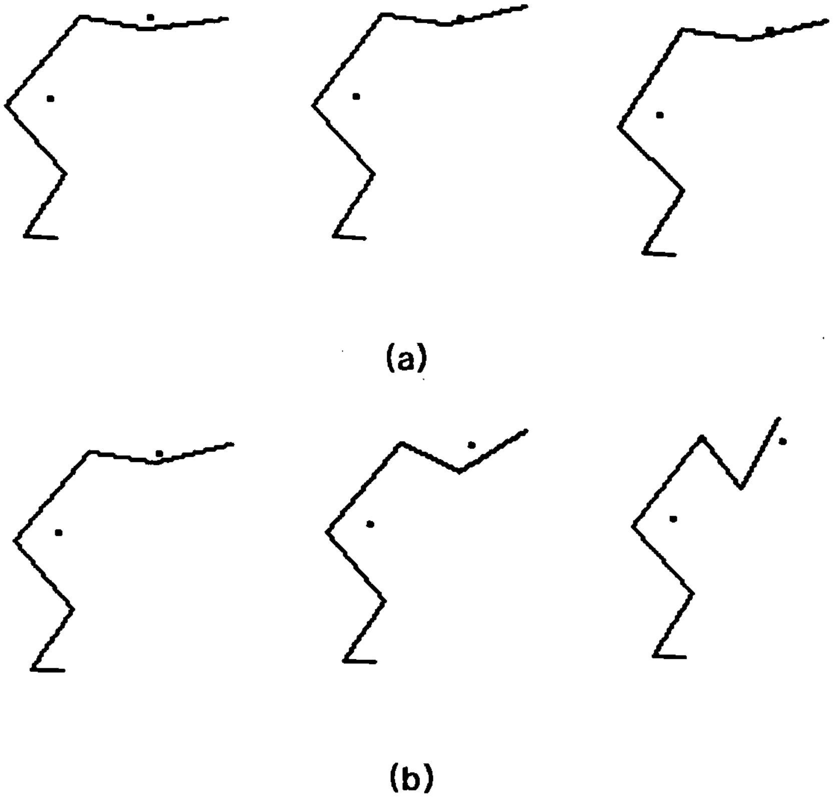

Fig. 2. (a) shows how the simple iterative algorithm fails when the line of action of the "desire" vector passes close to the joints that need to be changed. The configuration is shown on every third iteration. (b) shows how the behaviour is improved by adding a synergy that controls the shoulder and elbow angles together and is invoked when the desire vector aligns with the direction from the tip to the shoulder. The configuration is shown after every iteration, so a single iteration with the synergy produces more progress than six iterations without it.

SYNERGIES

Changes in the angle of the elbow change the length of the arm ,but they also have side effects. They make the hand move along a circular arc centered on the elbow, and they change the hand's orientation in space. To make the hand move in a straight line away from the shoulder that cancel out the side-effects of the changes at the elbow. If the upper and lower arm segments are the same length, this can be done by increasing the elbow angle by 2α and decreasing the shoulder and wrist angles by α.

In cases like the one shown in Figure 2a, the iterative computation can discover the need for compensating changes at the shoulder. It first changes the elbow angle, and this changes the line of action of the desire vector so that it no longer passes directly through the shoulder. The desire vector then generates a torque about the shoulder which causes a change that compensates for the side-effect of the change in the elbow. However, this method of generating side-effects and then figuring out how to get rid of them is not nearly as efficient as avoiding unwanted side-effects by using "synergies" - combinations of changes whose side effects cancel.

Synergies are easy to incorporate within the parallel iterative framework. In addition to the processors for the individual joints, we simply add a processor for each synergy. This processor knows about the combined effect of a number of changes, and it is invoked whenever that effect is desired. When it is invoked it attempts to change the joints it controls in the appropriate way. Since a given joint may be under the control of several synergies as well as having its own processor, some principled way of combining different influences is required. As with the coordination of reaching and balancing, the correct combination rule is simply to add together the changes requested by the various synergies and by the joint processor itself.

Figure 2b shows how reaching is improved by adding a synergy that controls arm length by coordinating changes at the shoulder and elbow. The synergy is invoked by the degree of alignment between the desire vector and the vector from the shoulder to the distal end of the arm. If the angle between these two vectors is ϕ, the changes requested by the synergy on the current iteration are:

$$\Delta A_{\text{elbow}} = 2.k_s.\cos(\phi).I$$

$$\Delta A_{\text{shoulder}} = -k_s.\cos(\phi).I$$

where k_s is a constant that determines the relative importance of synergies as compared with the primitive changes computed by the individual joint processors, and I is the effective inertia.

ADDING NEW SYNERGIES

The simple additive rule for combining the changes requested by the various synergies makes it very easy to add new ones. They can just be thrown into the pool of existing synergies. Provided each synergy has its own processor, and provided the basic joint processors can perform the necessary additions of the requested increments in parallel, additional synergies do not increase the time required per iteration.

THE FUNCTION OF SYNERGIES

The use of higher order synergies to speed up a parallel iterative search is quite different from the use of higher order schemas to eliminate surplus degrees of freedom by imposing constraints on lower level variables. In the parallel iterative scheme, there may well be a hierarchy of synergies, each influencing the values of lower level ones, but every synergy in the hierarchy has access to the global goals (e.g. the desire vector and the measure of imbalance). Because surplus degrees of freedom do not present a problem to the parallel computation, there is no need to place each lower order variable strictly under the control of a single higher order one. All the processors for all the different levels of variable can compute in parallel, and every synergy can be activated to the extent that it helps reduce the difference between the current state of the system and the desired final state.

5. THE ROLE OF FEEDBACK IN MODEL-BUILDING

In the preceding sections I have described some computational advances that help to solve some of the problems that Bernstein raised. This final section contains meta-level comments on the role of feedback in model-building and on a new source of potential feedback for theories of human motor control.

At the start of chapter 4 there is a long discussion of feedback from historical and philosophical perspectives. Bernstein points out (as Gibson did in the field of perception) that most of the interesting issues in motor control were ignored for many years because of the laboratory study of artificially simple situations. This is a familiar modern view which is probably as uncontroversial now as it was radical at the time. His philosophical views, however, are more questionable.

Bernstein correctly points out that internal symbolic representations can be objectively correct, and that they do not require the implicit internal observer assumed by idealists (see Dennett, 1978, pp 89-108 for an excellent recent discussion of this issue). Bernstein also points out that the development of a valid internal model is aided by two-way interactions with the domain being modelled. This is a point of great practical significance, but Bernstein tries to elevate the point to a philosophical principle that feedback of the effects of actions is *essential* for objective knowledge. This is a far more dubious propositionel. If it is true astronomers are out of luck.

Returning to the practical significance of feedback, recent technological developments have created the potential for a new source of feedback about the adequacy of theories of motor control. Progress in robotics clearly yields insights that supplement the experimental data on biological motor control, but there is always the worry that many problems in robotics may be the result of using conventional digital computers and may have little relevance to biological computation. The fundamental differences between the architecture of a general purpose computer and the architecture of the brain have led some people to suspect that computation may work in very different ways in these two types of machine (Von Neumann, 1958; Hinton & Anderson, 1981). Even though general purpose computers can be made to simulate any other kind of computer, there is still the suspicion that the sensible way to do a particular computational task in a conventional computer may be quite unlike the sensible way to do it in the brain.

Until recently, computers were very expensive to design and very expensive to build. This meant that they had to be general purpose, because the cost of designing and building dedicated hardware for a specific task was prohibitive. It was much more cost-effective to use a general purpose computer which was tailored to a specific task by its program. Now, however, very large scale integration makes computers cheap to build, and advanced computer aided design makes it relatively easy to design novel hardware structures. This opens up the possibility of tailoring the hardware to specific tasks, and thus avoiding the sequential bottleneck that is imposed by insisting on the architecture of a general purpose machine. It is now possible to design and produce a chip with hundreds of simple processors all computing in parallel and all communicating directly with specific other processors. The sequential execution of stored instructions is no longer necessary, and it may be possible, using dedicated hardware, to perform computational tasks hundreds or thousands of times faster by making good use of the parallelism (Mead and Lewicki, 1982). There is just one snag. There are very few good ideas about how to organize parallel computations using a hard-wired network of local processors each of which is much simpler than a general purpose computer. Perhaps this will be an area where theories of human motor coordination can suggest practical hardware designs, and where experience with these designs can provide useful feedback.

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SECTION 5

Chapter V **TRENDS AND PROBLEMS IN THE STUDY OF INVESTIGATION OF PHYSIOLOGY OF ACTIVITY**
N. Bernstein

Chapter Va BERNSTEIN'S PURPOSEFUL BRAIN
J. Requin, A. Semjen, M. Bonnet

Chapter Vb TOWARDS A HOLISTIC CONCEPTION OF MOVEMENT CONTROL
L. Pickenhain

CHAPTER V

TRENDS AND PROBLEMS IN THE STUDY OF INVESTIGATION OF PHYSIOLOGY OF ACTIVITY

N. Bernstein

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New concepts and problems which have arisen out of the development of cybernetics have powerfully seized the attention of physiologists. The overwhelming majority of these problems appearing in our physiological journals indicate a desire to establish a *rapprochement* between the current state of cybernetic theory and the attitudes and achievements of classical Russian physiology. This forces us to consider the history of the subject intently and thoughtfully. It seems, however, that the uninterrupted development of strictly materialistic concepts and points of view which are our heritage from native physiologists of the schools of Sechenov, Pavlov, Vvedenskii, Ukhtomskii and others depends on their further integration into the contemporary body of biological science. It is now more important to look ahead, and to attempt to discern, though perhaps only in the most general terms, *problems* (though still remote from a solution) deriving from the new facts and concepts, and some new *directions* which still need to be carefully considered, but which, it seems, may allow us to attain the discovery of new and wide horizons. We may justifiably relate the study of the *physiology of activity* to these new directions of investigation.

I

The classical physiology of the last hundred years is characterized by two sharply defined features. The first of these is the study of the operation of the organism under quiescent inactive conditions. Such conditions were artificially secured wherever possible by decortication, by anaesthetization of the animal, or by fixing it in a stand under conditions of maximal isolation from the external world. This analytical approach to the study of quiescent conditions derived from the attempt to study every organ and every elementary process in isolation, attempting to exclude side effects or mutual interactions. In general terms this approach corresponded to the predominance of mechanistic atomism in the natural science of that era. The absolutism of that point of view led to the conviction that the whole is always the sum of its parts and no more than this, that the organism is a collection of cells, that all behaviour is a chain of reflexes, and that a sufficient acquaintance with the individual bricks would be enough for the comprehension of the edifice constructed from them.

The second characteristic feature consisted of the concept that the organism exists in a state of continuous equilibrium with the universe surrounding it, and that this rigid equilibrium is maintained by means of appropriate reactions, unrelated to each other, and made to each successive stimulus impinging on the organism from the surrounding environment. The whole existence and behaviour of the organism was visualized as a continuous chain of reactions on the stimulus:response model (nowadays we would talk of input and output). The standard of the classical materialistic physiologists was the *reflex arc*, and their central aim was the analysis of regularities in *reactions* considered as rigidly determined input-output relationships.

The general technico-economical conditions maintaining after the First World War sharpened interest in the *working* condition of the organism. New branches of applied physiology arose - the physiology of movements (here, and from this point on, we shall use this term to indicate goal-directed motor acts and not insignificant fragments of movements such as the withdrawal of a paw because of pain, or the knee-jerk reflex). If this department of physiology has not been given sufficient weight and attention in the pages of the older physiology of rest and equilibrium, its preeminent importance now began to be apparent. In fact movements are almost the only expressions of the life activity of the organism. Movements are the means by which it does not simply passively interact with the environment, but actively *acts upon it* in whatever way is necessary. Sechenov had already indicated the general significance of movements in his remarkable book *Cerebral Reflexes* one hundred years ago. If we add that time has since revealed the integral nature of the participation of movements in all acts of sensory perception, in the education of the sense organs during early childhood, in the active development of an objectively consistent *world image* in the brain by means of the regulation of perceptual *synthesis* through practice, it is easy to understand the displacement of the centre of gravity of interest which is becoming increasingly felt in contemporary physiology.

The progressive growth in the complexity and power of technical devices has demonstrated very clearly that problems of control and regulation form an independent area of study - a study which is in no way less complex, important or comprehensive than that of the energies which are subordinated to these controls. The problem of the rider has begun to overshadow the problem of the horse.

Analogously in physiology, beginning with a study of the energetics of the operating (working) condition of the organism (gas exchange, the control of secondary systems which do not directly participate in external work - breathing, circulation, sweating, etc.) interest has gradually begun to centre on the more comprehensive problems of the regulation and central control of the activity of living organisms.

II

For further discussion it is necessary to pause briefly on one particularly important feature of the control of motor acts which has been established with complete objective reliability for both animals and men[*]. This is the fact that the relationship between the *result* - for example, the movement of a limb or of one of its joints - and such commands as are delivered to the musculature from the brain through the *effector nerves* is very complex and non-univocal[†]. This absence of univocality is a result of the fact that muscles are elastic belts (imagine the connecting rod of a steam engine replaced by rubber, or by a spiral spring) and the effects of their action on the organ which is moved depends essentially on the position and the particular velocity of that organ at the beginning of the

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[*] For a bibliography enabling the reader to become better acquainted with experimental and clinical data see the author's paper "Immanent problems in the theoretical physiology of activity" in the textbook *Problems of Cybernetics*, publ. 1961, and the journal *Voprosi Psikologii*, No. 6, p. 70, 1957.

[†] The indicated relationship between muscular excitation and the resulting movements is very different from the picture which was confidently described by physiologists of the last century (Bekhterev, Munk, etc.). It then seemed natural to treat the motor area of the cortex as a sort of keyboard on which somebody's hand, in sovereign control, described the program for a given motor stereotype. The pressing (excitation) of one of these cell buttons always brought about a given degree of flexion at a given joint, the pressing of a second brought about extension, etc.

muscular activity. This absence of univocality is further explained by the fact that, as in any physical multijointed pendulum, involved and complex involuntary forces (*reactive forces*) arise in a moving limb. The lack of one-to-one correspondence between messages to the limbs and the actions produced at particular junctures is finally also due to the fact that any meaningful movements overcome external forces which are quite beyond the control of the participant (gravity, friction, the opposition of an adversary, etc.) and which cannot be predicted by him. It is clear that organisms, whose only channels of operation upon the surrounding world are commands given to their muscles, may achieve controlled movements serving a particular purpose only by means of continuous monitoring and control achieved by the participation of the sense organs. The physiology of the remarkable and varied signalling equipment with which the organism is equipped, located in the muscles themselves, in the joints, and in the observation post of the body - the head incorporating the distance receptors of vision, hearing and smell - is now well understood. This apparatus provides for perfectly accurate and continuous *circular control* (i.e. feedback control) and revision (i.e. correction) of movements, even of movements comprising such complicated multiple-jointed structures as our limbs, and effected by the non-rigid muscles which drive them[*].

It is possible that these complex dynamic features of motor acts which have just been described are the reasons for yet another characteristic feature appearing most clearly in habitual movements; that is, the remarkable structurality and completeness of a motor act, which makes it impossible to treat it as an arbitrary collection of successive reflex elements (see Chapters II and III). We will not dwell on this feature at present. It is more important to turn to another set of characteristics of goal-directed movements which are still very far from clear, and which still require careful investigation. The analysis of the peculiarities of this group of properties allows us to find a bridge between the *physiology of reactions*,

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[*] Here we have a typical case of *control by means of negative feedback*, a device which is widely used in simple forms in modern technical devices. We may incidentally note that this principle of negative feedback control has been observed by physiologists in recent years to operate in the most various forms of control exerted by the organism - the pupilliary reflex, regulation of blood pressure, of the heart-beat, of chemical equilibrium, etc. It is now beyond any doubt that the most general and prevalent form of organization in live organisms is not the reflex *arc*, but the reflex *ring*.

with which psychophysiologists have been exclusively concerned for some time, and *the physiology of activity*.

III

What is the standard invariable determinant of this involved structurality of motor acts which we have just discussed? We cannot suppose *effector commands* to be such a standard determinant. These commands are emitted into a system involving at least two types of independent forces (reactive and external forces), they act upon the organ through a non-rigid musculature, and they must also vary between very wide limits in order to accommodate to signals coming from the sense organs. These afferent (incoming) signals also cannot act as standard determinants, because signals giving the degree of match or mismatch between movement and effect can only be as variable as the cues which provide them, and, more importantly, the information which they contain is a description of "what is" and not of "what must be done". The brain mechanisms by which the signals giving the progress of a movement, are coded and deciphered, and the degree of mismatch between emitted commands and the operation of the required muscle at the required time, also cannot be considered for this role, because they are necessarily just as variable and non-standard as the codes which they transform.

All our long experience in the study of motor activity, of motor habits and of clinical disorganization has demonstrated with great clarity that the standard determinant both for the programming of motor activity and its effection and correction by feedback connections can only be the formation and representation of a *motor problem* by the brain in one way or another. The analysis of this concept, and of the wide circle of relationships and facts which it entails, will be the particular business to which the remainder of this chapter is devoted.

If we do not, for the present, avoid simple commonplace terminology, then the sequence of the arousal and realization of any action of the class of so-called voluntary movements may be represented in the form of successive stages.

1. Perception, and the necessary evaluation of the *situation* and of its bearing on the individual caught up in it.

2. The individual determines in what way it is necessary to alter this situation; what, by means of his activity, the situation *must become* instead

of *what it is*. The motor problem has already appeared at this stage. It is not difficult to guess that this motor problem must contain more information than is included in the bare perception of the situation, some of which is at least partially not present in the latter. Animals in a herd, or people in a crowd, may be confronted with the same situation, but the motor behaviour of each individual will be different. Examples of this may be readily found.

3. The individual must next determine *what* must be done and
4. *How* it must be done, and what are the available resources.

These two micro-stages already represent a program of the solution of the problem, and after these there follows the process of its actual solution in terms of motor activity. It is scarcely necessary to emphasize that the control and evaluation of successive moments of the actual activity, the variability of the situation itself, with the fact that it is, generally speaking, possible to program only rather roughly movements which have some duration in time, all explain the adaptational variability of programs and of acts, permitting changes ranging from small corrections to widespread alterations in strategy.

It would be false to suppose that the micro-stages in the transition from the situation to the act which have been described above are found only in highly organized nervous systems. The same stages must also necessarily be found in such primitive acts as, for example, the pursuit of live prey by predatory fish. In this case we also have a situation which is perceived in the necessary form and measure, and a motor problem with a program for its solution. The precise way in which either of these aspects of the process is coded in the nervous system of a predatory fish is quite unknown to us, but it is beyond doubt that neither consciousness nor a particularly high level of nervous organization is necessary for them to take place.

Concerning the topic of the higher information content of the problem in comparison with the actual perceived situation, we must add the following. From the point of view of the dependence of the actions of an organism on the stimuli which provoke them, or on the input in general, we may draw up an imaginary series in which we may rank all actions (confining ourselves here to the actions of human beings) according to their degree of dependence on such activating stimuli. At one end of the series we have movements which can be fully explained in terms of the stimuli which activate them. Among these we have all the so-called unconditioned or innate reflexes. We may also include all reflexes conditioned during life experience which are nevertheless dependent on the activating stimulus - reactions of the general class of *conditioned reflexes* found in humans and animals alike.

We may place next in our rank order, movements for which the stimulus or signal continues to play the role of an activator, but which have a meaningful content that is increasingly independent of the stimulus. For motor acts of this class the activating signal increasingly takes on the features of a trigger signal, analogous to the pressing of a button which sets in motion the whole complex process of firing a rocket, or to the interjections "hep" or "arch" after which follow sequences of activity which are very little related in significance to these interjections. Finally, at the other end of the series, we find acts for which the activating or triggering signal does not play a decisive role, and in which it may be entirely absent. These are actions for which the program, and also the initiative, are entirely determined within the individual, and which, with greater accuracy, may be described as the operations to which the term "voluntary movements" may properly be applied. It is not difficult to see that a progression along our scale coincides with the gradual shift from passive acts, to acts having an ever-increasing degree of active involvement*.

IV

Now we shall see what can be said at present about the necessary concomitants of every act of translation of the perceived situation into a motor action - of the phenomenon which would be called "looking forward" in Chinese, and which in more scientific terms may be called *extrapolation to the future*. Indeed, planning a motor act (irrespective of the way in which it may be coded in the nervous system) necessarily involves the recognition in some form of the pattern of what must be, but is not yet, the case. In a similar way to that in which the brain forms an *image* of the real *external world* - an image of the factual situation at a given

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* Conditoned reflexes must be referred, not to the intermediate class, but to the first of the types described here, as in the case of established and differentiated conditioned reflexes a stimulus fulfils not only a *releaser* function, but also acts in an informational way, completely determining whether it will be followed by an appropriate act, by differential inhibition, or by an act of another type, etc.

moment, and of situations which have been exprienced in the *past* of which we have impressions in our memory - it must possess to some degree the capacity to form a representation of (or, what is the essence of the matter, to plan in advance) situations which are as yet unrealized, and which the biological requirements of the organism impel it to realize. Only such an explanatory image of the necessary future can serve as a basis for the formulation of problems and the programming of their solutions. It is certain that this image of the future is qualitatively very different from images of either past or present reality but, as the ensuing discussion will show, the possibility of its existence in some coded form in the brain (which need not in the least involve subjective consciousness), both in animals and in men, does not admit anything which is methodologically undesirable.

In a number of cases similar to the illustration provided and, it is possible, in experimental situations also, this "looking into the future" which we are discussing is accessible both to introspective observation and to chronometric measurement. We have, in the first place, cases where the program for meeting the motor problem is formulated as a physical code which is accessible to the sense organs. A musician playing by sight or any one of us reading a text aloud, advances his gaze some interval ahead of the notes or words sounded at any given moment. This is to say that both acoustic and psychomotor images of what is to be realized by motor means within a second, or fraction of a second, must be present in the brain all the while. It is possible, in this connection, to carry out an illustrative experiment on oneself. Try, without hurrying, to declaim to yourself (in the same way as reading to yourself silently) any piece of poetry which you know well by heart. Mentally listening to yourself doing this, you will clearly perceive that two texts pass before the inner ear: one text in the tempo of the mental declamation, sometimes accompanied by lip movements which are often intense enough to be perceived, and a second which runs ahead, easily outstripping the first text, as if you are preceded, stanza by stanza, by some internal prompter. I have no doubt but that psychologists can suggest many more successful and fruitful types of demonstration for this phenomenon.

There is one very idiosyncratic group of phenomena, which have been observed on more than one occasion, indicating that the image of the future discussed here is not only limited to effects on the course of the programming of motor acts but may in certain cases possess a great physiological reality, as if this foreshadowing of the future already gave it an existence in the present. I have in mind sources of the *emotion of fear*, an experience which can be aroused solely on the basis of a clear image of the imminent future in the brain. In both ancient and contemporary scientific literature we encounter a whole series of descriptions of *death from fear* - so severe may be the autonomic-vegetative shock which is produced by this coded image. In literature we encounter examples of this in Gogel (*Vyi*) and Edgar Allen Poe (*The Fall of the House of Usher*).

In order to approach an experimental investigation of those principles by which the image of the future, in the widest sense of this term, may be coded in the brain - that is to say, the nervous processes serving as the basic guiding compass of the organism in all its behavioural manifestations - and its essential difference from the image of the present and immediate past, we must begin with a small theoretical discussion.

Let all elements of a given set E (of any number of dimensions) be related in terms of a determinate law to the elements of a second set J so that each element of the second set will be brought into correspondence with one or more elements of the first, and vice versa. We will refer to the set E in this case as the *primary image*, to the set J as its *reflection*, and to the principle or law of connection as the *law of reflection, or projection of* E *on* J. The elements of the primary set may (all or partly) be time functions. The elements of the set J would then be functions of time in the same way, and with them, the set J as a whole. Even the laws of reflection may be functions of time, and in this case the reflection J will be doubly variable in time for both these reasons. As physical examples of systems of the type described we may take the optical image on the retina of a human being watching the movements in a street from a motor car. The continuously occurring changes in this image will be due both to the motions of the objects in the street, and to the changes in the law of optical projection on the retina of the eye because of the motion of the latter.

It is difficult for anyone who is not mathematically sophisticated to visualize the enormous number of possible laws of reflection amongst which we may search for the true law or laws of the formation of the image of the real external world in the brain. Meanwhile, repeated lack of information on theories, because of which the enormous number of such laws are naively reduced, has already frequently led both physiologists and clinicians to erroneous conceptions, sometimes to harmless errors, but often very far astray from a correct comprehension of the essence of the matter.

A characteristic example of such a discarded misunderstanding, is the problem of how we see objects in the external world the right way up in spite of the inversion of the visual field on the retina. This question attracted serious attention, and several hypotheses were put forward to describe a reinverting mechanism. The error in these explanations of the mechanism responsible for reinversion lay in the assumption of the separate existence of "percept" and "observer" in the brain. The former was in some way supposed to be reproduced element for element from the evidence available in the optical projection on the retina. The latter in some way perceived or saw this image, in a similar way to that of the subject seeing the external world. From this, it was a small step to postulate that this observer had his own visual area.

The error which has just been described has now already been exposed, and it has been mentioned only because it is instructive. Another important error is still alive and we must consider it.

V

The genesis of the concept of one-to-one correspondence between the cell network of the cerebral cortex and the elements of the perceptual fields of the organs of sensation - vision, hearing, touch, muscle and joint receptors (proprioception) - is not difficult to determine. An enormous amount of material gathered from the experimental physiology and pathology of the brain from 1870 onwards (the date of the first discoveries in this area) indicated beyond doubt that not only was it the case that each type of sensation has its own corresponding separate *zone*, but that within these zones there was some sort of precise correspondence with the elements of the peripheral territory of a given sense organ. This correspondence was more or less highly differentiated depending on the particular zone involved. Among these projection areas of the cortex the most detailed appeared to be those subserving touch and proprioception, in which it was actually possible to trace a sort of cartographical projection (accurate, however, in very general features only) of the sensitive surface of the entire body.

The dynamic variability of the information from the sense organs, changing from moment to moment as it reached the primary projection areas of the cortex described above, forced investigators to postulate the existence of secondary zones alongside them which would subserve the functions of retention (memory) of sense impressions transmitted by the primary projection areas. It was assumed that these secondary zones had the same point-to-point projection characteristics as the former.

In order to turn now to the analysis of the error of atomism which is inherent in these postulates, which clearly was in great part a result of inadequate information in the field of projection theory, and which was discussed above, we must again make a small digression.

It is not difficult to demonstrate by means of two or three extremely simple visual examples that there may indeed exist pairs of sets such as project fully upon each other, but that while each of them is easily and completely divisible into its component elements, a relationship in terms of projecting elements, such as described above for the projection of the type E on J, leads to an obvious absurdity.

A blueprint for a machine is made up of a thousand lines. In order to make this machine the craftsmen must carry out a thousand operations. Does this mean that the matching of every line demands a separate operation?

I make a statement containing 1000 words. My opponent entirely refutes my case, also in 1000 words. Does it follow from this that each of the words which he uses refutes one of the words in my statement?

Or finally, 1000 people read a book of 1000 pages. May we understand from this that the first reader reads page 1, the second page 2, and the hundredth page 100?

The technique of *reductio ad absurdum* is often a very useful device for the detection of an erroneous line of thinking.

Armed with the preceding examples let us turn again to the discussion of the basic position of the theory of the secondary fields, after which we may attempt an approach to a theoretical generalization of what has been said above.

What do we feel, touch or perceive with each of our organs of sensation? Things, objects. What are the elements of the multitude of environments which we encounter? Objects. What must be the elements for a cerebral projection of the type E on J in the corresponding cortical systems and in their cells? It is clear that it must be objects which figure here, both as separate reflections by the brain of the elements of the external world and as the stimulus signals for reactions to it.

If the erroneous nature of discussions of the brain image in these terms is not already clear enough from what has been said, we may proceed further along these lines.

A type of brain function which is the monopoly of man alone is articulate *speech*. We already have a collection of representations of the external world in the form of the first signal system. But words are the names of objects, so that it is necessary to add yet another projection in terms of elements arising out of this signal system, and we will obtain a second signal system, so to say, a projection of a projection, in which every object signal, image element of the first system will have its corresponding name in the second. Clearly, atomism has a very powerful hold upon the imagination if there still exists such an interpretation of the second signal system as reduces speech, the inexhaustibly powerful tool of thought, which no less than the hand makes a man a man, to the level of a dictionary of terms for concrete objects in the nominative case singular. Neither space, nor the aims of this statement, allow us to develop the theme we have touched upon. We shall accordingly limit ourselves to two or three questions. (1) In the second signal system what exactly are the signals for which such elements as "again", "twice", "*y* function", "without", "indeed" and "or" stand? (2) How and where in the atomistic second signal projection system are such signal words as "you think, he does not think, we will think, you would think, they will not think" incorporated into its structure? (3) Is it perhaps better not to discuss at all such verbal signals as "wave function", "quaternion", "antinomy", "transfinite"?*

An example of the same category of erroneous connections between two sets which are indisputably divisible in their elements, and also indisputably related to each other, is found at the boundary between psychiatry and neurosurgery. That is, the theory of psychomorphologism (now exposed as an error, and discarded) which classified and subdivided elementary psychic functions and the symptoms of their deterioration in

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* The most convincing example of the inconsistency of the second signal system theory arises out of a practical problem. It must be pointed out that it has been quite impossible so far to use the results of many years' attempts to delineate the second signal system as algorithms for machine translation, whereas a true physiological theory of speech and language ought to play a leading role. A comprehensive analysis of the essence of this argument and the reasons for it are given in L. Uspenskii's *Word about words*, Ch. 6, p. 283, Molodaya Gvardia, 1960. Also in O. Kulagin's paper "On the operators describing the algorithm of translation" in the textbook *Problems of Cybernetics*, 2nd. edn., p. 289, 1959.

relation to their connection with determinate locations in the brain. In this case also we can trace the confusion to the same error, which, in reference to the first of the examples of absurdities which we provided, we may term the error of the "blue print and the machine".

VI

Now, to turn from illustration to generalization: let us suppose that there are connections between the elements of the set E, which in one way or another join these elements into subsets by determinate laws, or even that we ourselves impose such ordering laws on the set as divide it up into families or subsystems of elements. The simplest example of an operation of this sort is the relation of a system of coordinates to a plane or a spherical surface. It is possible to give many examples of such sets in which an ordering of this type is not imposed from without, but exists within the set itself, and it is only necessary that the system should be observable and describable.

Let us now suppose that the elements of a given set M are related, not to separate elements of E (as was the case in the projection of E on J in terms of sub-elements), but to whole subsystems of its elements - representing their systematic ordering. In cases of this type we will describe the set M as *a model* of the set E, and the principle of the given ordering and relationships as *the operator of modelling*.

It is not necessary to emphasize how varied the forms and principles of modelling may be. In some cases, to every function of the families making up the set of the first order E there corresponds a determinate in M (termed in the functional). In other cases the operator of modelling determines an ordering or a grouping of functions of the primary set, in series, either continuous or discrete, so that such a series answers in M a certain type of a "function of functions". The very forms of selection of representative functions from the first set may be qualitatively very different from each other. The representation in the model may include exhaustively all the endless multiplicity of systemal functions which cover the first representation, or they may select discretely among them (for example, selecting only integer numbers as values for their parameters, etc.). This may also relate to each element or any constellation of elements in the model - discrete function axes of the first representation E - incorporating probability determinations of the strips of territory along them, etc. It is now necessary for us to formulate some basic points emerging from all that has been said so far, and which directly bear on our theme.

In the first place, describing this widening of the limits of the principle of representation, we may affirm with confidence that in the brain a *representation* (or representations) of the world is *constructed along the principles of a model*. The brain does not receive an impression of the external world in the form of a passive inventory of elements, and does not employ such primitive means of subdividing the world into elements as first come to mind (phrases for words, and plans for drawings), but applies to them such operators as most accurately model the world, casting the models in the most consistent, exact and comprehensive forms. This process, or act, of mentally modelling the world is, under all circumstances, undertaken *actively*. In reality if the principles of analysis, systematization and reflection of the set by the system are applied to the primary image by the brain itself, this process of the formation and use of operators is active by its very nature. And if the regularities in the internal ordering of the set E are inherent, then it is only possible to note them, recognize their significance, and employ them in the capacity of operator principles by means of active observation and investigation.

Something more may be said about the general characteristics and properties of active operational modelling of the external world.

Imposing on the first representation E one or other systematic regularity, or formulating a regularity which is already observed in the first representation, the brain brings some degree of additional information to the task, and by this means the information we obtain from the first representation is quantitatively extremely economized, but enriched in its meaningful content. It is possible to compare this additional information introduced from within to an enzyme, a small quantity of which, secreted by the organism, produces the optimal conditions for the digestion of a large quantity of a food.

It becomes clear in passing that the arguments hypothesizing an internal isomorphic representation of the sensory periphery (the retinae, skin, etc.) are false in principles, and cannot be accepted as circumstantial evidence for the existence of the observer in the brain whom we described above. The consequent duality of the observer and the percept, which alone of all schemes requires this detailed isomorphism, becomes superfluous and unnecessary from the point of view of active operational modelling. A model does not contemplate anything which confronts it from without, but is a coalescent indivisible unity of processes and mechanisms transforming received information, which constantly changes without losing its continuity, or unity, and directs the course of the active behaviour of the organism.

The phenomenon of looking into the future which has already been mentioned as the basis for every motor problem (or, as we may now say, for every *model of the future*) forces us to recognize that in the brain there exist two unitary opposed categories or forms of modelling the perceptual world: the model of the past-present, or what has happened and is happening, and the model of the future. The latter proceeds directly from the former, and is organized in it. These are necessarily distinct from each other, chiefly because the first type of model is single-valued and categorical, while the latter can only operate by means of extrapolation to some or other *degree of confidence* or probability*.

Problems related to the model of the present lie beyond the scope of this paper. Many psychological investigations have already been undertaken along these lines. We shall limit ourselves to a single example which may incidentally indicate the great practical importance of explanations of the nature of operators and operational models of the objects of the perceptual world. This example concerns the processes underlying the perception of *configurations*.

The visual image of a circle has five degrees of freedom (or represents

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* It is interesting to note that the two aspects of models of the surrounding world co-existing in the central nervous system are very clearly connected localizationally to different parts of the hemispheres. The neurosurgical clinic brings evidence of the contrasting forms of disturbances resulting from the lesions in the posterior and the anterior parts of the cerebral cortex. Lesions of the lower parietal fields, surrounded with the primary and the secondary zones of principal categories of reception (vision, hearing, touch), bring with them all sorts of disturbance in the simultaneous perception, in the ordering of objects and movements *in space*, briefly, in the structural synthesis of sensory information which are all the essential features of a model of factually existing reality. On the other hand, in the clinics it is demonstrated that with prefrontal and frontal lesions there are various types of loss in the planning and programming of active behaviour, in ordering the links of the chain of a motor act *in time*, in the chief premise of every form of activity, the so-called orientation reaction - that is to say the orientational prognosis of surrounding events and changes. All these forms of nervous activity indisputably belong to the domain of modelling of the future in the sense discussed above.

a five dimensional continuum) in terms of the multiplicity of its optical projections on the retinae. A triangle has six degrees of freedom, while the projection of the letter *H* has twelve, and the projections of some of the other letters of the alphabet have even more. This does not, however, prevent the operator processes in the brain (although their structure is quite unknown to us) from correlating the enormous variety of such optical projections and (we may suppose) of cortical projections, with a single meaningful code of symbols. There can be no doubt that when it is possible in the future to build a machine modelling this process which will recognize *letters* irrespective of their sizes of type faces, it will surely operate not by means of passive *scanning* (as do present experimental examples), but solely in terms of the cerebral principles of operator modelling, when these are understood.

VII

That important form of cerebral modelling which was only recognized by investigators after the arousal of interest in the physiology of activity - that is, *the modelling of the future* to which we now turn - is logically possible only by means of *extrapolation* from whatever the brain is able to select from the current situation, from the fresh traces (see Chapter IV) of immediately preceding perceptions, from the entire previous experience of the individual, and finally from those active trials and assays belonging to the class of actions which have so far been summarized briefly as orientational reactions and whose fundamental significance has certainly been underestimated.

The complex of nervous processes which makes up a model of the future is so unclear and enigmatic that very little can be said about it. Apart from the indisputable statement that such a complex exists, and plays a most important directional role in the active perception of the surrounding world, as has been described above, we may make the following observations.

In sharp distinction to the model of the present the model of the future has *a probabilistic* character. The anticipation or expectancy of the possible outcome towards which the current situation is moving is only possible by means of extrapolation and never, generally speaking, can be brought to a categorical result. At any phase of this process the brain is only in a position to survey a sort of table of probabilities for possible outcomes.

Outcomes	A	B	$C...$	$M...$	X	Y	
Probabilities	P_A	P_B	$P_C...$	$P_M...$	P_X	P_Y	$\Sigma_\rho = 1$
Motor problem	0	0	0...	1	0	0	

It is hardly necessary to make the point that, in the interests of analysis, we have restricted ourselves to a very simplified schematization.

Meanwhile, the motor problem which the individual determined for himself is formulated as a cetegorically unique outcome of the current situation, whatever its *a priori* probability may or may not be in the table (even if it is equal to zero). In this way the organism's activity is directed against the probabilistic model of the future, and the determination of problems that arise is the dynamic struggle of the individual to raise the probability P_M of the desired outcome until it reaches unity, or becomes an accomplished fact. This struggle implies the reduction of the probabilities of all other outcomes to zero. It is clear that this struggle must result in the lowering of the entropy of the system involving the individual and his immediate environment, that is to say, this must always be a process endowed with *negative entropy*.

The struggle described above takes place in a complex field of conditions with a multiplicity of variables. In the first place, the extent to which the brain is able to make successful extrapolations, and its estimation of perspective and of possible outcomes must necessarily be very approximate. (It is, however, probably no more approximate in relation to the requirements of a given living creature, in the case of extremely elementary organisms, than in the case of creatures with very highly developed brains). The coarseness of the possible extrapolation must necessarily increase with the interval of time, Δt, over which the organism attempts to exercise foresight. In the second place, the success of the extrapolation also depends on the term which the subject sets for its completion. If conditions are rapidly changing, and the organism is involved in time trouble, it may be obliged to limit itself to primary, coarsely exploratory techniques and responses, since it does not have sufficient time for more accurate ones. However, even when caught up in the toils of a threatening situation, where time is important, the selection of strategies of behaviour always involves a choice between responses which may be rapidly effected, though these may also be less accurate, and slower responses which have been more reliably evaluated. Thirdly, and finally, the field of conditions encountered by the organism is itself variable in time, and is both dependent on, and independent of, the activity of the individual, so that the organism is, in fact, constantly involved in a sort of conflict situation with the environment. It is already apparent from the foregoing that the evolution of Theory of Games is of great importance to the physiology of activity.

Among the related questions which require the combined attention of physiologists and mathematicians is the problem of the forms of extrapolation which are employed by the nervous systems of organisms of high and low development, and the particular mechanisms which they employ to achieve this guidance. Considering the lower, purely biomechanical, types of regulation which antecede a particular action by a minimal period of time, we apparently encounter extrapolation of the same type as that incorporated in a Taylor series with the use of two primary derivatives as information, that is, data from the joint and muscle signalling systems. (This is sometimes described as gradient extrapolation). Considering more complex and meaningful types of plans for movements, such as may require reprogramming during their course, the higher co-ordinational brain systems and the synthetic processes involved will be found to include forms of probabilistic extrapolation among their equipment, and these will doubtless include just such methods of active sampling as have been formulated and described in the contemporary mathematics of estimation as methods of non-local search (46). It is necessary to emphasize the decisive difference in principle between the appearance in the physiology of activity, of extrapolational search which has been described above, and the concept of trial and error described by the behaviourists. The latter indicates a sequence of attempts, each of which is unrelated to preceding ones, and is, like them, made at random. In this case it is only the external form which is active, the sum of trials being essentially treated as a passive statistical computation of successes and failures. Figuratively, we may say that each trial of this sort gives information of the type "this is not the way", but gives no information as to where or how to do "what is necessary". It is no accident that this principle is easy to imitate in machine models. On the other hand active non-local search, which is apparently a real component of orientational behaviour, after the first couple of attempts are either made at random or else directed in some approximate way by elementary mechanisms working on the principle of gradient extrapolation, results in the deduction of how and where the next step must be taken. In this way each attempt renders more accurate a progress towards the optimum means by which the maximum amount of the most useful information can be obtained.

VIII

What is known at present in experimental physiology about the manifestations and effects of this model of the future, and by what experimental techniques may these best be described in the light of exact modern knowledge? Let us turn again to the consideration of concrete examples of movements.

Among the multiplicity of functions of the central nervous system involved in the control of motor acts, the first to be explained are the processes by which movements are corrected while they are in progress. This is achieved by a system of feedback connections served by the numerous informational sources available to the body. The mediation of this uninterrupted system of correction is a most important biomechanical premise for the production of any purposeful motor act: the mastery of the enormous number of degrees of freedom possessed by our motor organs, and their conversion by these means into a directed system. This function may be regarded as the technical aspect of *motor co-ordination*.

During the course of the co-ordinational process (considered in micro-intervals both of time and of the path of the movement) one characteristic peculiarity of all excitable organs plays a decisive role. All such organs possess *finite* and also *variable* values of *thresholds of arousal*. The absolute values of these thresholds are remakably varied for different organs, and for each particular organ they may further vary between wide limits, depending on overall physiological conditions.

This is a circumstance which has very important consequences for the control of motor acts. The central brain systems organizing and co-ordinating motor acts, primed by the wealth of information from sensory sources, are enabled to do more than correct such disagreements as may arise between intended and actual movements *post factum*. Proceeding with a determinate program of operation, the central nervous system can, and indeed does, achieve *anticipatory adaptations* in terms of the tuning in advance of the arousal of all the sensory and motor elements which are employed.

These interesting but still barely investigated examples of regulation *ante factum* outstrip, as it were, movements by micro-intervals of time, and are closely bound up with the mechanisms of anticipation and extrapolation which have been discussed above. They have been described under various experimental conditions, and by various investigators, now as neuromuscular *tonus*, now as physiological *sets* and, in recent years, as functions of the reticular formation of the brain, although a growing amount of evidence suggests that we are here concerned with the same wide range of interrelated factors. In terms of contemporary electrophysiological techniques these processes of anticipatory adaptation can only be observed with some difficulty and as disconnected manifestations. It is naturally most convenient to observe them *before the beginning* of a movement, when the weak bioelectrical manifestations of tonic commands are not masked by the far more powerful potentials accompanying muscular activity. These tonic impulses, which precede the beginning of a movement, are the neuromuscular concomitants of *sets*. Improvements in the techniques of recording bio-electrical phenomena in nerves and muscles will make it possible to study these processes of set (switching processes in Lapicque's metaphor) during the course of the entire motor act.

The most interesting problems in this area, which have only just begun to be considered, are naturally related to the central nervous regulation of processes which involve set. Some of the problems encountered in this area bring us once again, and in an unexpected way, to the central problem of the cerebral representation of reality and of the types of coding which the brain imposes on its evidence. We approach this topic a little indirectly.

Physiologists have distinguished for some time between two very different forms of arousal process which exist concurrently in the neural and muscular substrate. One of these forms, which appears to be more recent in terms of evolutionary history (it may properly be called neokinetic), is manifested as a rhythmic sequence of bursts of excitatory impulses (sometimes called peaks, or spikes) following the all-or-none law. (That is to say, they have the same height, whatever may be the strength of the supraliminal stimulus impinging for the given variable degree of their excitability). These impulses travel at considerable speeds (of the order of tens of meters per second), and are transmitted without damping along nerve fibres. Because these impulses are transmitted over the entire course of the reflex ring through fibres enclosed in isolating myelinated envelopes with dielectric properties, the neural impulse codes running along adjacent fibres in a nerve do not suffer from mutual interference or leakage. This allows us to regard them as a *"channelized"* form of nervous process.

A second manifestation of nervous activity in neurons and muscular units, which is much more ancient in terms of its appearance during phylogenesis (it may be called palaeokinetic) has retained, in man and the higher mammals, the monopoly of the control of the smooth musculature in internal organs, and has also taken over the role of the tonic transmission of adaptational impulses to sense organs and effector apparatus, as discussed previously. These impulses differ sharply from neokinetic phenomena. Firstly, they are dosable, that is, they do not obey the all-or-none law; secondly, they have two types or signs of significance - or, in other words, they may promote either excitation or inhibition. Thirdly, their activity is not explosive - that is, instead of discrete peaks occurring at millisecond intervals they exhibit slow waves of various forms and heights. Finally, their most typical property lies in the fact that the dielectric coverings of the fibres *do not constitute obstacles* for their passage, so that they, or at least their major components, are able to spread *across the fibres.* For these reasons it is more correct to regard this form of nervous activity as being of *wave form* in distinction to the channelized form of neokinetic impulses. This latter property does not have much practical scope within the comparatively narrow peripheral nerve fibres. However, in the main brain mass itself we may say with some certainty that it is precisely these processes, penetrating considerable masses of brain tissue, and even the carapace of the skull, which are nowadays accessible to electroencephalographical investigation, being frequently called, in loose terms, *cortical bioelectrical currents.*

It is important to bear in mind that the neokinetic chains of impulses in the cortical neurons, being channelized and rigidly isolated from each other, cannot escape from their channels and appear on electroencephalograms (E.E.G. records). It is again unnecessary to make the point that encephalograms are quite unrelated, either in terms of their frequency or their overall form to chains of impulses of the all-or-none variety such as are observed in nerve fibres, and that they do not also represent the result of the superimposition of patterns of such activity upon each other. A whole range of clinical observations, particularly of cases of abnormal E.E.G.s accompanying pathological conditions, leaves little doubt that wave-form processes in the cortex play a certain important role in the regulation of channelized impulses. It is more than probable that this regulational activity is bound up with the function of the *reticular formation* of the brain, as we remarked earlier. We shall not consider this aspect of the problem any further - it is currently under extensive and successful investigation by experimentalists and clinicians. We may now choose another line of approach to the problem.

IX

During the last 200 years the history of the science of brain function has undergone wide oscillations between two opposing points of view as to the relationship of these functions to the substrate of the brain. After the work of Flourens and his contemporaries in the first half of the 19th century, the final triumph of the anti-localizational point of view appeared to be assured - that is to say, there was a wide recognition of the wave type of brain processes, acting on an undifferentiated substrate. The discovery of the projection zones of the cortex (in about 1870) swung the pendulum sharply in the opposite direction. In particular, the accumulation of an ever-increasing amount of information about the primary projection areas led to the intensive development of all the theories of cellular centrism with which we have been earlier concerned in this chapter. The cortex began to be regarded as a highly differentiated receptacle solely for channelized processes taking place in neural conductors (axons) with corresponding cortical cells acting as trigger-buttons for actions in the motor sphere, and as storage receptacles for the acquisition of experience of the environment in the perceptual areas.

The narrowness of these concepts was so clear that even within the memory of the living generation of scientists, in the thirties of this century, the "extreme left" of antilocalization again raised its head (the schools of Lashley, Paul Weiss, et al.), attempting to demonstrate the concept of mass action of the cortical cells and to shift the centre of gravity of the study of nervous processes towards a search for specificity among the codes of impulses transmitted along the nerves in the brain mass itself.

There is now no reason to decide the question in terms of either of these extreme cases - to put the problem on an either/or basis. Although the enormous mass of data now available provides solid arguments in favour of both points of view, it would be useless to bring them to discussion, or to seek for reconciling solutions. It is time that the following was clearly understood.

The high degree of differentiation of the cortical substrate, particularly in the higher animals, is now beyond question. However, just because of this great conglomeration of active excitable neural elements packed in electrolytic substance, there exist all the nessecary conditions for the development of wave processes acting transversely to neural paths, and involving the interaction of very large numbers of these elements. It would further be difficult to deny that the greater the degree of morphological localizational differentiation and subdivision of the cortical substrate, the more favourable are the conditions for an intensive development upon it of nonlocalized wave processes. Any electrician who is concerned with alternating currents and fields will confirm that it is a real problem to protect, by means of shielding, the function of the aggregate apparatus from the effects of mutual inductance and capacitance between its components. This must naturally apply in a far greater degree to the extremely complex living ensemble within a fluid electrolytic mass, where properties and charges vary both as functions of time and of the co-ordinates of each of their points.

It would be incorrect to visualize wave processes in the cortex as macroscopic fronts which are comparable in extent with the size of the entire skull. On the contrary, in correspondence with the microscopic non-homogeneities of the brain mass, its variable electrical parameters, and the momentarily altering pattern of its potentials, one must regard these processes as having a very delicate lace-like spatial and temporal structure. Such fluctuations in potential which can be recorded through the skull as the E.E.G. are naturally no more than a fused "hubbub".

It is hard to doubt that the wave processes which are composed of innumerable transverse interactions between the neurons and conductive pathways of the brain are not dominated in some way by the tonic regulationary activity of the reticular formation, and possibly also of the cerebellum and of the cellular centres of the brain stem. In these areas there is certainly infinitely more to be learnt than has been investigated.

The whole of this discussion points to the necessity for the consideration by physiologists of the study of central-nervous processes as an indivisible synthesis of channelized and wave-form components, and this once again, and for the last time, brings us to the problem of models of brain action, and to the question of the separation of the observer and the percept.

Cellular centrists have always diligently avoided the problem as to what exactly may be said about the cells of secondary projection areas in respect to the content with which they are supposed to be entrusted. What (besides the indeterminate chronic excitation postulated by the conditioned reflex school) is imprinted upon a cell which must store for months and years the images of chairs and lamps, and hypotenuses, or the terms for these things? If these contents are represented by the brain in the form of corresponding codes, then what determines the selection of an appropriate non-occupied storage cell for this code, and in what form is it retained?

The problem of the form taken by informational codes employed by the brain, and of their storage in the mechanisms composing memory, is still far from a solution. It is, however, necessary to approach it in terms of the most modern concept.

The falsity of the view of the opposition of the observer and the percept in the brain has already been emphasized. Instead of the passive expectation of information by an observer we now visualize active operators co-operating in synthetic and dynamic ways to capture information, and the modelling of preformulated and anticipatory actions. We now also expect to encounter, in place of stationary cells which select, and in some way store, microscopic atoms of a representation of the world, dynamically synthetic neural processes which are simultaneously multiply channelized and wave-like in form, and which we have hardly begun to consider in this way. There now appears to be more evidence for the view that the distinction between the cells of the brain (supposed initially to be empty and undifferentiated from each other), and the externally introduced and alien meaningful content, is just as inaccurate as the distinctions drawn between the internal observer and percept.

If every active process of perception and action is represented in the brain by the formation of a corresponding operator, then the most probable form which the latter may take is the formation of a determinate kind of contour, resulting in a new path for the circulation of both channelized and wave-form processes - a contour, the existence and characteristics of which are not determined by the nature of some hypothetical content of the cells, synapses, interstitial tissues, etc., but by the very dynamic form of its organization and connections. To put the matter as briefly and schematically as possible, we may say that the meaningful content and adequacy of a given portion of the model of the environment does not lie in what is or is not contained in it, but is no other than this operator, in the sense in which this term has been defined here.

The present account of the directions and problems confronting physiology does not pretend to fulfil the functions of a program, and so is not an exhaustive survey of the problems which exist in this area. The problems of the *affective motivation* of voluntary actions and of the physiological relationship between affective activity and its conscious intellectual forms has also been entirely neglected. Further, it has also not been possible to include details of processes in which negative entropy is clearly expressed, such as in the development and growth of organisms, beginning at the stage of the impregnation of the ovum and the coding or modelling within it of the *future* organism which will grow out of it. We have neglected the cardinal problems of structuring, expressed in terms of the qualitative and quantitative interaction between schemas and (metrical) forms in the processes of growth and activity. Nevertheless if, within the range of problems on which this account touches, we may provoke ideas, crucial objections or counterarguments which are important for future investigation, the purpose of this account will have been fulfilled.

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Human Motor Actions – Bernstein Reassessed
H.T.A. Whiting (editor)

CHAPTER Va

BERNSTEIN's PURPOSEFUL BRAIN

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INTRODUCTION

Having been invited to play somewhat the role of a referee for a scientific book which was alaborated, written and transmitted to the scientific community in exceptional conditions, we think it indispensable to specify the context of Bernstein's activity as well as the context in which his work became familiar to us. This gives the present volume a very unusual character, namely the significance of a rehabilitation. This context explains in particular the form of our contribution which will constantly reflect the difficulties to make comments on the chapter of a book published 15 years ago, in which were collected articles written between 1934 and 1961. The issue of this book suddenly made available to Western scientific workers a volume, the outcome of which remains ambiguous. Bernstein's work rapidly became a sort of bible for those who considered him as a "laboratory genius", to quote Phillips and Porter (1977), who anticipated the necessity of a renewal in the approach to voluntary motricity. For many others, the value of the book lies only in the fact that it reveals the conceptions and critical considerations of a researcher who was certainly creative and undoubtedly a victim of historical vicissitudes that scientific exchanges may sometimes undergo. To them, the exhumation of Bernstein's writings no longer has the power of modifying profoundly the development of studies devoted to the elaboration and control of movements, which have known a considerable expansion in the last two decades.

A first element of this context stems from what could be called the marginal character of the work of Bernstein (who, on several grounds, appears to have been an independent scientist). Let us mention first the troubles he had with the directivism of the scientific establishment of his country which emerge on several occasions in the course of his book in an explicit controversial form: his discussion of the dominant ideology of classical reflexologism and associationism not only resulted in excluding him from experimental research but also in distancing him from his scientific environment for many years. On the other hand, there are good reasons to believe that Bernstein, especially in the last years of his life, received only limited information on the development of Western studies concerning the fields in which he was interested. In particular, if one takes into account the variety of questions dealt with, which often go far beyond the organization and control of motricity and concern very general problems of epistemology, one is struck by significant lacunae in his bibliographical references. However, for a number of questions, the effects of these omissions are attenuated or even masked by Bernstein's penetrating intuitions. Finally, while Bernstein probably only had partial information about the views and works of Western physiologists and psychologists, these scientists were also totally ignorant of Bernstein's work until the translation of his collected articles was published, and even today his importance is not recognized by all. In a way, Bernstein's marganilization continues, thus favoring controversy duly amplified by the diversity of opinion concerning the value with which his work should be credited some twenty years later. As an illustration, in the Handbook of Physiology issued recently, neither Keele's (1981) contribution nor Granit's article (1981)[*] refer to Bernstein. This can be considered as clearly revealing partiality by those who see themselves as his natural disciples and who contributed to the rehabilitation of this work. Nevertheless those who think Bernstein's work not very decisive and no longer interesting will consider this lack of reference as a simple omission. They will underevaluate this work all the more as they feel bitter for having ignored it for so long and not having benefitted from it earlier.

A second element of the context which runs counter to the usual role of a referee is due to the very form of the book "The Coordination and Regulation of Movements". It does not offer a structured synthesis of Bernstein's view on the different physiological and psychological problems involved in the organization of motor activity. It is rather a conglomerate of articles, each of them intended to be self-sufficient. Articles are presented in the order of their publication, their selection being made on the basis of the topic dealt with, namely the control of movement. However, the book is far from being devoted to this one question and Bernstein is often led to put forward

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[*] Granit's article, however, was devoted to a history of conceptions in the domain of the control of motricity.

more general considerations about, for instance, the philosophy of biological sciences. Let us note that the many readers who knew Bernstein indirectly will be surprised when going back to the origins of a work known mainly for nourishing contemporary debates on the elaboration and control of movement. Beyond the "biomechanician", which remains the label most generally linked to Bernstein, they will discover not only the physiologist and the psychologist but also the pertinacious, often aggressive epistemologist who occasionally criticizes his immediate or distant circle. Such a juxtaposition of texts, while bringing some light on the evolution of the author's views regarding a number of problems, also underlines, through the redundancies of a discursive reasoning, Bernstein's main preoccupations and reveals the unavoidable internal contradictions of a train of thought which was sustained for 30 years. In this sense, Bernstein's writings do have something in common with the bible in which, as it is known, when exerting the art of using quotations one may find an adequate support for whatever limited point of view one chooses. Consequently, it was not only impossible but hazardous to restrict critical comments solely to one article without taking into account the previous or subsequent ones.

The last and probably the most important element of the context within which a new reading of Bernstein's writings is made nowadays, is evidently the whole set of exegeses, developments and comments to which his work gave rise. From this point of view, the systematic analysis that the Science Quotation Index provides is quite interesting. In effect it reveals that there are very few references to "Coordination and Regulation of Movements". These references can be classified into two constrasted categories, which is a good indication of the attitudes which contribute to the perpetuation of what we called Bernstein's marginality. The first category, the most important, refers with some exception to Haskins Laboratories' studies which played a decisive role in the circulation of Bernstein's view. Haskins' scientists, amply using paraphrases and quotations, presented, summarized or developed Bernstein's ideas within the framework of a new formulation of the problems raised by the organization and control of motor activity. It should be noted that it is at times difficult to draw the dividing line between what should be attributed to the influence of Bernstein's views and what constitutes the original, indisputable contribution of these authors. Finally, the amplification of Bernstein's views, within the perspective of an innovative thesis, naturally goes together with a will to disassociate from conceptions of motor control considered to be classical and outdated.

This amplification leads inevitably to a selected or truncated exploitation of Bernstein's views and arguments, which is facilitated - as already underscored - by the very content and form of the book. Consequently, it is now quite impossible to ignore the controversy which was unintentionally provoked by Bernstein. The second group of references to Bernstein's work is characterized by extremely general allusions to his views. They would not deserve any comment if they did not attest to the fact that it is nowadays well thought of, if not prudent, to quote him - which is easy in many cases, given the extent of Bernstein's views. Consequently, when making comments, it seems difficult not to adopt one of the two asymmetric alternatives: either to follow those who, since they became the disciples of a prophet, both contributing to the circulation of his ideas and proselytizing, can only be laudatory, or to maintain a distance which may conceal potential criticism as well as ignorance or lack of interest. This is why we thought that a real reintegration of Bernstein into the dynamics of the views and studies on the organization of movement could not be done without a critical evaluation of his work, a scientific requirement which probably comes rather late but which is one of the reasons for the present book.

The content of the chapter entitled "Trends and problems in the study of investigation of physiology of activity" seems to be characterized by three main themes. Our comments written within the above-mentioned perspective will concentrate on these. They are devoted in the first place to Bernstein's epistemological concerns, which are still relevant. They justify the basic idea that in the relationships between the environment and an organism the former does not always have the initiative. An organism acts through its motor activity, i.e. it intervenes upon the environment and constructs a perceptual synthesis which allows for a consistent image of the world. Such a view implies a radical criticism of atomism directed against the use of the reflex arc as a unit of analysis in the domain of motricity and, in the domain of perception, against the principle of an accurate and amorphous image leading to an insoluble antagonism between the "percept" and the "internal" observer. The world representation, as a source of information and a target for action, follows the principle of a dynamic modelling which cannot be separated from the activity of the organism.

The second main theme concerns the problems raised by the planning and execution of movement. In this chapter, emphasis is placed upon the necessary implication that Bernstein discovered in the "structurality" and "completeness" of motor acts. A lesser emphasis is given to the principles of organization and the modes of functioning of a sensorimotor system, which integrates motor commands and information derived from afference and reafference. Bernstein stresses the role of primum movens assigned to an organisatory entity (what he calls "the motor problem") which gives biologically meaningful motor activities their adaptive finality. Taking into account the criticisms addressed to the theories of motor control, for which the concepts of program and programming are crucial, we shall emphasize the view that Bernstein's work cannot be used as an argument to solve the conflict between "peripheralists" and "centralists" since it is concerned with what may be centrally processed, once the state of the periphery has been evaluated.

Finally we shall expand upon one aspect of Bernstein's views which seems to have been somewhat disregarded by his exegetes, namely the role of the representation of the future in the planning and execution of a motor act. This representation is at the origin of permanent anticipatory activities which range from the probabilistic image of what must be and/or will be realized in the environment, to the functional preparation of sensory and motor elements which will be involved in action. Even though less elaborated than the views concerning the circularity of movement regulation, these references to expectancy as an important dimension of motor control nevertheless complement Bernstein's considerations devoted to the application of concepts of cybernetics to the control of voluntary motricity.

I. BERNSTEIN AS A PRECURSOR OF COGNITIVE NEUROBIOLOGY

Bernstein was well aware that Russian physiology could benefit from contemporary developments in biological sciences, especially from the application of the cybernetic model to living systems. This concern for ideological openness, as well as his own ideas on the theoretical foundations of a physiology of activity, led him to adopt a critical attitude concerning the systems of explanation advanced by orthodox Pavlovian reflexology. He claimed that it was not possible to base a theory of the elaboration and control of biologically and behaviorally significant motor acts on the sole principles of a sequential chaining or a hierarchical series of elementary reflex pathways, which would restrict the organism to respond passively and stereotypically to the unceasing appeals of the environment. The only function of such a chaining would be to maintain a stable equilibrium between the organism and the environment. We would like to emphasize that

Bernstein's critical argumentation and presentation of an alternative theory - in which the decisive notion of the active intervention on the environment replaces the notion of a passive interaction - refers to an oversimplified view of the concept of reflex; such an oversimplification was no longer supported by the data on the functional characteristics of reflex reactions which were available to Bernstein, and are still less supported by the results obtained in most recent studies on reflex activity.

It is extremely surprising that Bernstein was content to consider that movements triggered by reflex activity were "fully explained in terms of the stimuli which activate them", given that even Sechenov, as early as the end of the last century, had mentioned that the spatial and temporal characteristics of a reflex reaction in the spinalized frog depended not only on the parameters of the stimulus but also on the initial joint configuration of the stimulated limb. The tendency to systematically underestimate the factors of variability of the most elementary reactions - especially factors of central origin - reflects a general conception in which reflexes receive a trivial functional interpretation which barely acknowledges their evident biological finality in the restoration of the equilibrium that a stimulating agent imperils. For example, it is because the motor reaction tends to develop toward a compensation of the load imposed upon the muscle, without truly compensating it, that the role of the stretch reflex in movement regulation was at first accepted, before gradually being questioned. At this point, it is worthwhile to note Granit's position in 1972. With good reason, he was impressed by the power of the structural organization of the myotatic reflex which is not only converged by the fastest sensory fibers of the neural system (which are strongly and monosynaptically connected with spinal motoneurons), but also benefits from a specialized servomechanism, the gamma system, which ensures it a permanent efficiency. On these grounds, Granit admitted the principle that the control of voluntary movement was submitted to the progressive stabilization of a chaining of reflex responses. "Even those movements which we regard as voluntary are largely automatic (then reflex in nature). Most of them intrude upon consciousness only at the moment when they are triggered off into action". The perturbing effect caused by any attempt of consciously controlling the execution of an automatic motor sequence was interpreted within this perspective. Even if the view that motor spinal organization would exert a sort of leadership is challenged, the contribution of this organization to the processes underlying the execution of movement can no longer be reduced to the sole function of load compensation, an interpretation that was facilitated by the experimental conditions in which the stretch reflex was investigated.

Analogous uncertainty dominates the interpretation of long latency electromyographic responses triggered by muscular overload. Even though the problems raised by the nature of these responses - are they reflex, as claimed by Evarts (1973), or voluntary? - and by the level at which their pathways loop - is it transcortical or not? - are controversial issues (cf. Wiesendanger, 1978; Bonnet and Requin, 1982), it was suggested and admitted that they had the functional meaning of compensatory responses. Such an interpretation is more and more often disputed (cf. for instance Bizzi, Dev, Morasso and Polit, 1978) and more cautious proposals are being advanced. It is now suggested that long latency electromyographic responses would reflect the intervention of pathways that continuously convey sensory feedback to central structures, so as to adapt motor control to the peripheral conditions of movement execution (Conrad and Meyer-Lohman, 1980). This hypothesis, if supported by experimental data, would probably have been attractive to Bernstein and led him to reconsider his own position, especially his underevaluation of reflex chaining in the elaboration of motor activities that are behaviorally meaningful. Let us add, within this perspective, that while there are several experimental arguments in favor of a preeminence of central programming in such an automatic activity as stabilization of standing posture (Bonnet, Gurfinkel, Lipshits and Popov, 1976), cortico-spinal commands were considered by Bernstein's followers as playing a double role: on one hand, responsibility for the activation or interruption of the automatic program and, on the other hand, control of motoneurons and interneurons including the oligosynaptic reflex pathways (Gurfinkel & Schik, 1973).

It should be pointed out that Bernstein's more or less explicit scepticism concerning investigations conducted within the reflexologic approach is partly based on the fact that the problems raised by the functional meaning of reflex reactions in motor activity regulation were not well differentiated from the methodological interest that such reactions present for the study of the characteristics of supraspinal processes. In spite of Paillard's (1955) princeps work, where he clearly distinguished the two perspectives and showed how they could be exploited, there still remains some confusion. It should be underlined that studies devoted to proprioceptive reflexes and, in more recent years, to long latency electromyographic responses are justified in part by the functional role of these reflexes in motor control (even though this role, while important, still remains unclear). The main justification lies in the fact that methods of reflexology provide a powerful tool for the investigation of the functions of the central neural system of awake and active subjects (cf. Bonnet, Requin and Semjen, 1981). Let us add that the very principle on which these methods are based consists in the analysis of controlled factors regulating the reactivity of reflex pathways, thus taking advantage of a fundamental characteristic of reflexes, namely their variability. This characteristic is totally opposed to the notion of rigidity that Bernstein used as an argument in his criticism of the conception of reflex activity as a functional module underlying motor organization.

In short, Bernstein was indisputably ahead of his time in denouncing the inadequacies of an atomist and mechanistic physiology, which claimed the possibility of adopting an approach based on the sole principle that the whole is the sum of the parts to analyze the processes controlling and regulating the activity of the whole organism and , a fortiori, the action of the individual. It should be stressed that Bernstein's perspective became, with a few exceptions, a source of innovation for Western research in physiology, both because it emphasized the conceptual and methodological autonomy of the "physiology of activity" and because it anticipated the increasing interest that studies in this field would elicit. ("The problem of the rider has begun to overshadow the problem of the horse"). Bernstein's originality may be evaluated by reference to the fact that it is only recently that some of the most famous neurophysiologists admitted the limits of the classical approach. For the first time, in 1973, before the publication of a book with a suggestive title (The Purposive Brain, 1978), Granit expressed doubts as to the universal power of the strict analytic approach adopted in studies devoted to the control of motor activity. Because such an approach neglects the "teleological" questions raised, on the one hand, by the nature of the goals that the organism tries to reach using motor systems and, on the other hand, by the processes which enable motor systems to respond to variations in the demands of the environment, it can produce only a corpus of knowledge consisting of an "amorphous conglomerate of well-documented facts". A similar remark might be directed toward Mountcastle who began by applying this analytical neurophysiology to the cortical mapping of sensory projections and later on, when it became possible to investigate unanaesthetized and active animals, contributed to the introduction of concepts derived from psychological studies (e.g. the concept of attention), using an experimental strategy combining the concepts and methods of neurobiology and psychology. It does not seem superfluous to insist on the present relevance of Bernstein's message, since it is still true that every technological or methodological advance in the field of neuroscience regularly cause the reappearance of the reductionist temptation. Scientists who promote these new sophisticated tools have a tendency to consider themselves equipped for a "direct" approach of the processes underlying complex behavior, and thus able to avoid the theoretical elaborations and the study of concepts which characterize the evolution of the scientific approach embraced by those for whom the central concern was the investigation of behavior. As Neisser (1967) wrote in a book published the same year as Bernstein's volume: "Psychology should not be viewed as just something to do until electrophysiology comes around to solve the problems".

However, we think that Bernstein's disciples sometimes went far beyond their precursor's conception, especially when developing the idea that a molecular investigation of movement, reduced to a fragment of activity isolated from the behavioral context, should be replaced with a molar approach to action, the motor expression of which being inseparable from the goals pursued. One may have two purposes in mind when saying that the distance between experimental paradigms used in laboratory conditions and the conditions in "real" life is so large that the heuristic value of the data obtained in laboratory is of little interest for the comprehension of the control of movement in "natural" conditions (for instance, Kelso, 1981). This position may constitute a reasonable warning against the dangers that the artificiality of certain experimental paradigms presents, i.e. against the risks of overgeneralizing conclusions to conditions which are very different from the ones in which experimental data were collected. This position may also be understood as an agnostic attitude toward every experimental investigation conducted in laboratory conditions; this raises a serious epistemological problem since it leads to a rejection of any reductive approach. Most certainly, Bernstein did not share this view, as testified by his concern for making the study of the "physiology of activity" benefit from a multidisciplinary approach. Neither is such an attitude consistent with the concerns of the Haskins' group: the reductive approach is constant in their work, as exemplified by their legitimate interest in modelling, borrowing concepts from physics and mathematics. However, this position is adopted by some authors, as illustrated by the astonishing conclusions of Reed (1982) who proposed to "integrate" Bernstein's physiology of activity and Gibson's ecological psychology into a theory of action. "There is no place in the theory for S-R analysis (...). S-R psychology and S-CNS-R psychophysiology are both to be rejected. Cognitive theories, with their Pandora's boxes of intervening variables can no longer be accepted". One may easily assume that Bernstein would have been reluctant to publish his book, not wanting such a disintegration to appear one day as the logical outcome of his critical - though certainly not destructive - analysis. This analysis was conducted with the resources of a subtle reasoning, trained to dialectic, within the perspective of promoting a new expansion in the conceptualization of motor control and in the paradigms used for its experimental study.

The abusive exploitation of Bernstein's explicit antireductionist positions in order to promote syncretic views based, in particular, on a systematic ecologism would lead to an underestimation of the modernism of a creative train of thought. Bernstein's conceptions seem to be the result of a universal evolution of ideas in behavioral science which were, for a long time, smothered by coercive mechanistic conceptions: Russian reflexologism or Anglo-Saxon behaviorism pretended to provide these disciplines with a "scientific" structure, proposing to them as a unique model the approach used in the physical sciences, and thus refusing them any conceptual or theoretical originality or methodological autonomy. Bernstein's ideas, as well as the conceptions of some of his Western contemporaries (e.g. Hebb, 1949; Wallon, 1942), announced both the resurgence of neo-mentalism, which enriched the cognitivist trend of contemporary psychology, and the spectacular recentering of biological sciences on the problems raised by the active organism. Bernstein's views contributed to the creation of conditions propitious to the suppression of separations between disciplines; their vitality, already evident but mainly potential, is now expressed in the dynamics of the concept of cognitive neurosciences (cf. Posner, 1982; Requin, 1982).

II. BERNSTEIN AS THE PROMOTOR OF A DYNAMIC PHYSIOLOGY OF ACTION

For about the last 10 years an important trend of thought, particularly well illustrated by the work of Kelso, Turvey and their colleagues, contributed not only to popularize Bernstein's ideas, but also to develop them within the perspective of a renewal, if not a reformulation, of the problems raised by the organization and control of motor activity. Before we propose some comments on this dimension of Bernstein's work, not distinguishing his own contribution from what his disciples attributed him and from their own further developments, we shall attempt to show that the perspectives he opened are indissolubly, but probably improperly, linked to an often radical questioning of motor control theories in which programming is a central concept.

1. The fallacious controversy on motor program

We find rather excessive the criticism addressed to theories which assume that a central executor is in charge of translating the schedule implied in an action project into a pattern of commands directed to peripheral executants.

In the first place, they crystallize on a narrow, oversimplified, conception of motor program which those who have exploited the fecundity of this concept for the comprehension of movement organization cannot accept. It is unfortunate that Keele's (1968) lapidary formulation - which was not intended to summarize in an exhaustive definition how a motor program is constructed, structured and executed, but rather to outline his conception - became the privileged target of pertinacious criticisms. We consider the formulation he proposed in 1968: "a set of muscles commands that are structured before a movement sequence begins and that allows the entire sequence to be carried out uninfluenced by peripheral feedback" as a summary of the methodological principles on which are based experiments that consider the concept of program as necessary for interpreting the data, mainly the demonstration that movements can be executed in the absence of sensory control, either because an experimental procedure prevents that type of feedback, or because the movement is executed within a time course such that feedback of peripheral origin cannot yet modulate the central command. Keele, however, does not claim that in most cases motor activity develops in such conditions, as testified by a later formulation (Keele, 1973): "If* neither visual nor kinesthetic feedback is needed for the execution of patterns of movement, then* the movement patterns must be represented centrally in the brain". Since the logic of this proposal could bot be disputed it did not invite oversimplified criticisms.

In the second place, through a constant but often imprecise reference to Bernstein, the ideas developed in the volume "Coordination and Regulation

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* Underlined by the authors.

of Movements" are presented as foreshadowing and justifying the entire reformulation of the problem of motor control; this reformulation is set out as an original synthesis of two radically antithetical conceptions underlying the "peripheralist" and "centralist" theories of movement. A good illustration of such a dichotomic view is provided by a recent critical review by Reed (1982) who writes: "At the same time that Lashley and other Western scientists were showing the limitations of the peripheralist theory, the Soviet physiologist N. Bernstein was demonstrating* that the central program theory was also untenable".

Any attempt to submit to an evaluation both the validity of these criticisms and the arguments they find in Bernstein's conceptions necessitates, beforehand, the softening of the extreme expressions which tend to abusively radicalize if not completely forge the above mentioned oppositions.

Let us consider for instance the status of the motor program which is referred to by its belittlers not only under the lable of concept but also of theory. The peripheralist theory, in which movement is considered as a reaction triggered by a stimulus bringing into play either a prewired circuit or a gradually built associative chaining, is contrasted either with the concept of motor program, thus creating an asymmetrical opposition between levels that are not equivalent from the formal point of view, or with a theory of motor programming assuming that the sole intervention of an omnipotent central executive gives a comprehensive account of movement organization; as a result, this theory is improperly considered as exclusively and radically centralist. As far as we know, Keele is the only one to have mentioned, probably unwisely, a "theory" of motor programming and, besided, the considerations presented in his paper do not allow one to think that to him motor program is more than a concept, though certainly a basic one in a theory of motor control.

Similarly, due to a constant metaphoric assimilation to a simplified version of computer programs, the concept of motor program was gradually reduced by its decriers to a rigid set of commands, a conception which strongly diverges from the view - advanced regularly for more than 50 years - that a central representation of action contributes in some way or another to the structuring of the outputs responsible for movement execution. It should be noted that the concepts of "Bewegungsentwurf" (Wacholder, 1927)

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* Underlined by the authors.

"scheme of action" (Del Bianco, 1947), "motor scheme" (Head, 1920), "kinetic formula" (Liepmann, 1900), "action plan" (Miller, Galanter and Pribram, 1960), all variants of the same basic idea that was to be developed in various modern theories of motor programming, do not allow so easily for simplistic "anticentralist" criticism, perhaps because they are more "vague" and do not suggest so strongly neurobiological models, as underlined by Gallistel (1981), but mostly because they did not become spoiled by the reductive metaphor of computer program. This metaphor results in a grotesque point of view and, furthermore, in an artificial homogenization of conceptions in which the concept of motor programming is not given the same status, which opens the way for extreme oversimplifications (e.g. Reed, see above, p.470).

A thorough critical analysis of the concept of motor program cannot be limited to the restrictive points of view adopted by scientists who are considered to be in Lashley's (1917) tradition - such as Keele (1968) and, to a lesser degree, Schmidt (1975). In effect, such restrictive positions imply either that the operationality of this concept is restricted to the very few motor activities in which there is no sensory feedback, or that the spatial and temporal characteristics of movement are exclusively determined by a "context free" central executive, in Kelso's view (1981). A detailed critical analysis should also consider theories of motor control in which the central and peripheral origin of motor command though well differentiated are closely integrated. This critical analysis would include for instance the conceptions in which feedback and feedforward mechanisms are the source of the adaptive flexibility of control functions exerted by different hierarchized levels of the neural organization (cf. for example Brooks, 1979), as well as conceptions in which the concept of program is enlarged so as to include either the elements of response elicited by sensory feedback (cf. for example Sternberg, Monsell, Knoll and Wright, 1978) or postural adjustments accompanying and/or anticipating movement and taking into account actual and/or predictable context elements (cf. Gahery and Massion, 1981). Finally, even the partisans of an extreme peripheralist conception, in which information transmitted by closed loops is given an almost exclusive role in motor elaboration, cannot totally discard the notion of program: "I must point out that a very limited idea of a motor program is necessary for any theory of movement because a movement must be started and feedback does not occur until a fraction of a second later" (Adams, 1976).

The objection that conceptions reducing the role of the central program of action reveal the explicit or implicit influence of Bernstein's ideas cannot be rejected; we would like to emphasize however that such an objection simply acknowledges the integrative process underlying the advance of any scientific approach. It should be noted, however, that at the time when Western scientists ignored or underestimated Bernstein's conceptions, the notion of hierarchization in the organization of motor control systems, of permanent remodelling of the central command by the reafferences from movement execution and of anticipation of the changes in the context where the action is to take place were already considered as necessary and not mutually exclusive elements of a synthetic theory emphasizing the adaptive flexibility of systems responsible for movement elaboration and execution (Paillard, 1960).

We believe that Bernstein's conceptions arose in this context where, at the same time, the deciphering of the functional organization of motor systems progressed rapidly and where the fecundity of applying the principles of cybernetics to the regulation of the activity of living systems became quite apparent. In our opinion, it is within the light of this context that Bernstein's book should be read.

In the article published in "Questions of Philosophy" in 1961, Bernstein uses more than twelve times the concepts of program and programming, not from a critical point of view, but in order to specify a stage in the chronological analysis of the operations underlying motor acts; this prefigures the later widespread recourse to serial models referring to processing stages on, in information processing theories that was to be observed from 1969 under Steinberg's influence (cf. for example, Requin, 1980a, b; Sanders, 1977, 1980; Shaffer, 1980; Theios, 1975) as well as in the study of functional organization of motor systems by neurobiologists following Allen and Tsukahara (1974) (cf. for example, Brooks, 1975; Kornhuber, 1974; Paillard, 1982; Thach, 1975). Bernstein outlines a rather broad definition of the concept of program, incorporating "what has to be done" and "how it should be done", which does not leave any uncertainty about his interest for this concept. Even though this definition is immediately tempered, according to the principles of the art of dialectic to which Bernstein was well trained, by underlying the necessity of a large adaptive variability of the program which has to take into account considerable strategic modifications, it would be fallacious to conclude that Bernstein abandoned the concept of motor program. From this point of view, no lines revealing Bernstein's recognition of the necessity to admit a central determination of action are more illustrative than the passages where he proposes a taxonomy of motor activities as a function of their dependency on the stimuli that may trigger them, and locates at one end of a continuum, unconditional reflexes, and at the other end, voluntary activities in which stimuli would play no role: "These are actions for which the program and also the initiative, are entirely determined within the individual...". Let us note that this quotation, if taken out of context, could elicit criticisms quite analogous to the ones addressed to conceptions as excessively centralist.

Such a point of view would clearly be partial, since it would neglect what the decriers of the notion of motor program underscore and consider as Bernstein's main contribution, namely his considerations on the necessary properties of a system of control: such a system, in order to ensure the adaptation of a motor act to its goal implies an invariant determinant, hence a system of reference, despite the multiple internal and external sources of variability that may complicate or disturb its functioning. However it should also be emphasized that the analysis on which this conception is based - which was to be often paraphrased and/or developed later on (cf. for example, Turvey, Shaw and Mace, 1978; Kugler, Kelso and Turvey, 1980; Kelso, Holt, Kugler and Turvey, 1980; Kelso, 1981) - led Bernstein to a conclusion that could be abusively used as an argument to close the pseudo-debate between centralists and peripheralists, since Bernstein rejects the possibility that the pattern of muscular commands as well as sensory feedback might be invariant determinants of motor control activity (p.445). Thus, we disagree with Turvey et al. (1978) who wrote: "Indeed, for Bernstein, the decisive factor in coordinated activity is not the efferent impulses but the complex system of afferentation...".

To us, it is much more important to stress that Bernstein's adhesion to a conception which would explain the problem of motor control would be explained in terms of sensory afferentation rather than in terms of motor efferentation. The contributions he made, most probably not intentionally, to promote this view, are at times expressed in somewhat vague formulations, and consequently easily distorted; these formulations are far removed from the central idea that the guideline of an action control system is not to be found either in the motor output or in the sensory input, but beyond that, in the elaboration of a motor problem or of an action representation. It is for instance very surprising to read (p.444) that "gravity", "friction", and "the opposition of an adversary" are the origin of unpredictable forces, a very questionable view that is totally in contradiction with the thesis that Bernstein clearly considers as crucial for a theory of action and which he presents in a detailed way: the importance of modelling the future, the source of a permanent anticipatory activity of the organism the aim of which is, precisely, to master what the future holds for the active individual.

2. Autocratic centralization vs democratic distribution of motor control

An essential premise in Bernstein's conception concerning the organization of the motor control system is based on the observation of a non-univocal relationship between the pattern of corticospinal commands and motor output. This relationship is expressed in the variability observed in the details of the execution of movements which have an identical purpose. This non-univocal relationship is interpreted, on the one hand, as a consequence of the more or less predictable variations of the environment in which the action is executed and, on the other hand, as a consequence of the complexity of a biomechanical effector device composed of several relatively independent elements with compliant connections. This absence of univocity suggests that the organization of the motor control system is incompatible with the notion of a unique central process, entirely responsible for the details of the pattern of neuromuscular commands, and having at its disposal periodically - and not continuously - updated information about the peripheral conditions in which these commands are executed. Control decentralization and "regulation circularity" thus constitute for Bernstein the two basic concepts enabling the elaboration of a coherent theory of action.

It should be noted that at roughly the same time, some Western authors also put forward ideas concerning the hierarchization of motor control systems. In 1960, Paillard wrote: "the spatial and temporal pattern of impulses required for a purposeful movement (...) is progressively built up by the spread of central commands through the lower structures (...), is remodelled at each way station of the executive system in accordance with the modulating influences which converge from the peripheral sensory mechanisms". Before analyzing experimental data supporting Bernstein's thesis and its implications, it is interesting to ask why the micro-differences, all things being kept equal, observed in the repetition of a globally identical movement, would be related to the variability of the conditions in which the transduction of a univocal command is realized by the biomechanical instrument of execution, rather than to the variations of the pattern of neuromuscular commands, once it is admitted that mastering the complexity is not an insurmountable problem for a unique or multiple controller. The idea that a new program needs to be elaborated each time there is an attempt to give an adequate response to the same motor problem was advanced by Welford (1974) for interpreting the gradual decrease with practice in the amount of imperfections of a goal-directed movement. A subject, when confronted with a new task, would first generate an approximate solution to the problem that must be solved, and later would actively modify this solution, thus avoiding the stabilization of the initial programming error. The movement variability would not then reflect random variations - random because unpredictable - in the conditions in which the biomechanical executant is prompted, but rather controlled fluctuations of the central command that attempts to optimize its effects. These adaptive fluctuations would reflect the diversity of strategies of actions available to motor control systems. This would explain that an identical goal-directed movement may result from very different patterns of electromyographic activity. This observation is precisely the second argument on which is grounded the view that the relationship between the neuromuscular command and the motor output is non-univocal. Thus, we do not think that the interpretation of a univocal relationship involving solely, fluctuations of the pattern of centrospinal commands might be so readily excluded.

In the first place, a major problem posed to motor control systems is the mastery of the variability of the context in which action will take place. This variability is a function of the initial joint configuration, of the reactive forces linked to the involvement of a limb and of the external forces that may interfere with those which will be called into play by moving one or several segments of the body. Without disputing the relevance of Bernstein's analysis of these factors of variability, we find it hard to conceive of variability as a truly original argument in favor of the indispensable role of the information conveyed by the complex system of sensory afferentation in the control of movement elaboration and execution. Paillard's (1960) review of the organization of skilled movements suggests, undoubtedly, that from this standpoint Bernstein belonged to the trend of thought and research initiated by Exner, expanded by Sherrington and Walcholder and later by Rusch, Gellhorn and many others. This school of thought considered the concept of sensorimotor integration to be the basic concept of any theory of motor control. "Even at the level at which relational impulses originate, the modulating action of sensory messages at every moment keeps the activity of central structures in harmony with the varying positions of the body parts in movement and with the state of the ever-changing external field of action" (Paillard, 1960). Such a formulation summarizes, without ambiguity, what the penetrating vision of almost half a century's work and theoretical developments in this area of research had allowed, by the fifties, to be considered as well-established facts.

The many studies which, since then, contributed to enrich our knowledge of the structural and functional organization of proprioceptive afferentation systems, and which today constitute the necessary basis for the elaboration of models of movement coordination, support the notion of a central executor monitoring the multiple variability factors of the intrinsic and extrinsic conditions in which motor activity occurs. It is above all a system constituted by neuromuscular spindles and their gamma motoneurons:

> In addition to conveying information about the body state mechanical parameters, they provide a picture of the peripheral reception of the central control. Because spindle responses to gamma motoneurons depend upon the kinetic state of the muscle, proprioceptive efferents thus make it possible to evaluate the correlation between parameters of motor control and conditions under which the latter are executed (Bonnet, Requin and Semjen, 1981).

The accuracy of the link between the command and the peripheral effect seems ensured by the specificity of muscular receptors for length and tension. This specificity permits a distinct coding of the active forces produced by the command, and of the passive forces coming from the environment (cf. for instance, Miles and Evarts, 1979).

Concerning the absence of linearity in the functioning of proprioceptive receptors and of muscles, it does not necessarily constitute, as Bernstein suggested, an insurmountable obstacle to the control of effectors. On one hand, there is evidence of the existence of filtering mechanisms intervening at the level of sensory relay neurons and participating in the command of the movement itself (Coquery, 1972). These data suggest that the central processor monitors and modulates anticipated sensory effects. On the other hand, by initiating a phasic contraction by action potentials "en doublet" in order to reach instantly a high level of tension, the central control system uses the typical non-linear relationship between the activation frequency of motor units and the tension level in order to improve its efficiency (Burke, Rudomin and Zajac, 1970; Gurfinkel and Levik, 1973).

In the second place, it is now classical, within that perspective, to advance as an argument the difficulty of the task that a possible unique central executor would have to carry out. This executor would have to determine the "optimal" solution among the many configurations of possible muscular activations when monitoring the execution of a spatially oriented movement mobilizing a plurijoint mechanical set which, consequently, involves a rather high level of degrees of freedom. It is of course necessary to inquire into the meaning of the notion of optimal solution, which only makes sense if referring to some variable in relation to which the optimum could be located.

A first formal approach which consists in conceiving of this problem in terms of biomechanics, i.e. borrowing concepts from physics, seems to us of limited relevance when considering an organism actively engaged in a behavioral sequence. As noted by Hollerbach (1982), pointing movements, executed most often with a roughly rectilinear trajectory, minimize neither energy consumption nor displacement duration, contrary to what would be the case if the problem posed in terms of control of a biomechanical system was given a rational solution. Within this perspective, it is necessary to specify the constraints which minimize this "optimal" solution. One may think that they are not limited to the physical constraints, mentioned by Hollerbach on the basis of the models provided by robotics, but rather that they involve constraints imposed by the biological and behavioral meaning of action, including its ontogenetic, socio-cultural, and possibly aesthetic determinants which participate in what Bernstein called "structurality" and "completeness" of motor acts. However, as already observed, a relative constancy of spatio-temporal movement parameters may be accompanied by a high variability in the pattern of underlying muscular activations, and there is also a high diversity in the strategies adopted by different individuals confronted with the same problem. These observations provide different arguments - if one does not consider only elementary acts of very limited meaning from the behavioral standpoint - favoring the view that, on the contrary, there does not exist an "optimal" solution to the control of action but rather a set of solutions, probably "equivalent" because of the multiplicity of points of view from which their "advantages" and "disadvantages" may be evaluated. It seems to us that the size of the set of solutions and motives for strategic choices this plurality makes necessary is beyond the scope of an analysis which makes sole use of the concepts derived from a theory of the control of a biomechanical system. Such an analysis is far removed from a theory of action within which Bernstein wished to integrate the behavioral and ecological meanings of the motor act.

In this sense, Gel'fand and Tsetlin's (1962) analysis - which underlines the difficulty, if not the impossibility, given the available techniques of computation, of rapidly determining this biomechanical "optimal" solution - besides the fact that it prejudges the capacity of the neural machine, which possibly is far superior to the logical and technological resources at our disposal - appears to be an objection of limited bearing to the hypothesis of a unique central monitor. The neural system probably works less according to the formal principles of a mathematician trying to find the solution of an equation with several unknowns than according to the more pragmatic style of a computer scientist who is content with one solution among the range of possible solutions. To conclude, it is not certain that the analogy with robotics whose "relevance to the analysis of the brain remains speculative" (Arbib, 1981) is more fertile in the domain of motor control than was and still is the metaphor of the computer. Bernstein was aware of this when he wrote in his book's conclusions: "...We are becoming increasingly committed to the importance of studying the principal differences between living systems and artificial systems".

A second formal approach likely to provide relevant critical arguments against the unicity and omnipotence of a central executor is based on the application of the theory of complex systems to the functional organization of living systems interacting with their environment. It consists in questioning the principles of organization enabling the realization of the best management of a system whose complexity lies in the structural differentiation of interconnected subsets presenting a certain degree of functional specialization for processing afferent and efferent information, this management being conceived of in terms of economy of means and/or efficiency of decision and control processes. It should be stressed that this type of analysis does not lead to clear cut solutions either. Arbib (1972), for instance, when comparing, within this perspective, the merits of a centralized control system versus a hierarchical control system, noted that the economy of means, defined in terms of storing and duration of programming, attained in the second case was compensated for by the greater efficiency, defined in terms of accuracy and adaptability, obtained in the first case. It does not seem to us that the choice is simple when, once the principle of a decentralization of control exerted by relatively autonomous subsystems is admitted, one inquires into the respective merits of a hierarchical organization or a heterarchical organization. In a hierarchical organization the preservation of a distinction between actors and instruments, implying that the flow of information within the system is unidirectional, seems to be an obstacle to plasticity but a guarantee for the conservation, at least temporarily, of action identity. In contrast, in a heterarchical organization the functional pluripotentiality of subsystems implies a reciprocity in exchanges of information permitting a "free dominance" which, while ensuring a total flexibility of the control system, risks to lead to an anarchic disintegration of behavior.

Irrespective of the outcome of the debates on these problems of formalization, it is not at all clear to us that, in this domain, the metaphor of the computer is without promise, as noted by MacKay (1980) in a different context. Oscillators and "coordinative structures" may probably be assimilated by analogy to the subroutines of a computer program when the function of this program is to control a complex process for which a certain number of parameters remain unspecified, hence, unpredictable to a certain extent. It is the case, for example, of a program devised for an on-line experiment on the acquisition and control of sensorimotor behavior in animals (Schematically, the computer-animal is a sort of ecosystem in which the activity of the animal-prey constitutes a source of variability that the computer-predator intends to constrain within the limits of the goal, its strategies of intervention being necessarily continuously adapted). Such a program involves a set of subroutines, relatively autonomous and adapted functionally to specified sequences of activity, their interconnections defined within the main program, such that a routine calls another as soon as the animal's behavior changes and does not remain within the control capacity of the first subroutine. These programs are based on a modular conception of systems and are conceived of as a catalogue from which are extracted certain subsystems that are put together to fulfil a function, that become disconnected and are then restructured in order to reach another goal. It should be noted that models of this type have already been proposed to account for the ontogenetic development in the learning of motor skills (cf, for example, Fitts, 1964; Bruner, 1971).

To conclude, we do not deny the theoretical interest of the sophisticated speculations to which a formal approach to degrees of freedom may lead (From this point of view the heuristic value of the ideas put forward by Bernstein is unquestionable). It appears necessary to inquire, however, at least in the present circumstances, into the operationality of these formalizations as possible guidelines for experimental research in the field of motor control. Our intuition, possibly erroneous, that they leave unexplained an entire body of experimental data which are perhaps abusively considered as well-established, constitutes an obstacle to an unreserved adhesion. To take only one example, what is their compatibility with the experimental data obtained in the theoretical context of the neural system considered as a channel of limited information processing capacity, which implies, if not the concept of a unique central monitor, at least the concept of a hierarchical organization of control systems? In other words, where would be the "bottleneck" in a system which would function according to the principles of a heterarchical coalition of "coordinative structures".

To us, the most important implication of Bernstein's ideas, and the most original in its historical context, is not the door he would have opened to conceptions of the functional organization of control systems based on an information and execution division of labor between relatively independent and specialized subsets coordinated according to a hierarchical or a heterarchical mode. We consider Bernstein's most important contribution to reside in his emphasis - that has, in our opinion, been underestimated and that we find preminotory - to set out the invariant frame of reference of the organization of action within an elaborated model of the future which reduces uncertainty and takes into account the variability of the context as well as the potential multiplicity of strategies of action. Let us note, moreover, that if one did not wish to remain confined in the artificial controversy concerning "centralism vs peripheralism", it would be possible, in playing the devil's advocate, to find a strong argument in favor of a decidedly centralist position in the many passages Bernstein devoted to the role of an internalized and anticipatory representation of action. However, as underlined in our introduction, Bernstein's epistemological and historico-critical concerns appear to us as bearing on a quite different level.

III. BERNSTEIN AS THE THEORETICIAN OF AN ANTICIPATORY ORGANIZING ACTIVITY

One of the main merits of Bernstein's contribution is to have envisioned the problems raised by movement coordination and regulation within the more general perspective of the activity of higher living organisms, human beings in particular, who do not simply interact with their environment but, on the contrary, take the initiative to act upon it, using all the means they believe necessary. Bernstein's insistent concern for meaningful goal-directed motor acts, his questioning of the use of insignificant fragments of movement as units of analysis, such as the knee jerk reflex, reflect epistemological, if not philosophical considerations which, in the more limited field of physiology and psychology, could lead him only to challenge S-R models.

These models are totally backward oriented, as noted by Poulton (1950) who pointed to the fact that "expectations about the near future" are the essential determinants of an individual's behavior. Bernstein opposes the narrow image of an organism mainly confined in procedures aiming at maintaining its initial equilibrium, to the image of an organism which anticipates its future, which somehow constructs it and whose behavior is consequently determined as much by what must be as by what has been.

It is this orientation toward the future, the representation of the "necessary future", that causes the emergence of the motor problem, whose apprehension and solution enable the organism to go from the actual state of affairs to the desired one. The solution of a motor problem is nothing else, indeed, than the undertaking of a program which both anticipates the result of an action and the means that the action mobilizes. This notion of motor problem is at the core of Bernstein's thought on the elaboration and structuration of motor acts. It does not seem, however, to have aroused all the interest it deserves. In a first series of remarks we shall attempt to determine the function played by this notion in a model of motor act organization, as well as to appreciate its more general relevance to the physiology and psychology of activity.

The occurrence of a motor problem in a given situation is but one of the expressions of organism's orientation toward the future. In the motor problem and in the program for its solution, the "image of the future" is represented under very general features. It must be admitted though, that in the actual solution of the problem in terms of motor activity, the image of the future is expressed in a detailed way as the outcome of a permanent anticipatory activity. This activity relies both on the predictable properties of the ever changing surroundings in which the motor act is accomplished, and consequent effects of the motor act on the environment and on the organism itself. Reading Bernstein's writings does not leave any doubt about the importance he gives to the diverse forms of anticipation, prediction, extrapolation and preparation activities in the organization and accomplishment of a goal-directed motor act. From this standpoint, the chapter that we comment on is particularly illustrative. It is striking, however, that these different forms of anticipatory activities are often loosely-defined or, at least, very unequally elaborated. In a second series of remarks, we shall attempt to make explicit the function of anticipatory activities in Bernstein's theory and to confront the often implicit potentialities of this theory with the developments that the notions of anticipation and preparation have undergone in recent years.

1. The formulation of the motor problem and its solution: an act of intelligence.

How are voluntary movements triggered and how is their internal cohesion ensured? It is not possible to sketch the evolution of ideas concerning these questions since, in order to do so, one should have to go back at least to W. James and to what he said of the anticipated image of movement. Let us note, however, that at the time of the publication of Bernstein's first work, the German physiologist Wachholder underlined that a voluntary movement in unfolded into its slightest details as a function of a "subjective" event, the planning of a movement (Bewegungsentwurf), and that it continues until the attainment of the best correspondence between what was projected and what was actually done (Wachholder, 1927).

In his search for a factor which could account for "the homogeneity of a movement and its unity in terms of the interrelations of its parts in space and time", Bernstein envisioned in his earlier writings the existence of "guiding" or "directional engrams", i.e. the existence of a motor image which would correspond to the planned movement in its real factual form, as well as the contribution of more general engrams representing classes of responses taken as "topological classes". The organizing entity that Bernstein proposes in the present article, as well as in "Some emergent problems of the regulation of motor acts" which was published 4 years before, is less precise, less "structural" but, in its generality, involves very interesting implications. The problem considered goes beyond the simple question of the image of movement and anticipation of the result: "the standard determinant both for the programming of motor activity and its effection and correction by feedback connections can only be the formation and representation of a motor problem by the brain in one way or another". If the questions of a problem and its solution are to be discussed separately, goal and means must be differentiated, at least under a very general form. The solution implies either the selection of the most appropriate means among the already existing means, or a novel (and original) combination of these means, or else the elaboration of entirely new means. If the regulation of motor-activities is conceived of as depending on problem solving types of processes, it becomes possible to consider motor activities in a broader perspective of active and adaptive exchanges between an organism and its environment. Such exchanges result not only in a transformation of the environment, but also (and in some cases mainly) in the transformation of the organism, since it acquires, in the course of its activity, a better and more reliable knowledge of the surrounding world as well as of its own possibilities. In turn, this knowledge provides the means of a new "adaptation". If the word intelligence is a shortcut which links knowledge and adaptation, the reference to "the motor problem" and its solution places the motor act in the class of intelligent behavior or, more exactly, underlines what renders the motor act a manifestation of intelligence.

"The conception (...) that we defend involves, as an evident epistemological consequence, the statement that the external world is not "given" with already prefixed structures and that the object, while existing independently of the subject, constitutes only a limit, in the mathematical sense, toward which are tending approximations of the subject who is trying to interpret it through steps of increasing objectivity; there then exists a proper activity on the part of the subject (...) which can be detected in the contribution he makes over and above the information extracted from the object". When one bears in mind Bernstein's statement that every meaningful motor act command supposes a qualitatively and quantitatively reliable representation of the environment and that every corresponding action constitutes "an active implement for the correct cognition of the surrounding world", and when one also bears in mind the way he opposes a purely "formal" activity to a directed search activity, - the former being characteristic of a behavioristic description of behavior in terms of trials and errors, the latter implying that any attempt to a solution, whether successful or not, gives a defined orientation to further attempts of solution, through processes of stochastic extrapolation -, one might think that the above quotation comes from Bernstein's writings. As a matter of fact, this is extracted from a study by Piaget(1960) and illustrates, in a way that we hope is convincing, convergences, or at least common preoccupations in the "physiology of activity" that Bernstein wished to promote, and in Piaget's conception of operational intelligence. However, to establish a parallelism between these authors would have but a restricted interest if it were not to improve our understanding of movement coordination and regulation. We think it very likely that the confrontation of Bernstein's and Piaget's ideas could arouse some suggestions which would constitute guidelines for further investigations, particularly in the field of motor skills acquisition.

To formulate a motor problem consists, among other things, in discerning the constraints which, within the whole situation, hinder the achievement of the desired objective. Once these constraints have been detected and understood, they orient the selection of the means to be used. Insofar as a motor problem is referred to, these means necessarily imply a particular mobilization of the motor system, in other words they imply a particular form of coordination and control. The question is whether the particular form required for solving the problem is already available, which would imply a selection among already existing means or a combination of these means. If such is not the case, the problem is no longer the achievement of a goal conceived of in terms of changes within the environment, but rather the elaboration of the means necessary for the achievement of this goal, i.e. the elaboration of a novel and appropriate form of coordination and control. In this case, and only in this case, the solution of the problem does not differentiate from the acquisition of a motor skill. It seems that this double interpretation of the motor problem and its solution hardly attracted Bernstein's attention. However he noted that: "the process of practice towards the achievement of new motor habits essentially consists in the gradual success of search for optimal motor solutions to the appropriate problem". This very idea was paraphrased by Fowler and Turvey (1978): "In this perspective, learning a skill involves discovering an optimal self-organization".

The question is then to know what exactly is "discovered" and how the result of this discovery is stored. In effect, one of the most surprising properties of acquired motor skills lies in their resistance to forgetting, which distinguishes them from most "arbitrary" acquisitions obtained by conditioning or association which, per se, are temporary. It is on this issue of how acquisitions are preserved that we share Piaget's (1960) concern when he states: "It is remarkable that current theories of learning mostly emphasized acquisition and partially neglected the problem of the stability of retention, as if it were not a problem. They do indeed pose a problem and this is why the intervention of a factor of equilibration seems indispensable, and this factor probably does not obey the laws of learning in a strict sense. Furthermore, as soon as one invokes regulatory mechanisms leading to equilibrium, some new acquisitions may depend only on them and not on experience" (Piaget, 1960). Let us recall that Piaget refers essentially to the structures of operational intelligence, in which equilibrium is ensured by operations' reversibility. Contrary to endogenous rhythms which ensure equilibrium through an oscillatory process between two extremes, and to regulations which ensure equilibrium through corrective changes triggered by negative feedback, reversibility ensures equilibrium by accompanying every change by a simultaneous correlative process, which ensures virtual or actual compensation of certain effects that may result from this change (cf. Berlyne, 1960). Thus, the acquired structure of operations would be "reinforced" and hence preserved, insofar as the organization of compensatory processes goes, toward a more and more stable equilibrium.

What are the common elements in Piaget's and Bernstein's ideas concerning the process by which the organism overcomes "the internal physiological indeterminacy of the periphery", i.e. the coordination process? It would certainly be difficult to find in Bernstein's works explicit references to equilibration processes and to equilibrium structures in Piaget's sense. But we believe, and this requires further investigation, that several elements in Bernstein's theoretical elaboration refer to such functions. Let us note, firstly, the decisive role given to (directed) active search for the maximal amount of useful information, this searching behavior being under the control of stochastic processes which participate in orientation behavior, in Bernstein's as well as in Berlyne's (1960) views. It also should be noted that this active search for information is directed, in the first place, toward "essential variables" which define the structure of coordinations. In other terms, they could be the "organizational invariant" which, instead of being, as Fowler and Turvey (1978) said,"an information about something which preserves its specificity over relevant transformation",could well then be an equilibrium structure with a set of intrinsic compensations between operations and functions. This remark is very important since, depending upon which solution is adopted, the genesis of motor "forms" and "formulae" - in other words the genesis of invariants of which Bernstein underlined the "topological" character - can be explained by gestaltist laws of perceptual organization, or by functions (operations) implying a certain degree of reversibility. Let us finally recall the diversity of anticipation and extrapolation forms that Bernstein considers as possible sources of "ante factum" regulation, as opposed to feedback regulations. It is this multiform anticipatory activity, expressing a more and more reliable and thorough knowledge of the possible consequences of a motor act for the environment and for the organism itself, that enables it to reach "a degree of coordination at which the organism is not only unafraid of reactive phenomena in a system with many degrees of freedom, but is able to structure its movements so as to utilize entirely the reactive phenomena which arise". Evaluating the possibilities and utilizing the consequences of the action is certainly an intelligent act, and it is based, according to us, on the gradual discovery of a structure of relationship, within which simultaneous correlative processes of compensation, hence of equilibrium, certainly play a decisive role.

2. The images of the future

To Bernstein, the problem of action is the problem of the objective and subjective requirements with which the organism will be confronted. It is therefore entirely dependent upon and tributary to the image of the future constituted by the organism. The image of the future can only be probabilistic, since it originates in an extrapolation whose reliability depends on the temporal span that anticipation encompasses. It also depends on the degree of stability of the context within which it is elaborated, and on the time available for its elaboration. Therefore, the actualization of the future is a conflictual process, a struggle against the obstacles - be they anticipated or not - the action encounters, a fight whose finality is to increase the probability of the desired outcome up to the unit and to nullify the value of other equally probable but undesired issues. This struggle for the accomplishment of an anticipated future may be conceived of as a process reducing the entropy of the system which embraces the individual and his immediate environment and, consequently, it may be assimilated, as it was by Bernstein, to processes of "programmed morphogenesis" through which the individual constructs himself, in spite of the obstacles, according to a predetermined model. "And so, what in a particular case of motor functions in organism appears to be (1) the modelling of future requirements in terms of a problem of action, and (2) the realization of an integrated program of this action by the conquest of external obstacles and by active struggle for the result, turns out to be a manifestation of the general principle of activity running through the whole of biology".

One may wonder whether this dialectic view of the foreseeable and the unforeseeable is the result of Bernstein's apprehension of the structural properties of the meaningful motor act (meaningful insofar as it refers to an image of the future) or whether, on the contrary, this view oriented his scientific approach. One may also ask to what point the analogy between morphogenesis and self-realization of an intended motor act may be pursued. It seems to us that the limits would rapidly be reached: the genetic program does not need an image of the future for its functioning, whereas the image of the future cannot ensure the realization of the desired state of affairs directly. Nevertheless, given that the anticipation of the future is normative, it inevitably leads into a program which maintains directionality of action as a result of the "motor problem". It could then seem that the two extremes of the dialectic conflict which presides over the action, namely the conflict between the foreseeable and the unforeseeable, would be replaced by the command coming from the "central" program and the information feedback by the "peripheral" sensory device, respectively. This superficial and, in the last analysis, erroneous view is induced by Bernstein's reiterated remarks concerning the unforeseeable character of independent external forces which act upon the organism and the unpredictable nature of reactive forces which arise in the course of movement execution "in the multi-linked biokenetic chains of the motor organs".

What is the precise scope of these remarks? It is hard to conceive of an organism realizing its "program of the future" if it were reduced to receiving and processing unexpected and bizarre information about an environment lacking in any consistency and/or predictibility. In Bernstein's view, the complete mastery of coordination consists in a judicial utilization of reactive forces, i.e. the introduction of the necessary impulse at the proper time. Such a "skill" certainly could not be exerted without the permanent and powerful intervention of the processes of anticipation of the spatio-temporal characteristics of events. The circular and uninterrupted flow of information between the "periphery" and the "center", whose importance was stressed by Bernstein, is the immediate condition as well as the consequence and the subsequent expression of coordination. It expresses the functioning of a system of foreseeable relations, which is projected both on the external and internal events elicited by the action, and on the events which, while independent of the action, accompany it and serve as potential cues. At this stage of exercise and practice (apparently, Bernstein did not like the term "learning") the informational load of centripetal messages which flow within the system is less heavy, insofar as these messages are known in advance and expected. Their local regulation and "sanctional" afferentation functions (Bernstein borrowed the term "sanctional" from Anokhin) are certainly more important than their function of informing about a totally unexpected event.

In these conditions, there is not one image of the future but rather several images, all extracted "from whatever the brain is able to select from the current situation, from the fresh traces of immediately preceding perceptions, from the entire previous experience of the individual, and finally from those active trials and essays belonging to the class of (...) orientational reactions". They cooperate in different ways to direct the action toward its final accomplishment through the preparatory activities that they elicit. However, while the image of the action's general goal throws light on the whole action and is located at a strategic level, other images of the future which anticipate "intermediary" events are located at a tactical level.

The reader is entitled to ask whether the analysis of intermediary anticipations that we have proposed is simply a paraphrase of Bernstein's more or less implicit ideas or, on the contrary, if we projected on Bernstein's writings some essential elements of a new trend of thought; a trend which, fed with an ever increasing number of experimental data in the field of perceptual and motor activities as well as in the domain of cognition, has insisted unceasingly on the decisive importance of anticipatory activities in actions (cf., for instance, Pew, 1974; Requin, 1980a). This question may be answered by saying that the context of contemporary works constitutes evidently a factor of sensitization to Bernstein's approach of anticipation. Furthermore, we would like to say that even though Bernstein did not give the various forms of anticipatory activities a definite status, he was well aware of the importance of these activities for a general theory of action.

In the remainder of this chapter, we shall attempt to show, with the help of some examples, how the different forms of anticipation are referred to in Bernstein's writings, and to outline their importance for, and their function in, the theory of action. In order to simplify, we shall follow Poulton's (1950, 1952) categorization in which he distinguished three types of anticipation concerning the receptor, perceptual and effector processes.

Receptor anticipation intervenes when, using a suitable mode for presenting the signals, the subject receives useful information prior to the time at which it will be used. Whether partial or exhaustive, this prior information enables the execution of preparatory cognitive or motor adjustments which may thus facilitate action efficiency and fluidity (Requin, 1980a). Bernstein gives the example of reading aloud: the eye is searching for new information while the previous word is being uttered. This example also shows the lack of a clear frontier between receptor anticipation, considered as being the less sophisticated, and perceptual anticipation which would participate in more complex functions. In effect, the coordination of verbal responses, of text coding and of the control of ocular motricity is extremely complex and the anticipation, in other words the more precocious reception of patterns of visual stimuli, is an activity inscribed within the frame of more general processes of anticipation and prediction.

Perceptual anticipation intervenes when the subject employs his capacity to learn the predictable characteristics of signals. On the basis of this knowledge, his anticipation of temporal and spatial events enables him to set up preparatory adaptations before the occurrence of the events. As a result, perceptual, cognitive and motor preparations permit partial compensation of the inertia of systems brought into play. Works devoted to the study of reaction times as a function of situational entropy (uncertainty concerning the events, their time of occurrence and their sequential dependence) investigate these anticipation and preparation problems to differing degrees and in different manners (cf. Requin, 1980a, b). Perceptual anticipation seems particularly important with respect to behavioral activities in which sensorimotor activity is not discrete but, on the contrary, continuous. In such cases, the detection of stable configurations within the input, and of the regularity of changes in the error signal (i.e. the detection of higher order derivatives of this signal), constitutes the very condition of an "ante factum" control capable of compensating the delays inherent in control systems which react to the error made. Bernstein pointed to the significance of this predictive activity for the regulation of rapid (ballistic) movements as well as tracking movements and, more generally, for all conditions where it is more important to prevent an error than to correct it.

Preventing error does not depend solely on the organism's capacity to anticipate changes in the environment, but also on its capacity to produce the most appropriate movements, i.e. on its capacity to anticipate their effects accurately. This type of anticipation, termed effector anticipation, is based on the information available to the subject concerning "the transmission properties of the muscle system, the limbs and any external devices being controlled" (Pew, 1974). This conception is very similar to Bernstein's view that one of the basic problems of coordination is the adaptation of active forces to the dynamic properties of the control system. In Bernstein's description of the functioning of the control process, the micro-instructions prescribing the direction, strength, velocity, etc... of infinitesimal segments of a movement are presented as prediction and anticipation activities. In this model, the triggering and pursuit of movement are ensured by the desequilibrium created between the momentary state and the desired state of affairs by the emission of these normative instructions. However, it is likely, and from a logical standpoint it seems necessary, that movement anticipation, in terms of instructions concerning its execution, should be accompanied by anticipation of the consequent sensory effects, not simply of the planned movement, but also of that particular movement which has the highest probability of achieving the goal of the action (Adams, 1971; Schmidt, 1975; Semjen, 1977). The anticipated sensory image of the movement that is the most appropriate to the goal is the only one that may serve as a reference against which sensory information generated by the executed movement can be compared for efficient interpretation.

The last aspect of Bernstein's ideas on anticipation processes concerns "the anticipatory adaptations in terms of tuning in advance of the arousal of all the sensory and motor elements which are employed". These anticipations do not really constitute a separate class of events but rather simply correspond to preparatory adjustments. We suggested that these anticipatory adjustments depend on other forms of anticipation concerning the requirements and regularities of the situation with which the organism is confronted. In a different article, Bernstein mentioned the role of "the preparatory organization of the motor periphery". The appropriateness of Bernstein's views is confirmed by the many studies providing evidence of preparatory organization, specific to a particular context, of not only peripheral but also central regulation loops (cf. Bonnet, Requin and Semjen, 1982; Wiesendanger, 1978). However, the explanatory value of the mechanisms proposed by Bernstein (neuromuscular tonus, physiological sets, reticular formation) seems to us rather limited by virtue of the poor level of specificity of these mechanisms.

The essence of preparatory adjustments so far mentioned is that they are latent; they become overt only when an appropriate stimulation questions underlying neuronal circuits. On the contrary, postural adjustments constitute overt forms of adaptation and one of their functions, according to Bernstein, is to anticipate the dynamic and static forces to which the body is submitted when executing a movement. Many current studies confirm this intuition and specify the feedforward mechanisms intervening in that particular form of preparation (Gahery and Massion, 1981).

In conclusion, what elements of this analysis of the "images of the future" can be retained? At the heart of Bernstein's theoretical elaborations on motor coordination and regulation, these images "anticipated", in our opinion, certain essential aspects of current preoccupations in the behavioral neurosciences. Bernstein certainly does not have the monopoly on invention and utilization of the notions of anticipation, expectation or preparation in a theory of "voluntary" movements (cf. for instance, Paillard, 1959). Even so, in 1960, one year before the publication of Bernstein's writings on this question, it was possible to present a synthesis of learning theories (Mowrer, 1960) by referring mainly to Tolman for introducing the concepts of expectation and anticipation. This remark is not at all intended to criticize Mowrer's review, it simply permits appreciation of what constituted Bernstein's originality in his time.

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CHAPTER Vb

TOWARDS A HOLISTIC CONCEPTION OF MOVEMENT CONTROL

L. Pickenhain

> Yes, we shall question all, again and again. And we shall not proceed with seven-league boots, but at a snail's pace. And what we find to-day, we shall erase from the board to-morrow, and we shall only write it down again, if we have found it once more. And what we wish to find, and do find, that shall we look at with particular suspicion... But if every other assumption than this one has been banished from our hands, then no more mercy to those who have not studied, but still speak about (Bertolt Brecht, The Life of Gallilei).

Bernstein started his scientific work with diligent and exact experimental investigations into the coordination of movement in healty humans both under working conditions and during different types of locomotion. Later on, he extended this study to subjects with lesions in the central motor system. Summarizing and generalizing the results of these investigations, he developed a new and comprehensive conception of the principles and mechanism of human motion. This completely new and prospective idea was in contradiction to some officially accepted dogmatic interpretations of Pavlov's ideas which in the Soviet Union prevailed in the forties and fifties. Academically, Bernstein was viciously attacked - especially during the Scientific Session on Problems of the Physiological Theory of Pavlov in 1950 in Moscow - and was accused of propagating idealistic and vitalistic positions. He - like Anokhin and many other scientists - was relegated from his laboratory and had to leave Moscow for some years. After Stalin's death (1953) he was able to return, but he never again had the possibility to participate in experimental investigations. Instead, he intensified his efforts to create a holistic conception of movement control in animals and humans. On the basis of the extensive experimental results of himself and of his pupils, together with an appreciation of the latest results and hypotheses from physiology and mathematics, he was able to develop fascinating, future-oriented ideas about the principles and mechanisms of programming and control of movements which consequently led him to postulate a physiology of activity.

Bernstein's paper 'Trends and problems in the study and investigation of the physiology of activity', written at that time, has not lost any of its fascination during the twenty years that have passed. Moreover, many of the assumptions underlying his theoretical framework for what he called the 'science of movement' have been confirmed by new research data and many of his critical remarks are equally valid today. His predictions and ideas about the discovery of new and wider horizons have turned out to be true. But, these horizons have become somewhat more precisely specified in some of the topic areas, and new questions have arisen on the lines of his prognostic thinking. In what follows, an attempt will be made to indicate the state of knowledge with respect to some of the questions he raises and - as he did - to accentuate especially those questions about human motor activity which seem most important for further developments.

In particular, the following issues will be addressed:

I. The model of the structure of motor acts and the determinants of their central control.

II. The reflection of the image of the real external world and the modelling of the future in the brain.

III. The programming of movements and the hierarchical complexity of their control - localized in numerous centres and systems of the brain.

IV. Some philosophical implications of Bernstein's views, especially regarding the activity of the organism as the leading life-developing principle in physiology.

I.

Evaluating and summarizing his immense experimental material on the coordination of movement during the performance of working acts, locomotion and other activities, Bernstein very soon concluded that the organization and control of movement cannot be understood as a chain of reflexes based on the conception of the simple reflex arc. In an article (not included in the present collection), he emphasised:

> The reflex is not the element of action, but the reflex itself is an elementary action (Bernstein, 1965).

The performance of purposeful movements requires continuous information from, and evaluation of the effect of movement, both as an instrument of reinforcement and for making the necessary corrections. From the beginning of the thirties Bernstein used what he termed the reflex ring to operationalise this experimentally based fact.

This statement parallels propositions formulated by Anokhin (1975) arising from his experimental work between 1932 and 1934 on the mutual connections between the activity of the central and peripheral parts of the nervous system. Both authors recognized the significance of recurrent afferentation for understanding the biological phenomenon of regulation and for which, Wiener (1948) later coined the term Cybernetics. In 1978, Anokhin, in his paper 'Theory of the functional system as a starting point for the construction of physiological cybernetics', refers to the work of Bernstein. He writes:

> During the last years N.A. Bernstein has elaborated this physiological architecture of behaviour acts very clearly (Anokhin, 1978).

Bernstein several times mentions the conformity between his idea of the reflex ring and the principal assumptions of Anokhin on the architecture of the functional system.

If Anokhin's model of the general architecture of the functional system (Fig. 1) is compared with the block diagram of an apparatus for the control

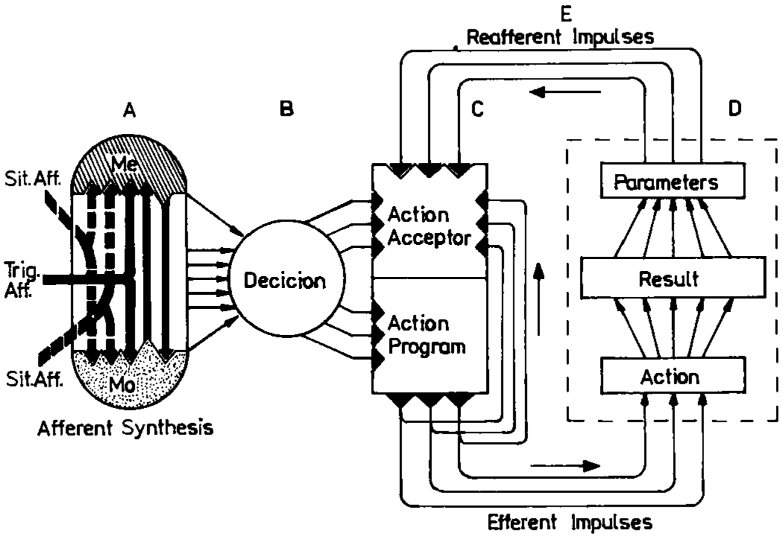

Fig. 1. General architecture of the functional system of behaviour according to Anokhin. A - afference synthesis: Sit. Aff. - afferences from the situation; Trig. Aff. - triggering afference; B - stage of decision; C - formation of the acceptor of the results of action and formulation of the efferent action program; D-E - analysis of the results of action and formulation of the reafferent impulses for the comparison of the preprogrammed and the real results; Me - memory; Mo - motivation (changed from Anokhin, 1978).

of movements designed by Bernstein (Fig. 2), the coincidence of the principal ideas on the participating elements is obvious. Both have the same circuit of efferent and afferent impulses (Anokhin calls them 'reafferent' - a term not acknowledged by Bernstein), the same comparator system (Bernstein: comparing system; Anokhin: acceptor of the results of action), and the same decision system for central control (Bernstein: commanding system; Anokhin: decision as effect of the actual afferent synthesis). Similar ideas on the continuous control of motion by comparing the motor command (feed forward) with the afferent feedback signals concerning the performed motor acts were developed in 1950 by Sperry on the basis of experiments with fish and by Von Holst and Mittelstaedt (1950) on the basis of experiments with insects. Independent of one another they postulated the existence of a trace of the motor command persisting for some time in the central nervous system and being compared with the afferent signals arising from the movements. This temporary trace was named 'corollary discharge' by Sperry (1950) and 'efference copy' by Von Holst and Mittelstaedt (1950). Until now, the intrinsic physiological nature of this short duration persistent trace has not been clarified, and it may be that its structure and mechanism are completely different in distinct situations, i.e. it may be more a principle than a determinate mechanism. Newer discussions about the role of efference copy ('effecence as a feedforward process') are more speculative than elucidative (Schmidt, 1976).

Bernstein does not discuss the possible intrinsic mechanism of the 'comparing system'. He thinks the most important question to be: 'What is the standard invariable determinant of this involved structurality of motor acts?' The answer to this question plays - as he asserts - the key-role in the complicated control system for goal-directed purposeful movements:

> ...the standard determinant both for the programming of motor activity and its effection and correction by feedback connections can only be the formation and representation of a motor problem by the brain in one way or another.

The greatest part of the relevant article (Chapter V) deals with the analysis of this statement and the search for the type of mechanisms which might be participating in the organism's solution of such a problem.

It is instructive that Anokhin (1968), independently, comes to the same conclusion. He stresses the necessity to regard the results of action as an independent physiological category. While the scientist is more interested in the 'reflectory action', for the acting animal or human subject the most important information is the result of his action. The prediction and control of the results of action play the leading role in all purposeful behavioural acts.

Astonishingly, the leading role of the results of action is not to be found in the schema of Anokhin (Fig. 1) nor in the block diagram of Bernstein (Fig. 2). In the motor response schema of Schmidt (1976) the 'desired outcome' also plays a leading role in the formation of the correct 'motor response schema'; but 'knowledge of results' is not connected with the 'desired outcome', and therefore the real dynamics between the planned program and the information on the real outcome which again influences the desired outcome is not shown. A similar block diagram is given by Batuev and Tairov (1978). Later (Fig. 3) a schema is presented in which this backward influence of the outcome on the formulation of the desired goal is considered.

This principal assumption that the formation and representation of the motor problem in the brain plays the deciding role in the performance and control of goal-directed motor acts raises some important questions concerning the type and mechanisms of reflection of the actual and the future situation of the surrounding world, the relation between the actual and the future model of the external world within the brain, and the plasticity of the motor coordination during the execution of the planned program to reach the desired goal. In addition, the way in which Bernstein and Anokhin treat the problem, taking the goal as the deciding element, raises both methodological and philosophical questions. In particular, a clear answer needs to be found to the question: Must this approach be considered as a teleological (vitalistic) one or not?

II.

The reflection of the real surrounding world within the central nervous system is one of the preconditions for building up movement programs which are determined to reach any external goal and to satisfy any important need of the organism. Neurophysiology has collected an immense experimental material related to this question. Stimulating any receptor organ one can find nerve cells or sets of nerve cells in the cerebral cortex as well as in distinct subcortical structures which are activated (or inhibited). The so-called primary, secondary and tertiary projection areas in the cerebral cortex illustrate the results of such investigations. In every textbook of physiology can be found the well-known homunculus for the representation of sensory and motor afferents in the post- and pre-Sylvian areas. Very precise study has been carried out on projections within the visual system. While it can be shown that there are distinct topographical projections from the retina to the geniculate body and from there to the cortical regions - as Bernstein indeed postulated - these projections never show a one-to-one correspondence. Instead it is found that the projecting pathways work as analysers for special features of the incoming visual information. Thus, in the primary projection area of the visual cortex can be found special neuronal networks for the representation of orientation, binocularity, depth, colour, and movement of visual stimuli. Starting with the retina and proceeding via the ascending pathways, it can be shown that the projecting visual system progressively extracts more and more abstract and general features of the visual information.

The experimental investigations of the last two decades have confirmed the assumption made by Bernstein that the projection of sensory information in the brain does not display a one-to-one correspondence between the elements of the perceptual fields or the organs of sensation and the cell network in the cerebral cortex. But the danger of atomism, about which Bernstein gives warning, arises again at the level of generalization. Thus, Gross, Rossa-Miranda and Bender (1972) believe that they have found cells in the monkey's cortex that respond only when the animal sees a monkey's hand. They do not even respond to a human hand. Such hypothetical cells for recognizing complicated features have been called 'gnostic neurones'. It is clear that this is a complete misinterpretation which does not take into consideration the very intricate function of the brain as a system (Young, 1978).

The question is even more difficult if the projections to the secondary and tertiary cortical fields and the multiple intervening connections between the different areas of the cortex and between the two hemispheres are considered. Obviously, in these complex areas of higher order there is not only a coding of intermodal influences, but also integrated neuronal circuits which are able - within the systemic organization - to organize and control special elements of complex motor behaviour. The most convincing study in this field is the investigation of the neurones of area 7 in monkeys by Mountcastle and his co-workers (Mountcastle, 1975; Lynch, Mountcastle, Talbot & Yin, 1977). They trained monkeys to execute tasks requiring the fixation of stationary visual targets, the tracking of slow moving targets and the making of saccadic movements to foveate those which suddenly jumped from one locus to another within the field of view. During these tasks they found, in area 7, different classes of cells with visuomotor neurones, in both a special and conditional sense: visual fixation neurones, visual tracking neurones and a great number of cells whose properties in the behavioural task used could not be specified precisely. Mountcastle does not try to give an atomistic interpretation of these results; rather, he supposes that there exists within the parietal lobe a neuronal mechanism for directing visual attention (only within the distance which can be reached by the monkey), for the fixation of gaze on objects of interest (the task must be highly motivating), for maintaining visual grasp of the object if it moves slowly, and for loosening fixation and initiating rapid saccadic movements towards new objects of interest presented within the visual field. He expresses the idea that in area 7 can be seen a command operation linked to certain motivational states and combined with a continually updated central representation of the relations between the body, head and eyes, and surrounding space. But such experiments, as subtle and interesting as they are, cannot yet answer the question put by Bernstein, according to which mathematical law the external world is represented in the brain and becomes the basis for goal-directed future behaviour. Most people would agree that this representation (or representations) must be 'constructed along the principles of a model', and that in no sense can the representation be isomorphic. So far no one has been able to demonstrate operator principles of this process. In this connection, the demand of Bernstein that these operator principles must ensure, under all circumstances, an active mental modelling of the world is very important.

During the last few years a completely new idea which attacks this very intricate set of questions was proposed by a scientist - Edelman (1979) - who earned his merits in a field seemingly far from neurophysiology, immunology, for which he was awarded a Nobel laureate. He suggests a selective theory of brain function in which the unit of selection is a neuronal group (Mountcastle, 1979). Such neuronal groups containing up to 10.000 neurones are formed during embryogenesis and during epigenetic development. Within each neuronal group intrinsic connections exist and can involve a variety of modes of interaction within a local circuit, including synaptic and all modes of non-synaptic interactions. These intrinsic connections - he assumes - have great variability from group to group. Among the groups, extrinsic connections exist, which are also specified by gene programming and synaptic selection. Neuronal groups of different structures and connectivity form primary repertoires (one repertoire comprising up to 10^6 cell groups) which can respond to or recognize a particular signal pattern. This many-to-one response implies that each repertoire is degenerate. By degeneracy Edelman means that, given a particular threshold condition, there must be more than one way of satisfactorily recognizing a given input signal. This implies the presence of multiple neuronal groups with different structures capable of carrying out the same function more or less well. Therefore, degenerate groups are iso-functional, but non-isomorphic. The polling of such <u>degenerate primary repertoires</u> by signals leads to associative recognition. Moreover, repetition of signals interacting with selected neuronal groups results in the emergence of secondary repertoires of groups with a higher likelihood of response.

Based on these principal assumptions, Edelman considers a hierarchy of responses that, in its later stage, will be non-linear because of the presence of feedback and feedforward loops with their associated alterations of temporal patterns and response times. In the hierarchy:

$$S \rightarrow R \rightarrow (R \text{ of } R)_n, \quad n = 1, 2, 3, \ldots$$

S represents transduced sensory input from the environment, R represents cortical cellular groups that can act as 'recognizers' of the input, and (R of R) represents groups of neurons in the association cortex, or in temporal, frontal, or prefrontal cortex, that act as 'recognizers of recognizers'. Thus, by the selection of certain subgroups, altering their properties as a result of experienced input and altering also the probability of future selection of these groups over their neighbours, a secondary repertoire as a collection of different high-order neuronal groups is produced Following this line of reasoning, the central nervous system can recognize modes of itself selectively and in a nonlinear and degenerate fashion. By this means, Edelman shows how degenerate selection and cell-group signalling in a re-entrant fashion could provide the necessary conditions for the explanation of consciousness at the cellular level.

In general, this type of modelling, the representation of the external (and internal) world within the brain, answers the principal arguments raised by Bernstein. It has the advantage of an associative system with successive recognitions across different levels of hierarchical nesting in a non-linear way. The system of group-degenerate selection shows all features of a distributive property, and it treats subjective phenomena like consciousness in a monistic manner as a process occurring in certain defined gross areas, for example the two cortical hemispheres, the thalamo-cortical radiations, and the limbic and reticular systems. It also explains the increasing plasticity at every succeeding hierarchical level of the brain. But - as Edelman (1979) states -, the described scientific theory:

> ...proposes a particular set of mechanisms to account
> for higher brain function, and it must stand or fall
> on their verifiability.

Nevertheless, it sets forth a principle of brain functioning which opens the way for future research into the intrinsic operator for modelling.

III.

Bernstein assumes that the basic mechanism by which the central nervous system controls, simultaneously, the large number of degrees of freedom of the multijointed, musculoskeletal system, must be a modelling of the future, and the programming of movement must be dependent on rules that permit original and creative motor solutions to problems such as reaching for objects in a structured space. During the last two decades, complex electrophysiological methods in combination with lesions and cooling experiments have been used to tackle this intricate question. Special attention has been directed towards the classification of different types of movement, as Bernstein had already done, to the hierarchical structuring of the initiation and control of different movement programs, and to the different types of feedback control.

Now-adays it is widely accepted that motor programming at different levels of the central nervous system is a basic principle of all active behaviour of animals and humans. Locomotion is one example of an action that has been extensively studied for this purpose. In higher animals and humans there are inborn local circuits in the spinal cord subserving the generation of the necessary agonistic and antagonistic impulse patterns for simple, more or less rigid locomotory acts. At higher levels of the central nervous system local circuits provide more complex programs, the execution of which is realized by using the simpler programs at deeper levels of organization in a more and more differentiated way. This 'multiplex hierarchical complexity, localized in the numerous centres and systems of the brain' ensures high redundancy and the possibility of learning by feedback, command and control flexibility, and command specialization and plasticity (Herman, Grillner, Stein & Stuart, 1976).

This is not a simple 'top-down' hierarchy, but a system that is distributed in space and time:

> Spatial distribution consists of parallel transmission on many kinds of information; somesthesia and kinesthesia ascending to the cerebellum as well as to the cerebrum. Temporal distribution results from unceasing activity in all afferent and efferent parts of the sensorimotor apparatus. Movements thus evolve in multiple ascending and descending loops whose activities are distributed in time and space (Brooks & Thach, 1981).

On the highest level of motor coordination this task is distributed in a very intricate and flexible manner between sensorimotor cortex, basal ganglia and cerebellum. The complex organization of motor programs is provided by these three main structures of the central motor system in such a way that the adequate type of movement can be performed at the necessary time with the necessary force and velocity (one including the other) to reach the intended goal in external space (Bizzi, Accornero, Chapple & Hogan, 1981; Miles & Evarts, 1979).

The actual existence of complex central motor programs completely sufficient to reach any goal, if learned in an orderly manner is confirmed by much experimental data. Thus, ballistic movements can be performed in the correct (learned) fashion, if all afferent control signals are excluded (Lamarre, Spidalieri, Busby & Lund, 1980). The cerebellum plays a special role in programming and controlling the execution and stopping of relatively simple ballistic movements (Kornhuber, 1971, 1974). The command for a ballistic movement occurs in the cerebral association cortex and passes through the cerebellum via the pons and returns to the motor cortex via the thalamus. Cooling the involved cerebellar parts of this cortico-cerebellar-cortical loop only delays, but does not exclude their exact execution. Cortico-cortical connections provide an alternative pathway for the initiation of ballistic movements, although after a longer delay. Fast and slow intended movements, on the other hand, which need continuous feedback control, are programmed through the special participation of cortico-striatal loops (De Long & Georgopoulos, 1981; Garibian & Gambarian, 1982); but cerebellar circuits are also included in this control mechanism (Miller & Brooks, 1982). It can be concluded that the brain has many operational motor programs at its disposal, at the level of the cortico-cerebellar and cortico-striatal loops including some thalamic structures. This gives the possibility to select the appropriate program according to previous experience in a very flexible way and to change the selection, if required by the actual environmental or internal (need) situation. Thus, the movement activity can be changed in time and space according to the goal that should be reached.

The choice of an adequate motor pattern is mostly determined by the influence of the association cortex on the previously mentioned motor systems. This part of the cortex receives and integrates the information coming from internal and external feedback which, in parrallel, also goes to the cerebellum. But, whereas the cerebellar circuit has only a self-sustaining control of simple programs such as ballistic movements and, in addition, helps to realize the higher intent, what is still under discussion is:

> ...how the necessary codes and commands are transferred from association cortex to the machinery of movement (Brooks & Thach, 1981).

In a well trained situation the task-relevant triggering signal determines at any level which preprogrammed movement pattern at which level of sub-programs will be initiated. On the other hand, feedback may order major program changes when the program is grossly inappropriate, and higher levels of programming must be included to prepare a more adequate program. But, feedback may also be used to monitor small errors and enhance modest corrections at deeper levels. In other cases, it may aid in coordinating one movement system with another, as when raising a limb requires background postural adjustments (Keele, 1981).

Thus, modern brain research has confirmed the assumption of Bernstein that the central nervous system has a 'highly homeostatic hierarchical system of coordination of motor function in all its complexity' at its disposal. This system can be used to realise those purposeful motor acts, necessary from moment to moment, to reach intended goals. The fundamental condition for this to take place, must be the:

> ...modelling of the future by means of extrapolation from whatever the brain is able to select from the current situation, from the fresh traces of the immediately preceding perceptions, and finally from those active trials and assays ... as orientational reactions whose fundamental significance has certainly been underestimated (Bernstein, 1967).

The parts of the brain which provide this leading role are the frontal lobes in continuous direct and indirect connection with the association cortex. The pre-frontal cortex is the substrate for cognitive operations of short-term memory and preparatory set that allow the appropriate choice, timing, and use of the necessary motor programs. Because it is continuously updating the model of the future in a probabilistic way, it appears indispensable for the protection of purposive behaviour from competing drives and for inhibiting the execution of untimely acts. It realizes these tasks by efferents to the striatal-cortical-cerebellar loops on the one side and by way of efferents to the limbic system and the diencephalon on the other side (Fuster, 1981). How this 'probabilistic extrapolation of the future' as the fundament for adequate choice and correction of the actual motor programs is realized in detail, remains open to question. But, the demands Bernstein has formulated for the basic mechanisms of this process, remain valid for future research. The approach proposed by Mountcastle (1979) and Edelman (1979) may be a high-road to disclose substantial mechanisms of this process in the brain.

Electrophysiological study of slow potential shifting in the human scalp records during the preparation of voluntary movements confirms the assumption of pre-programming preparation at the highest levels of the brain. About 800 msec before the execution of a voluntary movement a negative shift of the cortical potential bilaterally can be observed which extends far into the pre-central and parietal areas. This potential has been called Bereitschafts-potential (readiness potential)(Deecke, Scheid & Kornhuber, 1982). The amplitude of this potential is interestingly enough, connected with the probability of feedback guessed by the subject (Hink, Kohler, Deecke & Kornhuber, 1982). Ninety milliseconds before the start of a movement, the potential displays a pre-motor positive shift which is also bilateral and extensive. At least, 50 msec before a movement the negative, so-called motor potential, appears and is only present in the motor cortex contralateral to the moving part of the body; it is immediately followed by the EMG response of the involved muscles preceding their contraction. These slow potential changes, especially over the associative and frontal areas of both sides of the cortex, may be interpreted as a sign of the probabilistic adjustment of the modelling of the future and the selection of the most adequate triggering signals. The same is valid for the so-called negative contingent variation, a widely extended cortical negativity, which depends upon the contingency between some event and an expected, task-relevant stimulus such as a feedback signal (Walter, Cooper, Aldridge, McCallum & Winter, 1964).

Bernstein's assumption that in the central nervous system channelized processes ('impulse activity ...transmitted without damping along nerve fibres') and slower wave-like activity which is 'spread across the fibres' are operating together is confirmed by more recent investigations (Schmitt, Dev & Smith, 1976). It must be extended by the (at that time unknown) fact that there are triadic and other complex configurations of synaptic contacts, and substance exchange between nerve (and glial) cells through specialized channel structures (gap junctions, tight junctions). This must be especially effective within the local circuits of neuronal groups suggested by Edelman (1979) and others. The neurones within these groups are so densely arranged that every form of information exchange through channelized electric currents and electrotonic influences as well as through mutual substance exchange must be possible. The intra-neuronal and inter-neuronal transport of various types of substances suggests that the biochemical and bioelectrical parameters are functionally interwoven.

Also, Bernstein's statement that:

> ...proceeding with a determinate program of operation, the central nervous system ... achieves anticipatory adaptations in terms of the tuning in advance of the arousal of all the sensory and motor elements which are employed,

is completely confirmed by numerous experiments. This anticipatory adaptation, can be demonstrated at all levels of the motor system, from the highest goal-programming level at the frontal-associative cortical areas (see above) to the pre-innervation of agonistic and antagonistic motor units at the spinal level (Koc, 1975). The central movement program comprises all preparatory innervations that are necessary for the exact execution of the impending movement. They are acquired on a probabilistic base as result of learning and training and are ready to be changed on the same basis if they are not appropriate for the given task in the respective situation. In addition, the central motor pre-programming includes commands to the different vegetative functions which provide the necessary prerequisite to the planned motor action. This is clearly seen in the preparatory activation of metabolic, cardiovascular, and respiratory functions immediately before any intended performance.

This strong inter-connection between the motor commands and the commands to the vegetative systems may be used in special cases to disclose some preparatory and executive features of the model. For instance, this is possible by having subjects mentally perform a well-trained motor program and simultaneously recording the changes in some peripheral parameters. This may be illustrated by the example, shown in Fig. 2. A well-trained swimmer sits quietly in a chair. In front of his nostrils a thermosensitive sensor is attached by means of which his breathing may be recorded continuously. The subject is asked mentally to perform swimming over a given distance in a

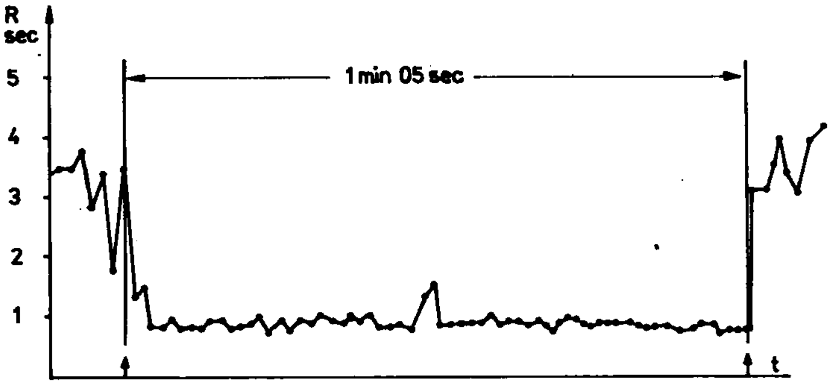

Fig. 2. Acceleration of the respiration frequency during mentally (ideomotorly) swimming a given distance in a predetermined time. R - duration of the consecutive breathing movements in sec; vertical arrows and lines - start and end of the ideomotor "swimming movements" (from Pickenhain & Beyer, 1979).

predetermined time. As is well known, there exists a strong correlation between the swimming movements of the arms and the respiration rate. This means that in a well trained subject the commands for the rhythmic respiration movements are integrated constituents of the complex central motor program. Therefore, the whole subjective performance of the mental swimming can be followed by looking at the respiration curve. At the first vertical line (Fig. 2) the subject announces, by pressing a button, that he is starting his mental swimming. The respiration curve indicates a strong acceleration which coincides with the frequency of the mentally executed arm movements. In the middle of the distance, the moment of the turn (a short deceleration of breathing) can be seen, and in the second half of the distance the subject evidently performs mental swimming with the same time program as in the first half. If the subject mentally reaches the goal, the respiration rate goes back to its original value; this the subject indicates by again pressing the button (the second vertical line) (Pickenhain, 1976, 1977).

This phenomenon is only observable, if the subject imagines the performance of the swimming movements very vividly and 'as really doing it'. By this way, the efferent impulses of the complex motor program of swimming reach all parts of the central nervous system which serve as subprogram effectors of the whole process of realization. The real movements of the extremities are inhibited by the given instruction to sit quietly and not to move. But, the EMG record may indicate a sub-threshold innervation of the involved muscles (the so-called ideomotor or Carpenter effect). Thus, can be shown, how precisely the subject is able to reproduce his trained motor program mentally and how well he executes his ideo-motor training program (Pickenhain & Beyer, 1979).

Ideomotor training is now widely used in the practice of sports and in the applied work physiology. The learning effect which can be observed is difficult to explain by the assumptions of the classic principle of feedback reinforcement. Because signals from the movement result are lacking, it must be supposed that internal feedback signals at the different sub-program levels involved are responsible (Fig. 3). The necessary pre-condition consists of the involvement of as many lower levels of the central nervous system as possible in the mental reproduction of the leading motor program. This supplies the higher centres with the necessary reinforcing feedback information. The occurrence of respiration changes in the respective case (Fig. 2) is an obvious indication of the participation of lower levels, as is the evidence of sub-threshold EMG in other cases.

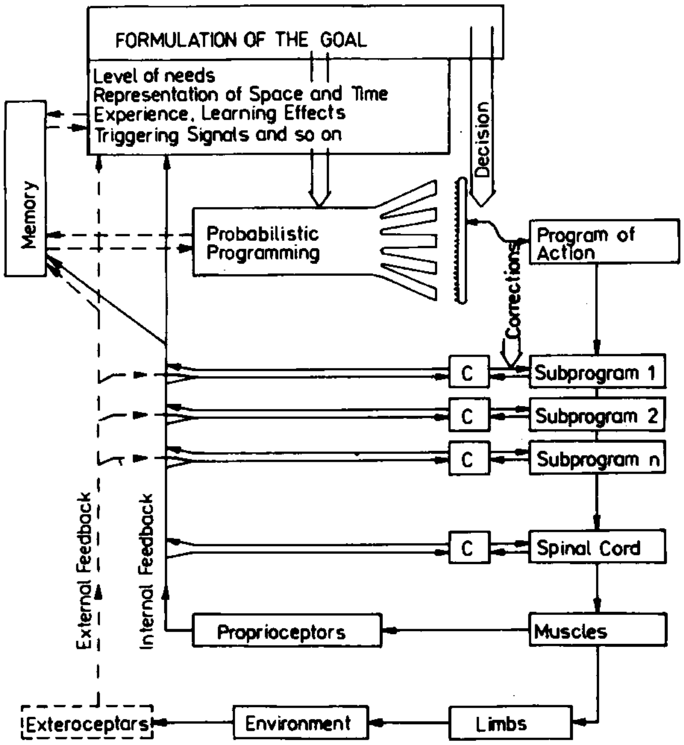

Fig. 3. Scheme of the goal-directed programming-and decision mechanisms with the external and internal feedback pathways and their correction influences (C) at multiple hierarchival levels. Continuous lines - obligatory information pathways; interrupted lines - facultative information pathways (changed from Pickenhain, 1977).

The question as to the manner in which time is represented in the brain is difficult to answer. It is a crucial question for Bernstein's idea of the modelling of the future by means of extrapolation leading to a probabilistic image of the future external world. He recognizes this difficulty when he states:

> ...that in the brain there exist two unitary opposed categories of forms of modelling the perceptual world: the model of the past-present, or what happened and is happening, and the model of the future. The latter proceeds directly from the former, and is organized in it. These are necessarily distinct from each other, chiefly because the first type of model is single-valued and categorical, while the latter can only operate by means of extrapolation to some or other degree of confidence or probability.

It must be added that this question is even more intricate, because in biological subjects neither space nor time have a linear structure. There are many observations which show very clearly that the biological structure of time is not congruent with that of physical time. There is no simple continuity from future through present to past. Rather, there exists an asymmetry and - as observations of brain-injured patients have shown - possibly a difference in central representation (as a very variable function), the future being more connected to the left and the past to the right hemisphere (Bragina & Dobrochotova, 1981). If this is correct, it would have serious consequences for the processes which are participating in the probabilistic modelling of the future and their transition, by the decision in the present, to the 'single-valued and categorical' experience of the past. The origin of the model of the present-past resulting from the probabilistic model of the future is up to now completely unclear and urgently needs further research and elucidation.

IV.

The philosophical background of Bernstein's work was acrimoniously disputed. Therefore, some remarks on his methodological position seems necessary.

Firstly, his monistic conception of the mind-brain problem is completely obvious. Without special discussion he treats the psychic processes as functions of the highest levels of brain structure, and he uses psychological and neurophysiological terms without any mutual exclusiveness. He considers modelling of the future equally as a mental as well as a biological phenomenon. The leading factor of all purposeful motor acts - the desired goal - is the expression of the needs of the organism as well as an idea the organism wishes to realize. In these terms there is no argument by which he might be accused of adopting an idealistic (or vitalistic) position about the mind-body problem. His position is much nearer to the conception of emergent materialism proposed by Bunge (1980).

Secondly, Bernstein has severely criticized the oversimplification of the findings of Pavlov by some of his pupils and followers. He did this especially in two papers which are not included in this collection (Bernstein, 1975): 'New trends in the development of the physiology and biology of activity' (published 1963) and 'On the road to a biology of activity' (published 1965). Acknowledging the immense merits of Pavlov at the beginning of this century in the discovery and investigation of the conditioned reflex, Bernstein at the same time opposes the exaggeration implicit in talking about a 'slave reflex' or a 'reflex of liberty'. He also contradicts the extension of the conditioned reflex conception to all mental processes and the dogmatism of some of Pavlov's followers who 'have distorted the reputation of this excellent scientist known all over the world'. On the other side, Bernstein really developed the reflex conception by adding the afferent feedback mechanism ('closing the reflex ring') and so connecting it with the concept of cybernetics.

Bernstein's reduction to absurdity of the idea of the second signal system as a scientific term which could explain the existence of words, verbal thinking, and articulate speech in humans has to be accepted. Whereas the first signal system is a collection of representations of the directly perceived objects and events in the external world, the second signal system should be a:

> ...projection of the projection, in which every object signal, image element of the first system, will have its corresponding name in the second.

This would presuppose a two-fold one-to-one projection which is not yet valid for the first system alone (see above) and which never can explain the nature of language, this 'inexhaustibly powerful tool of thought'. Pavlov expressed the idea of the second signal system during the last years of his life in a more symbolic manner to demonstrate the new quality of the human higher nervous activity. He never did support it by any experiments, and his followers also were not very successful in this direction. Therefore, this term may maintain its significance for symbolic demonstration and didactic purposes, but cannot be fundamental for scientific work.

Thirdly, Bernstein was accused of propagating vitalistic and teleological opinions, because he assumed that the modelling of the future requirements of the individual, a model of that which is not yet, but which must be the case, determines the motor actions of the present, i.e. the goal guides the on-going behaviour. This assumption does not fit in with ideas about linear causal connections between all natural events. But, as has been shown by other authors, the idea of linear causal connections is not sufficient to explain the real events in the world of living organisms. Therefore, the term teleonomy was created (Pittendrigh, 1958). This term is intentionally opposed to the term teleology which tries to interprete biological regulatory mechanisms in an unscientific, idealistic way. Teleonomic connections comprise complexes of causal connections which, uniting necessity and chance, are interwoven into a closed (cyclic) process, thus determining the goal-directed and purposeful behaviour of organismic systems. Already in 1961 the term teleonomy was successfully used by many biologists during the Cold Spring Harbor Symposium. Monod (1970) used it in his analysis of 'Change and Necessity' to characterize:

> ...objects that are equipped with a plan which they represent by their structure and which they realize by their performance.

This means, that a new philosophical approach and terminology are necessary to explain the inherent laws of closed biological systems which are both self-regulating and goal-directed. This is a complete confirmation of Bernstein's conception of modelling the future requirements, and on the other hand, Bernstein's arguments are important contributions to support the idea of teleonomy.

Fourthly, Bernstein repeatedly stresses the importance of extrapolation as a means which the brain uses to select the adequate solution to the probabilistic possibilities of future modelling. It is very interesting that the Soviet biologist Krusbinskii (1962, 1977) during his experimental study of the behaviour of different kinds of animals (birds, mammals) has found extra-polation behaviour to be an essential and very important constituent of the complex behaviour of animals. In conclusion, he states that the extra-polation behaviour and the modelling of future events are leading factors for the different purposeful motor acts of animals and must be interpreted as elementary reasoning (Krushinskii, 1977). His experimental data are important confirmations of the theoretical implications of Bernstein's idea.

Fifthly, up to now, the conception of a homeostatic balance between organism and environment is widely acknowledged. But, scientists who follow this line do not realize the real consequences of this idea. Bernstein (1963) - in a paper not included in this collection - points to the fact that such:

> ...homeostatic balance would sentence every individual to be entirely dependent on the environment and on its changes; a program-regulated morphogenesis with the preservation of constant species features would be completely impossible.

By this, Bernstein does not deny homeostatic partial mechanisms, but he refuses to acknowledge that the homeostatic principle plays the leading role in directing the behaviour of the organism. Life is by no means a:

> ...homeostatic balance with the environment, as the thinker of the classical period of mechanics assumed, but the overcoming of the environment. In this connection, the goal of the organism is not to maintain homeostasis, but the further development in the direction of the species-specific self-sustaining and developing programs (Bernstein, 1963, 1975).

Again, can be seen that in Bernstein's opinion the deciding and leading element in the behavioural architecture of movements is the desired goal the realization of which is pre-programmed in the model of the future. This leads back to his assumption that the formulation and execution of the model of the future is by its very nature an active process. This active process starts with the representation of the world in the brain which is only reached by active exploratory and checking processes and which is followed by the active performance (or active inhibition) of motor acts that are adequate (or inadequate) to reach the desired goal. This includes a temporary disturbance of the homeostatic balance between the organism and its environment which is re-established if the desired goal is reached.

Therefore, the activity of the organism is a particular category in itself which ensures the existence of the organism and of the species. Comparing the probabilistic model of the future with the desired and necessary goal, the organism's activity represents the dynamic struggle of the individual and raises the probability of the desired outcome, until it becomes an accomplished fact. This means realization of the genetic program by continuous battle during epigenetic development in a changing world. Bernstein postulates that this fact must be the topic of a special discipline

- the Physiology of Activity. He outlines some essential principles of this special direction of research. But, during the twenty five years which have passed since this postulate was first stated, no one has taken over this difficult task, although many scattered results in nearly all life science disciplines have underlined its great importance. The realization of this postulate remains open for future research.

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SECTION 6

Chapter VI	**TRENDS IN PHYSIOLOGY AND THEIR RELATION TO CYBERNETICS** **N. Bernstein**
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CHAPTER VI

TRENDS IN PHYSIOLOGY AND THEIR RELATION TO CYBERNETICS

N. Bernstein

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The introduction and development of new methods in industry have acted as a spur to the new trends in physiology with which we are concerned in this article, and to the formulation of the general problems which led, after a series of attempts, to the establishment of cybernetics as a science. If, when we think of cybernetics, we have in mind not the doctrine of Wiener, Shannon, Ashby, etc., but a general science which is concerned with the regulation of complex systems in conjunction with information and communication (and it is this which we shall consider from now on), it will be seen that a considerable number of the problems confronting modern physiologists are closely related to the more general ones for which cybernetics was first intended.

As the amount of heavy physical labour steadily decreases in everyday work, applied biology, starting with the energetics of work, biomechanics, protection and hygienics of work, etc. has begun to turn its attention to problems concerning intellectualized work in man-machine complexes, rationalization of control and communication, distribution of function etc., and it is precisely in elucidating such problems that the methods and concepts of cybernetics have turned out to be particularly valuable. An important area in modern applied psychophysiology is undoubtedly the study of work under conditions where very high demands are made on the subject's attention, adaptability and will, etc. (astronautics, high-speed flight, work at high attitudes, underwater or underground).

There are some recent offshoots of theoretical physiology which deserve mention and consideration here. The first one - the physiology of regulation - was established at about the same time as cybernetics and was to a certain extent its forerunner; the second - the physiology of activity - is emerging and developing, opening up a wide range of problems which have not been realized before.

In increasing our knowledge of the principle behind the cyclic regulation of the life processes we have discovered more and more about activity which is bound up indissolubly with that principle. The active form and structure of all the processes of reception and of central processing of information is no longer a matter of doubt, not to mention the phenomena and forms of activity - in the strictest sense of the word - namely, motor functions (where the prevalence and significance of the cyclic principle of regulation and correction have been described repeatedly). Nowadays Sechenov's thesis has been fully vindicated. He declared that we "listen rather than hear, and look rather than see". All our main peripheral receptors are equipped with efferent innervation and a muscular system on which depend functions of optimal adjustment (in a very broad sense), and also the countless phenomena of search, guidance, haptic tracking, etc.; here, also, are included all types of verification through practical experience of both concrete receptions and the entire system of sense organs, cross-checking, and the synthesization of evidence obtained from the various receptors in the organizational system of the sensory receptors; finally, selection of the necessary minimum of information and rejection of superfluous noise are active processes.

The significance of active forms of operation has been felt most strongly perhaps in the central cortex processes which deal with the construction of a dynamic image of the external world inside the brain. Whereas ideas concerning cellular centrism have been essentially inseparable from an assumption of the passive nature of the brain's intake and registration of sensory information, performed by primordial cells intended for this purpose, modern psychophysiological thinking tends to regard the cognitive processes as active simulations differing in principle from the mechanistic element-to-element relation. The selection of the principle by which perceptible sets of numbers are put into order is an active one, and so is the internal classification of separable subsets and the regulation of the haptic in the widest sense of the term, that is, regulation of the processes of active reception that were mentioned above (see, also, Chapter V).

The source of the principle of activity in living organisms, however, lies much further back than has yet been stated, and from this principle attains the significance of a significant general biological factor. Research into the subject should proceed from an analysis of motor functions.

Motor functions comprise a basic group of processes by which the organism not merely reacts to its environment but ever acts on the latter.

This fact allows the following conclusions to be drawn.

Each significant act is a solution (or an attempt at one) of a specific problem of action. But the problem of action, in other words the effect which the organism is striving to achieve, is something which is not yet, but which is due to be brought about. The problem of action, thus, is the reflection or model of future requirements (somehow coded in the brain); and a vitally useful or significant action cannot be either programmed or accomplished if the brain has not created a prerequisite directive in the form of the future requirements that we have just mentioned.

From this it would seem that we are looking at two connected processes. One is a probabilistic estimation based on the current situation - a kind of extrapolation ahead over a period of time; both neurophysiologists and those engaged in clinical studies are already gathering empirical data and observations which throw light on these processes.* Together with this probabilistic extrapolation of the course of surrounding events (it would be a course under conditions of non-interference), there is the programming of action which is intended to fulfil the future requirements. This (simple or chain) programming of action already resembles a kind of extrapolation between the actually existing situation and what the latter has to become in the interests of the subject. We shall not delay over the fact that programming and accomplishment of action usually both take place under conditions of vital pressure, that is, an inner conflict between urgency and accuracy of estimation; nor over the fact (completely obvious) that the practical accomplishment of action takes place necessarily as a struggle or active surmounting of variable external obstacles, whatever form these may take (insuperable external opposition, opposition from an enemy, surprise, etc.). It is interesting to note here that acknowledgement of the reality of a model coded in the brain, or of an extrapolation of future probabilities, and of a reflection of problems of action in the brain as a formula of future requirements, allow a strictly materialistic interpretation to be given to the concepts of singleness of purpose, expediency, etc.

Facts which are nowadays established, such as the coded reflections of informational material, whether original or recombined by the brain, were still completely unknown when scientific physiology was in its early

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* The so-called orientational reactions are a category of responses made by the actual reception to a divergence from or disagreement with the current probabilistic estimation (a low probability response).

period of development. Therefore most ideas such as the problem or aim of action answering to the requirements of the organism, i.e. a coded program aimed at producing the best possible conditions of existence for the organism, etc., were regarded as inherently part of psychology - a highly developed branch of science capable of formulating for itself the routine problems and aims of action. Scientists holding materialistic views were faced with a choice of two alternatives; either to admit the presence of a psychic force and consciousness in the earth worm and the tree - this of course was rejected as being absurd - or to believe that not one of the classes of motion discussed could be applied to the vast majority of organisms. The only ones who felt at ease in this sphere were the adherents of vitalism whose unsupported hypotheses enabled them to proceed as far as they desired in the direction of finalism.

It is precisely the discovery that an organism can construct and combine material codes that reflect all the countless forms of activity and extrapolation of the future, from tropisms up to the most complex forms of directed reaction to environment, that enables us to speak now of singleness of purpose and endeavour, etc., in the action of any organisms, ranging perhaps from Protista upwards, without the slightest risk of our slipping into finalism. The empirical data now supplied by comparative physiology testifies to an unsuspected variety in the material substrates of regulating codes and in the actual coding forms and principles themselves, among which the psychological codes of the human brain are seen as only one of the individual forms (although the most highly developed one).

It would be appropriate to add the following to this brief discussion of the problem of the simulation and programming of actions designed to bring about the optimal future state.

The physiologists of the classical period, by sticking to the idea of the reflex arc and confining their attention strictly to response processes, were able, with only slight schematization, to represent the effector processes of the organism as strictly (and in the majority of cases unequivocally) determinate signals arriving through an afferent semi-arc. Now, when we are forced by the facts to regard all phenomena pertaining to the organism's interaction with the world, and its even more active influence upon it, as being cyclic processes based on the principle of a reflector circuit, our evaluation of the existing correlations undergoes a fundamental change. Unlike the processes that supposedly take place along an open arc, the closed-circuit (ring) process may just as easily be started from any point on the ring; and as a result the processes of interaction termed (in the old sense) reactive (i.e. those originating from the afferent semi-ring) and those termed spontaneous (i.e. originating from the effector semi-ring) are both here combined into a single general class. In many respects it is precisely this latter subclass which includes the most vitally significant phenomena of activity. Meanwhile, in all such cases the organism not only reacts to the situation or to some particular singificant element discernible in it but also encounters a situation which, being dynamically variable, forces it to make a probabilistic forecast and then a choice. To use a metaphor, we might say that the organism is constantly playing a game with its environment, a game where the rules are not defined and the moves planned by the opponent are not known. It is this peculiarity of real relationships - the evaluation of situations by the organism is probably no less sensitive than its choice of action - that distinguishes the living organism from a reactive machine no matter how sensitive and complex the latter may be. Reactive mechanisms play an important role as technical components in the adjustable regulation of action, but never as direct determinants of action and behaviour. It is very easy, perhaps for just this reason, to construct a reactive model capable of forming both unconditioned and conditioned reflexes (see, for example, the models of G. Walter, etc.), but to build a model which carries out (or improves) a choice of optimal behaviour under conditions of purely probabilistic information about the opponent's moves entails difficulties which up to now have not been successfully solved.

Recent research has led to re-formulations of the concepts of organism, which are in many respects different from the formulations of the classical period - in which the organism was treated as a reactively equilibrating or self-regulating system. An organism must be considered to be an organization possessing two determining features.

Firstly, it is an organization which preserves its own (systemic) identity in spite of the continuous flow of energy and substrate matter passing through it. Despite the fact that not a single individual atom in the organism is retained in the cell structure for longer than a certain and (with few exceptions) comparatively brief space of time, the organism remains the same now as it was previously and its present life activity is determined by its previous life (and, of course, not only previous).

Secondly, the organism is continually changing in a set direction at all stages of its existence. The fact that thousands of examples belonging to the same animal or plant type develop into species that are identical in all their basic determining features (in spite of the often enormous differences in living conditions between the various individuals) is ample proof of this directional trend in ontogenetic evolution. As regards the embryogenetic stage, we know today the carriers of heriditary traits, their chemical structure and code alphabet, by means of which the organism possesses from the moment of fertilization of the ovum a coded model of its future development and a coded program of the consecutive stages in this development.

It should be clearly realized that the chromosome "mastercopy", i.e. the determining factor in the future of a species, is unquestionably material, in the sense that as regards the genus as a whole and the immediate ancestors of the species in particular, it springs from the past and is determined by it (as we are not competent in the field we shall not go into the question of the possible origin of mutations). But the most significant and striking fact lies not in this totally incomprehensible programming of the future by ancestors for their immediate offspring, but in the dynamic source (eventually also coded somehow, probably, and possessing its own somatic substrate carrier in the cell), which generates in the species an active, anti-entropic, overwhelming urge to realize the model. This movement towards structurization, that is to say, towards lower probability of the images which are being created, involves large metabolic expenditure of valuable energy, as does any anti-entropic process. This liberality displayed by the organism in the face of all obstacles, whether great or small, in order to realize its programmed morphogenesis is a most impressive phenomenon.

We have a close analogy (in miniature) to this supposed conflict between the future (for the immediate species) and the past (for the genus as a whole) in the case which we considered above, where the modelling of future requirements was seen as the formulation of a problem of action. The result that a living being requires for a given action is in the future; but, of course, the forecast, and program, and even the co-ordinating technique of this action, are based on past experience accumulated by the individual and by the genus, only recombined actively anew.

Let us now return to our main theme after this digression. The above-mentioned identity in all the essential features of morphogenetic development, seen against a background of variable conditions and independently of their variability, tells us that the organism actively overcomes possible or unavoidable external obstacles, which might prevent it from accomplishing its program of morphogenesis. Experimental data concerning damage and partial amputation to which, for example, the buds of the extremities have been subjected in embryogenesis, and which did not prevent them from growing into complete extremities; the facts of anatomical and also functional regeneration; the mass of clinical material that testifies to the struggle made by the organism and its functional system to overcome pathogenetic factors - all this data supports the view that both the organism as a whole and, very possibly, each of its cells struggle actively for survival, development and reproduction. The life process is not a balancing with the environment,* but a conquest of the latter, aimed not at preserving the status quo or homeostasis, but at an advance towards fulfilment of the developmental and self-preservational program of the genus.

And so, what in a particular case of motor functions in organisms appears to be (1) the modelling of future requirements in terms of a problem of action, and (2) the realization of an integrated program of this action by the conquest of external obstacles and by an active struggle for the result, turns out to be a manifestation of the general principle of activity running through the whole of biology. The principle emerges in the forms we have discussed and in the processes of development both of animals and of plants and in all features of their struggle for existence.

A very interesting problem, and one which seems relevant to the entire domain of biology, now arises. It is connected closely with the theoretical principles of biological model-making and also with the directional evolution of the individual. This question and its ramifications call for comprehensive study.

We shall begin by drawing a number of parallels between processes that are, externally, highly unlike, with the intention of formulating their common characteristics.

On an oak or a maple there are several thousand leaves, no two of which will be found to be exactly alike; in fact all their metrical features vary over a wide range. Nevertheless, we can confidently say that

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* Such a balancing would mean that every species is completely dependent on its environment and the changes therein, with the result that we could not hold the theory of a programmed morphogenesis and the retention of persistent features of the genus.

each leaf belongs either to an oak or a maple, because of certain characteristics which we have to call essential whether we like it or not, in spite of the difficulty of formulating them mathematically.

A person carries out repeated habitual movements (the best examples of these are the movements performed during writing, since they leave behind them obvious documentation). He may write out dozens of pages, write his signature a hundred times, write with a pen on paper or with chalk in large letters on a blackboard; he may write (as we have shown in experiments) using various limbs, or his mouth; and in all this we shall not be able to find two outlines that are exactly the same. Yet, one individual handwriting is retained in all these different examples. The trajectories of separate cycles in chronocyclograms made for all kinds of cyclic habitual movements corroborate this fact. Our perceptions, without the support of any exact formulation, have created concepts similar to handwriting, such as gait, touch (on the piano), pitch of voice, pronunciation or accent, etc. The same demarcation is applicable in all these cases, viz. fundamental affinity, that is, congruence in some of the characteristics, and absence of congruence and also rankability in variation ranges according to other (usually metrical) characteristics.

We may also cite here (we will list without giving any details) the recognition of outlines, especially of letters, of all sizes and type faces, etc. (it is curious that this can be done with equal ease for white letters on a black background, and with black on a white one); recognition of a person by sight with six degrees of freedom for variation of his image on the retina (and even with rejection of any mimetic variations); the ability of each one of us, right from infancy, to recognize a "dog" or "cat" in a definite instance of these animals, and so on. Some of the last examples relate to what psychologists have called for a long time correlation; but this term still does not explain either the mechanism of this process (which we have also attributed to cerebral model-making), or, more important, the principles by which the brain is directed while the attributes of the object are being separated into contrasting groups. Even less clear as yet is the obvious analogy of psychological processes of correlation during perception with habitual movements and the results of morphogenesis, that is, the very clearly different relationship between an organism, in all the cases we have compared, and attributes or characteristics of category.

It would seem that there are far-reaching perspectives ahead for the application and development of I.M. Gelfand and M.L. Tsetlin's mathemetical idea (Some methods of regulating complex systems, *Advances in Mathematical Sciences* 17, 1 (103) 1962, p. 3), which consists in applying to the problems which here concern us, a class of functions of a large number of variables, designated by these two authors as "well-organized functions". A function is well organized if (1) its arguments can be separated into essential and non-essential variables and if (2) all the arguments retain firmly their attachment to one or other sub-class. The non-essential variables may cause abrupt changes and discontinuities in the function, sharp graduations in value, etc.; at the same time (and this explains their name) they do not exert any determining influence on the course of the function as a whole or over large ranges or on the location of its extremes, etc. The marked variance from the non-essential variables may interfere with, and conceal the influence of, the essential ones over narrow ranges, but in the end the form and course of the function are determined mainly by the latter. Apparently an argument's appurtenance to one or the other subclass is not so much determined by its basic, underlying, concrete, physico-chemical (or other) process, as by the actual form of functional link which the argument has with the function.

It appears both tempting and reasonable to make use of this class of functions, by representing each aspect of the development and active life of a living organism by such a function of many variables, where each subclass of variable is directly superimposed on the behaviour of the corresponding essential or non-essential characteristics, as subdivided above. Then, with reference to the morphogenesis of say a leaf, a flower, etc., we may say that the traits which determine the species, and which are clearly coded in the chromosomes, are seen as the result of essential (in the Gelfand-Tsetlin sense) variables, whereas the material characteristics, each producing variational ranges, are seen as the result of non-essential variables. The same would apply to the co-ordination of movements, for example, to cyclic habitual acts such as writing. We have already discussed above a classification of the characteristics of such acts. The fact that a completely similar organization of determining characteristics occurs during acts of perception, first of all in form perception and then also in kinds of acts of correlation, indicated that the nature of these remarkable functions is inherent even in active brain model-making, during processes of perception and reflection of the environment.

The first attempts at using these functions to depict mechanisms of active life are already presenting important and promising new information about their characteristics in addition to what is already known. It has already been observed how differently an organism behaves under the influence of its surroundings with reference to essential and non-essential variables. As regards the latter type, it is reactive and, so to speak, yieldingly adaptable: if one leaf on a tree receives more food than another, then that leaf grows more vigorously than the other one; and if it is placed in better illumination it accumulates a higher concentration of chlorophyll, and so on. But essential characteristics of structure and shape such as those which determine the plan of the flower, or negative geotropism (i.e. the tendency to preserve the vertical direction of the trunk or stem under all conditions), are only relinquished by an organism if it is subjected to very violent interference, and that usually after a longer period than anticipated (regeneration serves as an example). Thus the function, that is the organism, may be said to be reactive as far as its non-essential variables are concerned, but highly non-reactive, or active, with regard to its essential ones.

Structural analysis of motor acts and of their co-ordination reveals exactly the same picture. As was shown by our research, the co-ordinational regulation of each complete cognitive motor act is constructed on a kind of multiple hierarchical system of circuits of regulation and adjustment. The need for this multiplicity of stages is due to the very great number of degrees of freedom in our multiconnected organs of movement, and by the enormous number of muscular units which actively co-operate to guarantee a posture or to perform a required body movement. In addition to this, there are also the attendant facts of muscular elasticity, complex reactive dynamics of the organs of movement, and, of course, the entire combination of those independent and, for that reason, unforeseeable external forces of resistance; the expedient conquest of these is the very essence of most of our arbitrary motor acts. In the co-ordinational regulation of movement, numerous types and grades of circuital corrections are distributed between the level systems of the brain, on the one hand, according to the composition and quality of the sensory syntheses inherent in them, and clearly, on the other hand, according to the relative importance and significance in the cognitive relation of any other corrections aimed at fully realizing a program of movement (see the author's book *The Construction of Motions* and also his paper in the symposium *Problems of Cybernetics*, No. 6).

In the context of the present discussion, some importance attaches to the fact that the apparatus for regulating movements displays two different types of co-ordinational tactic: when secondary technical disagreements or hindrances are encountered it operates reactively and accommodatingly without fearing any variational tendencies; but when aspects of regulation that are essential to the program are involved, this apparatus struggles for the required results actively, whatever might happen, overcoming obstacles and, if necessary, reprogramming during operation, etc.

There is now another problem which, although it is at present very topical and is closely related to the subject of the Gelfand-Tsetlin well-organized functions, can be mentioned here only in passing. It concerns the interrelations between the concept of biological systems and the concept or class of discrete number. The traits, arguments, correctional functions, etc., belonging to the non-essential type are obviously continuous, and form variational series correspondingly. What is the situation, then, regarding essential variables? In particular, is the question "to what extent does the apparatus we are dealing with 'know' how to count?" admissible in relation to hereditarily transmitted traits coded in the chromosomes?

This question is raised today in a wide variety of works. Judging, for instance, from the data provided by anatomy and comparative anatomy, this assured counting extends approximately to two to the sixth power (number of teeth, vertebrae, cyto-architectonic fields of the brain, elements of the collateral line of a fish, etc.). After this there begins the idea of "many", and of course it is impossible that in a gene apparatus there should be a code, for example, of the number of hairs on the head, or the number of cells in the cortex of the brain.

Undoubtedly, the border regions of the mumerical series offer, in the main, the greatest interest and significance. The cyto- and myelo-architectonic fields of the human brain cortex are calculated and standard; but how far does this calculation extend, and at what moment does randomization start in the number of cells and in the plan of their synaptic interrelationships? Are the numbers of glomerules in the kidney, of islets of Langerhans, Pacinian corpuscles, muscular units in a muscle, calculated or chance? How does the apparatus of hereditary transmission behave when it comes to numbers of the order of hundreds, that is to say, where is the limit of its informational capacity?

The following is of some importance in the light of the topic we are discussing. The informational capacity of the gene apparatus is clearly not imposed on it from outside, but expresses the evolutionally determined need of the animal, plant, cell, etc., in question. Therefore, an analysis of the mentioned boundary relationships and regions of transition from the necessary to the haphazard is at the same time an analysis of the distribution between essential and non-essential arguments, which corresponds to the evolutionally determined requirement of the organism. At the same time this is an analysis of where and how the organism demarcates the boundary between active and reactive processes, between number and multiplicity (calculated or continuous), between areas for applying the theory of well organized functions and those for applying the theory of casual processes.

In conclusion, another must be made to consider a fundamentally significant problem.

Right from the beginnings of scientific cybernetics, when there first appeared a parallel between the urgent basic problems of physiology and the problems which established cybernetics as a separate science in its own right, a rewarding exchange started between the two sciences with regard to practical data, theoretical formulations and correlations. The entire period from the publication of Wiener's work right up to the present day is full of the search for and use of analogies between living and artificial systems - analogies which, on the one hand, have aided physiologists in working out system interrelationships in the organism, and have, on the other hand, provided technologists with valuable new ideas on the construction of automatic systems.

It is difficult to say whether or not the "honeymoon" between these two sciences is over, and with it their common quest for and use of analogies and other similarities; but problems that suggest an opposite line of development have been increasingly coming to the fore in recent scientific literature: is there, after all, a fundamental difference in principle between living and non-living systems, and if there is, where does the "watershed" forming the boundary between them lie?

We are naturally not concerned here with differences in the materials out of which systems are made, or even with quantitive differences which would make it impossible for modern technology to imitate the 15 billion cells of the brain. Quite clearly, also, the difference we are concerned with has to be formulated in accordance with the principle of the unity of nature, whose laws control both living and non-living matter alike.

However, before the above concept of biological activity may be advanced as a working hypothesis, a reply must be made, if only in very general terms, to the question of whether one can talk about any deep-seated specificity of life processes without at the same time surrendering strict mathematical rigour, and without slipping into a form, albeit disguised, of vitalism.

As early as the 18th century, when militant mechanistic materialism firmly defined its scientific positions, natural science was faced with a choice which appeared unavoidable at that time (and also for a long time afterwards). On the one hand, the contrast between the phenomena of life activity and the then-known processes of inanimate nature was so striking that some explanation was demanded. On the other hand, the store of knowledge concerning deepseated physico-chemical processes, biophysical and biochemical principles on the molecular level, was still very small. The result was that many scientists, finding that their physico-chemical knowledge did not offer them an adequate means of explaining the specificity of life, put forward the notion of a non-material life force by way of explanation; this was completely satisfactory for the ones who readily entertained notions on all sorts of non-material factors and entities, and who consequently joined the idealist camp. The materialists, however, could generally do nothing but repudiate all research into vital specificity which could not be supported by the physics or chemistry of the day. This notion has been preserved even up to the present day. Meanwhile, during the more recent past, a great deal of fresh information has been collected and this has permitted a close enquiry into the nature of many processes, primarily those that take place on the cell and molecular levels, in a manner that was inconceivable in the last century. Indeed, these new discoveries have made it possible now to raise the problem of reviewing the traditional opinion outlined above. It is neither within the scope of the present paper nor the author's competence to undertake any detailed elucidation of the problem, but at least some indication as to its nature should be given here.

First of all, in the past only the most rudimentary information was available concerning the process of catalysis. Nowadays, however, the increase of knowledge concerning these processes reveals an ever wider area of operation and a hitherto unknown range of functions. For instance, there is the role played by enzymes in the regulated synthesis of high-molecular compounds; the reduplication of these compounds; the enormous variety and peculiarity of chemo-autotrophic micro-organisms; with their assistance, processes which would require enormous temperatures and pressures, etc., under laboratory conditions take place intensively. Nothing was known in the last century about stochastic processes (unless one counts the kinetic theory of gases and solutions); nor was anything known about anti-entropic processes in open systems, or under what conditions they took place and how they could be regulated, whereas every year now sees an increasingly rich fund of knowledge being amassed on this subject. Biological codes and their role in structurization and self-organization have already been mentioned above.

We will not prolong this enumeration; its aim has been merely to show that the extensive increases in factual knowledge up to the present will no doubt provide an impetus for scientific research to apply the newly discovered principles of biochemistry, biophysics and the new branches of mathematics to an unconditionally materialistic account of the phenomena of life, and this without prejudice to the dialectical principle of the transition from quantity to quality and without any risk of falling into idealism.

It is now necessary, whilst holding firmly and strictly to the principle of the unity of nature and its laws, to indicate and study the differences between living and articifial systems. It will be some time, perhaps, before the nature of this difference is formulated, but it seems reasonable to suppose that the features and character of physiological activity that we have been discussing here might at some later date form an essential part of this unknown characteristic. In any case, what has been said in this paper may facilitate the task of future exploration of the subject and indicate in some way how the problem of surmounting the division between the biological and technical sciences may be approached.

Human Motor Actions – Bernstein Reassessed
H.T.A. Whiting (editor)

CHAPTER VIa

FROM SYNERGIES AND EMBRYOS TO MOTOR SCHEMAS

M.A. Arbib

In this paper, we try to place Bernstein's "Trends in Physiology and their Relation to Cybernetics" in the perspective of two decades of research since its publication in Questions of Philosophy in 1962. It was reprinted in English translation as the last chapter of The Co-Ordination and Regulation of Movements in 1967, and stands in contrast to the other five chapters in that its emphasis is not primarily on the physiology of movement. Nor, surprisingly, does it have much to say about the discipline of cybernetics. Rather, in words taken from the last two paragraphs, its aim is to provide an impetus for "an unconditionally materialistic account of the phenomena of life, and this without prejudice to the dialectical principle of the transition from quantity to quality and without any risk of falling into idealism". Nonetheless, he asserts that it is "now necessary, whilst holding firmly and strictly to the principle of the unity of nature and its laws, to indicate and study the differences between living and artificial systems". The core of his paper is to draw parallels between embryology and the physiology of activity in arguing that self-directed activity is the distinguishing mark of living things. While my ignorance of dialectical principles precludes my offering an analysis of the extent to which this view is grounded in Marxist-Leninist philosophy, I can offer a brief survey of research in theoretical biology which addresses issues in embryology and in the physiology of activity, and then state my own opinions on the status of Bernstein's philosophical position.

A Brief Historical Perspective

Western cybernetics traces its roots to Maxwell's (1868) mathematical study of feedback, "On Governors", and Bernard's (1878) study of feedback systems in physiology, the mechanisms underlying what Cannon (1939) would call homeostasis. By contrast, as Bernstein implies, the Russians developed a separate tradition of the study of the physiology of regulation, which may be traced back to Sechenov's (1863) study of "Reflexes of the Brain". Western readers wishing a perspective on that tradition would do well to read the chapters on "Russian Contributions to an Understanding of the Central Nervous System and Behavior - A Pictorial Survey", "The Nineteenth Century Background of the Russian Neurophysiologists, and Sechenov" and on "Pavlov" in the volume edited by Brazier (1959). Clearly, Pavlov was the shaping figure of 20th century Soviet physiology, but too many scientists have taken his work on the conditioned reflex to downplay what Bernstein calls "Sechenov's thesis", namely "the active form of all the processes of reception and of central processing of information". To see that the study of reflexes is not incompatible with Sechenov's thesis we may turn to Sokolov's (1975) study of neuronal mechanisms of the orienting reflex, in which habituation is posited to depend on the organism's active formation of a model against which new stimuli may be matched for an estimate of their novelty. The Western reader may round out his appreciation of this distinctly Soviet view of physiology and neurology by reading not only Bernstein's work, but that of Vygotsky (1934) on thought and language, and of Anokhin (1935) on problems of center and periphery in the physiology of nervous activity. In addressing the neuropsychological problem of predicting lesion sites in the brain from symptom-complexes and vice versa, Luria (1973) explicitly bases his idea of "functional system" on the work of Anokhin, Bernstein and Vygotsky, asserting that the fundamental task of neuropsychology is to ascertain "which groups of concertedly working zones are responsible for the performance of complex mental activity (and) what contribution is made by each of these zones to the complex functional system". (I have developed this view within the perspective of Artificial Intelligence and Brain Theory in Arbib (1982)).

To trace the process whereby the term "cybernetics" became accepted in the Soviet Union, it may be of interest to quote the following from a survey of Soviet cybernetics:

> Before about 1955, cybernetics was anathema to the Soviets, even though many Russian scientists made contributions to what in the West was called cybernetics.
> ... Articles appeared which labelled cybernetics a 'bourgeois pseudoscience', 'the philosophy of the captains of a rotten ship'; one article was paranoid enough to suggest that cybernetics was a deliberate mystification devised by the capitalists to mislead Soviet scientists!
> ...
> In 1956, the Twentieth Congress of the CPSU not only heard

> Krushchev denounce Stalin, but also heard that 'automation of machines and operations must be extended to the automation of factory departments and technological processes and to the construction of fully automatic plants'. The Soviet Government formed the Ministry of Automation. ...
>
> Starting in 1959, the Academy of Sciences held a series of discussions on philosophical problems of cybernetics, which were published in 1961 under the same title. The general line of these papers was:
>
> 1. Cybernetics is the science of control, and will help build socialism.
> 2. Cybernetics is a science with a well-defined problem area, and must not be considered a philosophy. It cannot compete with material dialectics.
> 3. Bourgeois cyberneticians gloss over the vital distinctions between man, machine, and society. To understand the brain of man is the task of Pavlovian research on higher nervous activity. To understand society we need Marxism-Leninism.
> 4. Thought is an attribute of man alone, as a result of his social history. It is thus unquestionable dogma that machines cannot think. In fact, the question is considered highly improper, and attempts to define thought in a form suitable for discussion are not encouraged.
>
> Kolmogorov, however, stated in an interview that he believes thinking machines, which are operationally indistinguishable from humans, will be created by cybernetics. ...
>
> In 1961, the Twenty-Second Congress of the CPSU emphasized that cybernetics, computers, and control systems must be introduced on a large scale in industry, research, designing, planning, accounting, statistics, and management. (Arbib, 1966, pp. 196-198).

In 1962 came the publication of Bernstein's paper under review here. With this background, we may turn to an explicit introduction to Bernstein's central concept of synergy in his physiology of activity. We then study schema theory, theoretical embryology and Piaget's views on their relationship, before returning to the broad philosophical questions of Bernstein's paper.

From Synergies to Motor Schemas

Bernstein asserts that:

> classical physiologists stuck to the idea of the reflex arc to represent the effector processes of the organism as strictly determinate signals arriving through an afferent semi-arc. But cyclical processes may start anywhere, and so reactive and spontaneous processes are combined into a single general class. The organism not only reacts to the situation but, encountering a

> situation which is dynamically variable, must make a probabilistic forecast and then a choice. The evaluation of situations by the organism is no less sensitive than its choice of action.

In fact, Bernstein's work says rather little about the "evaluation of situations", and we shall sketch the theory of schema-assemblages in the next section to remedy this deficiency. For the present, however, we shall contrast two approaches to the 'units of control' employed by the brain in controlling movement. Both are called synergies. The first, due to Sherrington, posits a reflex unit above that of the motor unit; while the second, due to Bernstein, suggests that a restricted number of programs may underlie most of our behavior.

To understand Sherrington's views we must start with the notion of a reflex. Two familiar examples: In the knee-jerk reflex, the tap of the physician's hammer stretches a tendon, this is sensed by a sensor (proprioceptor) which activates a motor neuron which contracts the extensor muscle (which had 'appeared' to be too long) so that the foot kicks out. In the scratch reflex, an irritant localized to part of the skin activates receptors which in turn activate motor neurons which control muscles to bring a foot or hand to the irritated skin and rub back and forth. In each case, we have a reflex-loop which mediates direct stimulus-response behavior: from external world via receptors to the spinal cord where motor neurons respond by controlling muscles to yield movement in the external world. Now consider the scratch-reflex more carefully. We may see it as made up of two components. The rubbing component needs the limb movement to ensure that it contacts the (right place on the) skin, the limb movement is tuned into contact by feedback from the rubbing movement. The two reflex actions 'synergize' or work together. More generally:

> The executant musculature ... provides a reflex means of supporting or reinforcing the co-operation of flexors with flexors, extensors with extensors, etc. The proprioceptors of reaching muscles operate reflexly upon other muscles of near functional relation to themselves. Active contraction (including active stretch) and passive stretch in the reach muscles are stimuli for reflexes influencing other muscles, and the reflex influence so exerted is on some muscles excitatory and on others inhibitory; it is largely reciprocally distributed, knitting synergists together. (Creed et al., 1932, p. 129).

Thus, for Sherrington, the synergy is an anatomically based reflex linkage of a group of muscles.

The Bernstein school is informed by notions of control theory. The brain is to generate control signals which will cause all the muscles to contract with just the right timing to bring about some desired behavior. But there are so many muscles, they suggest, that to independently control every muscle to its optimum would be a computationally unmanageable problem. They thus see the crucial problem in the 'design' of a brain which controls movement to be that of reducing the number of 'degrees of freedom', i.e. the number of independent parameters which must be controlled*:

> In order for the higher levels of the central nervous system to effectively solve the task of organizing motor acts within a required time, it is necessary that the number of controlled parameters be not too large, and the afferentation, requiring analysis, not too great. (This is achieved) by the so-called synergies. ... Each synergy is associated with certain peculiar connections imposed on some muscle groups, as subdivision of all the participant muscles into a small number of related groups. Due to this fact, to perform motion it is sufficient to control a small number of independent parameters, even though the number of muscles participating in the movement may be large.

So far, the general framework is consonant with the Sherrington synergies. But these are restricted to stimulus-response patterns. Bernstein had a more general concern with dynamic patterns changing over time during some motor act:

> (A) complex synergy is involved in walking. ...'The biodynamic tissue' of live movements (appears) to be full of an enormous number of regular and stable details. ... (In old people) the synergy existing in normal walking between the action of the arms and legs is destroyed.

However, this was too global a view of synergy, and later work of the Moscow school came to view synergies as the functional building blocks from which most motions can be composed:

> Although synergies are few in number, they make it possible to encompass almost all the diversity of arbitrary motions. One can separate relatively simple synergies of pose control (synergy of stabilization), cyclic locomotive synergies (walking, running, swimming, etc.), synergies of throwing, striking, jumping, and a certain (small) number of others. (Gel'fand et al., 1973, p. 162).

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[*] The next two quotations are from Bernstein's paper "Biodynamics of Locomotion" in the present volume. He does not appear to use the term 'synergy' in other papers in this collection.

One thus comes to see a synergy in general as a program for controlling some distinctive motor performance extended in space and time, built upon synergies of coordinated reflexes as substrate.

The concept of synergy has much in common with that of schema as within the motor skills literature, e.g. in the work of Schmidt (1975, 1976). Schmidt' schemas seem suited to the performance of a single motion in the laboratory or in sports (e.g. swinging a bat) rather than to a complex manipulation or to goal-oriented performance in a dynamic environment. Each such schema is broken into two parts: The recall schema seems akin to feedforward, being responsible for the complete control of a rapid movement, even though environmental feedback may later signal errors. The recognition schema is responsible for the evaluation of response-produced feedback that makes possible the generation of error information about the movement. It thus seems to combine on-line feedback and identification procedures which may operate even after a movement is completed to better tune the schema for its next activation. We shall use the term motor schema for such a unit for the control of skilled movement, and use it as a synonym for synergy in the sense of Bernstein. We may compare this with Piaget's (1971, p. 7) notion of an action schema as "whatever, in an action, can ... be transposed, generalized, or differentiated from one situation to another, in other words, whatever there is in common between various repetitions or superpositions of the same action".

When we move to catch a ball, we must interpret our view of the ball's movement to estimate its future trajectory. Our attempt to catch the ball incorporates this anticipation of the ball's movement in determining our own movement. As the ball gets closer, or exhibits spin, we may find the ball departing from the expected trajectory, and we must adjust our movements accordingly. In this example, we think of the visual system as providing inputs to a controller (our brain) which must generate control signals to cause some system (our musculature) to behave in some desired way (to catch the ball). Feedforward anticipates the relation between system and environmen to determine a course of action; feedback monitors discrepancies which can be used to refine the actions. In describing the control of muscle Fel'dman (1966) notes that, for a mass-spring system, the control neurons must maintai a specific level of firing to hold a limb in a desired position - there is a functional relation between a desired output (e.g. muscle length) and a necessary input and a necessary input (e.g. maintained tension). In this case the feed-forward would be co-activated with the feedback system, so that feedforward sets and maintains the control level specified by the functional relationship, while feedback compensates for minor departures therefrom.

It is important to note that feedback can only be used effectively if the controller is 'in the right ballpark' in its model of the controlled system. However, in the real world the exact values of the parameters describing a system are seldom available to the controller, and may actually change (compare short-term loading effects on muscles and longer-term aging effects and weight changes). To adapt to such changes, the feedback loop must be argumented by an identification algorithm. The job of this algorithm is to monitor the output of the controlled system continually and to compare it with the output that would be expected on the basis of the current estimated state, the current estimated parameters, and the current control signals. On the basis of these data, the identification algorithm can identify more and more accurate estimates of the parameters that define the controlled system, and these updated parameters can then be supplied to the controller as the basis for its state estimation and control computations. The identification algorithm can only do its job if the controller is of the right general class and system parameters do not change too quickly. It is unlikely that a controller adapted for guiding the arm during ball-catching will be able, simply as a result of parameter-adjustment, to properly control the legs in the performance of a waltz. Thus an adaptive control system (controller + identification procedure) is not to be thought of as a model of the brain; rather each such control system is a model of a brain 'unit' which can be activated when appropriate. We may think of it as a motor schema or synergy. An important problem in analyzing human movement is that of the coordinated phasing in and out of the brain's manifold motor schemas.

Perception and the Schema-Assemblage

Having now viewed Bernstein's synergies as motor schemas, we now sketch a theory of assemblages of perceptual schemas, and their integration with planning in the action-perception cycle (Arbib, 1981), in addressing the problem of "evaluations of situations" that, we have suggested, has been relatively neglected within Bernstein's own work. To relate this theory to one's everyday activity, consider that, in walking down the street with a friend, one simultaneously engages in at least five movement processes: walking (including maintaining posture); breathing; talking; gesticulating;

and scanning the shop windows and passers-by. But each of these processes involves the co-operation of multiple processes: for example, stepping is determined inter alia by high-level route-selection processes ("turn left at the town hall"), visual feedback about the location of obstacles, and tactile feedback from the soles of the feet. And each of these in turn requires activity in a neural network linking an array of receptors with an array of motor neurons.

These behaviors involve not only 'externally-directed' movement, but also a variety of 'exploratory' movements that help update an 'internal model of the world' (here we may see such Western writers as Craik (1943), Minsky (1961), and Gregory (1969) making their contribution to Sechenov's thesis). In a new situation, we can recognize that familiar things are in new relationships, and use our knowledge of those individual things and our perception of those relationships to guide our behavior on that occasion. It thus seems reasonable to posit that the 'internal model of the world' must be built of units which correspond, roughly, to "domains of interaction" - a phrase carefully chosen to include objects in the usual sense, but to include many other things besides, from some attention-riveting detail of an object all the way up to some sophisticated domain of social or linguistic interaction for purposeful beings. We shall use the word schema to correspond to the unit of knowledge - the internal representation of a domain of interaction - within the brain.

The intelligent organism does not so much respond to stimuli as it selects information which will help it achieve current goals - though a well-designed or evolved system will certainly need to take appropriate account of unexpected changes in its environment. To a first approximation, then, planning is the process whereby the system combines an array of relevant knowledge to determine a course of action suited to current goals. In its fullest subtlety, planning can involve the refinement of knowledge structures and goal structures, as well as action per se. While an animal may perceive many aspects of its environment, only a few of these can at any time become the primary locus of interaction. In general, our thesis is that perception of an object (at least at the pre-verbal level) involves gaining access to routines for interaction with it, but does not necessarily involve execution of even one of these sub-routines. Our image for the control of the ensuing behavior is context-dependent interpretation (Arbib, 1972a) in that new inputs (e.g. coming upon an unexpected obstacle) can alter the elaboration of the high-level structures into lower-level tests and actions which in turn call upon the interaction of motor and sensory systems. We study programs which are part of the internal state of the system prior to action, and which can flexibly guide that action in terms of internal goals or drives and external circumstances. As Bernstein asserts, "problem of action ... is the reflection of the model of future requirements (somehow coded in the brain); and a vitally useful or significant action cannot be either programmed or accomplished if the brain has not created a prerequisite directive ..."

To better appreciate the intimate relation between perception and action, consider the perceptual cycle (Neisser, 1976). The subject actively explores the visual world, for example, by moving eyes, head or body (or manipulating the environment). Exploration is directed by anticipatory schemas, which Neisser defines as plans for perceptual action as well as readiness for particular kinds of optical structure. The information thus picked up modifies the perceiver's anticipations of certain kinds of information which - thus modified - direct further exploration and become ready for more information. For example, to tell whether or not any coffee is left in a cup we may reach out and tilt the cup to make the interior visible, and keep tilting the cup further and further as we fail to see any coffee until we either see the coffee at last or conclude that the cup is empty. We here see Sechenov's thesis in the form that one cannot understand perception unless it is embedded within the organism's on-going interaction with its environment. For as the organism moves in a complex environment - making, executing, and updating plans as it does so - it must stay tuned to its spatial relationship with its immediate environment, anticipating facets of the environment before they come into view. The information gathered during ego-motion must be systematically related to a cognitive map (Tolman, 1948; Lieblich and Arbib, 1982), which is not so much a mental picture of the environment as an active, information-seeking structure.

The "co-ordinated structures" of Gibson (1977) and the action-perception cycle remind the neuroscientist to eschew too Sherringtonian a view of a brain responding to an environment via a chain of reflexes. The "co-ordinated control programs" of Arbib (1981) remind us that this mutuality of organism-environment interaction must be embedded within a regnant plan of action which the organism brings to bear upon the pattern of interaction. To this context, we add that that portion of the environment which provides the current focus for the animal's interaction is part of a larger whole. Because of this, we may view the animal's 'internal model of the world' as being, functionally, a schema-assemblage - the assemblage being made up of a spatially-tagged array of parametrized schema instantiations. Let us unpack this mouthful. The spatial relationship between objects can be crucial, and so we speak of a spatially-tagged array. If we speak of a chair-schema, for example, as the internal representation of a chair and our possibilities for interaction with it, then the internal representation of a scene with three chairs may well involve three instantiations of the chair-schema, and each of these must be individually parametrized by pertinent characteristics (affordances) specific to the chair that it represents.

This notion of a schema as but a unit within an assemblage is the author's. Another root of the use of 'schema' in current psychology is found in the work of Piaget. The Piagetian schema is the internal representation of some generalized class of situations, enabling the organism to act in a coordinated fashion over a whole range of analogous situations. Reviewing this approach to the genesis and development of knowledge, Piaget (1971) relates his schemas to the innate releasing mechanisms of the ethologists and thus, via Lorenz (1941), to the schemas of Kant (1787).

Parallels with Embryology

About a third of the way through his article, Bernstein shifts emphasis from the physiology of activity to embryology, likening the regulatory properties of the embryo to the self-directed activity of the mature organism. In his extended analysis of the organism's growth, Bernstein suggests that the "life process is not a balancing with the environment, but a conquest of the latter, aimed not at preserving the status quo or homeostasis, but at an advance towards fulfilment of the development and self-preservational program of the genus". One is reminded of Waddington's (1957, 1968) introduction of the concept of "homeorhesis" in contrast to "homeostasis". Whereas, in homeostasis, organismic variables are regulated to fall within a certain physiologically acceptable range, in homeorhesis there is a stabilized flow rather than a stabilized state. Waddington views development as following a 'chreod', a canalized trajectory which acts as an attractor for nearby trajectories. However, he does allow that chreods may branch. Away from a branch, development is highly regulated; near a branch, external factors can control bifurcation, the switching to one branch or another. These notions received one mathematical form at the hands of Rene Thom (1975) in his theory of structural stability and morphogenesis. Thom, in what is known as catastrophe theory, seeks to give an abstract mathematical theory of bifurcations by looking at systems which belong to a parametrically-defined family of systems. A system of the family is structurally stable when small changes in parameter values do not change its qualitative behavior (i.e., the numerical description of its trajectories may change, but the overall pattern of its stabilities and instabilities remains unchanged). Only for an exceptional submanifold of the parameter space do the corresponding systems exhibit structural instability - this submanifold constitutes what Thom calls a catastrophe. Slight changes in a system characterized by these parameters can yield 'catastrophic' changes such as those exhibited at certain critical stages in morphogenesis. Thom gives an abstract topological characterization of a set of "elementary" catastrophes, and a number of theorists share his view that these topological invariants capture what is essential in morphogenetic processes. We see here a strikingly different approach to the issue of essential vs. non-essential variables from that, due to Gel'fand and Tsetlin (1962), discussed by Bernstein. While many workers do share Thom's views, others hold that applied mathematics offers less topological methods of handling nonlinearities and bifurcations which offer more detailed insights into biological processes.

Zeeman (1974) has used catastrophe theory to prove that when differentiation of a mass of cells occurs, forming two types of cells, then the boundary that forms between these cell types always moves from its initial position before stabilizing in its final position in the tissue. Ransom (1981, p. 62-65) offers an informal discussion of this theorem, and discusses the attempt by Cooke and Zeeman (1976) to apply it to explain the formation of the repeated pattern of vertebrate somites. It is beyond the scope of this article to survey the literature on cybernetic approaches to embryology - the reader may turn to Arbib (1972b) and Ransom (1981) for comprehensive reviews of automaton-theoretic and computational approaches to developmental biology; to the papers by Ede, Wolpert, Wilby, Bryant and Cooke in the volume edited by Ede et al. (1977) for a view of how modelling concepts have affected work on vertebrate limb and somite morphogenesis; and to the papers by Hope, Hammond and Gaze (1976), Willshaw and von der Malsburg (1979), and Overton and Arbib (1982) for a range of models of regulatory processes in the development of connections in the brain. Instead, we use the rest of the section to address certain questions raised by Bernstein by noting Piaget's concern for parallels between cognitive activity and embryology.

Piaget's studies of the cognitive development of the child were part of a broader philosophical concern to construct a 'genetic epistemology', a concern rooted in the embryological studies of his boyhood. In his Biology and Knowledge* (translated from the French of 1967), Piaget (1971) offers an essay on the parallels between organic regulations and cognitive processes. The most widely cited author in this volume is Waddington (Lamarck is second!) and, although Piaget does not cite Bernstein, his analysis of schemas and epigenesis make his text central to any discussion of the issues Bernstein raised in the paper under discussion here. The parallelism of active roles in embryology and in cognition is well-expressed in the following quotation:

> In genetic terms, the genome is in no sense the product of environmental influences but is an organized system, supplying 'responses' to environmental tensions (Dobzhansky and Waddington) and containing its own norms of reaction. In terms of embryology, however, epigenetic development implies a series of exchanges with some internal control imposing choices on the materials used... In neurological terms the nervous system is not subjected to some constraining influence from the stimuli but shows spontaneous action, reacting only to stimuli which sensitize it - that is, it assimilated them actively into schemata established before it made its response (Piaget, 1971, p. 31-32).

Note, however, that Piaget not only analyzes how stimuli are assimilated to pre-existent schemas. He also provides a theory of the process of accommodation whereby new schemas are formed from the old. Thus, although Piaget does seem to share Bernstein's notion that the organism has a "coded program of the successive stages in (its) development" (see Brainerd (1978) for a critique of the stage concept in cognitive development), he would not go so far as to assert that "the organism is continually changing in a *set* direction (my italics)". In addition to assimilation, Piaget posits a process of accommodation whereby the animal modifies its schemas to accord with an external world situation. Piaget thus sees adaptation as an equilibrium between the two functions of assimilation and accommodation, which he views as indissociable. He sees these processes of adaptation occurring first in embryology, and again playing an essential part in the cognitive domain. But he extends to intellectual growth a power which goes beyond the embryological:

> From the point of view of organization, intelligence succeeds in making structures that are both more stable and more highly differentiated, for although it would

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* I thank my students Donald House and Francisco Cervantes for their thoughtful schema-theoretic reviews of this volume.

> be possible to conceive of the mathematization of all biological structures, all mathematical structures could not be realized in the organic plane (Piaget, 1971, p. 213).

Philosophical Conclusions

Bernstein has stressed the material basis for embryonic development and for psychological processes, contra claims for vitalism or the postulation of mind as a separate substance, respectively. It seems that no reputable embryologist believes in vitalism any more - which is not to deny that the exact mechanisms whereby genes interact with cytoplasm in guiding development still hold many mysteries for scientists to resolve. But scientists do not doubt that the resolution will involve the explication of mechanisms, rather than resort to some elan vital. By contrast, some eminent neuroscientists still espouse a mind-body dualism, even though the majority are in some sense materialists. To conclude this essay, then, I briefly recall the dualist position of Sir John Eccles, the Australian Nobel Laureate in neurophysiology, and indicate why I do not find his case convincing.

Eccles follows Popper in distinguishing three "Worlds": World 1 comprises physical objects and states (including brain and body); World 2 comprises states of consciousness (mind and soul); while World 3, comprising the knowledge accumulated by society (social reality) elevates Cartesian dualism to a Popperian trialism. Eccles seems to misread Popper by locating records of intellectual efforts in World 3 - these records are clearly in World 1, and Popper is speaking more strongly, regarding theoretical systems and social systems as having a reality of their own apart from any embodiment. Thus Eccles and Popper see the physical, the mental, and the social as three separate substances.

Eccles addresses the question of their interaction. Where Sherrington, in Man on his Nature, had been a dualist - mind talks to brain, and vice versa - Eccles has to have World 2 interact with World 3 as well as World 1. Interestingly, he eschews the sort of direct communication between World 3 and World 2 that one might associate with such terms as "collective unconscious" and "racial memory", and has World 3 embody itself in books and artefacts which can then be sensed as is any other part of World 1 by the perceptual systems of the brain, themselves part of World 1. He then posits a specialized portion of the brain, the liaison brain which communicates with World 2, the mind.

Note that each mind is cut off from every other mind save to the extent that they communicate through their liaison brains, and thus through World 1.

Eccles then calls on the data on split brains. It is well known that 98% of people have the left hemisphere as the dominant hemisphere - it has the ability to use complex speech, whereas the right hemisphere has at best limited use of concrete words. When the corpus callosum, the massive fiber tract linking the two hemispheres, is severed, each half-brain can independently guide motor behavior, but only the left hemisphere can talk about what it is doing. This leads to two hypotheses: either the left brain alone has consciousness and personality, with the right brain providing additional computing power when the corpus callosum is intact (so that the functions of the right brain can then enter consciousness and affect personality via its modulation of the left brain); or both brains embody conscious personalities (which usually reach consensus via the corpus callosum when it is intact), but only the left brain can articulate this consciousness. Eccles opts, without further argument, for the first view, and then goes much further. He rejects the possibility that consciousness and personality could be functions of the left brain qua physical structure; rather he asserts that we have consciousness and personality as a separate entity in World 2, the mind, but that is is only the specialized structures of the left hemisphere that enable the mind to communicate with the brain. Unfortunately, this explains nothing. If the mind is separate from the brain, what tasks does it perform that the brain cannot? Eccles would answer that it makes those free decisions that constitute free will. I would argue that, in fact, our brains are sufficiently complex to make those "free" decisions. But even if Eccles is right, how does the World 2 mind make such decisions? Brain Theory offers potential mechanisms, Mind Theory does not - unless we postulate schemas in World 2 in communion with their neural embodiments, but somehow able to work independently of them. Eccles states that:

> I believe that my genetic coding is not responsible for my uniqueness as an experiencing being. ... Nor do my postnatal experiences and education provide a satisfactory explanation of the self that I experience. It is a necessary but not sufficient condition. ... We go through life living with this mysterious experience of ourselves as experiencing beings. I believe that we have to accept what I call a personalist philosophy - that central to our experienced existence is our personal uniqueness. (Eccles, 1977, p. 227).

Thus where we would argue that to the extent that we are more than our accumulated genetic and individual experience it is because of the people and physical world that surround us, Eccles explicity postulates a "something extra" - our uniqueness cannot be embodied in World 1, but requires the separate stuff of World 2. He makes this very explicit:

> I think that for my personal life as a conscious self, the brain is necessary, but it is not sufficient. In liaison with the brain events in World 1, there is the World 2 of my conscious experience, including a personal self at the core of my being. Throughout our lifetime this personal self has continuity despite the failure of liaison with the brain in states of unconsciousness such as dreamless sleep, anesthesia, coma. The brain states are then unsuitable for liaison, but the self can achieve in dreams a partial liaison (Ibid, p. 227).

But if the World 2 self "continues to be" while there is no liaison with the brain, why does it not have "things to tell us" when contact is re-established. Certainly, we sometimes "solve a problem in our sleep", but what evidence is there that this is the work of a disembodied mind reporting directly to consciousness rather than the fruit of the subconscious dynamics of the brain? The point I am making is not that we can prove Eccles wrong, but rather that there is no "this far can the brain go, and no farther" argument to prove him right. Thus we can hardly agree with Eccles that "there is *evidence* (my italics) that the self in World 2 has an autonomous existence, bridging gaps of unconsciousness when the brain fails to be in a state of liaison".

One final comment. If the above material argues for a fully materialist embryology and psychology, it does not augur well for Bernstein's aim to define the differences between living and artificial systems. As we come to develop cybernetic theories of the self-directedness that now distinguishes living systems, so will we come to build artificial systems that share this characteristic in increasing measure.

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Human Motor Actions – Bernstein Reassessed
H.T.A. Whiting (editor)

CHAPTER VIb

CONTROL THEORY AND CYBERNETIC ASPECTS OF MOTOR SYSTEMS

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This philosophical essay by Bernstein was a culmination of his long research work and was published in 1962, just a few years before his death. This was also the time when control engineers started considering the information aspects of a system and the concepts of adaptive and learning systems were being developed.

At the outset of the essay, Bernstein draws a distinction between the cybernetics of Wiener, Shannon and Ashby and the cybernetic concepts he proceeds to apply to physiology. The basis of this distinction is not entirely clear but is perhaps based upon the relative complexity of physiological systems when compared to classical engineering systems. This issue is not addressed again until the last paragraph of the paper when he proposes that "It is now necessary ... to indicate and study the differences between living and artificial systems" (p. 546). Whether this distinction exists seems unclear and whether we have enough understanding of living systems to make that determination is even more uncertain. This issue, like the issue of rigid materialism discussed below, seems just beyond determination by scientific methods at our current level of ignorance.

In the preceding paper Arbib has reviewed the historical development of the artificial intelligence field in the Soviet Union as it related to the biological problems. He has also extensively reviewed the concept of motor synergies and development of motor schemas. Our discussion on these topics will be limited to supplement Arbib's paper with an orientation based on modern control theory. Before going into the control theory and cybernetic aspects of motor systems, we wish to comment on two themes which are interwoven throughout the body of this paper: the unity of nature and its materialistic laws governing the phenomena of life. He means this with a vengeance.

Unity of Nature and Materialisric Laws

Most scientists would probably accept the notion that the domain of science is the material world. To the extent that some consider this an incomplete description of our personal world of experience, there is room for other "worlds" which do not contradict the material one. The distinction between mind and brain is one issue of this sort that has been addressed by such notable scientists as Sperry and Eccles (see Arbib). Bernstein, we believe, would disagree and insist upon "a strictly materialistic interpretation to be given to the concepts of singleness of purpose, expediency, etc." (p. 533) in the action of any organism.

What is the character of the natural laws governing living systems? Bernstein tells us that "An organism must ... possess two determining features. Firstly, it ... preserves its own (systemic) identity ... Secondly, the organism is continually changing in a set direction at all stages of its existence." (p. 535) "the organism as a whole and, very possibly, each of its cells struggle actively for survival, development and reproduction. The life process is not a balancing with the environment, but a conquest of the latter, aimed not at preserving the status quo or homeostasis, but at an advance towards fulfilment of the developmental and self-preservational program of the genus" (p. 537).

Furthermore, Bernstein goes on to state that "we are forced by the facts to regard all phenomena pertaining to an organism's interaction with the world ... as being cyclic processes ...; and as a result the processes of interaction termed (in the old sense) reactive ... and those termed spontaneous ... are both here combined into a single general class" (p.534).

Problems of Motor Functions

Having stated these very general principles, applicable to all living systems from single cells on up to human beings, Bernstein returns to the themes of previous essays (e.g. Chapters 4 and 5) concerning the problems of motor functions. These involve the same cyclic issues of action and reaction: a) a tuned perception of the state of the organism in its interactions with the environment; b) the definition of future requirements based upon extrapolation from the present state; c) a programming of the action intended to fulfil these requirements; and d) the execution of the action which alters the state of the organism and its consequent perceptions.

This addresses experimental problems which remain at issue today in the field of motor control. What sorts of programs are generated and how can they be related to future requirements (e.g. simply moving a joint from one state to another)? Do programs incorporate peripheral input only in their generation or during their execution. What are the operational rules that govern this and where does it happen?

Bernstein suggests that our understanding of these problems might be aided by Gelfand and Tsetlin's ideas on "well organized functions". Well organized functions have arguments which can be separated into two classes, termed essential and non-essential (Gelfand et al., 1971).

Non-essential variables can produce abrupt but limited variation in a function such that two different realizations of the same function (e.g. the shape of an oak leaf or a person's signature) may differ yet be recognizably related to each other. Motor acts are yielding and reflexive regarding those features that are non-essential.

Essential variables are dealt with in a more active, vigorous way. The organism will seek to preserve them in every member of a class. Structural examples easily come to mind such as all individuals having two arms although between individuals their lengths will differ. The former is an essential and the latter a non-essential variable.

We can find an analogous concept in the current search for invariant properties of movement. This notion that we can find elements in a diverse set of movements that remain the same has been and continues to be studied by many diverse groups (Asatryan & Fel'dman, 1965; Fel'dman & Latash, 1982; Bizzi et al., 1976, 1982; Soechting & Lacquaniti, 1982). The existence of such invariant (or essential) properties would enormously simplify the task of programming movements, at least as we now understand it.

Application of Engineering Methodology

In applying engineering tools to study the mechanism of human movements, one is faced with a serious methodological problem. The analytical tools have been developed to study cause-and-effect (or input-output) types of relationships one finds in engineering systems. Bernstein had noted that "Motor functions comprise a basic group of processes by which the organism not merely reacts to its environment but even acts on the latter" (p. 532). The implication was that the motor system was acting on a probabilistic extrapolation of the course of events (which would have occurred under conditions of non-interference) using some "common sense" simulation of the environment and anticipating future input-outputs of the system. The field of artificial intelligence is presently unable to define systematic rules to develop models of "common sense". At a recent meeting McCarthy of Stanford University noted that "much of the ordinary common sense ability to predict the consequences of actions requires going beyond the rules present in expert systems" (Kolata, 1982). Although considerable progress has been made in the last 20 years in applying control engineering and cybernetics concepts to motor control problems, some very fundamental questions about the functional organization and hierarchical information and control structures have yet to be answered.

The study of the motor system requires applications of four disciplines; namely biological control theory, neural modelling, artificial intelligence, and cognitive psychology (Arbib, 1982).

Biological control theory deals with the application of control theory techniques such as linear modelling and stability analysis applied to study problems such as the stretch reflex and tremor. Merton's hypotheses of motor control via the gamma loop (1953) - the follow-up servo hypothesis - was based on simple concepts of feedback control theory. This example also points to a very significant problem of identification in feedback systems when access to signals at various points in the system is very limited. Merton's original hypothesis was modified to the concept of alpha-gamma linkage by Granit (1955) emphasizing co-activation of alpha and gamma motoneurons and later on to servo-assisted motor control by Matthews (1972).

In Merton's hypothesis, the length of the muscle is assumed to be the controlled variable by a follow-up servo. Over the last 30 years, the question of what muscle variable(s) the nervous system controls in limb movements has been discussed repeatedly. In a recent article Stein (1982) has reviewed the arguments for and against the control of force, length, stiffness, velocity and/or viscosity. He concluded that different physical variables may be controlled depending on the type of limb movement required. Other possibilities also exist such as energy consumption or minimum response time which are frequently the design criteria for man-made systems.

On the output side of a muscle, there are two state-variables, length and tension, which are always linked by the external load and the internal electro-mechanical events in the muscle. The nervous system can only regulate the firing rates and the number of active motor units. This results in externally measurable force between limb and load which acts to move the limb and load through some trajectory in space and time. All intermediate variables are, in a sense, controlled in that the nervous system can affect and to some degree determine their future behavior.

In dealing with the central nervous system, we are faced with an enormous problem of the interacting nuclei with cerebrum and cerebellum as two dominant centers. Eccles (1973) has emphasized the role of the cerebellum in the control of movements. For example, he writes "Let us now try to visualize what would be happening in the cerebro-cerebellar circuits during some skilled action, for example, a golf stroke. In the first place we will assume a loop time of the cerebro-cerebellar circuit of a fiftieth of a second, so that a motor command to start the stroke will result in a "wise" comment from the cerebellum to modify the pyramidal tract (PT) discharge in accord with its learnt performance. The modified (PT) discharge is reported to the cerebellum and this in turn evokes a further corrective comment from the cerebellum. Thus there is this continuous on-going cerebellar modification of PT discharge. These hypotheses of the manner of cerebellar action in the control of movement provide great challenges for future research" (Eccles, 1973, p. 131). Eccles emphasized the concept of evolving movements and their continuous monitoring and control by multiple feedback loops through various levels of central nervous system (Figure 4-15, p. 132, Eccles, 1973).

The development of neural models to elucidate some of these concepts of cerebellar control of movements has been a difficult task. Several efforts have been made along these lines by Marr (1969), Albus (1971), Boylls (1976) and others. However, as Eccles pointed out there are good reasons to believe that the cerebellum functions as a special type of computer but there is not sufficient "hard data" as a basis for computer modelling.

Neurophysiological techniques of single cell recording and using correlation techniques, have made significant progress in tracing relevant connections. However, this frequently does not lead us to establish a cause-and-effect relationship in complex structures. Much remains to be done in developing techniques in the field of artificial intelligence to study such problems.

One clinical approach to study the function of a neural unit has been to observe the system performance by surgical removal of the region. As Granit (1977) pointed out this may not provide a final answer, because often it will merely tell us what the brain can do despite the loss instead of telling us what the lost portion did.

Motor Programs

Psychologists, artificial intelligence experts and researchers from the field of robotics in recent years have been focusing attention on the concept of the motor program (Mackay, 1980; Hollerbach, 1982). As Mackay points out, the motor theory has been split between two schools of thought; peripheralist and centralist. The peripheral hypothesis of motor control was pioneered by Sherrington and it essentially sought to explain all movement in terms of chains of reflexes, i.e., the control loops which are directly linked to sensory events at the periphery. The centralist dogma arose from the patterning of motor output within the CNS without the need of sensory information.

Considering the structure of the motor system one would think that neither approach is likely to be entirely correct. The truth lies somewhere in between the two concepts. It is quite possible that in certain experimental situations the peripheral or the central control hypothesis may be the dominant mechanism. For example, although Graham Brown and Karl Lashley demonstrated evidence of controlled motor output in the absence of sensory input this does not necessarily imply that sensory input does not play any role in an intact system.

Keele (1968) defined the concept of motor program as "a set of muscle commands that are structured before a movement sequence begins, and that allows the entire sequence to be carried out uninfluenced by peripheral feedback". Certain ballistic movements may seem preprogrammed but even in those cases the peripheral feedbacks contribute to load compensation as well as to modulate the central command to effectively use the most recent peripheral states of the system (Gottlieb & Agarwal, 1981; Gottlieb et al., 1983).

One final comment:

It seems then that the fundamental questions raised by Bernstein in 1962 remain equally fundamental and equally unanswered 20 years later. We may optimistically suggest that they will provide many individuals with a way to pass the time for the next 20 years.

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