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Ciba Foundation Symposium

TRANSCULTURAL PSYCHIATRY

Edited by A.V.S. DE REUCK, M.Sc., D.I.C., A.R.C.S.

and

RUTH PORTER, M.R.C.P.

With 10 illustrations

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THE Ciba Foundation, a unique international institution, owes its inception to the generosity of CIBA Limited, Basle. However, being established under British trust law, it enjoys complete independence in practice and policy.

Under the guidance of its distinguished Trustees, the Foundation offers accommodation to scientists from all over the world at its home in Portland Place. Foremost in its activities is the organization of small conferences, the proceedings of which are published in book form in the manner of the present volume. The Foundation convenes many other informal discussions between research workers of different disciplines and different nationalities and each year invites an outstanding authority to deliver a special lecture. An exchange programme between French and British postgraduates is conducted and a library service is available. Furthermore, the Ciba Foundation attempts in every other way possible to aid scientists, whether they be Nobel Laureates or young graduates making their first original contribution to research.

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Preface

THIS symposium was originally proposed by Professor E. D. Wittkower who suggested that one of the small interdisciplinary conferences convened under the auspices of the Ciba Foundation might afford an admirable opportunity for transcultural psychiatric research workers and anthropologists to share their observations, compare problems and exchange views. In planning the scope and membership of this meeting—the first devoted by the Foundation to psychiatry—the collaboration of Professor Wittkower and of Dr. H. B. M. Murphy were invaluable and are most gratefully acknowledged.

Not surprisingly the international membership of this meeting was as far-flung as that of any Ciba Foundation symposium. Of course, it is in the nature of their avocations that both transcultural psychiatrists and anthropologists tend to spend much of their time in the field, and in the event their duties overseas ultimately prevented a number of those who had accepted invitations from attending, including Dr. Gregory Bateson (Hawaii), Dr. Margaret Field (Accra), Dr. Paul Parin (Zürich), Professor Melford Spiro (Chicago) and Dr. N. C. Surya (Bangalore). A very august reason for dropping out at the last moment was that of Dr. Tigani el Mahi, who shortly before the meeting was appointed member of the Council of Sovereignty in the Republic of the Sudan.

Sir Aubrey Lewis was persuaded to take the Chair on this occasion and it is a sincere pleasure to record here how much the symposium owed to his light and masterly guidance, and also the debt the Foundation owes to him for his generous advice and assistance during the planning stages of the meeting.

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Transcultural Psychiatry
A.V.S. Deuck & Ruth Porter

CHAIRMAN'S OPENING REMARKS

Sir Aubrey Lewis

It is a privilege to be chairman of this meeting. The subject is of considerable importance, not only to us, but to a much wider collection of people who are concerned with the effect of diverse cultures on mental stability. It would be stupid of me to try to anticipate what is going to be said or what ground will be covered, as the discussion is likely to be wide and also rather deep at times. I was tempted, as I suppose most of you were, to embark upon a discussion of what is meant by "culture" and "transcultural", but I remembered in time that A. L. Kroeber and C. K. Kluckhohn had attempted this exercise (1952. *Pap. Peabody Mus.*, **47**, 181) and found that there were one hundred and sixty definitions of culture in English. It seems unnecessary to add to them, at any rate, at this stage.

The subject of our symposium is an old one; it is chequered with assumptions about national character which have often been deplorable both in their carelessness and in their consequences. David Hume put the matter well when he said, in 1741: "The vulgar are apt to carry all national characters to extremes; and having once established it as a principle that any people are knavish, or cowardly, or ignorant, they will admit of no exception, but comprehend every individual under the same censure. Men of sense condemn these undistinguishing judgements; though at the same time they allow that each nation has a peculiar set of manners, and that some particular qualities are more frequently to be met with among one people than among their neighbours." Unfortunately views on this matter have not always been so enlightened. In Hume's time, as one can see in psychiatric writings of the period, the disciples of Rousseau put it about that

mental disorder is a product of civilization, while others assumed that mental and moral degeneracy was the characteristic of those whom they despised as barbarians and savages. These crude notions have fortunately given way to a very different approach. At the beginning of this century psychiatrists began to take a closer interest in the matter; men of the standing of Auguste Marie in France and Emil Kraepelin and others in Germany recognized its importance, both for the furtherance of psychiatric knowledge and also because of its implications for the future of what we now call the developing countries. No doubt they were influenced by such studies as those of E. Dürkheim on suicide (1897. Le Suicide: Etude de Sociologie. Paris: F. Alcan) and by the political changes during this time, when colonies were becoming so much the subject of political discussion. There were also people—W. H. R. Rivers is an example—who were influenced by Freudian theory. The conclusions arrived at then, as in G. Voss' monograph (1915. *In* Handbuch der Psychiatrie: Allgemeiner Teil Abteilung 3, p. 1, ed. Aschaffenburg, G. Leipzig: Deuticke), were that there are no specific racial psychoses but that the national character can print its own stamp on individual disease patterns; the cultural development of a people has an effect on the form and content of mental disorders but cannot give rise to new types of illness. Kraepelin summed up current opinion when he wrote in his textbook (1883. Compendium der Psychiatrie. Leipzig: Abel) that there was very little that was assured about the tendency to mental illness in different peoples, and that it was impossible to separate the effects of different influences, such as culture, habits, climate, nutrition and physical disease. He believed, nevertheless, that the special disposition of a people was expressed in its mental disorders so that the incidence and forms of mental illness could provide rich sources of information and understanding about the peculiar social structure of individual countries. Kraepelin also asserted that new forms of mental disorder are nowhere to be found. In his empha-

s on the difficulties of the subject and his frank acknowledge-
ıent of ignorance Kraepelin was saying something which has
ad to be repeated since. Professor Leighton has noted that this
an area in which there is an extraordinary plenitude of ideas
ıd theory, and an extraordinary dearth of sound and firm con-
usions. That is rather the same view as Kraepelin's, although
ıere was much less information at his disposal, forty years ago,
ıan there is now at ours. The sophistication that has come into
ıe whole field is indicated by the papers which will be presented
ıd by our discussions.

It is the joining of forces by anthropologists and psychiatrists
ıat has made such a difference to progress in this field in the past
ventv years. The fruitful combination of psychiatrists and
ıthropologists is mirrored in the composition of this group.

Transcultural Psychiatry
A.V.S. Deuck & Ruth Porter

RECENT DEVELOPMENTS IN TRANSCULTURAL PSYCHIATRY

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I AM honoured to present the opening paper at this Ciba Foundation symposium. My introductory address will survey the field of transcultural psychiatry.

Recent interest in the field of cultural and transcultural psychiatry may give the impression that this is a new field. This is not so. It can be traced back to the beginning of this century when Kraepelin (1904), inspired by regional differences in the behaviour of the mentally ill which he had observed in Germany and in other European countries, left for Java with the explicit purpose of studying cultural influences on the frequency and symptomatology of mental disorders in this country. He noted that manic-depressive psychoses were uncommon and that depressive reactions, if they occurred, seldom contained elements of sinfulness. Other psychiatrists at about the same time, as well as social scientists, described culture-bound syndromes such as Amok, the Windigo psychosis, Latah, and Arctic hysteria.

A *rapprochement* between anthropology and psychiatry, and especially between anthropology and psycho-analysis, took place in the third and fourth decades of this century under the impact of

the work of Ralph Linton (1945), Abram Kardiner (1947), Margaret Mead (1952), Irving Hallowell (1953), and many others. The focus of interest of these authors was largely on the area of culture and personality, and psychopathology was of secondary concern though there were also some pioneers in the field of what we now call cultural psychiatry of whom these names may be mentioned (Aubin, 1952; Benedict and Jacks, 1954; Carothers, 1953; Devereux, 1961; Laubscher, 1937; Linton, 1945; Stainbrook, 1952; Tooth, 1950).

But major impetus to the field of cultural and transcultural psychiatry has been given only recently. If we ask ourselves why these developments have come about, several explanations can be offered. It appears: (A) that anthropology has left behind a stage, through which every science goes, where it concerned itself solely with a description of sociocultural institutions and has increasingly turned its scientific curiosity towards why human beings behave as they do; (B) that technological advance, the ease of travel, combined with an awareness of the interdependence of the world in the nuclear age, has brought psychiatrists face to face with psychological problems in other countries; and (C) that leaving taxonomy behind—earlier than anthropologists—and turning their interests to the dynamics of human behaviour under the impact of Freud's discoveries, a new brand of psychiatrist has arisen whose perspective is not confined to the boundaries of the mental hospital grounds or to the four walls of their office.

It is not irreverence on our part if in what follows we shall pay scanty attention to earlier publications; this omission is merely dictated by the exigencies of manageability of the material to be reviewed. Moreover, a fairly comprehensive review of earlier publications has been given in R. Linton's book, *Culture and Mental Disorders* (1956) and in M. Opler's, *Culture, Psychiatry and Human Values* (1956).

Our presentation will concentrate on three hundred publications which have been sent to the editors of our *Transcultural Psychiatric*

Research Review (1960–4) during the last five years or which have been traced by the editors in their scrutiny of the relevant literature. Our objectives in preparing this introductory paper have been to define, to delineate, and to delimit the field of transcultural psychiatry, as we see it; to state some of its investigative aims; and to give an overview of the problems which have been investigated. In other words, we shall *not* concern ourselves with the results obtained but rather with the question of "Who does what, where, why, and how in transcultural psychiatry?"

The specific purpose is to give a bird's eye view of the field even at the risk that a good deal of what I am going to say will be familiar to this highly sophisticated audience.

DEFINITIONS

It may be debatable whether cultural psychiatry should be subsumed under the heading of social psychiatry or social psychiatry under the heading of cultural psychiatry. Following the example set by Ruesch (1961) we regard cultural psychiatry as a branch of social psychiatry. It concerns itself with the cultural aspects of the aetiology, frequency, and nature of mental illness and the care and aftercare of the mentally ill within the confines of a given cultural unit. The term *trans*cultural psychiatry, which is an extension of cultural psychiatry, denotes that the vista of the scientific observer extends beyond the scope of one cultural unit on to another, whereas the term *cross*-cultural is applied to comparative and contrasting aspects of psychiatry in any of the areas named.

In the light of our present knowledge, transcultural psychiatry is predominantly a field of research though there are also practical applications. It is obvious that understanding of the cultural background of his patients' and of his own cultural background greatly assists the practising psychiatrist in handling his patients; and that in planning mental health services cultural considerations have to be taken into account. It is hoped that additional knowledge of

sociocultural variables noxious to mental health will assist in the prevention, or at least reduction, of mental illness.

DELIMITATIONS OF FIELD OF RESEARCH

My co-workers and I have spent a good deal of time on defining the boundaries of our field of research. We fully realize that knowledge about and understanding of the effect of differences in cultural environment on *normal* personality development is an essential prerequisite of understanding of the cultural aspects of *abnormal* thought, feeling, and behaviour but we believe that as a field of research this is a domain for study by cultural psychologists and anthropologists. Equally, we have separated off from our immediate research interest the field of international psychiatry. International psychiatry concerns itself with such comparative aspects of psychiatry as teaching and training facilities, hospital administration, shortage of qualified psychiatrists, and treatment problems with little specific regard to the cultures involved.

To repeat, I define my own position in the fields of cultural and transcultural psychiatry as the contribution which I, as a psychiatrist, can make to the understanding of mental abnormality against different cultural backgrounds. This statement implies reliance in my research efforts on close co-operation with social scientists.

RESEARCH APPROACHES AND METHODOLOGIES

There are two major approaches to the field of transcultural psychiatry: those focused on *quantitative* differences and those focused on *qualitative* differences of mental disorders in contrasting cultures.

Both types of studies may be started off either from the psychiatric or from the social science point of view. The scientific observer may have been able to demonstrate, for instance,

differences in the frequency and/or nature of mental disorders at two or more culturally different observation posts, and may ask himself whether and how these differences can be explained in terms of sociocultural variables; or he may choose the location of his research according to sociocultural factors such as adherence to tradition versus culture change and ask himself to what extent, if at all, these factors affect the frequency and/or nature of mental disorders.

Methodologies of transcultural psychiatric research consist of application of the same investigative technique to persons of contrasting cultures either by the same observer or by different observers. These observers may be psychiatrists, anthropologists, psychologists, social workers, or a team composed of representatives of any of these disciplines. Though obviously desirable, as will be shown, for a variety of reasons such team co-operation has only infrequently materialized. If it occurs, it entails one of the major problems of all interdisciplinary research, namely, the dovetailing of data obtained by different disciplines—different in the primary focus of their interest, in their theoretical orientation, and in the methodologies employed.

Research tools used in the three hundred publications surveyed comprise in order of frequency: (A) clinical observations; (B) field surveys embracing field observations, interviews with key informants, home visits to sample subjects, and census examination; (C) hospital, law court and government records; (D) psychological tests; (E) questionnaires; and (F) psycho-analytical techniques. As regards clinical observations, psychiatrists predominate over social scientists in a ratio of 2:1 and as regards psychological tests, social scientists predominate over psychiatrists in a ratio of 7:1. Surprisingly enough, about equal numbers of psychiatrists and social scientists have been engaged in field research. In interdisciplinary research projects questionnaires were most commonly employed, with field observations, clinical observations, and psychological tests following in this order of frequency.

INVESTIGATORS

As might be expected, the bulk of the research in the field of cultural and transcultural *psychiatry*, in contrast to cultural and transcultural *psychology*, has been carried out by psychiatrists. The ratio between psychiatrists and social scientists in the three hundred publications surveyed is three to one. The percentage of publications with a joint authorship of psychiatrists and social scientists is eleven. Even if instances are added of actual co-operation without joint authorship and if the few investigators with double training are taken into account, research projects with interdisciplinary teams are regrettably uncommon. One may speculate whether representatives of the two disciplines are reluctant to work *with* each other or *under* each other.

In order of frequency, publications based on observations in America come first, those in Asia second, and those in Africa third. Little research in this field has been carried out in Europe and in Australia. Most of the North American investigations have concerned themselves with Aborigines and with ethnic and religious minorities, and the Caribbean Islands, not only on scientific grounds, have been highly attractive. Though there is considerable interest in cultural psychiatric problems in Japan and India, the actual number of articles on this subject published by indigenous investigators in these countries is small; a large slice of Asiatic publications originate in the Middle East and especially in Israel. Peoples in Nigeria and Senegal have been the special focus of research interest in Africa. Remarkable is the almost total absence of research in this field in Scandinavia, in Soviet Russia, and in the People's Republic of China. As regards the nationality of the investigators who carry out research in countries other than their own, once again American psychiatrists are in the lead.

If one asks oneself how these differences can be understood and why the majority of investigators are psychiatrists from the United States, a number of answers can be given: (A) In countries

in which few psychiatrists are available, services have to come first and research has to take second place; (B) The more homogeneous the population of a country is, the less interest exists in cultural differences, as may be the case in Scandinavia; (C) an overall ideology obliterates interest in subcultural differences; and (D) very prosaically, research funds for research abroad are more easily obtained in the United States than elsewhere.

The survey of investigators shows that the desirable goal of joint research efforts between psychiatrists and social scientists is far from being attained.

SUBJECTS INVESTIGATED

As regards subject matter, a small percentage of the three hundred publications analysed present general surveys, theoretical issues, and problems arising from co-operation between psychiatrists and social scientists. Articles reporting on actual research deal with cross-cultural differences in (A) total frequency of mental disorders; (B) relative frequency of nosological entities and of symptoms; (C) nature of symptoms; (D) care of the mentally ill; and (E) community attitudes towards mental illness and the mentally ill.

(1). *Total frequency:* Comparative studies of total frequencies of mental disorders in contrasting cultures are fraught with methodological difficulties. Hospital admission rates, for obvious reasons, fail to give information on this issue. Only a few reliable comparative field studies, such as D. Leighton and co-workers' Nova Scotian (1963) and A. Leighton and co-workers' Nigerian (1963) studies are available. But even if absolute figures could be obtained they would be of little use. More can be, and is, learned from comparative figures, for instance, in relation to different periods of sociopolitical development, to birth rank or sex of the patients.,

Starting off either from case counts or from cultural variables, attempts have been made to correlate disease frequency with

cultural stress factors. These can be grouped under the headings of: (A) stresses due to *existing* value orientations; (B) stresses due to *coexisting* value orientations; and (C) stresses due to *change* of value orientations. Reports have reached us, for instance, from India and Japan, which show the pathogenic effect of existing orientations regarding such values as traditional family structure, role and status of women, and sibling rank. A second series of investigations concerns itself with the emotionally disturbing effects of clashing coexisting value orientations, as in the case of American Negroes, American Indians, Chinese mainlanders in Taiwan, Okinawans in Hawaii, and of displaced persons, intergenerational conflicts, and of political change anywhere. Thirdly, the impact of cultural change which largely amounts to westernization—with breakdown of family traditions and of other social institutions, with industrialization and with urbanization—has made itself felt in many parts of the world, and has been studied in its mental health implications.

(2) *Relative frequency of nosological entities and of symptoms:* Research efforts have been directed towards the ratio of various mental disorders and the frequency of symptoms against contrasting cultural backgrounds. It has been suggested, though not proven, that schizophrenia is more common in Oriental countries than elsewhere and that this relatively high frequency is due to the Oriental way of life with its emphasis on social and emotional withdrawal, on meditation, and on contemplation. Endogenous depression, at least as we know it, is said to be rare in some primitive societies; this alleged rarity, for instance in Africa, has been attributed to culture-determined personality differences. Equally, the rarity of obsessional neuroses in African and other societies has been attributed to culture-determined personality differences and/or absorption of what could be psychopathology into culturally shared behaviour.

Studies have been carried out on the high frequency of conversion hysteria in Arabic women in relation to their subdued

social status, the differential frequency of alcoholism in relation to prevailing drinking patterns, the differential frequency of drug addictions, the differential rate of suicides, and on differences in the frequency of some hysterical manifestations and especially of types of delusional content in different cultures.

(3) *Nature of symptoms:* The questions have been raised: (A) to what extent the clinical picture of established psychiatric entities changes historically in the same culture with culture change; (B) to what extent it differs in different cultures; (C) whether unusual symptoms or symptom patterns are to be noted in certain cultural areas; and (D) whether novel forms of psychopathology exist in some cultures which are absent in others.

In general it has been pointed out that the definition of what constitutes a symptom is subject to cultural variations and that identical symptoms have different meanings in different cultures on account of differences in culturally shared beliefs. In particular, differences in modes of thoughts, use of language, and ways of behaving of schizophrenic patients, in depressive ideas and in hysterical manifestations, have been subjected to phenomenological, psychodynamic and social science study in contrasting cultures. Striking differences in the delusional content of patients suffering from schizophrenia or from paranoid states have been demonstrated in different societies, ethnic groups, and religious groups.

Various atypical forms of behaviour, such as the *bouffée délirante aigue* or the frenzied anxiety of Africans—usually shortlasting psychotic episodes with intense fear of bewitchment—have been described. To what extent these disorders are due to infectious-toxic factors, personality factors, or to cultural and other environmental factors has been debated and requires investigation.

A special group among these unusual symptom patterns constitute so-called culture-bound disorders, also described as folk illnesses, such as possession states (Wittkower, 1964; Yonebayashi, 1964), Pibloktoq (Jussow, 1960), Windigo (Cooper, 1934; Landes

1938; Teicher, 1961), Moth Craziness (Kaplan, 1962), Susto (Vellard, 1961; Rubel, 1964), Koro (Mead, 1952; Rin, 1965; Yap, 1965) and Tarantism (de Martino, 1962). Detailed clinical descriptions of these disorders have been given and their aetiology has been discussed. There is no doubt that culture specific elements have an important pathoplastic effect on these disorders, but it is also true that there is a remarkable similarity in some of them, designated differently in disparate societies, and it therefore seems reasonable not to regard them as unique, culture-specific phenomena but rather as culture determined variants of known psychiatric disorders.

(4) *Treatment:* In many parts of the world, in primitive and not-so-primitive societies, the bulk of the mentally ill or at least a substantial proportion of them are looked after by non-medical personnel. They are treated by quacks, herbalists, religious healers and cult groups. The choice of these non-medical healers is by no means solely determined by shortage of trained psychiatrists but also by prevailing aetiological concepts. The nature of these concepts has been explored. If the belief is held that mental disturbances are due to evil spirits, demons, or sorcery, persons believed to be in possession of supernatural powers are likely to be consulted. The personalities of these native healers, the procedures which they adopt, and the psychodynamics involved in their treatment have been studied. In keeping with African and Indian cultural requirements, procedures such as family participation in the treatment of the mentally ill in hospitals and village settlements have been employed and appraised in their effectiveness. Reasons for resistance to western psychotherapeutic measures, such as psycho-analysis, in some countries have been explored and reasons for preference for such procedures as Morita therapy (Morita, 1928) have been given. Modifications of western type group therapy, based on cultural considerations, have been experimentally introduced in Latin-American countries. Noteworthy are investigations regarding the doctor-patient relationship and

management of the mentally ill in contrasting cultures. Somatic forms of treatment are, of course, universally used, provided that equipment and drugs are available, yet little is known about the differential response to psychotropic drugs in different cultures.

(5) *Community attitudes towards the mentally ill:* Plenty of impressionist evidence regarding community attitudes towards the mentally ill in different cultures has been presented but little systematic comparative research has been carried out. Recent research into community attitudes in rural and urban Japan, modelled on previous studies in the United States and employing similar investigative techniques, is an effort in this direction (Miura *et al.*, 1964; Terashima and Nareta, 1964).

SUMMARY AND CONCLUSIONS

In the preceding pages, by way of introduction to this symposium, I have tried to give a bird's eye view of the field of transcultural psychiatry. I have defined the terms cultural, transcultural, and cross-cultural psychiatry, have outlined the boundaries of the field as we see them, and, on the basis of the material surveyed, have identified the investigators operating in this field, the subjects which they study, the methods which they employ, and some of the problems which they encounter.

As indicated by the recital of the areas covered by transcultural psychiatric research, a vast array of data has been accumulated, many of which are incompletely understood. There are some definite gaps and shortcomings:

(A) The shortcomings of epidemiological research especially in its cross-cultural applications are generally acknowledged. Those foremost in the field would probably be the first to admit that only a beginning in studies of this kind has been made, and doubts have been cast on the value of the application of classical epidemiology to the study of mental disorders. Beyond this, it is an undeniable fact that the statistical method never gets to the heart of human

behaviour and its motivations, the study of which, after all, is the main goal of transcultural psychiatry.

(B) As regards clinical observations, integration of psychiatric theory and social science theory is far from being accomplished. Possession syndrome is a case in point. Social scientists and psychiatrists have repeatedly studied this phenomenon more or less in conceptual isolation. A greater cross-fertilization and interpenetration of the two disciplines is required. The setting up of centres for transcultural psychiatric studies, multidisciplinary in composition, in increasing numbers and the convening of this very symposium are efforts in this direction, though more can be done.

(C) As regards clinical methodology, though others may disagree, I believe that the descriptive approach and even a refinement in phenomenology will not lead us much further. At the present state of our knowledge, be it in the understanding of differences in depressive symptomatology or of such syndromes as Koro, more can be gained from a dynamic approach. This means that more psycho-analysts than hitherto must leave their couches behind and devote their time, energy, and skills to field research.

(D) Last, but by no means least, there is a dearth of young psychiatric research workers qualified to carry out research in our field. To overcome this handicap funds and extension of existing training facilities are required.

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DISCUSSION

Shepherd: Professor Wittkower, how would you incorporate subcultural psychiatry into this scheme? You spoke about different cultures, and you implied that they existed in different countries; but if you had been speaking about the differences between ethnic and religious groups or rural and urban populations in, for instance, Japan you would have been discussing differences *within* one national group or culture. Are these subcultural differences to be included in your definition?

Wittkower: We do, of course, consider subcultural groups as important subjects of study, and as sub-fields in the field of cultural psychiatry. In the work which is going on at the Section for Transcultural Psychiatric Studies of the Psychiatric Department at McGill University, and which Dr. Murphy discusses (this volume, p. 303), we are studying subcultural groups. In our province, Jewish, English, French, Polish and German Canadians, as well as other small groups, are included in the survey.

Rubin: Do you also include differences in social class?

Wittkower: We obviously take social class into consideration, but what we are trying to do is to differentiate between social psychiatry and cultural psychiatry, although the borderline between the two is not always distinct. The present concept of social psychiatry is so wide that we feel, although others may disagree with us, that the time has come to deal with definite, manageable segments. If you include, as well as the areas which I've just described, other sub-disciplines—marriage counselling is an example which could come under this same heading—then you find you are dealing with too wide a field.

Mead: One of the problems here is that you may be referring the differences between your groups of Canadians back to their countries of origin whereas these differences are in fact carried over from other cultural units in Canada or the United States, as later differentiations may occur within the same cultures. This happens, for example, in different groups in Tasmania and Australia, or in different parts of the United States.

Lewis: This is very well illustrated by O. Ødegaard's studies of Norwegian immigrants to the United States (1932. Emigration and Insanity. Copenhagen: Levin and Munksgaard). They are among the most valuable contributions to this field that we have.

Yap: I think it could be misleading, and even untrue, to suggest that in studying mental illness in cultures which are not western, one is not interested at all in social categories. In developing countries Lambo has noted that factors such as social class and education affect mental illness. I have also found, in my studies on Koro and on possession, some quite interesting and significant differences in the social backgrounds of our samples. Fortunately, we have fairly good control data for the general population in the community. I think that if we work in outlandish countries not much progress can be made unless we make use of social categories. In these areas the distinction between *cultural* and *subcultural* as such is not all that important, but sociological distinctions are important.

Caudill: I agree. To make any sense at all out of the natural variations which are found in any culture in which there are large amounts of data, you need to make use of social categories. In Japan, one of the categories that I found most helpful for understanding the symptom-patterns of psychiatric patients concerned the "style-of-life" of the family, that is, whether the principal earner in the family was a "salary-man", or an owner or employee of a small business. This distinction in style-of-life is an important one in Japan today as the ratio of salary-men or wage earners to small business owners and employees is about sixty to forty in urban areas. These within-culture variables help us to make detailed interpretations from our data rather than just demonstrate gross similarities and differences between the Japanese and the Americans.

Mead: I think it is useful to realize that variations within a culture by class, occupational differences, sex and age can be treated as versions of a whole culture in some instances. In the Middle East, for example, if you want to talk about class-differentiated culture, you must also talk about sex-differentiated culture, as the women have a different version of the culture from the men. In the United States at present it is reasonable to say that adolescents have a special version of the culture. We should be aware of these differences when choosing the

material for our studies; to look for a sex differentiation of the order of the Middle Eastern one in, say, the United States, would not be very fruitful. In the United States there are American-Indian cultures in which the women speak a different language from the men, and know it. Women speak a different language from men in England and in the United States, but they do not know it as the differences are inarticulate, below the level of consciousness and do not function in the same way as in some of the American-Indian communities. In a country like the United States, where we have been subjected to thirty years of class analysis, virtually everybody now knows about class and thinks in terms of class; it has become an element of conscious differentiation within the society. The president of a local club, for example, can give you the class breakdown for the entire community. All these differences in degrees of articulateness, consciousness and differentiation have to be included and allowed for in our studies. Would you agree with that general statement, Professor Wittkower?

Wittkower: Entirely.

Leighton: Taking off from that point, I would like to hear Professor Wittkower's comments on the emphasis that has been put on social class in the United States. I think one of the reasons for this emphasis is that we are a country composed of people who come from many cultures and who have retained parts of these cultures. Our behaviour, derived as it is from numbers of cultures, may promote awareness of disharmonies and conflicting values. I wonder if this has been a factor in generating our interest in the social categories.

Concerning the choice of who treats psychiatric disorders in different cultures, I wonder, Professor Wittkower, if you would include "effectiveness of treatment" in your list. You mentioned that even in places where professional psychiatric services are available treatment may be in the hands of herbalists, mystics or religious healers. One possible explanation for this is that formal psychiatric treatment has not demonstrated that it is more effective than these other methods.

Wittkower: This is very true. Professor Carstairs has written extensively on this subject and Professor Lambo and I have had many discussions about it. The view which we have adopted is that a rapid changeover to western psychiatry is not acceptable to the local

population. Professor Lambo, I am sure, has many examples that confirm this; in certain cases the native healer is undoubtedly more effective than the western-trained psychiatrist.

Leighton: Even in western society a "native healer" may not be notably inferior to the orthodox psychiatrist. I am sure Professor Wittkower knows better than I do that psychiatric treatment is something that is largely used by the middle and upper classes. Many studies have shown that people of the lower socioeconomic levels are hard to reach and maintain in treatment.

Rubin: I would like to emphasize this for the Caribbean area where social class, education, occupation and race or ethnic origin are very clearly and closely correlated. As Dr. Caudill has said, this is not just a statistical artefact but differentiates ways of life, especially in terms of cult membership and access to different kinds of psychiatric treatment. Before you can do depth studies in these areas it is important to be able to describe your population in this kind of framework. Whether a patient goes to a cult healer or to a psychiatrist is frequently determined by social class.

Mead: Professor Wittkower, I would like to add one more element to your inventory of the factors which are affected by different cultures, and that is the extent to which psychodynamic theories are influenced by the culture in which they are developed or to which they are transplanted. Would you include this? One can make very clear statements about the selective attention to different aspects of the same theory in different cultures. For instance, in the United States, certain psycho-analytical theories have been very widely accepted and others have not; and so, when Great Britain and the United States, or France and Germany, or Brazil and Israel are compared, these differences in psychological theories can be seen. I do not think that we have even begun to exhaust the study of this subject yet, because we do not know enough about the development of Japanese, Indian, and Chinese psychiatry and this will undoubtedly put a further imprint on the subject. We will not have anything like a real understanding of the potentialities of culture for developing theory until we have had contributions from as many of the great cultures and culture areas of the world as possible. We are at present in a transitional stage: we are far better off than we were twenty years ago, we are infinitely

better off than we were forty years ago but we still need to know a great deal more about all this than we do at present.

Wittkower: As Dr. Mead has said, we have not begun to exhaust this subject; I would say that we have hardly even begun to study it. There is still a wide area which should be examined and explored. A recent book (Parin, P., Morgenthaler, F., and Parin-Matthey, G. [1963]. Die Weissen denken zuviel. Zürich: Atlantis) was a very interesting effort in this direction.

Murphy: An extension of Dr. Mead's point which Professor Wittkower did not mention because there have been virtually no studies done on it is the demands on, and the role given to, the psychiatrist by different cultures. I have recently been asking our psychiatric residents what limits they would like to put on the role of the psychiatrist in our society. In Montreal, at the moment, they see their role as almost infinite; if they were allowed to they would take over the direction of all society! In other countries I am sure that there are particular areas outside pure treatment in which psychiatrists are thought to have some competence and that these areas vary from country to country. We have done no work on this but I think it is important for future studies.

Wolfenstein: Professor Wittkower, how are diagnostic categories established for transcultural psychiatry? I presume that some of the symptom-complexes that we call schizophrenia are called by different names in other cultures, then a translation or interpretation is made, and after this the syndromes which are thought not to have an equivalent in our nosology are given a special name as, for example, "possession states".

Wittkower: As you know, this is a major problem and one could enlarge on it a great deal. In a recent book, G. Devereux (1961. Mohave Ethnopsychiatry and Suicide. Washington: Smithsonian Institution Bureau of American Ethnology, bul. 175) has presented disease entities in terms of both standard nomenclature and indigenous aetiological theory. In our own study we have tried to appreciate the differences in the concept of schizophrenia in different cultures. This is, of course, something entirely different in North America and, let us say, in German-influenced countries such as Japan although the differences are actually not as great as we originally expected. Dr.

Murphy, does our Canadian survey contribute anything to the solution of this problem?

Murphy: No, I do not think that it does; much more needs to be done in this area. The word schizophrenia is used for a host of conditions which eventually, perhaps, we will be able to separate out. The tendency at the moment is to use the word as a blanket term to cover many syndromes.

Lin: I would support Dr. Murphy's comment that much has still to be done here although I do not think the cultural variation is as great as one might have presumed. It seems to me that the education and training background of the psychiatrist is more decisive than cultural variations in the identification of symptom-complexes. For example, when a person in India makes a diagnosis of schizophrenia he will probably follow the diagnostic practice of the United Kingdom, whereas psychiatrists in Japan are likely to follow the German school. These variations, therefore, relate more to the different psychiatric schools of thought than to the cultural differences of the psychiatric patients.

Carstairs: This alters the focus of our discussions to a study of psychiatrists. I thought that Dr. Wolfenstein was asking: "Do we know if we are studying the same disease entities in the same patients in different cultures?"

Caudill: I took part in a study in which about eight hundred Japanese patients who had already been diagnosed by Japanese psychiatrists were re-diagnosed in terms of the American Psychiatric Association categories (1952. Committee on Nomenclature of the American Psychiatric Association. Washington: American Psychiatric Association). For the psychoses, which made up about five hundred of the patients, we were in 95 per cent agreement with the diagnoses made by the Japanese psychiatrists. The neuroses, as you might expect, gave us a good deal more trouble. The great majority of the psychoses fell into the broad category of schizophrenia and we were able to agree that these people were schizophrenic, both from the American and the Japanese points of view. Using a part of these data, we found very interesting differences in the patterning of symptoms for schizophrenics in each culture. If you have adequate data on symptoms—and particularly on the pattern of

symptoms during the year preceding the current hospitalization—you can, in all probability, demonstrate clusters of symptoms that in some respects are characteristic for a particular culture (Schooler, C., and Caudill, W. [1964]. *Ethnology*, **3**, 172). I would strongly support the idea that, in a methodological sense, a great advance will be made in cross-cultural epidemiological studies when we can agree about what we mean by a particular type of illness.

Leighton: I would like to say something about diagnostic categories in West Africa. One of the most enthralling days that I can remember was when Professor Lambo took me to visit a series of native healers, all of whom specialized in the treatment of mental illness. Each of these men presented two or three cases and gave me the opportunity of questioning either the patient or the healer himself about the characteristics of the disorder. On one occasion a healer said to me, through an interpreter: "This man came here three months ago full of delusions and hallucinations: now he is free of them." I said, "What do these words 'hallucination' and 'delusion' mean, I don't understand?" I asked this question thinking, of course, of the problems of cultural relativity in a culture where practices such as witchcraft, which in the West would be considered delusional, are accepted. The native healer scratched his head and looked a bit puzzled at this question and then he said: "Well, when this man came here he was standing right where you see him now and he thought he was in Abeokuta" (which is about thirty miles away), "he thought I was his uncle and he thought God was speaking to him from the clouds. Now I don't know what you call that in the United States, but here we consider that these are hallucinations and delusions!" I saw no symptoms or behaviour that day that did not fit somewhere into my experience as a clinical psychiatrist, although obviously one day was not long enough for a full evaluation. Professor Lambo arranged later for us to study, systematically and in some detail, twelve patients of native healers. Each of the four psychiatrists in our team participated in this study so that four different psychiatric viewpoints were represented. Dr. Lambo's colleague, Dr. Asuni, interviewed the native healer himself, took histories of the patients, and got a description of each case from the healer's point of view. When we looked at our data we could not find anything that failed to fit into one or other of the usual western syn-

dromes. These patients were all psychotics, incidentally, and in some of them there was an organic basis for the psychosis, for instance, an epileptic with hallucinatory auras. The rest were very similar to what one sees, or used to see anyhow, in the public hospitals, labelled schizophrenia.

Margetts: The more I listen to discussions on transcultural psychiatry, the more I am coming to believe that perhaps there is no such thing (Margetts, E. L. [1965]. *Canad. psychiat. Ass. J.*, **10**, 79 [editorial]). I would agree with what I think Sir Aubrey meant when he intimated that we had not learned a great deal about it since the time of Kraepelin. As far as I am concerned, psychiatry is the same all round the world: the signs and symptoms of mental diseases are the same, the diagnoses are the same and there is probably just as much possession syndrome in England as there is in equatorial Africa. I would like to hear comments from other participants about this, because I think that many of us could spend less time on words and more on learning about the facts of psychiatry round the world. It seems to me that the problem is to try to find out more about the basic aetiology of mental illnesses, still largely unknown, rather than just to go on repeating our descriptions of the various syndromes in different parts of the world.

Lewis: Professor Margetts has raised a matter that is going to recur and which we will consider more fully later on.

Mead: I hope that this meeting is going to be a start for defining the transcultural field. There is more international participation here than we have had at previous meetings, so perhaps the various devices, distortions and omissions that have been characteristic of most of the recorded histories so far will be eliminated.

There are some very curious beginnings to this subject. The first article on culture and personality was actually written by Leslie White, who is not generally associated with this field (1925. *Open Court*, **39**, 145). The importance of Lawrence K. Frank in developing the field ought also to be mentioned; I described his work some years ago (Mead, Margaret [1953]. *In* The Study of Culture at a Distance, p. 3, ed. Mead, M., and Métraux, R. Chicago: University of Chicago Press). Some very important papers on transcultural psychiatry were written in the nineteen-twenties, for example, C. G. Seligman's early paper

(1923. *J. roy. anthrop. Inst.*, **54**, 13) and the work of E. Sapir (1949. Selected Writings of Edward Sapir on Language, Culture and Personality. Ed. Mandelbaum, D. G. California: University of California Press). It is just as important to have the bibliography in this field finally completed and brought up to date as it is to get Kraepelin's work into proper perspective.

Transcultural Psychiatry
A.V.S. Deuck & Ruth Porter

HOMINID EVOLUTION, CULTURAL ADAPTATION AND MENTAL DYSFUNCTIONING

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IT has been said by a biologist that "man is the historical creature par excellence" (Mayr, 1963*b*, p. 624). Not only is he a product of biological evolution; as a species he is also history-bound when we consider the widely varying modes of cultural adaptation and readaption he has continually undergone. It is not always sufficiently emphasized, however, that the explicit recognition of this dual aspect of man's historicity is one of the unique achievements in the intellectual history of western civilization. Nor have all its implications been fully explored, or understood, in biological, sociological, cultural and psychological terms. A novel stage in man's capacity for looking at himself objectively has been reached in western culture in so far as it has led to a continuing search by man for more and more reliable knowledge about himself, now set in a temporal framework of the most inclusive range. This general perspective is as important for psychiatry as it is for anthropology and other disciplines concerned with the study of man. What I shall be dealing with here are some of the implications that can be drawn from our present knowledge of human phylogeny that transcend the purely morphological facts that are presented by the palaeontological record. These implications are relevant for our understanding of the psychodynamics of modern man, representing as he does a behavioural level which marks the contemporary culmination of the hominid line in a very lengthy evolutionary process.

In the past, the necessary specialization required for the description, analysis, and ordering of the data in human palaeonto-

logy inhibited or discouraged discussion by non-specialists interested in the broader behavioural and psychological dimensions of human evolution. Indeed it was difficult to discuss trends of this kind in relation to the empirical facts of human palaeontology without resorting to pure speculation. Limits were also imposed upon a conceptual perspective which, in the nature of the case, required a more holistic approach than that represented by physical anthropology. Darwin himself envisaged a psychological dimension of evolution which led to the establishment of comparative psychology late in the nineteenth century. But, at the time he wrote, the best he could do was to refer to the work of Herbert Spencer. Obviously direct observation of behaviour at different stages of hominid evolution is still ruled out. On the other hand, the new data now accumulating from field observations on the behaviour of non-hominid primates is becoming more and more significant as an indirect means of filling this gap in a manner which laboratory studies of individual primates could not do. (For summaries see Carpenter 1954, 1958; De Vore, 1963.) Besides this, the palaeontological data now available have some important behavioural implications. As a consequence, a conjunctive approach to human evolution is no longer purely speculative and is likely to become less so. Evolutionary biologists themselves, with the adoption of the "synthetic theory", have been co-ordinating data from a number of specialized biological fields. Many aspects of the evolutionary process have been reinterpreted (Simpson, 1953; Dobzhansky, 1962; Mayr, 1963*a*) and behavioural evolution has been under discussion (Roe and Simpson, 1958). All these developments are relevant to a reconsideration of human phylogeny in a more inclusive frame of reference.

AUSTRALOPITHECUS AND HOMINID EVOLUTION

An adequate understanding of human evolution, as of any phyletic sequence, is not solely dependent upon the knowledge provided by the discovery of new fossil forms, important as these

may be. The identification and relationship of these in an evolutionary sequence is of paramount importance, otherwise the evolutionary unit with which we are dealing cannot be clearly defined and interpreted. One of the purposes of an orderly zoological classification is to indicate significant genetic relationships in a systematic form. Problems of this sort have harassed the study of human evolution for a long time. At the period when Darwin remarked in the concluding chapter of his *Origin of Species* (1859) that "much light will be thrown on the origin of man and his history", there was no pertinent fossil evidence that could be brought to bear on hominid evolution. But in the twentieth century, with the accumulation of fossil material, particularly from Africa, the opposite problem arose: hominid forms became more and more plentiful but were given generic and specific labels that became chaotic. They obstructed evolutionary thinking and the significance of much of the data already in hand (Winner, 1964). Besides this, the Piltdown forgery early in this century added to the confusion. Fortunately this period is now being transcended, with the consequence that the outlines of major evolutionary trends are becoming clearer than before and can serve as a matrix for the discussion of behavioural and psychological gradations, even if precise correlations with biological data are not possible. The recent simplification of classification, for example, which recognizes only two genera of the Family Hominidae, *Australopithecus* and *Homo*, is, at the same time, a direct index to a behavioural gradation, and a distinction that implies different levels of psychological integration. Washburn (1963, p. 196) comments as follows: "The adaptation of the genus *Homo* is based on technical skills, language, and many attributes of mind (memory, planning, etc.), and, if complex tools and intelligence account for the radiation, they must be used in classification." Thus a broad empirical base on which to form a conception of evolutionary gradation from a non-hominid level to that represented by *Homo sapiens sapiens*, is emerging.

In thinking about hominid evolution two categories of problems must be clearly differentiated. The first of these involves speciation, the branching off of a specialized line of hominids, distinct from the anthropoid line, that gave rise to the differentiated pongids (anthropoid apes). Among the latter it is now recognized that the gorilla and chimpanzee represent conservative lineages which, although specialized in their own way, are closest to the hominid line. The latter, in turn, is anatomically and adaptively the most radically distinctive of all evolutionary units belonging to the superfamily Hominoidea, which includes the apes and man.

Problems of the second category are concerned with the identification of the earliest hominids and the subsequent evolution of this branch as the evolutionary unit that culminated in modern man. A major break-through in our knowledge of human evolution has been the discovery and identification of the Australopithecines. Members of this genus are now accepted as representatives of the earliest grade in hominid evolution. These early Villafranchian hominids exemplify the closest link with our ape-like, hominoid ancestors of which we have knowledge. What we have now learned is that the connotation of the term "man", as we have been accustomed to use it in non-evolutionary contexts, is not synonymous with the technical term *hominid*. The Australopithecines, although hominids, were not "men" in the sense of "human beings"; they did not possess a "human nature" however ambiguous that term may be; they were not "men" biologically, culturally, or psychologically. This makes it more apparent than ever before that, whatever characteristics may be selected for emphasis, a "human" status necessitates a definition with reference to a position on some scale of structural gradation, as well as on some differential level of behavioural and psychological functioning. In terms of zoological criteria alone, the species comprising the genus *Homo* would suggest the lowest common denominator of a "human" status, while those constituting *Australopithecus* fall below it.

While fully recognized now as differentiated hominids, chiefly on account of their dentition, and pelvic-leg structures that permitted upright locomotion, the Australopithecines, although not established as the direct ancestors of the hominid sequence by palaeontological evidence, occupy an equivalent status when considered behaviourally as well as structurally. In Simpson's opinion (1963) "present evidence does not exclude, and may be taken to favour, the possibility that early *Australopithecus*, or an unknown genus close to it, was such a direct ancestor." And Hulse (1963, p. 187) writes: "They are the earliest members of our family yet to be discovered, and the least like *Homo sapiens*. As such, they are among the fossils of greatest significance to any student of human evolution, for they represent a stage through which our ancestors passed in the transition from being exceptionally clever animals to being human."

The major reason for the evolutionary position now assigned to *Australopithecus* is that the primary taxonomic criterion for the differentiation of the hominid line from the pongids is erect locomotion. It has primary significance because it marked the initiation of a new adaptive plateau, both structurally and behaviourally, by permitting "an entirely new system of selective factors to act upon the organism". Biegert (1963, p. 142) points out that: "The 'higher development' of the motor and tactile faculties of the hand, the loss of prehensibility of the feet, a reduction in size of the canines and accompanying alterations of the lower premolars and of the jaws, the additional expansion of the neopallium, the reduction in size of the masticatory apparatus (taken as a whole) and the accompanying changes in skull structure all are adaptations initiated by exposure to this new adaptive plateau. . . ." And, we may add, a terrestrial niche was opened up which, in terms of wider behavioural potentialities, involving novel ecological relations and subsistence activities, was not fully exploited until a later stage in hominid evolution. In this connexion, too, it is important to recognize that, although an upright posture became established in

Australopithecus, it was still incomplete, as detailed studies have shown (Biegert, 1963; Napier, 1963). It is not until we reach *Homo erectus* in the Middle Pleistocene, that we find the completion of this structural adaptation. By this time we have ascended another adaptive plateau. An expansion of the brain has occurred, about 1000 to 1400 cm.^{3} in less than a million years. This is one of the major features on which the generic separation between *Australopithecus* and *Homo* is based.

PHYLETIC INTERPRETATION OF HOMINID EVOLUTION

It is also possible now to discern a phyletic pattern which characterizes the hominid line of evolution, as compared with other known sequences. It has been pointed out by Dobzhansky (1963, p. 358) that anagenesis rather than cladogenesis predominated; that is, progressive evolution was maximal while splitting and branching of hominid forms was restricted. Both *Homo erectus* and *Homo sapiens* are polytypic species. The picture of gene flow that we must imagine is suggested by the absence, in Mid-Pleistocene times, of several reproductively isolated species or genera existing at the same time. If this had been so, *Homo sapiens sapiens* would have inherited its gene pool from one of these groups. On the contrary, it would appear that *Homo erectus* evolved into the polytypic species *sapiens*, thus carrying forward in time the genes present in Mid-Pleistocene and Upper Pleistocene species. If this hypothesis is accepted, the total framework of the biological dimension of human evolution is clarified and a consideration of concomitant sociological, cultural and psychological dimensions simplified.

It should also be noted that conceptually, contemporary evolutionary research in biology has replaced an "archetypal" interpretation by a "mosaic" one (Mayr, 1959, p. 5). That is, every character, or set of characters, does not evolve at the same rate; mixtures of advanced and primitive characters may be expected

in any phyletic line. *Australopithecus* remained anthropoid in some respects, but not in all. Brain expansion was not correlative with the set of characters that made upright locomotion possible. The large brain of the later hominids was not an initial differential feature of hominid evolution—an empirical fact for which there was formerly no palaeontological evidence.

The same mosaic principle, it seems to me, must be taken into account when we consider phenomena of a non-biological order in the total perspective of hominid evolution. We cannot project an archetypal pattern of culture, derived from the study of modern man, backward into the earliest stages of hominid evolution. If we do, some kind of macro-mutation is implied. Nor, in any consideration of the psychological development of the hominids, can we assume that a "human" nature or a "human" personality structure arose in this way. Archetypal concepts too easily become associated with saltatory hypotheses. Culturally and psychologically, as well as anatomically, such hypotheses are untenable. We can no longer speak of man, of culture, of language, or of a human personality as if they had leaped into existence; or, to use the figure of Howells (1950), refer to the appearance of man "as if he had suddenly been promoted from colonel to brigadier-general, and had a date of rank".

In this century, some social scientists, impressed with man's distinctiveness, have been more inclined than their biological colleagues to imply, or adopt, a macro-mutational view of man's cultural level of adaptation. Geertz (1964) goes so far as to say: "The reigning solution of the origin-of-culture problem has been what might be called the 'critical point' theory". Kroeber (1948), who used this term, postulated that "the development of the capacity for acquiring culture was a sudden, all-or-none, quantum leap type of occurrence in the phylogeny of the primates". This change, presumably in cortical structure, enabled an animal whose parents had not been competent, in Kroeber's words, "to communicate, to learn and to teach, to generalize

from the endless chain of discrete feelings and attitudes", to become competent. With him culture began and, once begun, set upon its own course so as to grow wholly independently of the further organic evolution of man. The whole evolution of the creation of modern man's capacity for producing and using culture was seen as a process of marginal quantitative change giving rise to radical qualitative difference. Geertz goes on to point out that it now appears that the Australopithecines had at least "an elementary form of culture", so that cultural development in some form was under way long before the level of the genus *Homo* was reached.

PROTOCULTURAL STAGE

Another way to put it is that *some* of the necessary, but not all of the sufficient, conditions for cultural development and psychological reorganization were present, not only in the earliest hominid phase, but even earlier. I have previously labelled this earlier grade of adaptation *protocultural* (Hallowell, 1961, 1963). The most important feature of the protocultural stage, exemplified by non-hominid primates and certainly by the earliest hominids, was the existence of social structures, or systems of social action varying in size and mating patterns but in which parents of both sexes were associated with their offspring. These structures were based on role differentiation which depended in part upon the socialization of individuals mediated by observational learning, some tutelage perhaps, and systems of communication both gestural and vocal. There was social transmission of some group habits, and perhaps *ad hoc* tool-using in some groups. It was a form of social organization in which learning was related to a system of social action and in which the transmission of social habits was possible. Regular or even predictable social relations existed and an animal had to discriminate in some way between individuals in order to play the roles which produced an organized system of

social action. In other words, in composition, structure, and behaviour, some of the basic conditions were present that are required for more elaborated forms of cultural adaptation to develop. Whatever the terminology used, some concept of gradation is necessary in dealing with the sociological, cultural and psychological dimensions of hominid evolution, to replace a saltatory hypothesis.

Terminology, however, is one of the inherent difficulties in dealing with these topics. While the nomenclature of biological taxonomy can express phyletic relationships in evolutionary rank, there is no equivalent terminology for handling cultural or psychological phenomena in the same way. This is why it now seems necessary to recognize that such familiar terms as culture, mind, human nature, language, tools and so on, intelligible as they may be when applied to *Homo sapiens sapiens*, have to be re-conceptualized if used in an evolutionary perspective where gradation and the mosaic principle must be taken into account. A simple equation such as man = language = culture = human nature, actually evades the evolutionary problem. If the mosaic principle be taken into account, the selection of any one feature of cultural adaptation as a reliable index of the presence of a cultural archetype and an associated level of psychological adjustment at an early period in the development of the hominids, is hazardous. Thus, if tools are taken as an index, are we referring to *ad hoc* tool using, or to a well developed tradition of tool manufacture? Was tool-making a saltatory event with no previous history? Can we assume that language, religious beliefs, moral values, the arts, and other aspects of culture were associated with *ad hoc* tool-using or only with a tool-making tradition? Does tool-making signify anticipatory behaviour, that is, conscious temporal orientation and planning for future events? (Napier, 1963; Hall, 1963.) The Australopithecines could hardly be expected to possess an archetypal pattern of culture fully equivalent in content and functioning to what we observe in *Homo sapiens*.

THE SOCIAL MATRIX OF BEHAVIOURAL EVOLUTION

Abandoning archetypal assumptions and the "critical point" hypothesis and adopting an inclusive frame of reference, a conceptual approach that gives full recognition to both the structural and behavioural dimensions of hominid evolution is possible if we give full weight to one basic fact: the unalterable social embeddedness of the hominids, their closest hominoid relatives, and their common ancestors. The primates have been continuously social animals, a categorical behavioural fact which, despite variations in group life, is as distinctive a feature of their basic mode of adaptation as are the common structural features that link them. Thus, whether we are dealing with anatomical or behavioural dimensions of the evolutionary process, events in both categories occurred in localized population units which were also systems of social action. The changes leading to the development of an erect posture occurred in population units of this kind as did the later expansions of the brain in the Pleistocene. It is well recognized by biologists that the unit of evolution is not the individual but a local population which is interbreeding. Consequently, whatever kinds of changes occurred in primate evolution must have had effects upon group living. And whatever sequences in gradation can be identified should be thought of with reference to modifications in the organization of social groups. Adaptation to a terrestrial niche on the part of the Australopithecines, territorialism and home range, and the development of tool-making evolved within organized group life; so did the social effects of the expansion of the brain, greater longevity, and extended infant dependency in relation to learning, communication, role differentiation, and the development of the necessary and sufficient conditions required for the functioning of a human personality structure. Although we may wish to deal with limited aspects of this multidimensional picture, the ecological, social, cultural, and psychological aspects of it were undoubtedly inter-

meshed. The actors in the population units involved were inevitably participants in systems of social action. Organized group living was the matrix of potential changes, just as the population units, considered as demes (the community of potentially interbreeding individuals) were the locus of genetic changes. We must consider hominid evolution as involving both. Dobzhansky (1962, pp. 18, 19) has asserted: "Human evolution cannot be understood as a purely biological process, nor can it be adequately described as a history of culture. It is the interaction of biology and culture. There exists a feedback between biological and cultural processes." Nor did man's capacity for cultural adaptation, he says, "appear all at once, complete and finished".

Cultural adaptation, then, in its most developed form in modern man, may be viewed as the culmination of processes rooted in the adaptation of prehominid gregarious primates who, in the course of hominid speciation and subsequent evolution, became specialized in their mode of social behaviour as well as their anatomical structure. This course of development involved selective pressures which induced changes in ecological relations, social organization, codes of communication and psychological organization. Among other things, a new level of adjustment on the part of the actors participating in systems of social action was achieved which was characterized by a novel form of functional integration.

LEARNED BEHAVIOUR AND CULTURE

For a long period it was customary to say that learned behaviour and its social transmission were the earmarks of culture as a distinctive human trait. Now the wheel has made a complete turn and biologists and others have discovered that among non-hominid primates and other animals, there is evidence for the social transmission of acquired habits. Thus, we now hear about the culture of animals other than man. While such a broad conceptualization is useful if we wish to emphasize the continuity between man and

other animals, it blurs the picture if we are concerned with behavioural evolution in the hominid line. Elsewhere I have pointed out (Hallowell, 1963) that the social transmission of acquired habits is more significantly conceived as a prerequisite of cultural adaptation as observed in modern man and an earmark of a protocultural behavioural plateau. Concepts of culture that lay primary emphasis on shared and socially transmitted behaviour without qualification, do not enable us to make a necessary distinction of degree between different levels of behavioural evolution in the hominids. Other capacities and conditions appear to have been required before the full-fledged level of cultural adaptation in *Homo sapiens* was realized. In fact, neither learning nor the socialization and transmission of learned habits seem to have reached an optimum level—in the sense of permeating every aspect of the operation of systems of social action—until the later stages of hominid evolution have been achieved. Furthermore, while the rudiments of an ability to be influenced by the behaviour of other individuals of their species may be characteristic of non-primates, phylogenetically it was only in the hominid line of the higher primates that capacities and conditions arose that led to the transcendence of more than a protocultural grade. An intermediate step may be related to the superior capacity possessed by some primates for observational learning (Hayes and Hayes, 1952; Munn, 1955; Hallowell, 1963). At the same time, when social transmission is dependent upon this capacity alone, there are inherent limitations imposed upon the cumulation of what is transmitted. Referring to the rudiments of cultural transmission among animals Dobzhansky (1955) points out that every generation learns the same thing which its parents have learned. In only a very few instances the evidence is conclusive that the learned behaviour can be modified or added to and that the modifications and additions are transmitted to subsequent generations. So long as social transmission was dependent on capacities for observational learning, the kind of acquired habits transmitted or any innovations

which could become significant in social groups were severely limited. On the other hand, it has sometimes been overlooked, as Margaret Mead has pointed out (Mead, 1958), that at the level represented by modern man there are non-cumulative as well as cumulative aspects of culture to be considered. This may well represent an important area of continuity with levels of earlier cultural development in the hominids. At the same time it should be recognized that the socio*cultural* systems of *Homo sapiens* could not operate through observational learning alone.

While it will be unnecessary to go into detail here, at least two other dimensions of the total evolutionary process require comment in relation to the essential factors that made it possible for the protocultural stage of the earliest hominids to be transcended. These involved (1) changes in systems of communication which must have radically transformed the operation of systems of social action, and (2) behavioural changes in the actors involved in these systems, consequent upon the release of new capacities associated with the expansion of the brain. Both of these are related to socially acquired forms of behaviour and the social transmission of experience. If we take a less categorical attitude towards the importance of learning as such, and consider it in relation to an advancing level of cultural adaptation, giving weight to its social context, correlative questions arise. One of these concerns the type of learning involved. The importance as well as the limitations of observational learning have already been mentioned. We also need to ask *what* was learned, how much, and under what conditions.

COMMUNICATION

It appears to be significant that the social transmission of a code is not characteristic of animal systems, whether we consider non-hominid primates or other animals. However, we have evidence, according to Hockett (1960, p. 89) that of the "basic features of design that can be present or absent in any communicative system,

whether it be a communicative system of humans, of animals or of machines", nine out of thirteen "were already present in the vocal-auditory communication of the protohominids—just the nine that are securely attested for the gibbons and humans of today. . . . The problem of the origin of speech, then, is that of trying to determine how such a system could have developed the four additional properties of displacement, productivity and full-blown traditional transmission" (and duality of patterning). The availability to individuals of a system of linguistic communication with novel properties that had never existed in combination before, released potentialities that can hardly be overestimated in relation to what and how much could now be learned and socially transmitted. Even fantasies, dreams, and other products of the imagination of individuals could be included in this latter process, since subjective experiences could be given utterance in meaningful symbolic forms.

In an evolutionary framework there was continuity in the basic functions served by systems of communication, but modification in their design. The new features added laid the groundwork for new functions. Some measure of the role it was now possible for systems of linguistic communication to play in the socialization process can be judged by the fact that population units of interbreeding individuals which were likewise systems of social action now became linguistic communities. It was necessary for the actors in such social systems to learn a linguistic code in order to participate in them. If this is recognized, there can be little dispute about language as a vital aspect of cultural adaptation at an advanced level and inseparable from it. On the other hand, if systems of communication among the earliest hominids are thought to have been of the same kind as those found in non-hominid primates, linguistic forms of communication cannot be considered integral with the protocultural grade of these hominids. While at an early hominid stage individuals were introduced at birth into a social community, they were not born into a linguistic

community. Thus one of the conditions necessary for the acquisition of a truly human status was not yet present. Yet this does not by any means preclude the transmission of other socialized habits, or even the use of simple tools at a protocultural level.

The second dimension that needs to be considered in hominid evolution is difficult to deal with briefly. It concerns changes occurring in the central nervous system which led to the exercise of new capacities. These capacities must be considered relevant to the development of systems of communication because we know that anthropoid apes cannot be taught a human linguistic code (Hayes, 1951). And, as Nissen (1951, p. 105) said long ago: "Experience will not make a man out of a monkey." The only point I wish to emphasize here is that the interpretation of selective pressures considered relevant to the expansion of the brain present a different picture if we bear in mind the social context of hominid behavioural evolution and the problem of cultural adaptation, than if we ignore these dimensions and consider the problem in organic isolation. Washburn (1959, pp. 28, 27) brings the former point of view into focus when, referring to the diagram in Penfield and Rasmussen (1950), which pictures the way the body is represented on the cortex, he calls attention to the fact that "different parts of the brain did not expand equally" and that "the areas which are largest are the ones of greatest functional importance. The area for hand increased vastly more than that for foot", a fact that "supports the idea that the increase in the size of the brain occurred after the use of tools, and that selection for more skilful tool-using resulted in changes in the proportions of the hand and of the parts of the brain controlling the hand". It is emphasized that: "our brains are not just enlarged, but the increase in size is directly related to tool use, speech and to increased memory and planning". Similar in basic pattern to the brain of other primates, the uniqueness of the human brain "lies in its larger size and in the particular areas which are enlarged. From the immediate point of view, this human brain makes culture

possible. But from the long-term evolutionary point of view, it is culture which creates the human brain". Whether this interpretation proves valid or not in all respects, the point of view enunciated is a far cry from the conceptualization of an anatomical evolution in isolation from social behaviour, or one in which a critical point —a Rubicon, defined by a brain size of 750 cubic centimetres—was once considered the crucial fact.

The same conjunctive frame of reference is of value when we consider the psychological restructuralization that the actors in systems of social relations must have undergone, as a consequence of the functional integration of new mental capacities and the rise of linguistic systems of communication. It is in these dimensions of hominid evolution that we find the realization of the behavioural potentialities in the neurological dimension. Among other things there seems no doubt that the role of cortical functions, operating as intervening variables, became increasingly important. Broadly speaking, these laid the foundation for increasing independence of conduct from determination by stimuli in the immediate sensory field, for delayed responses, for the development of ego functions and so on. Cortically localized areas concerned with verbal functioning were likewise involved. The transcendence of a protocultural grade was not solely dependent on organic changes, but involved a feedback affecting systems of communication and the cognitive functioning of individuals. For the acquisition of verbal tools through socialization in a linguistic community and the use of language in inter-individual communication were not the only factors of importance. While language, considered as a *code*, became socially transmitted, a linguistic system served a concomitant function; it facilitated intra-individual mental processes (Carroll, 1964), that is, conceptualization, thinking, and the choice of alternative courses of action. Such processes parallel the voluntary and conscious quality of the speech utterances of individuals. Just as the individual is the master of his own speech utterances, although their form is determined by an acquired

linguistic code, in the same way the cognitive processes of individuals are enhanced through the use of language. It is not implied that thinking and language are identical, quite the contrary. But "thinking" or some equivalent process, whatever it may be called, is restricted to narrower limits in the absence of the symbolic modes which a linguistic code provides.

NORMATIVE ORIENTATION AND PERSONALITY STRUCTURE

It was the exercise of new mental capacities, integrated with systems of symbolic reference, that enabled the hominids to transcend an earlier protocultural, non-linguistic stage in which observational learning predominated. Linguistic systems introduced new factors directly related to the operation of systems of social action as well as the mental processes of individuals. All the features of a protocultural grade were incorporated in a new level of sociopsychological integration—the level which is exemplified by the socio*cultural* systems of *Homo sapiens*. This more evolved stage is characterized by two distinctive and interrelated features: (1) a culturally constituted normative orientation, and (2) a psychological structuralization of the actors in these systems as *persons*. At this level it was not merely learning and the social transmission of acquired behaviour that was of paramount importance. What was involved was the psychological transformation of biological individuals at birth into autonomous persons through a socialization process that groomed them for active participation in a sociocultural system. Thus potentialities, rooted in both the genetic equipment of individuals and in the cumulative cultural heritage of a particular social group, could be realized through the novel level of psychological integration that we find in a human personality structure. It is only through the activities of persons that sociocultural systems can be perpetuated. Cultural adaptation, unless conceptually oversimplified and restrictively defined, involves the social and psychological adjust-

ment of persons. As Grace de Laguna (1963, p. 178), in her discussion of the meaning of person in a philosophical context, has observed: "Culture can be inherited only through becoming internalized in persons, the process of its inheritance is also the process of its *re-creation*" (italics mine). Whatever may be said to be culturally determined cannot be determined by culture without recognizing persons as intermediary agents. The social transmission of culture cannot be conceived, therefore, as self-perpetuating, autogenic, or simply as a transmission of acquired habits in a purely behaviouristic sense. Cultural tradition depends upon the functioning of persons, but not upon persons whose conduct is identical in all respects. Persons belonging to the same sociocultural systems may use the same linguistic code, but their vocal utterances differ, as does their thinking. Roles are traditionally defined, but they are played by persons performing individual acts that have idosyncratic components. Here we have the basic psychological fulcrum for creativity, cultural change, and variability in sociocultural systems, as well as continuity over time.

The normative orientation of sociocultural systems is related to this latter phenomenon and to the fact that persons are capable of volitional conduct, of making judgments, and choosing between possible courses of action. Traditionally recognized standards and values are characteristic of all sociocultural systems. Techniques are appraised as good or bad as are the manufactured objects themselves. Property rights are regulated according to recognized standards. Knowledge and beliefs are judged true or false, art forms and linguistic expressions are brought within the sphere of normative orientation, and conduct is evaluated in relation to ethical values. All cultures are infused with appraisals that involve cognitive, appreciative and moral values (Tolman, 1951; Kluckhohn, 1951). Relations between culturally constituted values, goals and the psychodynamics of the adjustment of persons are integrated with the role of traditional social sanctions in reinforcing the continuity of sociocultural systems considered as wholes.

Normatively oriented systems of social action do not operate through the social transmission of learned habits of avoidance (as in the choice of a mate) but rather through regularities that are the consequence of the choices of persons influenced by the traditional system of values with which they have identified themselves. Consequently, while some deviations from sanctioned conduct may be expected to occur, this does not interrupt the continuity of the traditional normative pattern.

Psychologically, a normatively oriented social order requires a capacity for self-objectification on the part of the individual actors. This makes possible self-identification over time, and an appraisal of one's own personal conduct and that of others in a common framework of socially transmitted and sanctioned values (Hallowell, 1955). Without the capacity for a psychological level of organization that permits the exercise of these and other functions, a social system could not operate at the level of normative orientation nor could moral responsibility for conduct exist. The relations between needs, motivations, socially recognized goals, and learning are more complex because cortical functions have become increasingly important. It is impossible to attribute an equivalent level of psychological functioning to the earliest hominids. What occurred in the psychological dimension of human evolution was the development of the necessary and sufficient conditions which laid the foundation for a human personality structure and the functional autonomy of persons. This required a distinctive integration of persisting interrelated mental functions that had multiple determinants: those derived from biological roots in phylogeny, socialization in systems of social action mediated by a linguistic code, and a culturally constituted normative orientation.

EGO FUNCTIONS AND SELF-AWARENESS

Ego functions played a central role in this psychological restructuralization. They became integral factors in determining

responses to the outer world in the interest of inner needs, particularly when delay or postponement of action, or choice, was required. They became intimately connected with such cognitive processes as perception, attention, thinking, and judgment; with mechanisms of defence against culturally unapproved impulses and with integrating and harmonizing functions and creativity.

Concomitantly with the exercise of ego functions the "inner world" of the human being could deepen, expand, and become socially significant as a factor in cultural adaptation. It has not always been sufficiently emphasized that *Homo sapiens* has been able to assimilate and integrate a wide range of subjective experiences in addition to the accumulation of pragmatic knowledge. David Beres (1960*b*), departing from the restricted sense of the term "imaginative" as the obverse of realistic, extends its psychological connotation to include "a *process* whose *products* are images, symbols, fantasies, dreams, ideas, thoughts and concepts". Imagination in this sense is a complex function entering into "all aspects of psychic activity—normal mentation, pathological processes, and artistic creativity" (Beres, 1960*a*, p. 253). His point is that "imagination is not opposed to reality, but has as one of its most important applications, adaptation to reality". In other words, reality can be best understood, not only as a relative indeterminate concept, but as one which is always infused with imaginative processes. Symbolic representations, derived from such processes, involve mediating ego functions between the external world and the inner drives of man. From this point of view dreams, fantasies, myths, art, and the world views of man, as articulated in cultural tradition, may be interpreted as making positive use of psychological resources in cultural and personal adjustment. Reliable knowledge of reality, in any scientific sense, cannot be assumed as a condition necessary for either biological adaptation or cultural adjustment to the necessary actualities of a human existence. Man has been able to survive and to make successful cultural adaptations in which his own imaginative

interpretations of the world have been fed back into his personal adjustments to it.

Self-awareness is co-ordinate with an advanced grade of cultural adaptation and is rooted in ego functions. The latter, as distinguished from id and super-ego functions in psychoanalytic structural theory, are postulated as a sub-system of a total personality in which super-ego functions are broadly related to the traditional values of a normative social order, and id functions to the biological energies and dynamics of the organism. Self, on the other hand, has a reflexive connotation, referring to one's own person. I can distinguish myself from other persons, I can conceive of myself as an object, I can develop attitudes towards myself. Self is a phenomenal datum, whereas ego is a psychological construct. Concepts of self, self-identification and self-objectification presume social interaction with other persons and a linguistic code. In all culture a self image is, in part, culturally constituted, as are other classes of objects in the phenomenal world. The way in which the self is conceived is important in relation to the premises for action which persons must assume in interaction with other selves and other classes of phenomena. Self-identification is culturally supported everywhere by the use of personal names. Through language and reflective thought self-related activities, both in the past and future, can be considered in the present. Self-objectification and -identification are required, of course, for the operation of kinship structures, as they are for an awareness of one's position in a spatial and temporal frame of reference. One of the universal features of language is a pronominal system that promotes self-objectification in self-other relations. As Grace de Laguna (1962, p. 175) has pointed out, becoming an object to one's self "carries with it the awareness of other persons not only as *objects*, but as *fellow-subjects*. An 'other' person is not only 'him' of whom I speak, but you *to* whom I speak and in turn an 'I' who speaks to me." As a consequence of self-objectification, sociocultural systems can function through the

commonly shared normative orientations of selves, in contrast to the systems of social action at an earlier hominid level where ego-centred processes, even if existing in rudimentary form, could not have become salient at the level of self awareness until subsequent evolutionary changes had taken place.

In the psychological dimension of evolution the development of ego functions played a primary role in the differentiation of the most advanced hominids. This phylogenetic aspect of human personality organization has been clearly recognized in psychoanalytic thinking by those who have been responsible for emphasizing the structural theory in contrast with the topographic theory of an earlier period (Arlow and Brenner, 1964). Hartmann (1964), for example, has emphasized the point that many functions which are taken care of by instincts in the lower animals are, in man, functions of the ego. He also points out that in man the id tendencies appear "to be far more estranged from reality than the so-called animal instincts are". It is also Hartmann who has developed the concept of a "conflict-free ego sphere" in human ontogenetic development; autonomous ego functions have to be recognized along with unconscious drives (Hartmann, 1958). Broadly speaking, the direction of ontogenetic development is towards a degree of personal autonomy that is related to normative values and the achievement of relative independence from erratic or unpredictable responses to impulsive demands This viewpoint reinforces other evidence which indicates the increasing importance of central cortical functions at more advanced evolutionary levels.

If ego functions came to play such an important role in the personality organization and cultural adaptation of the most advanced members of the genus *Homo*, there is ample evidence for identifying such functions as constant and vital factors of a "human nature". Reviewing this topic in its psychological dimensions a decade ago, Spiro (1954) concluded: "The structure and functioning of a human personality constitutes man's universal

human nature, psychologically viewed. Its universality is not only descriptively true, it is analytically true, as well. In the absence of human personality there could be no human culture". Human nature in this sense is an evolutionary product and could not have been characteristic of the early grades of hominid evolution. (For a statement of the problem in terms of semantic capacities see Wallace, 1961.) The "psychic unity of man", long assumed, implicitly or explicitly, as a foundation for significant comparisons of linguistic and cultural data, whether derived from literate or non-literate peoples, is given a sounder basis if we recognize its structural and dynamic foundations. To do so undermines the will-o'-the-wisp of a "primitive mind", associated with the idea of social and cultural progress and an evolutionary concept of "mind" within our own species. On the other hand, the limits of variability in the functioning of a generic personality structure under the conditions set by cultural change and the different patterns of sociocultural systems focus attention on important problems for investigation. In fact, cultural variability itself may be interpreted as a reflection of the adaptive potentialities of a human personality structure. Cultural variability has its limits, however, and personal dysfunctioning is an empirical fact in all sociocultural systems.

If we assume that a common personality structure constitutes the core of a human nature we have a common point of reference in psychiatry and anthropology to which variations in cultural and personal dimensions can be related. There is as sound a basis for the comparison of mental dysfunctioning, mental disorders, socially deviant behaviour, or whatever term is preferred, as there is for linguistic and cultural comparisons of all kinds. Even though symptoms may vary in form they are related to the experience and functioning of persons, to the phenomena of self-awareness in man, to underlying ego functions, and to conflicts between the demands of a culturally constituted, normative orientation and biologically rooted impulses.

Since the use of language, however different its codes may be, involves an ego function, the symptomatic importance of linguistic dysfunctioning whether in aphasia or schizophrenia is apparent. So is any kind of dysfunctioning that affects the orientation of the self in a spatial and temporal framework, whatever their traditional forms. For in every sociocultural system such a traditional frame of reference is basic. Without the maintenance of a known position in space and time a person is unprepared for action.

Since self-objectification involves self-appraisal in relation to moral conduct, we can see the social as well as the personal value of unconscious psychological processes such as repression, rationalization, and other mechanisms of ego defence in the adjustment of persons. Traditional systems of moral values inevitably impose a psychological burden since it is not always easy, at the level of self-awareness, to reconcile idiosyncratic impulses or needs with the demands imposed by the normative orientation of the self. Here we have a radical psychological gap between the advanced hominids, their ancestors of an earlier stage, and other animals. Feelings of guilt involve the assumption of self-awareness and some sense of responsibility in choice of conduct. A human personality structure involves affective responses to self-related acts. The content of what the self feels guilty about, or what acts arouse anxiety is, in part, one of the consequences of a normative orientation, associated with the operation of social sanctions as Freedman and Roe (1958, p. 461) write: "Only in man is there simultaneously such a rigidity of social channelling and such a degree of potential plasticity and flexibility for the individual. Incompatible aims and choices which are desirable but mutually exclusive are inevitable conditions of human development. This discrepancy between possibility and restriction, stimulation and interdiction, range and construction underlies that quantitatively unique characteristic of the human being: conflict".

In *Homo sapiens*, mechanisms of ego defence may be seen in evolutionary perspective as an adaptive means that permits some

measure of compromise between conflicting forces. For, at the human level, the capacity for self-objectification also means that the self of awareness becomes an object of primary value. Any kind of self-depreciation, loss of self-esteem, insecurity or threat to the self impairs the integral functions of the person and the performance of his roles in a sociocultural system. Ego defence mechanisms may promote self-deception and, in the amnesia of fugue states, the lost memories are those involving self-reference, concealing episodes that generated anxiety or guilt. On the positive side, the concept of ego strength, referring to the degree of integrative efficiency with which ego functions are able to operate without crippling restrictions, has emerged as a criterion of personal and social adjustment. A scale has even been devised using the concept of ego strength in the sense of "one's ability to cope with the problems of reality, to adequately and directly deal with problems which arise", in order to determine how "healthy" one society is, as compared with others (Allen, 1962). If ego strength and mental health are closely related, the way in which different sociocultural systems promote or retard ego strength offers an intriguing area for inquiry.

The emphasis given in recent years to ego functions in psychoanalytic structural theory, also accords with the picture we can reconstruct from other sources, of hominid evolution in its psychological dimension. It is not surprising, in this frame of reference, to note what appears to be a relation between the adaptive potentialities of ego functions in a human personality structure and their role in mental dysfunctioning. It is in some of the most severe and characteristic forms of mental dysfunctioning, for example, that we find symptoms that indicate a blurring in the boundaries between inner and outer worlds and disturbances in the ability to distinguish self from environment. Arlow and Brenner (1964), emphasize that; "What seems to be common to all psychotic patients is that various ego functions are disturbed as part of the defensive struggle against instinctual derivatives,

self-punitive trends, or both. If many ego functions are severely disturbed the individual's capacity to adapt to his environment in a socially acceptable way is almost sure to be compromised." At the end of their monograph these authors suggest that any psychopathological classification "must be formulated in terms of the patient's conflicts and, above all, in terms of the integrity or degree of disturbance of his various ego functions". This is what Karl Menninger (1964) offers in a recent book. Personality dysorganization and dyscontrol are scaled to the individual's need to maintain a "vital balance" in psychodynamic adjustment. It is assumed that one of the chief activities of ego functions involves *coping*, a key term in Menninger's discussion. At the level of mental health there is relative success in coping. At other levels, in a descending scale, ego functions become increasingly ineffectual in maintaining this vital balance.

CONCLUSION

Psychiatry and anthropology can approach man in complementary frames of reference. The psychiatrist is faced with dysfunctioning persons who occupy positions in ongoing sociocultural systems; the anthropologist is mainly concerned with the attributes and operation of these systems considered as organized wholes. If the psychiatrist recognizes the relevance of social and cultural variables, as well as organic ones, and the anthropologist recognizes the psychodynamics of persons as one of the necessary conditions for the operation of sociocultural systems, a common frame of reference is provided for the inclusion of the total range of human behaviour. A consideration of hominid evolution, moreover, provides the background which brings into focus, in historical depth, the multiple variables whose interrelations were vital in developing the necessary conditions of a human existence. In the earliest phases of hominid evolution, systems of social action became the locus of structural and behavioural changes that

conjointly affected both the dynamics of these systems and the actors in them. These changes included anatomical and neurological developments, the design of systems of communication, the quantitative ramification of what was learned and socially transmitted, and the paramount role that acquired systems of value came to play in conjunction with a personality organization in which ego functions and a sense of self-awareness were of central importance. Thus, organic, ecological, social, cultural, and psychological variables all need to be considered in relation to each other at all levels of behavioural evolution. The psychological re-structuralization that occurred made it possible for the actors in sociocultural systems both to acquire, and under some circumstances to transcend, their traditional cultural heritage. Potentialities existed for innovation, creativity, cultural reorganization, and change since the continuity of sociocultural systems through the socialization of the child required the constant recreation of the premises for action and value-oriented goals. But this process, in the nature of the case, could not be completely determinative nor was it a mechanical process. Among other variations, the dysfunctioning of the human personality could arise when the conditions necessary for the achievement of mental health were not present. The functioning of persons in the development of cultural adaptation, as well as their dysfunctioning, are both relevant to a comprehensive understanding of the nature of man in the perspective of hominid evolution.

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DISCUSSION

DeVos: I would like to relate Professor Hallowell's paper to what was said in the last discussion about social psychiatry. I interpret Professor Hallowell's meaning to be that, because of the universality of human nature, it is perhaps not necessary to assume that there is such a thing as transcultural psychiatry. I do think, however, that the special orientation in psychiatric thinking implied by such terms as transcultural psychiatry or social psychiatry does become important in this modern age as the degree of communication among different cultures increases. As much as we become conscious of cultural differences and as much as it becomes necessary for us to communicate with others living in other culture patterns, so much do we discover

within ourselves certain rigidities of behaviour that are of social concern. As cultures become more complex, and social and class mobility increases, previously unnoticed rigidities within individuals can hinder their adaptation to new situations. Transcultural communication changes the perspective of psychiatry within particular societies and psychiatry, therefore, becomes more social in orientation. It becomes both the study of social maladaptation and its amelioration, and the study of problems of internal intrapsychic maladjustment and their treatment.

We probably all agree that there are certain physiological or biological universals that can manifest themselves in forms of mental disorder in the same manner, whether they appear in Africa, Asia or America. When an individual or a society breaks down, the fissures that appear in human personality organization, given the universals of human nature, are structurally identical. Mental mechanisms such as those involved in the development of intellectual and affective controls, although influenced differentially by socialization practices found in particular cultures, depend upon certain physiological hereditary structures common to all *Homo sapiens*. Universals can therefore be found in intrapsychic adjustment.

The social stresses which affect social adaptation in the different cultures all differ greatly and there are cultural differences in the types of situation that give rise to inner feelings of distress in the individual. Therefore, social psychiatry becomes involved not only in universal patterns related to physiology but also in the understanding of particular patterns of adaptation related to social stress. The attention of modern psychiatry is not simply directed toward the investigation of such intrapsychic patterns as are found within psychotic breakdown, but it also relates itself to a study of the types of existential discomfort and potential "dis-ease" that are due to social tensions or psychic stresses caused by disruptive cultural patterns. The broad perspective of a transcultural psychiatry is needed to sort these out.

We now see, in situations of acculturation or social mobility brought on by modern communication, types of problems that do not occur in isolated cultures where individuals can live from birth to death with very little change. I think it is this factor of rapid cultural change that produces what we might call the need for social or transcultural psychiatry.

Fortes: I would like to begin by paying tribute to Professor Hallowell's impressive exposition, and then to try and correlate his and Professor Wittkower's papers. In emphasizing the similarities between cultures it is a pertinent although a rather paradoxical observation that up to the present no culture has been encountered in which something which we would call madness does not occur. I cannot say that there are no exceptions to this because we know very little about conditions among, say, Australian aborigines in their pristine state, but it is surely uniquely interesting that every society in its ordinary life apparently generates within itself this phenomenon. It is not only we who call this madness, but, as Professor Leighton made clear (see p. 23), every society everywhere does the same, and recognizes it as a part of life in the same way as they recognize physical illness. It is very interesting for me, as an anthropologist, to relate this to what Professor Hallowell has said. Paradoxically, one might ask oneself whether the phenomenon of madness is not itself an adaptive device in societies managing their internal affairs. Dürkheim has said that every society gets the crime it deserves, so that if we look at this problem in a natural-historical, positive way, schizophrenia might almost be regarded as a necessary sociobiological occurrence in societies. If we look at madness on this level we can see that transcultural psychiatry, like any transcultural study, is a rather roundabout device for getting at fundamental, theoretical generalizations by comparing variations in the cultural inventions and discoveries of humanity. In theory we could do without this help if we were able to make a study of schizophrenia in sufficient depth in any one, homogeneous, cultural setting. I think we might learn a great deal about the therapy of mental disorder if we asked ourselves in what sense we can regard psychosis, or any form of psychological disturbance, as mental ill-health. The term is one that we, the outside observers, use, and we know from anthropological discoveries that mental ill-health is very often canalized or exploited for apparently functionally useful, social purposes. What can we discover by looking at mental ill-health and madness as if they were necessary concomitants of social and cultural evolution, and what is the most appropriate way to do this? Perhaps, as Professor Hallowell has suggested, rather than look at these phenomena in isolation, we should study them in the context of the significance of intergenera-

tional relations, the processes of cultural transmission and even beyond these in the total texture of social relations in a community, in order to find an answer to our problems.

One word that Professor Hallowell did not use at all in his analysis was the word "relationship"; for anthropologists, this is a crucial concept. The work of the Harlows with monkeys (Harlow, H. F., and Harlow, M. [11 February 1965]. *The Listener*, review) is largely concerned with problems of relating. These workers have uncovered possible connexions, even at the evolutionary level of the sub-human primates, between psychosis and the nuclear, elemental relationships as, for example, between mother and child, sibling and sibling and so on, and this relates to what Professor Hallowell calls the normative orientations. I am intrigued by this because I agree with him about the way that society and culture—in general terms Man—has evolved from the outset in interbreeding populations, that is to say, in societies. A crucial factor here is the phenomenon of the rule, for until there are rules there is no society. Rules of behaviour are cultural norms, but the observance of rule by individuals is the connecting link between culture and personality.

The question of the "self" and the "person" is another very important one. I believe that every society develops a culturally defined notion of the self, that is, a consciousness in cultural terms, in its world-view, of the self. I think we could also learn something about the problems to which I have referred by approaching them from this existentialist point of view, and thinking about the relationship between self and society.

Mead: Professor Hallowell has raised so many issues that it will only be possible to discuss a few of them. In trying to understand the dynamics of human personality a better understanding of evolution, for example, such phenomena as incest and incest taboos and the early formation of preferences, heterosexual, homosexual and autosexual, will be very useful.

Other possibilities for increasing our understanding now flow from the comparative work that has been done on animal psychology. The recognition of the role of behaviour in evolutionary change is very, very new—the first symposium on behaviour and evolution was only in 1955 (Rowe, A., and Simpson, C. G., [ed.] [1958]. Behavior

and Evolution. New Haven: Yale University Press)—and most of the very interesting new experimental work in which we have found animals and birds acting in a sense as persons has resulted from studying them in conditions of domestication, that is in systems. Animal behaviour in these studies can be usefully compared with man's behaviour if one thinks of man as a self-domesticated animal. Even if these studies are rather limited, for example, observations on the Japanese monkey, they are nevertheless sufficiently bounded for certain kinds of interaction to occur (Frisch, J. E. [1959]. *Amer. Anthrop.*, **61**, 584; Imanishi, K. [1960]. *Curr. Anthrop.*, **1**, 393; Itani, J. [1958]. *Primates*, **1**, 84; Simonds, P. E. [1962]. *Curr. Anthrop.*, **3**, 303). In K. Lorenz's experiments (1959. *In* Group Processes, Transactions of the 4th Conference, p. 181, ed. Schaffner, B. New York: Josiah Macey Jr., Foundation) when, at our suggestion, psychiatric case histories were recorded on geese, the geese were found to have not only personalities, but, particularly under conditions of overcrowding, very maladapted personalities. So we have here a whole spectrum of possible evolutionary features that are important to psychiatry because they are crucial points of maladaptation in society.

There are also a large number of structural features which are relevant here, such as the hymen and the change in oestrus in human beings at the menopause (Mead, M. [1961]. *In* Sex and Internal Secretions, p. 1433, 3rd edn., ed. Young, W. C. Baltimore: Williams and Wilkins). The background of the oedipal situation itself can probably be carried back to a period when there may have been far less discrepancy in size between the six-year-old male, and his father or the males in control of the community (Mead, M. [1963]. *Bull. Menninger Clin.*, **27**, 185). Evelyn Hutchinson has indicated this (1959. *Amer. Nat.*, **93**, 81) by suggesting that there are biologically regular sequences of development and interrelations between parents and children, and that these can be violated by a variety of cultural arrangements including modern hospitalization and obstetrics. These factors, as they occur in different forms in different individuals, may account for much of the maladaptation that we find. The best way to study cross-cultural psychiatry would be through an understanding of these principles. This approach in no way contravenes the notion of the psychic unity of mankind or of the universal solidarity of human

nature within one species, but it gives us an evolutionary base for discussing a large number of maladaptations which are central to psychiatry. I think it is important to realize that when we study cross-cultural psychiatry we find that many of these crucial situations, the so-called nuclear and elemental situations, recur over and over again within every society.

Finally, I think that we will be greatly helped in our understanding of the types of thought that have been called the primary process, if we use this evolutionary approach (Mead, M. [1952]. *In* Dynamic Psychiatry, p. 401, ed. Alexander, F., and Ross, H. Chicago: University of Chicago Press; Mead, M. [1964]. Continuities in Evolution. New Haven: Yale University Press).

Caudill: If we look at this the other way round I think that these elemental situations, for instance the relationship between mother and child which is found in any society, provide for us the ground from which we can make comparative studies. There is such a tremendous variability in the way in which the relationship between mother and child is worked out in different cultures (or within a single culture) that this in itself justifies a cross-cultural approach in which the patterning of variability between cultures can be compared.

Mead: I agree with this, and I would not agree with Professor Fortes when he says that cross-cultural comparisons are clumsy. They are very fine tools provided by history and I would say rather that Professor Harlow's experiments (Harlow, H. F. [1963]. *In* Expressions of the Emotions in Man, p. 254, ed. Knapp, P. H. New York: International Universities Press) were clumsy.

I think that we can do more with genuine human situations than we can with animal experimental analogues when we study variations in interpersonal behaviour.

Murphy: The literature on transcultural psychiatry has exceedingly little contact with genetic psychiatry and the whole group of European schools which emphasizes the genetic, biological side of the subject. There is today renewed interest in the concept of race, without its adverse connotations in the social field in connexion with gene pools. We are recognizing, with the analysis of blood groups and other genetic variables, that there are associations between cultures and the distribution of genetic characters. Professor Hallowell, I would like

to ask you, in this context, about the interaction of social and biological evolution in the past. You spoke about the ego functions, and I wonder whether you think that social development may affect biological development through the selection of genetic variables which favour ego development and the strengthening of certain aspects of the ego. I think that there should be a bridge between the common attitude of the social psychiatrist towards mental disorder and the attitude of those psychiatrists who emphasize genetic factors, and I wonder whether what you said offers the basis for such a bridge.

Hallowell: I can only say that in my paper and in my own thinking, since I am not a biologist, I have used the word *identification* to try to indicate some of the essential variables, and to visualize what might happen if we think about these in terms of general interrelationships. This is rather a crude model, but the investigation of the relationship between deterministic factors and what actually happens is certainly a fascinating subject. I simply do not know the answer to your question. I wish you would tell me how you think the subject could be illuminated.

Mead: I do not think there is any work on genetics yet that could help with this problem, although Professor C. H. Waddington's research (1961. The Ethical Animal. New York: Atheneum) has provided a kind of bridge between social psychiatrists and geneticists. Waddington considers the possibility that the human ability to accept an authoritative statement about right and wrong, absolutely regardless of content, may have been an essential evolutionary feature and so can be regarded as a component of our evolutionarily developed human nature. It is possible that, at a particular period in human history, some hominids may have developed this capacity more fully than others and so it became a feature of a gene pool. It is more likely, I think, that this is a pervasive character rather than a single-gene-carried one. I think that a geneticist, using anthropological and psychological data, could usefully construct a hypothesis along these lines and this could then be examined scientifically. We might go further than this and postulate a population with ego weakness of some sort as a genetic feature. Another anti-evolutionary element might be contained in a population with greater dependence on auditory than on visual imagery (Mead, M. [1954]. *Psychiatry*, **17**,

303). This auditory dependence would be a disadvantage to the race as it would lead to greater credulity, because auditory cues are not scrutinized as carefully as are visual ones. Other examples of ego weakening characteristics which might be part of a gene pool might be a population with increased dependence upon skin contact; or a people like the pygmies who, as far as I know, are the only people who show paroxysmal laughter and fall to the ground when they laugh. This phenomenon, which has been described by Colin Turnbull (1962. The Forest People. New York: Doubleday), is exceedingly important to the society and permeates its music and communication. A characteristic of this sort might be associated with an isolated group that share in a single gene pool and might result in a variation in ego strength. I think it is in areas like these that genetic studies will be useful.

Lewis: Is this condition comparable to the physical syndrome, narcolepsy, in which people fall to the ground when they laugh? The symptom is called cataplexy and is well known.

Mead: No.

Fortes: I would like to emphasize something that I think Professor Hallowell implied. One important contribution which cross-cultural studies do make in this field concerns the notion of reality. The idea or vision of what is real and what is not real is a cultural product, and my own experience of the mentally abnormal in different cultures suggests that a great deal seems to hinge on just how these sick people, in contrast to ourselves and in comparison with other normal members of their society, see what is real. I think this is an evolutionary problem of great importance in transcultural studies.

Transcultural Psychiatry
A.V.S. Deuck & Ruth Porter

SCHIZOPHRENIC AND BORDERLINE STATES

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MACCURDY's (1939) observation that the nature of men is identical but what divides them are their customs, is valid even at the level of psychotic regression. Lewis (1949, p.5) has also observed that the "life experiences of persons from birth until death are on the whole rather similar in general pattern, that is, the event of birth, attachment to the parents, infantile dependencies, repressions created by the necessity of conforming to a particular culture, the problems of adolescence, heterosexual adjustment, the trials and responsibilities of adult life, and the regressive and compensatory reactions to frustrations and defeats are universally similar in their expressions."

With these opening remarks emphasizing an appreciable degree of uniformity in human response I should now like to discuss in detail some of our findings, and the findings of others, on cross-cultural variations in psychiatric symptomatology in schizophrenic and borderline states.

Schizophrenia and its clinical variants constitute the most common group of psychoses occurring in the African, irrespective of social and cultural backgrounds. Carother's (1953) observation that "paranoia, paraphrenia, and even paranoid schizophrenia are relatively rare in Africans" is inaccurate. This may be due to semantic and other difficulties inherent in the culture.

In spite of fundamental similarities which have been noticed in many cases, differences in the symptom-complex which may be attributable to cultural factors have also been recorded. Tooth (1950, p. 48) writes: "While it is likely that the clinical picture of

schizophrenic reactions may also be modified by the environmental and cultural background of the patient, all the varieties of schizophrenia commonly described in Europeans may be seen in Africans. But, whereas in Europeans, the distinction between an affective state with schizophrenic features, and a depressive phase in a primary schizophrenic psychosis, is a common stumbling block in differential diagnosis, in Africans, schizophrenia is more liable to be confused with one of the organic psychoses. Among the 'bush' peoples, a typically schizophrenic picture is most likely to be due to organic illness, while schizophrenia itself appears as an amorphous, endogenous psychosis. But the schizophrenic psychoses occurring in the urban, literate section of the population show more nearly the same forms as are found in Europeans."

Lambo (1955, p.250), writing on "the Role of Cultural Factors in Paranoid Psychosis among the Yoruba Tribe", observes: "Paranoid psychosis in the non-literate (rural) tribal group manifests some features which are apt to be indefinite, transitory and multiphasic. In some of these patients, this mental disorder genetically resembles a hypothetical mental disease lying midway between psychotic and psychoneurotic illnesses in Euro-American cultures". He continues: "Clinically, it is essentially atypical in its form, when compared with that of the literate tribal group, and, in the majority of cases, it closely simulates the picture of organic confusional psychosis that is usually found in Europeans and Westernized Africans. In most cases, a peculiar complex of symptoms (anxiety, repeated confusional attacks, absences, automatism) may be prominent, though transitory."

Laubscher (1937) has also observed that among African schizophrenics "the picture of mental confusion stands out clearly above any other syndrome".

Since 1955 when two groups of paranoid schizophrenics, literate and non-literate, were intensively studied, over two hundred patients (one hundred and thirty literates) suffering from all subtypes of schizophrenia have again been studied clinically,

The differences in the patterns of clinical response between the two subcultural groups, with both quality and quantity of the symptom complex as determining factors, were found to be due to certain features of the psychosis. In order to group together these characteristics for descriptive purposes, we have adopted Birnbaum's terminology. Karl Birnbaum (1931), in his *Strukturanalyse*, coined the word "pathoplastic" for those factors which contribute to the clinical picture in individual cases or groups of cases and which are attributable to cultural and other influences tending to modify the basic manifestations of mental disorders.

The symptom-complex which has been observed by us consists of anxiety state (pseudo-running amok), neurotic depression, vague hypochondriacal symptoms, magico-mystical (bizarre) projection symptoms, episodic twilight or confusional states, atypical depersonalization phenomenon, emotional lability and retrospective falsification of hallucinatory experiences. These features are usually present in varying degrees and in various combination in the acute phase of schizophrenia and its allied disorders. In the early stage of the course of the illness these pathoplastic factors, which are cultural precipitates, tend to be multiple especially in the older age group, but as the illness progresses the quantity is reduced.

Some of these features are paroxysmal in the acute phase, and assume an episodic nature as the patient settles down. On the basis of these qualities it is possible to differentiate sharply between the two groups of psychotic patterns of reaction. The variation in psychiatric symptomatology would seem minimal in the literate, sophisticated African whose pattern of illness approximates to the pattern which has often been described in Western Europe.

EFFECT OF CULTURE ON SYMPTOMATOLOGY

Individual symptoms which are frequently influenced by cultural factors include the following:

(A) Delusions

The form and content of delusions in the non-literate African tend to be culture-bound, but this is not entirely confined to this group. Schizophrenic patients from westernized homes, such as African students in Great Britain, may show the same phenomenon.

Delusions of persecution figure prominently in both groups. Delusions of grandeur are rare in the non-literate group in contrast to the expansive ideas of noble birth, with continual self over-valuation and extravagant themes, which one frequently encounters in the urban or literate group.

In the rural non-literate Africans in this tribe, the delusional contents are often centred around the concepts of supernaturalism and ancestral cults, while in the literate African, hypochondriacal delusions and bizarre somatic complaints, especially in the early stages, seem to dominate the picture.

This form of schizophrenia with "metaphysical delusions" in the rural African has presented us with some difficulty. Jaspers (1962) writes: "Patients may display their delusions in some supra-natural mode and such experiences cannot be adjudged true or untrue, correct or false. Even when empirical reality is concerned it is difficult enough to be decisive, though some evaluation can usually be made. We can study the metaphysical experience in its schizophrenic manifestations as it is conditioned by the morbid process and yet realize that the metaphysical intuitions (the images themselves, the symbols) that have arisen in the course of these experiences have acquired cultural significance in the minds of normal people for quite different reasons."

On the whole, in both groups, the ways the delusions are cast throw some light on the cultural interpretation of nature in the environment. One of the most important findings here is the readiness with which most of the members of the literate group, even most fervent Christians, regress in their delusions to super-naturalism or magico-mystical influences. This regression is usually transient and often the delusions in the primitive group,

cast within the framework of supernaturalism, lack the tenacity and conviction that are often encountered in similar delusions of the westernized group. The same observation has been made among the American Negroes (Acherman, 1938).

The rarity of delusions of grandeur is characteristic and this is of cultural origin. Bleuler (1924) has observed that "certain ideas of persecution and grandeur are only possible in definite strivings and in definite characterological relations to the environment". Most African cultures demand total allegiance to ancestral cults and nether world gods, and it is not unlikely that this submissive attitude influences their choice of delusion.

Due to the influence of pathoplastic factors, especially episodic confusional states and excitement, systematization is usually vague and rudimentary in most of the non-literate (rural) cases. In many of these patients, however, delusions are capable of elaboration.

(B) Hallucinations

The observations made on the form and content of delusion are equally true of hallucinations in the non-literate group. There are, however, one or two features which are of importance clinically: hallucinations tend to be transitory, predominantly auditory and ill-defined.

Temporal, spatial and causal relations are meanings we read into sense-impressions we receive. There are, however, many other forms of meaning (meanings which are culturally determined) which, in similar fashion, we read into or add to different perceptions. In most African indigenous cultures one is never struck by the conventional meaning of a word, but, rather, great emphasis is laid on the whole complex meaning of the situation, that is the complex of emotional relations between man and man. Verbalization is the most predominant form of communication of the non-literate African, hence the cultural emphasis on auditory sense-impressions and their symbolization.

The other important feature is the ability for retrospective

falsification of hallucinatory experiences. This was present in about forty per cent of the rural cases, and in about ten per cent of the cases it was associated with confusional states.

(C) *Episodic Confusional States, Incoherence, and Emotional Lability*

So far as cross-cultural variations in psychiatric symptomatology are concerned, confusion states of varying degrees seem to emerge as a prominent pseudo-psychotic-like symptom. Episodic confusion states have also been observed in many psychiatric disorders without psychosis in Africans, though they feature with monotonous frequency in schizophrenia and allied disorders. One or more of these symptoms is invariably present, and it is this constant feature of the clinical picture which distinguishes schizophrenic psychosis in the non-literate Yoruba tribe from that occurring in the literate tribesman.

It is not uncommon for marked emotional fluctuations in normal and diseased persons in any culture to lead to affective confusion at some time or other. This, however, does not usually assume a definite pattern. During the twilight state which accompanies this psychosis in the non-literate African, there is a much more profound confusion of thought process, disturbance of association and falsification of the situation by illusions, hallucinations and depersonalization phenomena than in the literate African. The most constant content of this state is that of paranoid anxiety.

This clinical feature has also been noticed by Shelley and Watson (1936) and Carothers (1947), in different parts of Africa with similar cultures.

Because of the African's cultural environment, intellectual and affective factors are closely interwoven (this is a form of autoplastic adaptation), but the affective sector predominates and it dominates his life; his interpretation of reality is not its relation to its temporal environment but the relation of men with other men, and of all men with the supernatural. This seems to be the most

important, single, cultural factor which conditions the evolution of varying degrees of affective outbursts associated with the initial phase of schizophrenia.

If one attempts to relate this syndrome, consisting of extreme affective disinhibition associated with clouding of consciousness and incoherence of thinking, to syndromes known in clinical psychiatry, one will have to think of hysterical and epileptic manifestations, but in none of the cases observed has there been any evidence of epilepsy.

Bleuler (1924) was one of the earliest authors to notice the clinical significance of "psychically determined excitements, or twilight states" in schizophrenia. He further observed that they may not be "directly connected with the process of the disease, but are only transient reactions of the diseased psyche to certain stimuli".

(D) Anxiety State

This condition may be an important component of, or an underlying factor in, the confusional or twilight state described above, or it may exist alone when it is usually seen as a precursor of the psychosis. Anxiety state as a concomitant of some other psychiatric condition may occur in any culture, but the form it takes is culturally determined.

In non-literate Africans anxiety is almost invariably interpreted as the result of a bewitchment which constitutes a threat to life. Thus psychogenic foci often excite activity of a kind. In a small proportion of cases, this anxiety state has only a taint of hypochondriacal features. In the literate group, on the contrary, the anxiety state (which usually but not always precedes the psychosis) shows very marked hypochondrial features, while the "action" component (that is, frenzied fury and homicidal tendencies) is conspicuously absent.

In both groups the anxiety may persist in the acute stage, but is usually absent when the psychosis becomes fully established in the

non-literate group, except when it is present as a component part of the twilight state. In the episodic twilight state or recurrent confusional state that dominates the clinical picture of the psychosis in the rural tribesman, anxiety is likewise recurrent or episodic.

(*E*) *Hysterical Conversion Symptoms*

The early appearance in some patients (less than 20 per cent in our recent study of 70 cases) of hysterical conversion symptoms has been found.

Thus the symptomatology of schizophrenic psychosis in the non-literate Yoruba is ill-defined, punctuated with multiple pathoplastic features and, on the whole, the picture of mental confusion is the most prominent feature. Hence its form, especially in the early phase, is essentially amorphous.

It should be stressed that these pathoplastic factors exist only in addition to one or more of the basic symptoms which are varying aspects of the fundamental schizophrenic disorder, common to both groups. These may include thought disorder, passivity feelings, affective disturbances and disturbed association of ideas. In the early stages of the psychotic process in the non-literate group psychoneurotic-like symptoms of an ill-assorted nature are also present.

SPECIFIC DIAGNOSTIC FEATURES

(*A*) *Diagnosis*

The diagnosis of schizophrenia and borderline states (paranoid schizophrenia, paraphrenia or any intermediate clinical entity) in the literate Yoruba tribe does not present any difficulty arising from the cultural aspect. This is equally true of the non-literate group when the disease has become well-established. This study presents no evidence that these psychoses (within the accepted diagnostic nomenclature) among the literate, urban Yorubas are

significantly different, from the point of view of aetiology, diagnosis, psychotic manifestations or prognosis, from schizophrenic psychoses in Western cultures. On the other hand, the tendency to diagnose schizophrenia and its clinical variants only when the typically "schizophrenic" or classical symptoms have become well-established excludes many patients of the type I have been describing in whom the manifestations are more indefinite, polymorphous and masked or are not recognized because of the presence of pathoplastic features. In African countries this may constitute a large group of patients and not just an insignificant category of exceptional or atypical cases. At Aro hospital and the University of Ibadan Psychiatric Unit we have studied hundreds of such cases during the past ten years and have evaluated our earlier impressions by follow-up studies on the patients; follow-up studies continue at the present time.

If the Westernized African has innate psychological qualities which differ from those of Europeans, we would expect him to react differently to the same environment or similar traumatic stress, because of a different constitutional pattern. This hypothesis is not supported by the observations in this work. Morris' (1951) observation lent indirect support to our findings. He found a similarity of constitutional factors in psychotic behaviour in India, China and the United States. Clinical observations in this work have shown that the pattern of schizophrenic disorder in Westernized Africans is similar to that which is usually encountered in Europe.

Our clinical work in Nigeria shows that the diagnostic criteria and assessment of prognostic possibilities which hold in Western culture are equally applicable to the Nigerian patients who have been in contact with this culture. However, this does not seem to hold for patients whose social and cultural background is as different from a Western background as that of the non-literate African. In this group schizophrenic symptomatology in the main shows a considerable degree of diversity, polymorphism and affective swings.

It is of great clinical importance to mention that the basic symptomatology is similar in our two groups and, therefore, similar to that which is commonly described in Europeans. The degree of variation in the basic symptomatology in individual cases in the two Nigerian groups is not unlike the differences which exist between individual cases in Western culture. Apart from this basic similarity the pathogenic factors are essentially heterogeneous and this is probably due to cultural variations.

The most important single factor in connexion with the diagnosis of schizophrenia and allied disorders in the non-literate African is the variation in its phases, which in our opinion is attributable to the admixture of its symptomatology. The frequency with which these heterogeneous categories of symptoms occurs remains to be determined.

The most important diagnostic features will be discussed under two headings:

Mental symptomatology of the initial stage (acute phase) of the psychosis and its diagnostic significance: During this phase, frank clinical expression of the underlying disorder is vague and unimpressive due to an overlay of psychoneurotic phenomena. It is possible that in this phase the affective sector may be so active that the basic feature of the illness is completely overshadowed by mental confusion and projection symptoms. Therefore the picture of mental confusion invariably dominates the acute stage of the disease.

The psychodynamic function of mental confusion in certain acute, psychotic states appears to be that of defence; confusion keeps out of awareness various affects which are conceived as intolerable to the ego and as disrupting the interpersonal relations which concurrently exist. Rosenfeld (1950) has expressed the view that in many instances, particularly in acute psychoses, the ego is threatened with the sudden access into awareness of an overwhelmingly great intensity of affect, so that in addition to a delusional defence, mental confusion is immediately erected to

reinforce the primary delusional projection. The essential feature of the acute stage, therefore, is the screening of psychotic reactions by psychoneurotic symptoms, a mental mechanism manifestly brought into play to prevent a break with reality.

Lewis (1949) has stated that the differentiation of the individual from his social environment and "the relationship of the person to the herd is an evolutional process which has developed late". While this state has been achieved by Western culture, the primitive African largely identifies himself with his social environment.

The diagnostic importance of pathoplastic factors: The symptom-complex referred to above represents the patient's elementary psychical responses, in his native environment, to external stimuli. The reality underlying the appearance of the symptoms is an activity similar to that which we know exists in psychiatric disorder complicated by physical illness such as diabetes and tuberculosis.

The cultural origin is supported by the following observations: (i) these ill-defined symptoms are present in varying degrees and severity in other psychoses in the non-literate population, with a greater predilection for the schizophrenic group as a whole, especially those in which psychogenic factors are easily demonstrable. (ii) they may disappear at any time and leave no trace in the further process of the underlying (primary) disorder. (iii) they are not uncommon in some Westernized patients who invariably show more permanent regression to tribal beliefs and ideological concepts.

While most of these pathoplastic factors are apparently of psychoneurotic origin, they have no distinct features in themselves, that is, they are in themselves atypical. For example, the type of anxiety state that may be associated with or precede the psychotic manifestation in the non-literate group is multiphasic. It usually starts in the classical form, assumes a crescendo and may terminate in a hysterical hypomanic state (state of frenzy, pseudo-running amok) during which the patient is often dangerous.

Thus these contributory factors to the basic manifestations of schizophrenic psychosis are in themselves sources of clinical confusion.

The diagnosis of schizophrenia and borderline states in this tribal environment should, therefore, take into consideration the important clinical finding that these diseases include a heterogeneous group of symptoms which nevertheless have a precise relation to the cultural traits of Nigerian personality structure.

(*B*) *Differentiation from Organic Disease*

It is important to point out here that when a schizophrenic psychosis in the rural tribesman is precipitated by organic illness, such as cerebral trypanosomiasis, the expression of bizarre symptoms is gradual and disturbances in association of ideas and affectivity become prominent. The pathoplastic features are not evident. In other words, when a patient with schizophrenia from the non-literate group shows a cluster of symptoms which, in Europeans or in the Westernized African, would be described as typically schizophrenic, trypanosomiasis or other organic disease should be looked for. This had also been observed in the Gold Coast by Tooth (1950) when he said, "Among the 'bush' peoples, a typically schizophrenic picture is most likely to be due to organic illness". In our experience the organic states are usually of a chronic nature.

(*C*) *The Diagnostic Significance of the Basic Psychotic Manifestations*

From what has been observed among our Nigerian patients it would be unwise to attempt to differentiate too rigidly (in terms of diagnostic categories) between the true schizophrenias and the borderline states. Bleuler (1924), for instance, did not think that the diagnostic differentiation of, for example, the paraphrenias from the other acute or chronic paranoid forms was ever possible. This is in contrast to Kraepelin's view. Bleuler also believed that most of these cases bear some genetic relationship to the schizophrenias.

While we can agree with Lewis (1949) that "it is quite difficult in the light of our present knowledge, to consider schizophrenia as an entity having any definite form, function or special characteristic behaviour", the basic clinical picture in the great majority of these cases seems to be related to the schizophrenias as they are understood today.

When this group of psychosis in the non-literate Nigerian is devoid of its pathoplastic traits, one is confronted with a clinical picture, which is comparable to that of the Westernized group, and between which it is almost impossible to draw any hard and fast lines of symptomatological differentiation.

SUMMARY

Our clinical experience of cross-cultural variations in the symptomatology of schizophrenic and borderline states has shown that sociocultural factors, when they are powerful, provoke secondary elaborations in the symptomatology. These elaborations appear through simultaneous psychic change and take the form of certain "symptom clusters" which have a well-defined place in psychopathology.

The grouping of such symptoms is predominantly, but not exclusively, encountered in rural and non-literate Africans in whom, at least in the initial phase, the entire character and form of this disorder may be affected—the pathoplastic influence. The development of these variations in symptoms seems to be the result of interaction of the patient with his environment. These secondary elaborations show some characteristic modifications within the true schizophrenic group because the schizophrenic disease process may overwhelm them selectively or completely as they appear.

In the borderline states, however, the primary psychotic symptoms fail to reach a diagnostic threshold or are qualitatively reduced.

Finally, I should like to stress the need for precise classification, based on detailed clinical studies in cultures outside the West, of this group of disorders.

DISCUSSION*

Lewis: Professor Lambo, you have emphasized the pathoplastic influence of culture on mental disease, but culture can also operate by favouring the overt manifestation of a previously latent mental disorder. Could you tell us if you have any dependable data for Nigeria which show the proportion of persons manifesting schizophrenia per thousand of the population, compared with this figure in other cultures?

Lambo: I personally have no data on this subject. The only information that we have at present comes from the work which Professor Leighton and his team did in Nigeria (Leighton, A. H., *et al.* [1963]. Psychiatric Disorder among the Yoruba. Ithaca, New York: Cornell University Press). I would be extremely cautious in using this data because his study is confined to a fairly homogeneous section of the population. We do have mental hospital statistics and although these are not particularly reliable they seem to show that the age trends of schizophrenia as a whole are comparable with those in other countries; we see the disease mostly in young adults. The patients are usually fairly sophisticated, but this is probably due to social selection and not an absolute feature of the disease.

Caudill: Professor Lambo, you mentioned that some of your schizophrenic patients showed affective symptoms, for example, an hysterical overlay, which you thought were due to cultural elements. Can you tell us in what way you think these symptom-patterns may relate specifically to the culture?

Lambo: We have not really indulged in psycho-analytical interpretations. I use my past experience of schizophrenia in the West as a control, and compare the symptoms in western patients and in non-literate, rural Africans. On the whole, the picture in the rural African is much more florid than in the western group. When we look at the literate, urban African, whose mental life is much more sophisticated and whose attitudes and social background are comparable to those

* The bibliographical reference to Professor Lambo's paper are printed on p. 384.

of the middle-class Londoner, we find that his symptoms are less florid than the symptoms of the African from the village. When we examine a specific symptom, such as aggression, we find that aggressive outbursts are a characteristic feature of both groups. However, in the urban African, aggression is internalized or passively expressed whereas in the rural African it is fully expressed. We can extend this further and say that in Nigeria homicide, for example, is much more common in the rural areas than in the towns. In the rural areas, powerful cultural factors engender suspicion, ideas of reference and, in some cases, persecution—witchcraft and so on. These culture-bound phenomena do not, however, constitute psychosis, although in the excited person such feelings may lead to expressions of overt aggression such as murder.

Caudill: Do you think that more florid symptoms are permissible in the bush than in the city?

Lambo: I think this is so on the whole, although it is difficult to compare an African schizophrenic living in the bush, with one who lives in the town. For example, in an Englishman with schizophrenia (who is not unlike a sophisticated Nigerian) the onset of the illness is often insidious and impotence may be a symptom. Such patients can be observed over a long period before it is possible to make a definite diagnosis, whereas in the rural African the whole picture is much more acute and florid with atypical features.

Mead: I think this means that we have spreading over the world what could be called a "cross-cultural culture". This follows the Euro-American pattern and has certain characteristics. For instance, in schizophrenia in the West, the pattern is more apathetic than in the East. We notice the same sort of contrast in the degree of floridness of schizophrenic symptoms between New York and Kentucky, as is found in Nigeria between the city and the bush. Anywhere in the world there is a systematic relationship between the symptoms of those who are part of a scientific, rational, urbanized, secularized culture and those who are on the periphery and are non-urbanized (Mead, M. [1957]. *Int. J. Psycho-anal.*, **38**, 369).

Studies on eidetic imagery (Klüver, H. [1933]. *In* A Handbook of Child Psychology, p. 699, 2nd edn. ed. Murchison, C. Worcester, Mass.: Clark University Press) were done in the United States before

this subject became unrespectable because of the use the Nazis made of it. We found this sort of imagery in children and among primitive peoples who lived in peripheral areas which had not been urbanized. We also found that in artists, in San Francisco and other urban centres, you may still find this type of behaviour. I think that Professor Lambo's findings are a remarkable confirmation of what has been found in other countries. All over the world, in quite different ethnic groups, we find this contrast between the type of imagery found at the periphery of a culture and the type of imagery and breakdown found in the centre.

Carstairs: I also would like to comment on the contrast between the literate and the non-literate. It seems to me that the literate have been exposed to certain experiences which colour the concrete content of their delusions. This may be simply because their way of life has led them into certain experiences, but I feel that there is another dimension here which concerns the familiarity with complex, imaginative experience. This can be quite separate from the question of literacy or non-literacy. In the non-literate community itself there are some who are well versed in the traditions and fantasies of their culture and one would expect them to elaborate a complex delusional system. Conversely, among the semi-literate, delusional content may be misleading. I remember a patient of Sir Aubrey's at the Maudsley Hospital eight years ago; she was a woman of modest intelligence, and yet she elaborated the most complicated and bizarre fantasy involving space travel. Sir Aubrey was clearly quite impressed by this flight of the imagination until one of his registrars pointed out that her fantasy was literally the contents of the current comic strip in a daily newspaper. The patient had not, in fact, soared imaginatively, she had just given us a literal reproduction of something she had read. I think that we should attempt to separate the accident of experience brought by literacy from the development of complexity in the imaginative life.

Margetts: I hope I will not be considered indiscreet if I quote myself. I wrote (Margetts, E. L. [1958]. *Med. Proc.*, **4**, 674): "Well defined and elaborate paranoid delusions are not commonly noted, though one emancipated young man who could read, and who went to the cinema, thought he was a space explorer!" This patient was a Kenyan African.

Professor Lambo, would you care to say anything about the family history of mental illness in your African schizophrenic patients? This part of the story interested me very much when I worked in Africa.

Lambo: We have not yet made a special study of the families of schizophrenic patients in Nigeria but our observations have shown fairly consistently that schizophrenia or other psychiatric disease occurs frequently in the near relatives of our schizophrenic patients. This is a subject in urgent need of scientific study.

Caudill: We have studied symptomatology in two groups of schizophrenics, Japanese and American, and found differences that we think reflect their different cultural backgrounds. For example, the Japanese patients are more physically assaultive (particularly toward their mothers) whereas the American patients show more hallucinations and bizarre ideas. Since Japanese culture stresses interpersonal decorum, and American culture stresses the dominance of cognitive over emotional processes, it may be that the schizophrenic in each culture is reacting in a strongly negative fashion to major cultural expectations (Schooler, C., and Caudill, W. [1964]. *Ethnology*, **3**, 172).

Leighton: I am most interested in the idea of the schizophrenic's negative reaction to the rules of his culture. There were only a handful of schizophrenics in the small sample that we investigated when we were working with Professor Lambo, and I appreciate that one should not draw general conclusions from a small number of cases. Nevertheless, in our limited experience, one of the things that struck me as the hallmark of the schizophrenic was his violation of accepted religious sanctions. I remember one woman who imitated a masked dancer—an *egungun*—something no woman is supposed to do, and I am wondering if this reacting against religion just happened to be the form the psychopathology took in these few special cases, or whether running counter to the religious sanctions of the culture is characteristic of schizophrenics. Profesor Lambo, have you studied this aspect of the problem at all?

Lambo: Not really. Imitation of *egungun* certainly runs counter to the cultural norm, especially when a woman is doing the imitating. Not uncommonly one of the earliest complaints from the relatives of a schizophrenic patient in a rural area is that he is doing what he is not expected to do or generally breaking the social code of behaviour.

This subtle alteration in behaviour is usually in the sphere of religion since this is such an immediate part of the life of the non-literate African. We have not really studied this in detail yet, but we will certainly do so as soon as practicable. Alteration of social role or negativisitic attitudes towards, or contravention of, established codes of behaviour may be subtle and will in any case need the participation of an anthropologist for its study.

DeVos: I think that this is related to the condoning of abnormal behaviour in a culture. One of the problems of establishing valid epidemiological surveys, I imagine, must be that people who are psychotic or otherwise mentally ill in a way that is tolerable to the society in which they live will not necessarily come to professional attention. I worked with Dr. Horace Miner in Algeria, and we pointed out (Miner, H., and DeVos, G. [1960]. Oasis and Casbah: Algerian Culture and Personality Change. Ann Arbor, Mich.: University of Michigan Press) that if a person became psychotic in one sort of way he could become a holy man, but that if his madness caused him to take off his clothes in public he would be declared crazy. In Algeria the least bodily exposure was defined as madness. Such exposure of the body was the unseemly act that defined the individual as no longer in control of himself.

Wittkower: Professor Lambo, I would like to ask you three questions about the aetiology and symptomatology of mental disease. First, you mentioned the toxic appearance of some of your patients; to what extent does the presence of infection or toxic states, for instance malaria, influence the clinical picture? Second, what do you think is the reason for the lack of systematization of symptoms in non-literate patients? Dr. Yap (1957. *J. ment. Sci.*, **97**, 313) attributed this to the paucity of education in non-literates; do you agree with this view? My third question is a little complex: it has been shown in Africa and other parts of the world that ego development and ego functions are different in literates and non-literates. For example, there may be differences in the weakness of repression, the frequency of introjection and projection, and the frequency of denial compared with the variety of reaction formations, isolation and sublimation. Is it possible that there is a relative brittleness of the ego in non-literates which predisposes to the short, acute, psychotic episodes

which have been called *bouffée délirante aigue* and also to a high frequency of schizophrenia?

Lambo: I will answer Professor Wittkower's questions in the order in which he asked them. In tropical Africa, the picture of schizophrenic states is not infrequently complicated by a wide variety of endemic diseases which may influence their course. It was originally thought that some pathoplastic features, especially the confusional episodes frequently seen in rural Africans, might be due to toxic phenomena, and many unexplained primitive reactions have also been frequently encountered in patients who are free from endemic disease; but these reactions would seem to be much more related to culture than to infection.

I agree with Dr. Yap that the lack of systematization in delusional content in non-literates may be due to lack of educational background. I do not think, however, that this is the whole story, because the rural Africans may show a great deal of elaboration when they express their delusions. Perhaps we are unable to appreciate the form their systematization takes; it certainly occurs, but in a way that has not yet been accepted in western psychiatry, perhaps because of the obvious lack of connexion and consistency in their thought processes. Strong affective overlay may also be a factor which reduces the quality of systematization so that it is difficult for us to recognize it.

We have not investigated ego strength, or ego development in our studies at all. We are cautious of introducing the psycho-analytical approach to our practice of psychiatry in Africa until there is a precise and clear concept of psycho-analytical theory in western culture which may be applicable to our countries. When we observe that a lack of inhibition in many of the people in West Africa predisposes them to excitement and confusion we may be inferring the same thing as Professor Wittkower, although we are not consciously using the concept of lack of ego strength. We may be introducing two other dimensions of the ego—"flexibility" and "rigidity"—and these may be assessed as strength or weakness depending upon the cultural norm.

Lin: I would like to underscore the plea for more fact-finding, descriptive, epidemiological studies of different populations and cultures. I do not think that this approach is contradictory to Professor Wittkower's psycho-analytical orientation at all; I think the two approaches are complementary.

Professor Lambo's presentation reminded me of our own study on aborigines in Taiwan. There seem to be many similarities in the symptomatology of our patients and Professor Lambo's, particularly the acute onset with acute psychotic symptoms of confusion and violence.

Professor Lambo, first, how do the indigenous inhabitants regard the psychotics? Do they feel that they are abnormal or ill or dangerous or what? And secondly, what is the duration of the psychotic process in your patients? In our observations on Taiwan aborigines we found that psychotic episodes were quite short compared with those we observed among the Chinese. The prognosis in these aboriginal patients was also fairly good. They do not seem to deteriorate into the chronic, schizophrenic state that we see in many Chinese and western communities.

Lambo: The question of the attitude of the community to psychotics is a most interesting one. Dr. DeVos has suggested that whether these people are regarded as mentally ill or whether they become a tribal variant of the normal depends on many factors, one of the most important of which is social behaviour. In my own experience, and I know that Professor Leighton will agree with me, even the most primitive cultures recognize mental illness but their reaction to it differs. A psychotic may be left alone and tolerated or allowed to roam around the village or he may be confined; this depends on the meaning his symptom has for the group as a whole. There is a difference between the basic recognition of mental illness, which is probably common to all cultures, and the measures that the group take to deal with it, which may be a reflection of their social attitudes.

We found the same type of picture in a recent survey that we carried out in villages in Nigeria on what constituted aggression and delinquency in children. We asked in various tribal areas if delinquency occurred and we discovered that the people had their own ideas of delinquent behaviour, but that their attitudes were flexible and accommodating until a person acted against the tribal system or became dangerous to the community. When this happened he was expelled from the village. There is a great difference between recognizing that a person is ill or antisocial, and taking measures against him.

I agree also with Dr. Lin about the duration of psychotic episodes.

In our experience in Nigeria some of these episodes are quite short and only last from a few hours to a few days. I also have recorded (Lambo, T. A. [1960]. *Brit. med. J.*, **2**, 1696) that there is not the same degree of chronicity in African schizophrenic disorders as there is in some other cultures especially the West.

Leighton: I would like to comment on the way that we have developed our concepts of psychiatric disorder. I support Professor Lambo when he says that we can agree fairly well over the symptomatology of mental disorders even if different words are used to describe symptoms in the various cultures. As long as we discussed only symptoms with the native healers of the Yoruba tribe we could understand each other and were in reasonable agreement, but there was chaos the minute we started trying to find common ground over aetiological conceptions; talking to a native healer about the difference between organic and functional, or between a schizophrenic psychosis and a severe psychoneurosis, led nowhere. Conversely, *we* had difficulty in trying to visualize clinical entities based on the idea that some mental disorders can be identified by the "fact" that they are the result of attack on the mind by the smallpox god which produces mental symptoms rather than manifestations in the skin. Similarly, it is difficult for us to consider the concept that mental disorders are due to bewitchment or taboo violation or a spirit. For the most severe mental disorders at least, there were no bridges between us and the native healers when we discussed aetiology, but when we considered what kinds of behaviour and feelings were looked on as illness, it was not too difficult to communicate.

We did find, in our limited studies (Leighton *et al.*, 1963, *loc. cit.*), some interesting differences in the attitudes toward mental abnormality. This applied particularly to fairly mild disorders such as some psychoneuroses, psychopathic personalities, senility and so on. In exhaustive discussions with members of the Yoruba tribe about what they considered illness, psychopaths and the symptoms of senility were never mentioned spontaneously. Eventually we described psychopathic people and asked about them. The answer was immediate: "Oh yes, we all know people like that but we don't call that illness." When we asked about disorders of later life the answer was: "Oh yes, sure, that's the way old people are, but that's not an illness, we don't send

anybody for treatment for that. That's the declining cycle of life, just as not being able to speak is the beginning part of life."

Fortes: In North Ghana, which I knew thirty years ago and in which I worked again recently, the people drew a definite distinction between madness as an illness and mental conditions which we were told were not madness as the sufferer had been "born that way". This also applied to senility. I was told: "We don't worry about old so-and-so, he's just old, that's what's the matter with him."

Leighton: Were these people recognized as having personality attributes which were undesirable although not defined as illness?

Fortes: Yes.

Leighton: I would like to comment briefly on depression. Our approach to depression was purely symptomatic, by which I mean that we tried to see how many people there were in our Nigerian sample whom we, as clinicians, would classify as depressives in symptomatic terms. We found a surprising number, comparable to the number in other populations that we have studied. These results, like those of H. Collomb and J. Zwingelstein (1962. *In* Report of 1st Pan-African Psychiatric Conference, p. 227, ed. Lambo, T. A. Ibadan: Government Printers), do not confirm previous writings which suggest that depression is excessively rare in Africans. An intriguing fact here is that there is no word for depression in Yoruba. When we described individual symptoms—crying spells, feeling blue, loss of appetite, waking early in the morning and so on—these were immediately recognized by the Yoruba, although the whole constellation of symptoms still did not form a syndrome in their minds; to us, the various features all fitted together but it had not occurred to our informants that they made a pattern. On the other hand, there was one disorder, puzzling to us, that was frequently reported, both by the native healers who were our key source of information and also by certain members of our sample who were questioned about their previous illnesses. They would quite often say that they suffered from *inorun* which was translated to us as heavenly fire. *Inorun* quite clearly is a syndrome in their minds, although I still cannot make it form a pattern in mine.

Transcultural Psychiatry
A.V.S. Deuck & Ruth Porter

PHENOMENOLOGY OF AFFECTIVE DISORDER IN CHINESE AND OTHER CULTURES

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CROSS-CULTURAL studies of affective disorder have not been systematically attempted, and such information as we have derives more from epidemiological than nosographic enquiry. However, it is hardly possible to separate the diagnosed incidence of the disorder from its clinical presentation, since the distinction of morbid from normal affect is essentially a matter of degree and also much influenced by sociocultural factors.

General surveys of affective and other illnesses in different cultures have been made (Benedict and Jacks, 1954; Krapf, 1959; Aubin and Alliez, 1955). Stainbrook (1954), in a cross-cultural study of depression, has reported some of his own observations in Brazil, but it is on Africans that most interest has been focused (Carothers, 1948; Tooth, 1950; Laubscher, 1951; Field, 1958; Lambo, 1956, 1960; Earle and Earle, 1956; Krapf, 1959; Asuni, 1962). There is agreement among these authors on the rarity of outspoken affective illness, the lightness and short duration of depression with an absence of sin and guilt ideas, the relative frequency of manic pictures, the common association with confusional symptoms, and a very low suicide rate. Carothers identified "involutional depression" in Kenya, and Savage (1963) in Nigeria, although in the former case it was not considered typical. Both Lambo and Field pointed out that depression might be missed because of a veneer of psychosomatic symptoms. It has been increasingly recognized that the illness is not in fact rare, being masked often by hypochondriacal ("psychosomatic") and

confusional symptoms (Lambo, 1956, 1960; Field, 1958; Gillis *et al.*, 1962; Savage, 1963; Leighton *et al.*, 1963).

Observations on Indonesians have been made by Kraepelin (1921), Osterreicher (1951) and Pfeiffer (1962, 1963). The first two authors arrived at conclusions similar to those reached for Africans. Pfeiffer, however, did not find more mania than endogenous depression in his cases, although he formed the impression that there was a tendency to euphoric moods in all his patients.

Among the Chinese in Indonesia Van Wulftten-Palthe (1936) and Osterreicher (1951) found a high prevalence of depression, and Pfeiffer (1962) has remarked on the frequency of hypochondriacal depression in this racial group. Statistics on affectively disordered in-patients in Shanghai have been published by Hsu and Ch'un (1959) and in Peking by Chu and Liu (1960). Their patients included manic-depressive as well as involutional-depressive cases. T. Lin (1953), in Taiwan, found that the expectancy for manic-depressive illness was low although not as low as in Thuringia. Yap (1958*a*, *b*) found a moderately high rate of suicide among Hong-Kong Chinese. The absence of delusions of sin in Chinese as well as Japanese depressives has been noted by Yap (1958*a*) and by T. Lin (1953). The rarity of severe self-reproach is claimed for most non-Christian patients (Murphy, Wittkower and Chance, 1964).

Among other studies, that concerning the Hutterites (Eaton and Weil, 1955) with their high incidence of guilt-ridden depression is of unique interest. Beaglehole (quoted by Benedict and Jacks, 1954) reported that the admission rate for manic-depressive illness in native Hawaiians and for Filipinos in Honolulu was comparable with the admission rate for Whites, and for Maoris in New Zealand it was even higher.

In the United States, but not England, admissions for manic-depressive psychosis are falling, especially as regards mania (Farber, 1959; Lachman and Abrams, 1963). Arieti (1959) has attempted rather unconvincingly to relate this fall to a weakening of super-ego

sanctions. Mayer-Gross (1954) has pointed out the growing prevalence of minor depressive illnesses today, and Ancherson (1964) has remarked that depressive ideas in present-day depressives are less dramatic, taking the form of guilt over social slips and failures without any religious content.

Scattered findings on economic, educational, and social class differences in affective illness are noteworthy. Carothers (1948) and Lambo (1956, 1960) reported that "classical" endogenous depression was confined to westernized sections of the population, and Aubin and Alliez (1955) stated that in African soldiers depression was typical only among non-commissioned officers. Stainbrook (1954) noted that among the Bahians in Brazil severe depression with self-punitive verbal behaviour was seen only in well-to-do patients. He has also reviewed reports suggesting that Negroes in the northern parts of the United States showed more depression than in the south. T. Lin (1953) found that manic-depressive illness was rare among the lower classes. Hes (1960) showed that the illness was commoner among western than oriental immigrants into Israel and Hollingshead and Redlich (1958) established that in New Haven neurotic depression, but not affective psychosis, predominated among the higher classes.

Such observations indicate clearly the need for caution in interpreting cross-cultural material. Far too often generalizations are made and associations drawn without due regard for the sources and the homogeneity of the samples as well as their precise nature. It is extremely important to control differences in sex and age structure when comparisons are made. Case-finding is particularly difficult in recurrent recoverable illnesses which often cannot be easily distinguished from normal, and which may be removed from view because of suicide. In-patients, always a highly selected group, are not directly comparable with out-patients, nor with patients identified by epidemiological methods. Above all the type of patients treated is greatly dependent on adequate provision of clinic facilities. We need, as far as possible,

systematic observation and analysis of representative cases over a period of time.

AN EXAMINATION OF HONG KONG MATERIAL

In 1961 a retrospective and follow-up study was made of 130 in-patients with affective illness who had been admitted to hospital for the first time between 9 years 6 months and 13 years 7 months previously. These were consecutive cases except for 16 which were rejected because of a doubtful diagnosis. All patients were personally examined at the Hong Kong Mental Hospital (the only one in the community) during their first, or in a few cases, their second or third admission. It was possible to trace the outcome in 62 cases, either by post or in person. The sample of 62 included three patients who had died, and in the one who died earliest the actual period of observation was only seven years.

The diagnostic criterion employed was a primary disorder of mood in the absence of schizophrenic thought disorder or organic cerebral symptoms. No special attention was paid to recurrence or outcome, nor were any cases rejected because of psychoneurotic symptoms.

CLINICAL FEATURES

Age and sex distribution, and duration of first attack: The median duration, from the beginning of the illness to discharge, in the 81 patients treated for their first attack ever was 11 weeks (range 1–54). The median age for men was 27 years (range 14–50) and for women 28 years (range 14–58).

Table I shows that there is an apparent tendency for the first attack to occur more frequently at an early age in women. Our findings are similar to those from another Chinese sample by Hsu and Ch'un (1959). Kraepelin (1921) found that first attacks occurred mostly in young adults, especially women and Lundquist (1945) and Kraines (1957) found that most first attacks were in young women.

Table I

AGE OF INITIAL EPISODE, ACCORDING TO SEX

	Number of patients				
Age in years	*Male*		*Female*		*Male + Female*
11–20	10		16		26
21–30	31	47	25	38	56
31–40	16		13		29
41–50	4	4	13	15	17
51–60	–		2		2
Total	61		69		130

Between the ages of 41 and 60 there were 15 women with affective illness, but only four men. This is out of all proportion to the distribution of the sexes at this age in the general population. Since in this age group all the patients had come under personal observation it was possible to make an exact diagnosis: of the women nine were depressed, five manic and one had mixed symptomatology; of the men three were depressed and one manic. Kraepelin (1921), like Lundquist (1945), showed that females predominated at all ages, and Kraepelin also discovered a sudden rise in the number of first attacks in women between the ages of 45 and 50. Kraines (1957) found more women than men affected at this age, although Stendstedt (1952) did not. Chu and Liu (1960) have recorded that there were more women than men admitted for "involutional psychosis" in Peking. Thus, among the Chinese too, involutional women are unusually liable to affective illness, especially depression.

Taking all ages, there were 69 women to 61 men affected. This preponderance of women, already noted in various studies, was also described by Cassidy and co-workers (1957). In our study the sex incidence of affective disorder was significantly different ($P < \cdot 05$) from the sex ratio in the general population between the ages of 16 and 55; in 1950 this was 120 male to 100 female (Medical Department Population Survey).

It appears therefore that, even cross-culturally, affective disorder possesses the following basic characteristics: onset frequently in young adults and a predilection for females as regards first or early attacks and during the involutional period, when depression is more likely than mania.

Variations in the nature of attacks according to age and sex: Table II, which concerns 180 episodes actually observed in hospital (or clinic), excluding chronic states, gives a clearer idea of the relationship of age and sex to diagnosis.

Table II

VARIATIONS IN 180 OBSERVED EPISODES, ACCORDING TO AGE AND SEX†

	Number of patients								
	Male			*Female*			*Male + Female*		
Age in years	*Manic*	*Depressive*	*Mixed*	*Manic*	*Depressive*	*Mixed*	*Manic*	*Depressive*	*Mixed*
11–20	5	1	2	6	4	–	11	5	2
21–30	15 (29)	7 (12)	1	15 (30)	5 (18)	3	30 (59)	12 (30)	4
31–40	14	5	1	15	13	6	29	18	7
41–50	11 (11)	4 (4)	1	6 (8)	17 (27)	7	17 (19)	21 (31)	8
51–60	–	–	–	2	10	4	2	10	4
	45	17	5	44	49	20	89	66	25
Total	67			113			180		

† Data from observations on 23 men and 39 women.

There is a clear predominance of mania compared with depression in men at all ages, but in women only before 41 years. Kraepelin (1921) found depressive first attacks clearly predominating only after the age of 25, for both sexes analysed together. For attacks in general depression was more common at all ages. Lundquist (1945) found manic first attacks commoner than depressive first attacks in young men, but mania and depression were equally common in young women. Kraepelin and also Lundquist showed, for first attacks, that depression increased with

age for each sex, but mania behaved in the opposite way. Olsen (1961) found that manic first attacks were much commoner before the age of 25. The predisposition of youth and the male sex to mania appears fundamental.

Of the 25 mixed attacks, 14 were commingled, and 11 alternating, mania and depression. In seven of the alternating attacks depression led to mania and in the other four, mania led to depression. Mixed attacks were commoner in relation to total episodes in women, and this was also found by Kraepelin. This is probably another fundamental characteristic of affective disorder.

Relapse after initial attack: Out of 62 patients, excluding four who became chronic, 33 had relapses. The distribution of these, according to the number of years which had elapsed since the initial episode, is shown in Table III.

Table III

RELAPSE AFTER INITIAL ATTACK

Time since initial episode in years	*Number of patients*	*Number expressed as percentage of total*
0–5	16	48·5
6–10	8	24·3
11–15	7	21·2
16–20	1	3·0
21–25	0	0
26–30	1	3·0
Total	33	100·0

The episodic nature of affective disorder, with relapses occurring over widely varying periods, is again shown to be a basic characteristic.

Outcome on follow-up: The four cases which became chronic were all women; two were taken ill before they were 30 and showed increasing blunting of affect without any schizophrenic primary symptoms; the other two became ill in middle life and one of them later developed hypertension with episodes of

clouding of consciousness. Anderson (1936) found that one-third of women over 40 years old never recovered; Lundquist (1945) discovered that depressed cases had a poor prognosis although there was no clear sex difference in the prognosis. The relationship of poor outcome to schizophrenia and arteriosclerosis is well known (Kraepelin, 1921; Lundquist, 1945; Kraines, 1957). (See Table IV.)

Table IV

OUTCOME ON FOLLOW-UP, ACCORDING TO SEX

	Number of patients		
Outcome	*Male*	*Female*	*Male + Female*
Well	18	25	43
Mild symptoms only, not requiring treatment	2	2	4
Under treatment as out-patient	2	1	3
Under treatment as in-patient	1	4	5
Chronic in hospital	–	4	4
Dead from physical illness	–	3	3
Total	23	39	62

Of our sample 8 per cent became chronic from the start. Comparisons must be cautious because of the varying lengths of follow-up and differences in diagnostic criteria, but we may note Kraepelin's figure of 5 per cent and Lundquist's 16 per cent. Our study confirms the universal finding that the great majority of attacks of affective illness are self-limiting and benign.

Types of clinical course: If, like Lundquist, we regard as the initial attack the one that first brought the patient to the doctor (thus making reliable diagnosis possible), our followed-up patients fall into the groupings set out in Table V.

Both Kraepelin (1921) and Lundquist (1945) gave figures similar to ours for the proportion of single to multiple episodes of depression and of mania. In our series, for all the different patterns together, single episodes came to 47 per cent; this may be

Table V

TYPES OF CLINICAL COURSE, ACCORDING TO SEX

Diagnosis	*Number of patients*			*Male + Female expressed as percentage of total*	
	Male	*Female*	*Male + Female*		
Depression					
Single Episode	5	11†	16	26	37
Recurrent Episodes	1	6	7	11	
Mania					
Single Episode	4	6	10	16	29
Recurrent Episodes	6	2	8	13	
Mixed					
Single Episode	1	–	1	2	10
Recurrent Episodes	1	4	5	8	
Diphasic Manic-Depressive					
Single Episode	1	1	2	3	24
Recurrent Episodes	4	9	13	21	
Total	23	39	62	100	

† Including 4 cases which became chronic.

compared with the 53–68 per cent reported by Kraepelin (1921), Lundquist (1945), and Stendstedt (1952). Our data also parallel Kraepelin's in finding that depression is the commonest kind of single episode, that recurrent depression is more common in women, and recurrent mania in men, and that recurrent mixed states with confusion and stupor are commoner in women.

The infrequency of depressive and manic episodes occurring together in the same patient has been pointed out by several workers. Kraepelin (1921) found that only one-third of his cases was of this kind and Rennie (1942) one-fourth. Our findings concerning such biphasic cases are in keeping with theirs. Olsen (1961), however, gave a much higher proportion for young patients. Nevertheless, the varied colouring of the illness in a not inconsiderable number of cases, as well as its irregular periodicity,

must be considered fundamental characteristics. We found 29 per cent of our cases with mania, compared to Kraepelin's 17 per cent, but differences in the age and sex composition of the two samples may account for this.

Symptomatology: During the observed initial episodes of illness in our 62 cases, the following outspoken secondary symptoms (not mutually exclusive) were recorded in the number of cases shown: Resistiveness (9—8 females); Agitation (8—7 females); retardation (7); paranoid ideas (6); clouding of consciousness (4); guilt and unworthiness (4); grandiose ideas (3); hypochondriacal ideas (2); Hysterical features (2); Hallucinations (2); Nihilistic ideas (1); and Obsessional ideas (1).

On the whole the clinical picture presented was similar to that described in the West, although in our cases women seemingly exhibited more resistiveness and agitation than men. Kraepelin (1921) noted that women, especially in youth, were prone to impure forms of the illness with anxiety, confusion and stuporose states. Olsen (1961) found hysterical features in many of his young cases. Of unusual interest is the rarity and mildness of the ideas of guilt and unworthiness expressed. There was no mention of "sin" at all, but only four patients were Christians. In two patients the ideas of guilt concerned bringing trouble to their family and a variety of personal faults. A third said he was incapable of earning money, stupid and unable to cope, and a fourth that he was hopelessly poor, uneducated, exploited and unable to help his country.

There was only one woman, aged 50, whose illness resembled the classical melancholia of involution. She exhibited much agitation, verbal hostility, crying and refusal of food, believing all her family dead, all her possessions lost and that she was being harmed. This was her first attack, four years after the menopause. However, she was irritable rather than obsessional by previous personality. There were four women, ranging in age from 25 to 44 years, with recurrent mixed episodes, and only one man. One

of the women always started with a brief depression, then entered into a mixed state with thematic content and finally passed through a phase of depression before recovery.

Everywhere, it would appear, female sex in affective illness is associated with more variegated symptoms. In keeping with present-day opinion, there is little evidence for a special "involutional melancholia" (Stenstedt, 1952, 1959; Kay, 1959; Cassidy *et al.*, 1957).

Attempted suicide: Out of 130 patients, seven men and 16 women made suicidal attempts. One woman attempted suicide twice and another three times. We have previously found that unconsummated suicide was particularly a female phenomenon, in Chinese as in Westerners (Yap, 1958*a*, *b*, *c*). We also showed that the suicide rate in Hong Kong for 1954 was 23·5 per annum per 100,000 population aged 15 and over, which, cross-culturally, is neither very high nor low. The Hong Kong rate has been slowly rising, like the number of new depressed cases seen (in contrast to manic cases), but we must not assume a one-to-one relationship between suicide and depression.

Precipitating causes: Precipitating causes were considered significant in 34 out of 62 patients in their first admissions. Twenty-one suffered from depression, eight from mania and five showed a mixed state. The stress was psychological in 19 cases, physical in twelve and combined in three.

The manic and mixed reactive-to-stress cases were interesting. While they were not much younger than the depressed cases on the average, there was only one patient over 31 among them; this was a mixed case who was febrile and whose mother had died. The precipitating causes were equally divided between psychological and physical, and included infectious fevers and lactation.

Our findings fit in with those of Kraepelin (1921) and Olsen (1961) regarding the readiness with which young persons react to stress with mixed and atypical symptoms instead of the expected depression, and confirm the intrinsic psychological link between

mania and depression. Young women everywhere are of course likely to suffer from puerperal and lactational fever, apart from possessing a greater affectivity. Young men are prone to mania, being commonly more aggressive and out-going than other groups.

THE SIGNIFICANCE OF CROSS-CULTURAL DATA FOR NOSOLOGY

I have previously (Yap, 1961) discussed the need in cross-cultural research for concepts with unequivocal meanings, sufficiently generalized and culture-free to be widely applicable. Comparative psychiatry is basically dependent on epidemiological method and the latter must have clear-cut and consistently applicable diagnostic categories to work with. Too narrow a definition of disease is likely to be culture-conditioned; but too generalized a nosological concept may include heterogeneous material, making it useless for comparative research, or it may be drawn at a level too abstract for observation in the field.

The problem in the case of affective illness is simplified by the fact that its core of depression (or elation), unlike schizophrenia or organic states, makes a direct appeal to our psychological understanding. It is only to be expected, therefore, that the unity of the symptom-complex has been confirmed by several workers using factor-analysis. Kraepelin (1921), in his diagnosis "manic-depressive psychosis", included patients with confusional symptoms—amentia. Regarded as an operational definition of illness, "manic-depressive psychosis" is not very different from the notion of "affective disorder", which, after all, is "neo-Kraepelinian". Most psychiatrists have in fact used the category of "manic-depressive psychosis" merely as an operational concept.

The application of a simple minimal definition of "affective disorder" to Chinese patients has been shown to be capable of identifying a group with general characteristics that parallel those found among similarly differentiated Euro-American groups. The

parallelism lies both in the conjunction of symptoms cross-sectionally and in the behaviour of the cases viewed longitudinally. An important universal phenomenon of abnormal human behaviour is found to be denotated, cutting across cultural boundaries. The practical usefulness of the diagnostic concept is thus demonstrated. While there may be variations in the ratio of mania to depression and in the intensity of depression, these are only differences of degree and not of kind.

THE VALIDITY OF CROSS-CULTURAL STATISTICS

The apparent frequency of mania in the hospital statistics of certain developing countries is probably an artifact produced by selection for admission on the basis of disturbing behaviour. The fewer the beds available the more will admission be restricted to obstreperous manics rather than subdued depressives; indeed, where psychiatry is ill-developed, the depressives may not be regarded as sick at all. The same consideration applies to cases with aggressive, destructive and acutely confused behaviour, said to predominate in the mental diseases of primitive peoples. It is interesting to note that where psychiatric facilities are relatively adequate, as in Hawaii and New Zealand, the rate for manic-depressive illness among the indigenous peoples is not found to be low (Beaglehole, quoted by Benedict and Jacks, 1954). Furthermore, when field studies are cited instead of admissions, as in Tooth's study (1950), manic-depressive illness is said to be relatively common. Socioeconomic and "threshold factors" of family tolerance may also explain the apparent infrequency of depression recorded for southern United States Negroes as compared with northern Negroes (Stainbrook, 1954). The reported association of "true" manic-depressive illness with education in Africans may arise simply because the educated and westernized have a better chance of coming to the attention of the town-based psychiatrist. Finally, it is possible that the pre-

dominance of mania may be due, as Stainbrook (1954) found in Bahia, to the practice of labelling as "mania" what is really schizophrenia.

Nevertheless it is credible that real variations in the depth of mood changes and secondary symptoms do exist. The decreasing incidence of manic-depressive illness in the United States may not be entirely deceptive; the phenomenon has been noted by experienced practitioners outside hospitals, and even if diagnostic standards have changed the crucial fact could be that the primary symptoms of affective illness have become less pronounced so that other diagnoses become feasible (Arieti, 1959). Field (1963) found variations in the occurrence of depression between different tribes within Ghana alone. Cross-culturally, the marked and consistent divergences in suicide rates are highly suggestive; so is the intimate relationship of guilt-laden depression in the Hutterites with their social and religious background. The frequently reported somatic and hysterical symptomatology of depression in primitive peoples may be a genuine variation due not simply to inability to communicate with the psychiatrist, but also to a somatic orientation in their ideas of disease, comparable to those of lower classes in developed countries (Hollingshead and Redlich, 1958).

SOCIOCULTURAL FACTORS INFLUENCING VARIATIONS IN SYMPTOMATOLOGY

An orderly, if superficial, discussion of this subject confined to cross-cultural issues must be based on a coherent theory of affective disorder. We accept the fundamental importance of the arousal of aggressiveness following deprivation of a loved object, in mourning as well as in frustration; and also the turning inwards of aggression as the underlying process in depression (Lindemann, 1944; Yap, 1958*a*, *c*). Some of the central concepts involved here have been generalized and tested by psychologists in the context

of general behaviour theory (Bandura and Walters, 1963). We agree with Linton (1945) rather than Kardiner (1945) in holding

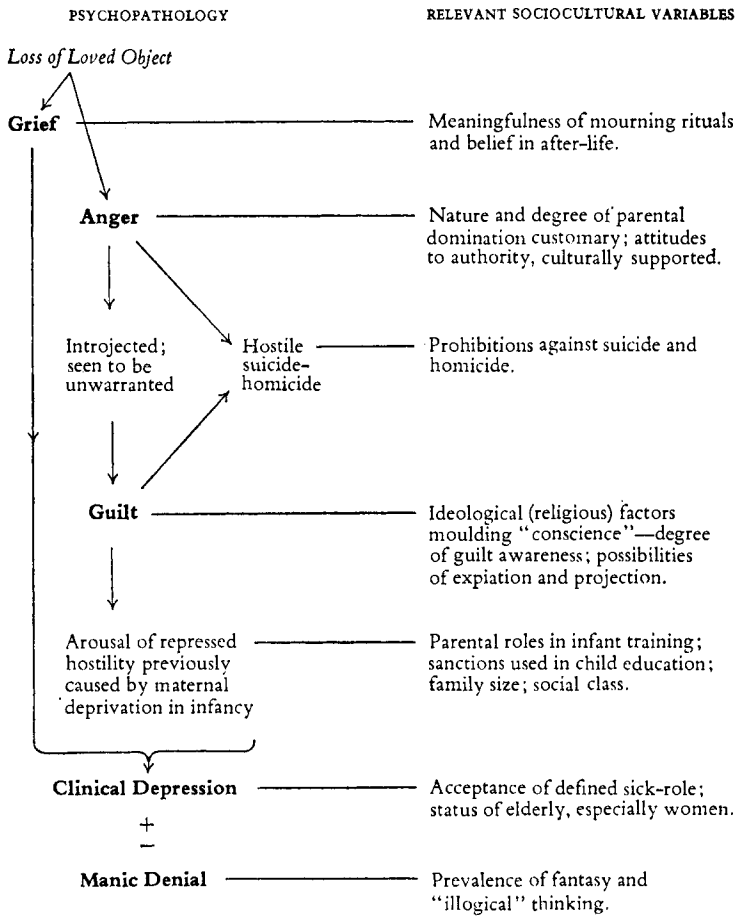

FIG. 1. Possible relationship of sociocultural variables to psychopathology of affect.

that social forces are of significance in personality formation throughout life, and not only during infancy. We also believe

that there are no grounds for separating depression with guilt from that without it.

Figure 1 is an inevitably over-simplified diagram of the psychopathological processes underlying affective disorder. These formulations are in the light of contemporary knowledge unexceptionable. Next to these processes I have indicated certain related sociocultural variables.

Mourning practices: A necessary though not by itself sufficient pre-condition for the appearance of depression is the process of grief or mourning. Religion gives promise of continued interaction with the deceased and the possibility of reparation for whatever wrong may have been done. Acceptance of loss with the allaying of guilt and resentment may be inhibited if customary rites and beliefs lose their significance in rapidly acculturating groups or in secularized societies. Whether mourning becomes pathological or not depends primarily on the degree of ambivalence in the subject's relationship to the lost person or object. This in its turn is affected by the nature of the interaction between parent and child; the relationship of father to child in patriarchal, traditional societies differs from that in individualistic western cultures.

Super-ego formation: "shame" and "guilt": The alleged rarity of depression in illiterate Africans has been ascribed to the absence of self-reproach and of a sense of personal responsibility; this is supposed to be due to a certain fatalistic attitude (Tooth, 1950), or a lack of individualistic competition (Carothers, 1948). Benedict and Jacks (1954) indicted the failure to introject hostility, and Carothers (1948) boldly speculated on a fault in the mechanism of repression. Hitson and Funkenstein (1959) found that depressed patients in Boston were taught to be responsible for their own actions and anticipate the needs of others, whereas paranoid patients were trained to be the passive recipients of action by people in authority. Arieti (1959) ascribed the decline of manic-depressive illness in America to a change from "inner-directed" character

structure to "outer directed" (Riesman, Glazer, and Denney, 1950), implying that "guilt" as well as "shame" sanctions have become replaced by sanctions arising from diffuse anxiety.

The role of the self-punitive conscience in depressive reactions need not be disputed, but it is unconscious rather than conscious guilt feelings that give rise to pathological depression. However, there is no sharp line of demarcation in this respect, and the development of the super-ego is rightly considered a central problem for the cross-cultural discussion of depression. This area of study has been in a state of chaos. It is increasingly clear that the opposition of "shame" to "guilt" is intellectualistic, arbitrary and without empirical justification. The early attempts of Benedict (1947), Mead (1937, 1948, 1950), and D. Leighton and Kluckhohn (1947) to distinguish shame from guilt cultures have not been successful largely because of semantic difficulties, the lack of agreement on criteria for their identification and the failure to detect surplus meanings in curiously ethnocentric formulations (Piers and Singer, 1953; Ausubel, 1955; Whiting, 1959; Bandura and Walters, 1963).

It is more helpful to think in terms of a linear ordering of unconscious guilt feelings, conscious guilt feelings, and conscious moral shame feelings generated by the anticipation of discovery of wrong conduct by others. Table VI, which is a succinct outline of this position, is perhaps of heuristic value. It emphasizes the fact that the contrast should be made between the first sort of feelings and the last two, rather than simply between "guilt" and "shame". Non-moral shame feelings arising at the time when, for example, nudity or any defect is accidentally exposed to others does not concern us. In real life situations of course all these different affective reactions can coexist.

Guilt feelings are not confined to particular religious systems and practices, such as Protestantism and the doctrine of sin which is to be relieved by ritual atonement. DeVos (1960) has discussed guilt reactions in the Japanese and identified an analogous develop-

Table VI

FACTORS IN SELF-CONTROL, VIEWED PSYCHOPATHOLOGICALLY

Characteristics	**Unconscious Guilt Feelings** ↔	**Conscious Guilt Feelings** ↔	**Conscious Moral Shame Feelings**
Pathogenicity	+	+ or −	+ or −
Degree of Internalization	+ + +	+ +	+
Witnesses	None	None, or fantasied gods or spirits	Anticipated discovery by real persons
Acceptance of a Moral Standard	Yes	Yes	Yes
Sense of Moral Obligation to Conform	Yes	Yes	Yes

− = none.
+ = slight.
+ + = moderate.
+ + + = severe.

ment to Weber's "Protestant Ethic" (1930) based on moral obligation to ancestors and the Emperor. In general his findings also apply to other East Asian cultures. Hu's (1949) analysis of Chinese concepts of "face" is also relevant. All societies enforce social control with moral teachings, both abstract and situation-centred, predicated on moral obligation. This is true even for primitive groups like the Winnebago (Radin, 1926). All have in addition external sanctions. The individualistic, competitive and aggressively striving Protestant cultures may specifically produce unusual psychological stress and the belief in original sin probably intensifies depressive self-reproach in a superficial, pathoplastic manner; but the more general and fundamental question concerns, not the effect of the content of moral-religious teaching, but what social forces determine the strength of self-control responses. This brings into consideration the significance of early child-training.

It is useful to keep in mind Jaspers' (1962) distinction between "understanding" and "explaining" psychology. Our actual object of study is the process of self-control, not the patient's values. In this respect it would be helpful for the psychopathologist to note the difference between the unconscious, aggressive, inhibitory, and heteronomous archaic super-ego on the one hand and the mature and rational super-ego with the opposite characteristics on the other (Flugel, 1945; Piaget, 1948; Dicks, 1948; Fromm, 1950; Ginsberg, 1956).

On the basis of rather unsatisfactory evidence derived from questionnaires, Murphy, Wittkower, and Chance (1964) and Chance (1964), in Montreal, have hypothesized that self-reproachful depression is more common in socially cohesive groups, among which are included the "highly traditionalized and tightly-knit". Thus they take a diametrically opposite view from that of ethnologists who associate tradition-orientated societies with shame sanctions and absence of intra-punitive reactions, and therefore with less guilt and presumably less depression. As far as suicide is concerned their position would also be opposed to that of Henry and Short (1954) and, basically, of Dürkheim (1951) who regarded strong social restraints and social integration as conditions unfavourable for suicide. It may prove important to take into account factors of child-training, which the Montreal group have not done.

Possibilities of expiation and projection: Where religion teaches that human nature is evil from a supernatural cause it also provides the means for absolution and atonement and the relief of guilt in the individual. Jung (1958) believed that Christianity heightened the sense of guilt in western man and in this he is supported by Guirdham (1959). Pfister (1948), looking at it from another point of view, has emphasized Christianity's guilt-reducing functions, and Flugel (1945) comments on the same problem. In the Orient the rites of ancestor worship or reverence serve a similar function, but the absence of teachings on original sin and the supernatural

basis of conscience has also made possible among the elite a rationalistic acceptance of moral values, with conscious acknowledgment of wrong and the need for further self-cultivation after the example of the Confucian "Superior Man" (M. Lin, 1939). If institutionalized figures of Evil exist, for example Satan and revengeful ghosts, guilt can be projected on to these, just as at a mundane level paranoid suspicions can be thrown on to witches. In either case the culture prescribes appropriate magical rituals for protection. Field (1963) has well described the dynamic relationship between guilt and witchcraft fear in Ghana. Projection can take a quasi-pathological form when depressive guilt is ascribed to some evil "personality" which then "possesses" the subject (Yap, 1960). All these are alternative reactions to depression, and they are conditioned by differences in education and social class.

The depressive sick-role: Within his own culture the sick-role of a depressed patient may have a special significance. Among the Hutterites depression is accepted as divine testing and is not regarded strictly as a disease. The "lost soul" belief in South America may similarly give cultural support to the depressed person in the condition called Susto, which will then not call for medical attention. The depressive sick-role is patterned into something else in the case of Amok, which is a culture-conditioned, quasi-abnormal solution to morbid states of hostility (or hypereridism) arising from acute frustration (Yap, 1958*a*, *c*). Linton (1956) pointed out that cultures encouraging violent expression of hostile impulses and not emphasizing logical thinking may show less self-reproach and guilt reactions. We have already mentioned that illiterate groups, such as the lower classes in advanced countries, tend to define the sick-role in somatic terms, so that they will present themselves to the doctor as physical cases.

Child-training: Parental severity in child-training may influence directly the degree of internalization of aggression and thus the genesis of guilt. Henry and Short (1954), and Gold (1958) have reviewed evidence suggesting that, while strict parental disciplining

leads to decreased self-aggression, the withholding of love and nurture has the opposite effect. Aggression is more readily turned inwards when the mother rather than the father gives punishment. Evidence of the relation of love-oriented rearing techniques and early weaning and toilet training to high guilt has been obtained by Whiting and others (Whiting, 1959). Within the framework of orthodox psycho-analysis Goldman-Eisler (1953) demonstrated the relationship of early weaning to "oral pessimism" and "probably depression". Other workers, however, have failed to show a relationship between early training and later personality traits. Nevertheless we would be rash to ignore altogether variables such as the type of parental dominance, the use of verbal, love-oriented techniques or of physical punishment, the size of the family in terms of number of siblings and presence of mother surrogates, and—embracing all these—social class or culture pattern. We badly need formal testing on actual clinical samples of hypotheses relating depression to child-training. The methodology for such research has been pioneered in ethnological field studies by Whiting and Child (1953), and Scofield and Sun (1960) have attempted, with the use of Whiting and Child's techniques, to correlate training experience with personality in Chinese and others.

The cross-cultural approach is interested especially in the intrinsic psychological relationship of mania to depression, seen particularly in the pathological form of an extreme excursion of the mood cycle in response to stress. The psychological interpretation of mania forces us to treat it along the lines of hysteria. It must be admitted that over-emphasis on psychodynamic processes can breed nosological confusion; but we should not on this account ignore the situation-centred aspects of the manic reaction and also the underlying shift in the balance of aggression that can so often be detected in reactive cases. If manic reactions are seen in the light of hysteria, the well-known sociocultural factors bearing on the latter also become relevant for our present discussion.

SUMMARY AND CONCLUSIONS

Study of a sample of Hong Kong patients initially diagnosed as suffering from affective disorder and followed up revealed clinical features that paralleled what had been described in the West, in particular the periodic, benign, often biphasic and sometimes obviously reactive nature of the illness. This is taken as evidence for the validity and usefulness of the diagnostic concept as a universally applicable, nosological category. Reports from different cultures give no convincing ground for believing that there are significant qualitative variations in primary symptomatology after correction for factors of age and sex. However, illiterate groups may present with a somatic emphasis understandable in terms of their ideas of disease and their own definition of the sick-role; and in Judaeo-Protestant cultures the illness may be characterized by a relative severity of depression because of the merely pathoplastic influence of religious ideology. Various sociocultural factors with a possible bearing on the illness are discussed.

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DISCUSSION

Hes: I would like to report a study of the different distribution of manic-depressive psychoses among the ethnic groups in Israel. My findings have now been confirmed, on hospital cases, by a W.H.O. epidemiological study group (1964. Preliminary Report on the Census of Mental In-Patients in Israel, ed. Halevi, H.S. Jerusalem: State of Israel Ministry of Health). The W.H.O. group found that manic-depressive psychosis was most common in European Jews in Israel, less so in North African Jews and most rare in Yemeni Jews. There are too few facts and too many theories in this field, but, nevertheless, I would like to offer some speculations about the psychodynamics of manic-depression. S. Arieti (1959. *In* American Handbook of Psychiatry, p. 441, ed. Arieti, S. New York: Basic Books) describes an inner-directed personality and a tradition-directed personality. These

theories of personality have been elaborated by D. Riesman (*cf.* p. 99) and both these writers think that the theories are important in explaining the difference in prevalence of manic-depressive psychosis amongst the different racial groups. I think there is a need for more intensive studies in this area. I am at the moment analysing a manic-depressive, Iraqi Jew, whose psychopathology seems to be different from that originally described by Freud and his school. This man's main pathogenic factor appears to be an oedipal fixation and not the oral one described by other workers (Abraham, K. [1953]. Selected Papers on Psychoanalysis. New York: Basic Books; Cohen, M. B., *et al.* [1954]. *Psychiatry*, **17**, 103; Freud, S. [1924]. Collected Papers, vol. 4, p. 152, trans. Riviere, J. London: Hogarth Press). It is, of course, not appropriate to base a theory on a single case at such an early stage of the treatment, but I think that this may be an important finding. It seems to me that J. H. Eaton and R. J. Weil, in their study (1954. Man's Capacity to Reproduce. Glencoe, Ill.: Free Press), described as manic-depressive psychosis a condition which was not a manic-depressive psychosis at all but a kind of *Anfechtung*. I think that what they were describing is actually a religious feeling which is culturally accepted and very prevalent among the Hutterites. Eaton and Weil describe this as a kind of basic temptation by the devil; the patients are depressed and there is no manic phase at all.

Pfeiffer: In Indonesia, depressive patients are almost unknown in the hospitals and even in private psychiatric practice they are rare, especially when we look for the depressive ideas to which we are accustomed in Europe. However, when we look for states of biological deterioration with sleeplessness, inactivity and diurnal mood swings we find that such cases are not infrequent. I think that the insignificance of depressive ideas can be interpreted according to the value system of the society in which the individual lives. In western cultures, the value of a man is measured by his working efficiency; the depressed patient, who is unable to make decisions and work efficiently, finds himself in conflict with the value system of the community and also, of course, with his own super-ego which results in feelings of inferiority and guilt. In Indonesian society, the value of a man is not measured by his efficiency, and a person will hardly ever be blamed for doing nothing. Because of this, and in contrast to the manic

patient, the depressive does not come into conflict with the *Adat*, or traditional custom, but rather finds a support in it.

Rubin: I should like to comment briefly on two of the problems that Dr. Yap raised. My first comment concerns the prevalence of manic-depressive psychosis in the Caribbean. We carried out a survey on mental hospitals (1961. Report of 3rd Conference of the Caribbean Federation of Mental Health. New York: Research Institute for the Study of Man) in eleven of the islands in this area under five different metropolitan centres of influence. This was a one-day census of in-patients and one of the methodological problems that we had to deal with was the differences in terminology, nosology and so forth in the different areas. This meant that we had to amalgamate our diagnostic categories into two broad groups—functional, and organic psychosis. In this study we found that only 9 per cent of the population were classified as manic-depressive whereas 56 per cent were classified as schizophrenic. We used the American Psychiatric Association's classification of symptoms (this volume, p. 22) and we hope eventually to correlate the diagnostic categories and the symptom-check list to determine whether there are areal differences in diagnostic criteria.

Many of us are interested in the guilt-shame controversy, and agree with Dr. Yap's ideas about it. We have done a comparative study of Negroes and East Indians in Trinidad in which questions relating to guilt reactions were asked (Rubin, V., *et al.* [1959]. *Int. J. soc. Psychiat.*, **5**, 20). We found that the Negroes who were Christian and the East Indian Hindus reported different reactions in guilt situations. The Negroes tended to react with feelings of guilt when they had done something which they thought involved guilt whether this was in public or in private. The East Indians, however, reported feelings of guilt primarily if they had been seen committing the "guilty" act. There was a difference between internal feelings of guilt and what might be called shame.

Mead: The discussion following Dr. Yap's paper is particularly fruitful in showing us the number of different kinds of categories with which we could be working. An example is the influence of biological change on the manic-depressive psychosis. M. Roth and J. P. Morrissey (1952. *J. ment, Sci.*, **98**, 66) have noted that in England depressives over the age of 65 years were classified as senile and not treated. On follow-

up, however, these workers found that this classification of depressed individuals under 65 years old as organic depressives and over 65 years as senile was quite arbitrary and unhelpful. The high incidence of depression in women at the menopause may be due to a biological factor which is quite distinct from the cultural influences we have been discussing. It is also quite possible that there is a biological factor associated with late adolescence which gives rise to various manic or depressive activities. Attempts to integrate these findings, together with Dr. Yap's suggestion that as people become sicker they are no longer interpreted in the same terms as before, give us a picture which is very difficult to interpret. I think we could usefully study some of these problems by sorting out such factors as the involutional processes in women and the processes of senescence from cultural factors. This problem for men in their middle years is described by K. Soddy and R. H. Ahrenfeldt (1965. Report of Scientfic Committee of the World Federation for Mental Health. London: Tavistock Publications).

Caudill: I agree that depression is a good category for sorting out some of the problems about mental illness, but I think that some of the sub-categories of other illnesses, such as the schizophrenias, could also benefit if they were studied in this way, from both theoretical and methodological points of view.

Mead: Theories about the guilt-shame dichotomy are often the result of truncated, insufficient and inadequate thinking in which the question of pride has been disregarded. The whole problem stems from a failure to distinguish an early period of childhood in which neither shame nor pride are of primary importance, and a later period in which they are highly significant. The true continuum ranges from people who look for approval or disapproval from those they admire (pride) to people who are afraid of the disapproval of those they despise (shame) (Mead, M. [1961]. Cooperation and Competition among Primitive Peoples, p. 516. Boston: Beacon Press). Pride has been systematically left out by many workers in this field, which makes nonsense of the psychodynamics of the problem.

E. H. Erikson (1950. Childhood and Society. [1964. 2nd edn., rev.]. New York: Norton) has built up a general picture of the development of these feelings. He postulates a very early sense of sin as a deep

pervasive identification, followed by a period of shame and pride associated with the development of autonomous feeling, and then guilt again at a still later stage. From this we can see that guilt is associated with particular kinds of character formation and religious ideas and we can build a picture that has some coherence. In discussing the shame-guilt dichotomy often only part of this picture is included,

DeVos: I would like to support what Dr. Mead has just said by referring to the Japanese. It is obvious from the early missionary reports that guilt feelings were not located where the missionaries looked for them, in relation to sexual and sensual bodily expression, but instead they were to be found in connexion with family obligations. Once one knows the context in which guilt appears it is not difficult to find it, although the Japanese do not call all forms of guilt by the same name. For example, they use the term *on-gaeshi* which literally means "repaying one's parents", and infractions of this obligation arouse in the Japanese the feeling that is called guilt in the West (DeVos, G. [1960]. *Psychiatry*, **23**, 287). Different peoples feel guilty about different things depending upon their culture, and if we bring to the subject a preconceived notion of what they should feel guilty about or depend solely on the names they use for their feelings, we just create problems for ourselves in cross-cultural work.

Lin: Dr. Yap, can you give us any figures about the educational and social background of your patients? Have you any data which compare the background of patients with and without a guilt-shame complex?

Yap: I have not made any special attempt to analyse these factors, but most of the patients had at least a few years' schooling. The guilt feelings were slight and consisted of mild self-reproaches about such problems as not earning enough money, not helping the father and mother, being unable to hold a job or help the community and not paying taxes.

Lin: I asked this question because I think that the kind of sin or guilt complex that we see in Christian communities associated with the doctrine of original sin is very difficult to find among the Chinese. When we widen our definition of sin and guilt to include failure in fulfilling the duty to parents and ancestors, then I think we do see guilt complexes among the Chinese depressives. Among the more educated Christian Chinese, however, we find depressives with the

western type of guilt complex. When we studied the aborigines in Taiwan, we found many manic-depressives among them with this non-western type of guilt complex. I think that our previous idea that oriental peoples do not suffer from guilt complexes should be reconsidered in the light of these findings.

Caudill: Dr. Yap, do you have any data on birth order in your cases?

Yap: No.

Caudill: Certainly in the Japanese material we find more first-born than last-born males among our depressives.

Yap: This is not surprising, because in the Confucian system the eldest son has heavy responsibilities especially when the father dies. I had an interesting case recently where the patient was a doctor who had to treat his father in an emergency in his last illness and failed to save his life. My patient became very depressed but he could find no solace in the Confucian rites of mourning because he was too westernized and too deracialized—he qualified in Edinburgh—to derive comfort from them. For this man belief in an after-life where he will be able to make amends for fancied wrongs and where there may be some interaction with deceased persons is no longer possible. This is probably because the impact of a rational education has deprived him of his belief in the meaning of Confucian rites of mourning.

Murphy: Dr. Yap, have you observed any difference in the responsiveness of your manic-depressive patients to external circumstances, for instance a change in the family situation? It is my impression from studying the literature that, whereas in the West most depressions run their course irrespective of what happens in the environment, in the Chinese and especially in the Indian, the depression may disappear in a few hours with news of a change of situation. Is this true, and if so how do you interpret it?

Yap: Certainly we get our share of reactive cases—I am using the word reactive in the Anglo-Saxon sense—and my impression is that these cases occur mostly among economically deprived and therefore poorly educated people. These cases frequently show hysterical reactions and one often feels tempted to call them hysterics because the psychogenesis of their symptoms is so clear. Whatever this controversial word hysteria actually means, the psychological mechanisms

in these patients are easily identified. This applies especially to certain cases of so-called reactive mania where one might easily diagnose a hysterical reaction with manic features. These patients are very responsive to external circumstances and improve with a change in the circumstances of their lives. Of course, many of them are poor and do suffer from real difficulties, so that their illness is not necessarily a culture-influenced complex arising from early childhood but may be due to real external difficulties.

Transcultural Psychiatry
A.V.S. Deuck & Ruth Porter

HOUSING AND MENTAL HEALTH IN DEVELOPING COUNTRIES

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IT was a practical problem which aroused our interest in this subject. Many of the hypochondriacal patients under treatment in our clinic apply to us for recommendations that will enable them to acquire new flats. These patients complain that their mental condition is influenced by their poor housing and they are convinced that this will improve with a change of abode.

We decided to investigate this problem from three angles:

(1) The literature on the influence of change of abode on mental condition.
(2) Man's attitude to his home.
(3) Study of our own cases.

STUDY OF THE LITERATURE

Grootenboer (1962) demonstrated a connexion between housing conditions and behavioural disturbances. Before the second world war young couples started their conjugal life in a separate flat away from their parents. Due to a shortage in the post-war period young couples had to live with their parents. In several cases the removal of the young couple to a separate new apartment relieved tensions between grandparents and parents. On the other hand it was frequently found that the first child born to a couple during their stay with the parents served as a kind of reminder of this unhappy time. This child would find difficulties in adapting

itself to the new home and would yearn for the coddling and attention provided by the grandparents. As a result of the parents' rejection of the child in whom they saw a remnant of their past sufferings, and because of the inaccessibility of the grandparents' love, behavioural disturbances developed in the children observed in this research which found their expression in the home and at school. In such cases a change of abode reduced parental tensions, but created mental problems in certain of the children.

Martin, Brotherson and Chave (1957) found a negative relationship between housing conditions and mental pathology in a study from England. It was found that the number of psychiatric hospitalizations in a new housing estate was from 23 per cent to 74 per cent higher than the expected number when compared to the average number of hospitalizations in England and Wales. The greatest abnormality was shown by the fifty-five and over age group. Women of forty-five years and over, who made up only 13 per cent of the population of a development town, formed 55 per cent of the admissions of the psychiatric hospital. Martin concluded that change of abode had constituted a serious psychological stress and caused mental breakdown particularly in the case of aging women.

An additional finding in this study was that the rates for neurosis and ill-defined nervous conditions from records of general practitioners greatly exceeded the rates found by Logan (1953, 1955).

Mention should be made here of the work done by Tyhurst (1951) which stresses the appearance of depressive-paranoid patterns after a change of abode and after a change in social strata.

A most impressive piece of research by Wilner and co-workers (1962) investigated a thousand families (four hundred experimental families and six hundred control families) and examined the influence of change of abode upon the general and mental health of the subjects. The investigators interviewed the families eleven times over a period of three years and one of the interviews took

place before the move. One of the most outstanding findings of this work is the discovery of a rise in morbidity in the under-twenty age-group during the first five months after the move. A further finding was the even higher morbidity in the thirty-five to thirty-nine age-group, which was, however, found to be due to a small group of subjects who had suffered from chronic diseases before the move. This work teaches us not to draw hasty conclusions from the effects of moving. General and mental health is liable to change after the move has taken place. The authors close their work with the statement that no drastic changes take place from a statistical point of view, on moving house, but that there is a tendency to improvement in general and mental health after a period of one year.

This is not a comprehensive review, but is intended to emphasize various aspects of the problem of the connexion between change of abode and mental health.

MAN'S ATTITUDE TO HIS HOME

What do we know of man's attitude to his home? The information at our disposal is drawn from various sources including psycho-analytical research (Abraham, 1924), projective tests (Buck, 1948), and phenomenological studies (Langeveld, 1953).

Man's first home is his mother's womb. From the day of his birth to the day when he enters his own dwelling as a grown man, there unfolds a long series of developments. On the one hand the child is bound to, and dependent upon his mother, whilst on the other hand he feels the urge to break away from her, and to experience his own individuality. This development covers the differentiation of the self from the object world and reaches its peak in the successful solution of the oedipus situation, when the male child finds himself capable of discarding the mother as a sexual object.

Langeveld, who describes this development from the pheno-

menological aspect, draws our attention to the child's need for a hiding place. Children love to hide in cupboards, and there is no doubt that this lure of the cupboard represents a regression to the mother's body, and the need for privacy, at one and the same time. Importance is also attributed to the look-out which allows the child to participate in activities and occasions from which his age should preclude him. The author stresses the stages of development in this need of a hiding place in the life of the child. The tiny child is satisfied with his place behind the furniture or the curtain where he can be alone yet close to the rest of the family. The school child isolates himself further afield and creates a place for himself in the garden, in the woods, and elsewhere. In adolescence, boys and girls find such hiding places unsatisfying and desire a private room of their own. The adult lives in his own house and does not relate to it purely as a place of abode, for this place is also significant as a status symbol.

This development applies to children of western cultures, which stress the importance of the child's independence. We have insufficient knowledge of the function of the home in the development of the non-western child who has been reared in a large extended family where no importance is given to independence and individuality.

The projective tests which were brought into daily psychological use by Buck (1948) emphasize the fact that man expresses himself in the drawing of a picture of a house. In the house-tree-person test, the house represents femininity and the tree masculinity. Tree and house also represent father and mother or the masculine and feminine aspects of man himself.

ILLUSTRATIVE CASE REPORTS

I will now describe some cases from our clinic which illustrate certain aspects of the problem of housing and mental health.

(A) Simcha, A., female, aged 40 years, born in Morocco, referred

to our clinic for mental tension, social withdrawal, and hypochondriacal complaints. The patient is dissatisfied with her present home. During the intake process it emerged that she had moved to this apartment at her own volition, in order to avoid contact between her daughter and a neighbour of the previous home. When, however, her daughter persisted in this relationship with the man of her choice and indeed became his wife, our patient grew exceedingly dissatisfied with her new home. This had disappointed her as she had expected it to prevent her daughter's undesirable marriage. It had also separated her from her relatives who lived close to her former home.

(B) Sason, E., male, aged 40 years, born in Iraq. The patient is of medium intelligence with antisocial behaviour, referred to our clinic for depression. Preliminary talks revealed that the patient's family consisting of himself, his wife and six children, existed in wretched living conditions. The wife endured great hardship, as all six of her children had had rheumatic fever and her husband was frequently unemployed. As a result of intervention by the Anti-Rheumatic Fever League, this family was allotted an apartment in a residential area on the easiest terms. The patient's wife, the pillar of the family, had been able to bear the burden of her children's sickness and her husband's unemployment because of the proximity and support of her relations; she was broken since moving away from them. The father of the family, a passive and weak personality, was sent to the clinic for treatment after the collapse of the wife who had been his support until the change of abode.

This case stresses the importance of the extended family unit in the maintenance of balance between the roles of husband and wife in oriental communities.

(C) Nissim, M., male, aged 32 years, born in Morocco. This head of the family was referred to our clinic for stomach pains, nervousness and excessive smoking. The patient is fourth in a family of ten children, very dependent upon his parents, and incapable of expressing anger—"Eats himself up inside". There

is no verbal communication between husband and wife. In the event of family conflicts the husband flees to his parents who reside in the south of the country and the wife turns to the social service bureau to complain about her husband. The wife is the youngest of seven children and her mother died when she was six months old. The wife has a domineering and demanding personality while the husband is of a passive and dependent nature. When both husband and wife are invited to the clinic the husband sits on one bench in the waiting room while his wife sits on another as far away from him as possible. When they come for a joint discussion the husband is mute and the wife does the talking. The wife makes many demands including different work for her husband, financial support from the social services, an apartment closer to her husband's place of work, and transfer of the family to where her parents-in-law reside. The wife has great expectations of this move, since her father moved from Rabat to Casablanca at a time of great financial hardship and thus improved his family's condition: "change of place, change of luck".

Particularly in patients from non-western communities who have married by family decree and not by mutual choice, we find a tendency to seek family support when friction arises between man and wife. In this present case, the family quarrel was caused by the conflicting personalities of husband and wife. She saw the solution of their strife in the experience of her childhood when a change of abode had brought a change for the better.

DISCUSSION

When we examined the case records of patients who turned to our clinic for help in their housing situation, we were impressed with the fact that not a single family returned to the clinic in order to report the favourable results of the move. If we wish to learn which factors are operating in cases of improvement in the general and mental health condition after moving to a different apartment

and/or a different residential area, we will have to invite these patients for a follow-up study. It is clear from our material, and from Wilner's study (1962) that an evaluation of the effects of housing on mental functioning is a costly and time-consuming enterprise with many pitfalls.

The preponderance in our material of non-western patients, that is those from Mediterranean countries such as North Africa, Iraq, and Turkey, was the second phenomenon to draw our attention. We suggest as a speculative explanation of this phenomenon that families of European background know ways and means for improving their housing situation.

In the value system of western immigrants the house occupies a different position from that of their non-western neighbours. In the western family, the home is a place for the nuclear family exclusively and stands also as a status symbol. Emphasis on hospitality and food are predominant in non-western families and housing itself seems to fulfil a secondary need. Of course these categorical statements do not hold in our present society influx, in which many non-western families pattern their lives according to European standards.

In this paper I have not paid attention to the importance of the neighbourhood, to moving within the neighbourhood and moving to a different residential area. There are many studies on the influence of slums and crowding on mental health, criminal behaviour and the like, but these are mainly of a sociological nature and would not add much to the solution of our problem of the vicissitudes of housing and mental health.

Study of our cases showed us that unrealistic expectations characterized the problem of Mrs. Simcha. She did not suffer from crowding or a bad neighbourhood, but regarded the move as a last resource to solve the problem of the undesirable relationship of her daughter and a neighbour. After the move proved unsuccessful the patient became very dissatisfied with her new apartment which was far removed from her close relatives.

We learned from Mr. Sason's case that in recommending rehousing it is not just one factor such as the children's health which should be taken into account. A thorough study should be made of the family situation as a whole including the relationship of the head of the household and his wife and their specific needs, such as closeness to parents and relatives, proximity to the husband's job, and school for the children.

The last case, Mr. Nissim, emphasized the importance of the extended family but also underlined the role of childhood experiences, which determined the expectations about moving. When things went badly, the wife recalled a similar situation in her childhood, when her father had decided to move to a different city and conditions had improved.

In reviewing our cases we find that we do not know which factors in the move to a new apartment favour improvement in the mental state.

Particular aspects of this problem which seem to be of some importance and which might usefully be assessed in future studies are:

(A) The motives which lead a patient to apply for a recommendation for relocation.
(B) The patient's attitude towards his present living conditions: advantages, disadvantages and unrealistic attitudes.
(C) The expectations he has about the new apartment.
(D) The dynamic structure and the social and ethnic affiliation of the family of the applicant and the differential needs of the various family members.
(E) Living conditions in childhood and the significance of previous moves.
(F) Attitudes towards parental figures of the applicant (especially mother).
(G) The place of the house in the value system of non-western patients.

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DISCUSSION

Wolfenstein: The idea of the house as an influence in mental illness is a very interesting one. We are not talking here about the realistic aspects of housing but its excessive valuation. I think that this symptom may be due to an inability to form good object relations, so that material things have to be over-valued. An example of this phenomenon occurs in hypochondriacal patients who are probably suffering from an intensification of narcissism which fits in with their reduced capacity for object relations. All Dr. Hes' patients seemed to be in poor relation with their intimates, particularly the woman who moved because of her daughter's friendship with a neighbour.

We have found that in families where children have lost a parent if, at the same time, they lose the house they may show a tremendous nostalgia for this, and may, indeed, feel permanently homeless. I have an adolescent patient who has become a kind of "beatnik", she is just drifting and going down in the social scale; she is homeless because she has lost the house where she used to live when her father died. I think this is also related to J. Robertson's work (1958. Young Children

in Hospital. London: Tavistock Publications). He reported that if children are in hospital for a long time they become "thing-orientated" instead of "person-orientated". When their parents come to visit them, at first they cling to the parents and long for contact with them, but later, if they stay in the hospital long enough, when the parents come they just say: "What did you bring me?" and they want the things, not the persons.

Caudill: Dr. Hes, did you study the question of sleeping arrangements in your cases, who sleeps by whom and so on?

Hes: Everybody sleeps with everybody because housing is so short; most of them sleep all in one room.

Caudill: I am interested in this because I have studied sleeping arrangements among the Japanese. In Japanese hospitals, the public as well as the private ones, many patients have a *tsukisoi*—a twenty-four-hour attendant—who, in the private hospitals at least, is in a one-to-one relationship with the patient and sleeps next to him at night. These attendants are always women whatever the sex of the patient (Caudill, W. [1961]. *Amer. sociol. Rev.*, **26**, 204). Sleeping arrangements in the home are also very different in Japan from those generally found in the Anglo-Saxon West. For instance, I worked with a 27-year-old female patient, who had had a sad affair with a man at work and ended up with a severe hysterical reaction; she was a hospital in-patient during the week and went home at weekends. Her father would say: "Well, it's cold where you are, why don't you come and sleep beside me?" And she would do this. If this occurred in the West, we would probably regard the emotional make-up of the father as abnormal but it is not viewed in this way in Japan. David Plath and I are exploring the question of who sleeps by whom in about three hundred, urban, Japanese families at the moment but this work is unpublished. Japan is, at least by Western standards, an overcrowded country and one might suppose that people sleep together because of overcrowding, but we have not been able to confirm this. We split our material into high (one or more sleeping rooms available per person) and low (less than one sleeping room per person) density families, and we found that even when there was no overcrowding they still all sleep together. The mother, father and baby sleep in the same room, and when another child is born then he usually sleeps in the same room,

too. This is because of preference, not because of crowding, and is a very different kind of arrangement from that found in the West. There are possible mental health implications here, for if the nuclear family splits up for any reason they split so that parent-child pairs sleep with each other rather than splitting on generational lines with the husband and wife in one sleeping space and the children in another. Almost nobody sleeps alone in Japan. I think this picture gives some indication of the emotionally "asexual" environment of the Japanese home (Caudill, W. [1962]. *In* Japanese Culture: Its Development and Characteristics. Chicago: Aldine Publishing Company).

In spite of all this co-sleeping, under conditions which in the West would be considered as crowded, we found that quite strict social structural lines were drawn around who sleeps by whom. Nuclear family members slept together, but they did not sleep with extended kin; extended kin slept together, but they did not sleep with non-related household members such as maids and employees and so on.

Mead: I think the recent interesting work on environmental imprinting, so-called, is relevant here (unpublished data). Baby chicks, for instance, have been put, immediately after birth, in a highly-patterned, coloured space and then taken out again. Several days later the chicks were given a choice between that and other sorts of space, and it was found that a significant number chose the physical environment which was present immediately after birth. This material might be related to the observation that Swedish emigrants may travel all the way from Sweden to Minnesota to find a physical environment similar to the one they have left so as to continue living the sort of life they are used to. In studies on refugees and immigrants it has been suggested that there may be an actual physical configuration which people need. Part of this is the man-made house environment, its shape, size, lighting, and general spatial configuration, but the wider environment may also be involved. It may be important for you to hear the sound of the surf or of running water, to live on a mountain, in the lee of a hill or on a plain where you can see in every direction. This sort of thing may be one of the factors causing disorientation and breakdown in refugee and immigrant situations, and it is worth further study. There is, of course, a difference between these two situations, as the refugee, because of political and economic considerations, usually

cannot choose where he lives, whereas the immigrant can make a relatively free choice because he pays his own way. The refugee and immigrant problem is a very important one in the world today, as the mental health of millions of people is involved. There has been a tendency in psychiatry to emphasize interpersonal aspects of the situation and to minimize the importance of non-interpersonal, ecological factors, although these may have almost equal psychological importance.

Lewis: Does the work on imprinting suggest that a child born in hospital, in a highly sterile delivery room and whisked away from its mother each night, will later differ considerably from a child born at home?

Mead: There are many suggestions that this is so, but there is such a variety of maladaptive events when a baby is born in hospital that it is at present impossible to isolate them (Mead, M. [1949]. Male and Female, p. 265. New York: Morrow. [1955. Reprinted, p. 198. New York: New American Library]). There is the question of anaesthesia, local and general, and work during labour against the force of gravity as well as the lights, the hygiene and the separation from the mother; there are too many variables to deal with in one answer.

Caudill: I have been studying intensively 30 Japanese first babies and their mothers, and 30 American first babies and their mothers (Caudill, W., and Weinstein, H. [1965]. In preparation). These are well-matched samples from urban middle-class families and the differences in the behaviour of the two groups are already pronounced at three to four months. The differences between the American and Japanese infants are greater than differences between the mothers of the two cultures. The Japanese infant is much more often with his mother, whether she is actively taking care of him or just sitting near him, and when she tends the child she does it more quietly than the American mother. The Japanese mother also touches and holds her baby more than American mothers do. Possibly as a result of this, the Japanese infant at three to four months tends to be quiet and non-active. When he wants something he usually just gives short whimpers and the mother responds to these. These children very seldom cry explosively as American babies do.

Lewis: What do they do when they are in pain?

Caudill: They start to whimper, and almost immediately the mother intervenes. During my observations none of these children was in severe pain, and I suspect that they would have cried if they had been. The American child, when he wants attention, often starts with a loud cry but, even so, his mother may wait for quite a long time before she comes to take care of him. The American infants, however, are much more active, and play more with objects on their own, than the Japanese infants.

Lewis: I infer from what you have said that the main differences in maternal behaviour in the two groups are the quietness of the mother and the rapidity with which she hurries to the child when it whimpers or cries.

Caudill: Yes, these are two differences, but there are also others. For one thing, the Japanese child is in his mother's arms more often than is the American child and for another, because of the "groupiness" of Japanese life, people are constantly around the child immediately after its birth. An American house, in contrast, is constructed differently from a Japanese house so that this difference in the physical environment is related to the degree of separateness or aloneness of the baby, and this is also true for adults.

Lewis: Have you checked the child's response to the different types of maternal behaviour?

Caudill: Yes. When the American mother is with her baby she talks to it more than the Japanese mother does and the American babies respond to this, so there is a positive correlation between high-talking American mothers and high-talking American babies. In Japan there is no correlation whatsoever between the amounts the baby and the mother talk. This is probably because the bulk of the Japanese cases cluster in the low-talking baby, low-talking mother quadrant. Perhaps there is a need for a threshold of talking stimulus from the mother before a mother-baby talking interaction can develop, and this threshold is not reached by the Japanese mother.

Mead: Do you think that there is also a different response to houses in your two groups?

Caudill: Yes. Japanese houses are more open and have fewer and thinner walls than American houses do, which places the Japanese in closer contact than the Americans. Another difference is that one

sits on the floor in Japan which brings the adults down to the level of the children; this does not happen in the States.

Lewis: Professor Lambo, does the Nigerian baby have a lot of attention from his mother and correspond to the Japanese baby in this?

Lambo: As far as I can remember the child in early infancy, in Nigeria, is constantly with the mother and there is a great deal of fondling and taking care of him. In childhood, too, there is very little crying because of the mother's constant anticipation of the child's needs. I think that there are similarities here with the American mother from the point of view of constantly talking to the child and sensory stimulation, and with the Japanese mother from the point of view of being constantly with her baby.

Lewis: Does the Nigerian baby whimper or cry?

Lambo: There is a good deal of crying. The Nigerian baby cries when it wants something, or is in pain, but probably does not need to cry as often as the American baby because the anticipation of the mother is so quick.

Fortes: It is an interesting observation that in most sedentary African societies there is no word for family which is not also the word for house; the concepts of family and house are regarded as the same. When you are familiar with a culture you can walk into a house and read the family pattern and also see the developmental cycle at that particular time. Here the house, the external integument of the family, *is* the family, and the family is being lived through in terms of its interaction with its external integument, the house.

The linking concept between house and family in the Anglo-Saxon world, of course, is "home"; house, home, family are terms which represent different aspects of the same relationship. One of the big differences between Great Britain and the United States is that in Britain we tend to incorporate the "house-home" into "family", so that we have the idea of a "family-home", whereas in the United States the house is like a garment, you outgrow it, throw it away, and move on.

Firth: I have been very interested in this discussion on housing and all that goes with it, including sleeping together. I would like to add some material about oceanic communities. In the community of

Tikopia in the Solomon Islands, in which I have worked, there is not this great focus of the family on the house, except linguistically. In practice, and this surprised me, the people in Tikopia can, to a large extent, just take up their very simple personal possessions, consisting often of only a sleeping mat and one or two bark cloth garments, and move into another house with some relatives. This very easy transition from one house to another seems to me to be something which facilitates certain of their ordinary social arrangements.

I am not quite sure where all these observations lead us. The general argument in this part of our discussion seems to have been that early home conditions may have the same sort of effect on a child as imprinting has on chicks, and that this, in one way or another, may lead to stressful conditions in the personal mental life of the human being as he grows up. Yet I am not quite sure what kind of weighting should be given to the two different sorts of home environment. Is it less stressful to be deeply attached to the house in a way that gives you security, or does it make for fewer problems in adult life if you have adjusted in childhood to your parents moving you and the sleeping mat at a moment's notice? In other words, although the range of behaviour in connexion with the house is fascinating anthropologically, its bearing on mental health is not clear to me. I would like to ask what others here think about the effects of early attitudes to the house on mental stability in adult life.

Caudill: I think that these attitudes do have a direct relationship to mental health, and this is clearly shown when one studies these phenomena within the context of a single culture. In Japan, for instance, the bulk of psychiatric patients are in the 15- to 24-year age group. Japanese psychiatric patients are, on the whole, younger than American psychiatric patients and there are proportionately more late adolescents and young adults in psychiatric hospitals in Japan than there are in the United States. I would relate this to the home situation; the Japanese have their breakdowns at a time when they have to leave home and make a transition from the home into marriage, or the university, or into the occupational world.

I think, as Margaret Mead has said, that we must try to think of the culture as a whole and see how the various pieces are put together. What is an adaptive way of life in one culture may be quite

inappropriate in another. It seems to me that because of the characteristics of the home in Japan, and their effect on the maturing child, a Japanese man as an adult will be comfortable in his occupational life if he can operate in a rather tight, small network where he does not have to take much direct responsibility, and where responsibility is shared among the people in this network. If, however, he moves to the very different culture of the United States, he may break down. Japanese students in America have a much higher rate of emotional troubles than students from many other countries; the expectations in the environment of the United States are very different from the expectations at home in Japan.

Mead: I think we can say that where there is an intricate relationship between the individual, the family, the physical aspects of the house and the environment, this favours stability so long as you stay in this same environment. E. H. Erikson (1950. Childhood and Society, p. 141 [1964. 2nd edn., rev.] New York: Norton) has studied Californian Indians who live in an area where the river and the salmon and so on are locked in. Provided the Indians stay in this environment these factors are all invaluable elements of stability. Choice of a woman as a mate in this group is a function of many intricate, elaborate, beautiful and unchanging arrangements. She is standing there in a house of this colour and dressed in a particular way; she is your wife and you know who she is. This is the traditional behaviour. If, however, you have to move, then the more intricate and special the relationship with family and house and so on has been, the greater is the possibility of derangement. This is most important for the refugee and the migrant.

When we study another level of society to which, in one sense, both Professor Firth's Tikopians and my Samoans belong, the representations of home are so skeletal that you can take them with you wherever you go. We find examples of this in the United States. People who move very often have, perhaps, a single object such as a clock, or a picture or one small blue bowl which can be packed into anything. They move, they put the blue bowl down, they are at home. All the stability and serenity of the familiar environment is combined with its total portability or reproducibility, and I think that this is also true for the Australian aborigine. Father has a shield with which he defends himself and in which he carries the baby; the family has a lean-to and,

although it can be made anywhere, there are very special ways of making the camp. These people are exceedingly mobile and, at the same time, extraordinarily non-productive as far as civilization is concerned. Perhaps we could take these dual elements, the serenity and stability on the one hand, and the mobility on the other, and use them to plan for increasing mental health in the future.

DeVos: The urban Japanese now have portable ancestral tablets made of plastic so they can take the family with them wherever they go!

Wolfenstein: M. Balint has described two types of people (1959. Thrills and Regressions. London: Hogarth Press). I do not know how universal these are, but one type can only feel comfortable surrounded by all his belongings and established with his traditional possessions, and the other carries his life in a suitcase and feels at home wherever he is: sometimes we describe these two attitudes as agoraphobia and agoraphilia. I suppose they both have their hazards, it depends on your circumstances.

Rubin: It seems to me that for the cases which Dr. Hes has presented, particularly where North African Jewry has been transplanted to Israel, the mechanical act of moving may be a token of a deeper uprooting. Dr. Hes, have any studies been done on this uprooting of extended families, or whole ethnic communities, to new housing areas? How do you think the findings here would differ from your findings for individual family moves?

Hes: I do not know of any such studies. The Yemenite community, about 60,000 persons, was almost completely transported from the Yemen to Israel, but scattered all over the country in many different villages and kibbutzim and other housing situations. A few ethnic kibbutzim were established but I do not know of any studies on them.

Mead: I think enough work was done to establish and recommend that if the Yemenite groups could be left together in their original native communities they were less likely to break down. Certainly there was less trouble in the Yemenite kibbutzim than there was with scattered Yemenites in Tel Aviv. The incidence of delinquency, runaway wives and so on was much less in the kibbutzim than in Tel Aviv. Isn't that right, Dr. Hes?

Hes: I am not sure about that. Immediately after the 1948 resettle-

ment the Jewish agency tried to introduce different kinds of communities into a single settlement; this was extremely unsuccessful.

Margetts: Perhaps a little philology might be of interest here. "Ecology" is a word that we have heard today. It means a study of organisms in relation to their environment and is derived from the Greek, *oikos* which means house, and *logos* which means discourse, indicating that man relates to the environment from his house. The same roots are also the basis for the word "economy", which in commerce signifies a money system and in biology an aggregate of functioning organs; for example, we use the expression "bodily economy". This root is also interesting psychiatrically because of an archaic psychiatric term, "ecomania". This is a type of mental derangement which is characterized by perversity or uncontrolled ill-temper in one's domestic relationships!

Hallowell: I have studied the Canadian Indians, although not under completely aboriginal conditions. The Ojibwa Indians, for example, used to live in multiple family dwellings in which individuals were associated together as an operating domestic unit. You can learn from these dwellings a great deal about the people themselves. I think it is clear, although this has not been recorded, that what the dwelling does is to establish spatial boundaries that are essential for the operation of a social unit. This unit does not have to be named, for it is known that here is where you share food and live together and so on. The dwelling here is simply one way of defining, in terms of a spatial boundary, the operation of a social unit of some kind.

Fortes: One can go further than that for Africa. In sedentary Africa the dwelling is not just a spatial boundary, it is also an objective symbol of the identity of the family; this is "we" in a very special sense. In the Tellensi tribe in Ghana, the first totemistic mode of differentiating social groups into units and sub-units, or clans and sub-clans, is in terms of whether the house is painted white or black or has a straw roof or a flat roof. The whole course of development of the woman, through stages of increasing autonomy from young bride to mother to grandmother is in terms of the spatial allocation made for her within the total dwelling space. Conflicts, struggles and emotional difficulties arise over the right moment for a woman to be recognized as having the status of mother of a family or grandmother and so on. This is worked

out in terms of the external domestic arrangements for house-space and in this sense the dwelling is much more than just a boundary.

Lewis: How does this relate to nomads such as the Australian aborigines who run up their lean-tos wherever they happen to be?

Fortes: My remarks referred to sedentary people. I was most interested in Professor Firth's material. In a sense he is talking about a community in which there are so many ramifications and filaments of kin connexions, and these cover such a wide area, that I suppose these people feel themselves to be at home wherever these filaments reach.

Murphy: I wonder if there is any connexion between the degree of attachment to the home as a dwelling and the dynamics of family relationships. Dr. Caudill mentioned that in Japan mental breakdown occurs especially in young people, presumably at the time when they leave their family. Could this be related to the relative strength of the child-mother attachment in Japan compared with the attachment to the house? In the Singapore Malays described by Judith Djamour (1955. The Family Structure of the Singapore Malay. London: Colonial Office), there is also an intense child-mother attachment which persists well beyond the latency period. The importance in adult life of the parent-child relationship is probably greater than the importance of the relationship between spouses. The Malays present a similar picture of mental disorder to the Japanese in that they normally exhibit very little in the way of mental disturbance until they have to leave the maternal home and establish a new home of their own. It seems possible that the actual physical home may be felt as an extension of the mother in these cultures, and so it assumes great importance in situations where the child has not, at an earlier age, been able to free itself from the mother.

Caudill: I think that the interpersonal relationships in the home are more important than its actual physical structure. The strongest of these, of course, is the mother-child relationship but there are also the wider family relationships through which various conflicts may be acted out. The container for this set of relationships, the house itself, has not so much meaning to the Japanese as to the peoples of some other cultures because most of the houses in Japan do not last a particularly long time. There are a few "stately homes" out in the country

which last for several hundred years and people can certainly become attached to these, but I think that for most families in Japan it is not the house itself but the life that is lived in it that is important.

Carstairs: I notice that nearly all the studies that have been cited have drawn attention to the importance of the home by looking at the situation when you move away from home, although as soon as you do this qualifying factors appear. Margaret Gildea (1959. Community Mental Health. Springfield, Ill.: Thomas), in her study of children showing emotional disturbances, found that when the family moved home—a frequent experience in young American families—the children were disturbed if they moved school as well, but if they stayed in the same class, with the same friends, Gildea could not detect emotional disturbance. J. W. Douglas, in this country (1958. Children Under Five. London: Allen and Unwin), has shown rather similar responses in children whose mothers were taken away. If the child was left in a familiar setting this compensated for the interruption of the relationship with the mother.

I am also reminded of the fact that nearly all attempts to show the positive effect of putting people into better homes have had negative results. D. M. Wilner and R. P. Walkley, in their study of relocation in Baltimore (1963. *In* Effects of Housing on Health and Performance, chap. 16, ed. Duhl, L. J. New York: Basic Books), were able to show unequivocal improvement in the incidence of infectious diseases in young children, but they found that minor dissatisfactions such as complaints about the neighbours were just as common in the moved families as in the control group.

There have been three major studies on this subject in Great Britain (Hare, E. H., and Shaw, G. K. [1965]. Mental Health in a New Housing Estate. London: Oxford University Press; Martin, F. M., Brotherston, J. H. F., and Chave, S. P. W. [1957]. *Brit. J. prev. soc. Med.*, **11**, 196; Taylor, S. L. J., and Chave, S.P.W. [1964]. Mental Health and Environment. London: Oxford University Press). These studies reported surveys of the incidence of psychiatric disorders in a dormitory housing estate near London, in Harlow New Town, and in old and new parts of Croydon, a London suburb, respectively. Their authors were unable to show any significant reduction in the frequency of minor psychiatric disturbance in their samples, even

though, objectively, the physical surroundings were improved and, for Harlow, even the social setting appeared to be better. These findings raise the possibility that when you move into a new house in a new community you may change your expectations and the demands that need satisfaction. It is possible to be just as dissatisfied in good material circumstances, and even in good social circumstances, as in poor ones once your expectations for a better level of existence have been kindled.

Leighton: In our Stirling County study (Leighton, A. H. *et al.* [1965]. *Sci. Amer.*, **212,** 21) my colleagues and I have been following a community consisting of a group of about thirty families which has changed in its style of housing over a ten-year period from what was essentially a slum to a reasonably middle-class type of area. With this change there has been a marked improvement in various aspects of mental health, including a reduction in psychoneurotic and psychosomatic symptoms, and particularly in sociopathic symptoms. The difference between this community and those mentioned by Professor Carstairs is that the group in Stirling County has accomplished this change for themselves, whereas the other groups have had the changes imposed on them.

Lin: In my epidemiological research on mental disorder in Taiwan (unpublished), I think that my findings suggest that there is some relationship between certain *external* environmental factors and mental health. I do not think, however, that we should limit ourselves in our discussion to the absolute degree of poverty or education or material comfort involved, but we should also look at subjective reactions to environment. I agree with Professor Carstairs that when you move to a better dwelling your expectation of satisfaction may increase and so a relative emotional deprivation may be the result of the move. In our study we have now started to investigate this aspect of deprivation as it relates to social mobility and housing.

Hes: I would like to mention the question of changed expectations after a move. The few cases of mental disorder that we have been able to follow up in Israel have shown no improvement after the move but have been characterized by a repetition of the original stressful situation. In Israel you cannot rent a house, you have to buy it, and in order to improve your house at all you may have to buy a new one. As a

result, the head of the household gets into debt, so he has to work harder and he often has to take on an extra job after his regular work is finished; or he may have to take work away from the family to pay for the house, so that the mother is deprived of psychological support and shouts at the children, and father is not at home and so the children get out of control. In fact, although the housing has improved, the total situation has deteriorated and I think that this may partly explain why we have seen no improvement in our cases.

Dr. Mead, is anything known about the psychodynamics of hiding behind a curtain or behind furniture in non-western cultures? It seem to me that this behaviour may be an early stage of looking for your own apartment and for your own living space.

Mead: In children's play, the various forms of "peek-a-boo", "hide-and-go-seek" and "now you see it now you don't, now you're here, now you're not, you're hidden and you can't be found", occur all over the world. I suspect that this type of game is related more to the early mother-child relationship and to a child's early autistic behaviour than it is to housing.

Hes: I am sure that this is really the same subject whether we are overtly talking about houses or about the mother-child relationship.

Mead: I do not agree. I think that there is something about houses that starts perhaps with caves and trees, and not with wombs. One aspect of the house is that it is an extension of the mother's body, I agree, but the other is that it is a hole into which you crawl to get out of the rain. I think that these two ideas are separate although they tend to be muddled.

Lewis: You might call the second aspect "territoriality".

Mead: I think that the external reality of the house has been present for millions of years and is just as real as the mother-child aspect. We should include both in our studies of this subject.

Transcultural Psychiatry
A.V.S. Deuck & Ruth Porter

SOCIAL ASPECTS OF IDEAS ABOUT TREATMENT

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THE treatment of mental disorder is essentially a social process which may be described, for purposes of analysis, as consisting of three principal interdependent elements: a set of techniques, a system of beliefs and concepts, and a system of social relations. This paper does not set out to discuss techniques of treatment, nor does it attempt any direct examination of the ideas and relationships involved in therapy; the aim is to direct attention to what may be called the pre-treatment situation, and to discuss some of the factors which seem to be important influences on people in their search for treatment.

In speaking of a system of relationships or a system of ideas as part of a social process one may seem to suggest that such systems are the material or reality with which the anthropologist is confronted in the field. This is not by any means the case. The reality consists of social situations in which individual people are continually making choices. Decisions to seek treatment are thus seen as part of a process in which "choice is exercised in a field of available alternatives, resources are mobilized and decisions are taken in the light of probable social costs and benefits" (Firth, 1964, p. 17). For the anthropologist, one of the principal problems here is how to manage the translation from individual action to social regularities. Whether the choices and decisions that people make in seeking or not seeking treatment are wholly conscious or not is irrelevant for present purposes. What I am concerned with is to delimit the alternatives open for action and to examine some of the social factors making for, or associated with, the lines of action followed. In doing so I make use of field material relating to three

somewhat dissimilar societies. Some of this material was obtained in the course of general medical practice in a rural district in south-east Africa; other material is derived from studies in which I have been engaged as an anthropologist working in certain areas in South Wales and among islanders from Tristan da Cunha.

For many psychiatrists the significance of any analysis of decision-making in the pre-treatment situation depends upon their particular professional interests. Those who are primarily clinicians, or whose principal concern lies in the provision of mental health services, may wish to know how this aspect of the study of social action bears upon the differential use individuals are likely to make of treatment facilities. Those whose predominant orientation is towards the epidemiology of mental disorders may be concerned to know how, for example, the concept of social organization, as developed by Firth (1964), might help them to discover the way in which choices and decisions affect the prevalence of psychiatric symptoms; for not only may the process of decision-making be crucial in understanding the way in which "cases" select themselves for treatment, it may also be seen to be an essential part of the process by which people identify particular feelings and behaviour, their own and those of others, as indications of illness rather than evidence of lack of religious faith or of weakness of character.

Both social values and individual preferences are involved in ideas about illness and about doctors and treatment; values and preferences in turn imply the actual or potential making of choices and decisions within particular social contexts. These are among the most important of the many social factors which affect both the number of treated "cases" found in a population and the number of "cases" identified in an epidemiological survey. For example, the particular patterns of behaviour which members of a particular society recognize as the signs and symptoms of mental disorder involve choices. This may well mean not only that observers "choose" (in the sense of identifying or rejecting)

certain signs as legitimate evidence of illness, but also that the sufferers themselves "choose" (in the sense of selecting, maybe at unconscious levels), certain symptoms as those which are known to be acceptable. Here it should be noted that we are concerned with the form into which behaviour is moulded by organizational processes, with the evaluation of such socially moulded behaviour as constituting symptoms or signs of mental illness, and with the way in which the social framework of particular communities enables them to handle disordered behaviour. As well as this we are faced with the notion that the very emergence of particular patterns of unusual behaviour, which may or may not be identified by the people themselves as symptoms and signs of mental disorder, can be related to structural features in the society concerned. Evidence for this view may be found in many instances of rapidly spreading aberrant behaviour under favourable social circumstances, of which the Dancing Mania in mediaeval Europe is a good example (Rawnsley, 1960).

No very useful discussion of people's ideas about psychiatric treatment is possible without also considering their ideas about illnesses of all kinds and about the treatments deemed appropriate. Furthermore, in many if not most societies, consideration of illness and its treatment can only be undertaken in the general context of beliefs about misfortunes and their causation and about the steps to be taken to prevent them or relieve their ill effects. In this connexion it has seemed useful to differentiate between two main kinds of misfortune in terms of the relative importance of unexpectedness (Loudon, 1965). On the one hand, there are misfortunes pure and simple, events which are acknowledged to be the universal lot of man and on the other there are what may be called, for want of a better word, catastrophes. These are events subverting the order of things, events which are unexpected and not easily explainable to everyone's satisfaction by whatever standard interpretations or institutionalized responses are provided by particular philosophies or by individual cultures. In this sense,

in many societies, major mental illness is a catastrophe rather than a misfortune.

Where a single occurrence, such as a case of madness, evokes a number of different and maybe contradictory expressions of belief among informants, and where occurrences which seem to be of the same order of phenomena are not so regarded by the people themselves, the anthropologist is faced with one aspect of what has been called the problem of circumscription, of how most usefully to limit a field of study or discussion and yet be sure that there are significant interconnexions within the field of events demarcated (Gluckmann, 1964). Mental disorder and its treatment, as a focus of conflicting opinions where people are driven to make choices in the evaluation of unusual behaviour and where their actions may be governed by such choices, emphasizes the importance of ideas about accountability.

There are a number of institutionalized expectations involved in what has been called the "sick role" (Parsons, 1952, p. 436). Someone who is ill is not expected to fulfil all the obligations of his usual social roles, though the degree to which he is exempted from them depends to some extent on the nature and severity of his symptoms. At the same time the absolution from social obligations in itself imposes certain duties on the patient. It is taken for granted that if the state of being ill is acknowledged by him as something unpleasant and undesirable it necessarily involves the duty of wanting to get well and of taking appropriate steps to that end. Failure to come up to these expectations may arouse suspicions that he is malingering and thus tend to abolish exemption from usual role responsibilities.

In Western society the border land between sickness and non-sickness is characteristically occupied by a variety of syndromes which may, without further discussion here, be termed psychiatric illnesses. Where people draw the dividing line between sick and non-sick depends on the extent to which they feel that the particular actors concerned can be held responsible, or called to

account, for their feelings or behaviour. Actors labelled as "neurotics" may well be regarded as ineligible for the sick role if people think that they are, or ought to be, able to control their feelings and behaviour. In other cases it is the appropriateness of the sick role rather than the actors' eligibility for it which is questioned. Thus it is often thought more suitable for those suffering from what are generally described as mental or nervous complaints to make an effort of will to change their outlook or circumstances, perhaps with the help of priests or other non-medical advisers, than to seek treatment from doctors. Whether this is so or not often depends, among other things, on the nature of the symptoms and signs presented by the sufferer.

In some non-Western societies the boundary between sickness and non-sickness is hard to define in terms which are easy to employ cross-culturally. To some extent this is because the steps which are socially defined as those appropriate in cases of illness are often substantially the same as those considered appropriate in relation to other misfortunes. Forces which are held accountable for illness are also held accountable for accidental injuries and for other mishaps whether serious or trivial, and in many instances the fact that the same ideas about accountability apply to disease and to other misfortunes is reflected in the lack of role specialization among the indigenous experts usually consulted by sufferers. In many societies, however, the more or less radical social changes accompanying the introduction of advanced technology in recent years have widened the range of choices open to individuals seeking aid in the face of misfortunes. How much or how little this may in fact mean as far as the treatment of mental illness is concerned may be seen from studies carried out in some of the older, so-called plural societies such as New Zealand and South Africa.

SOUTH AFRICA

A number of the issues raised so far in this discussion may be illustrated by considering a minor mental disorder which occurs

among many of the Bantu peoples of southern Africa. The condition appears to be a manifestation of anxiety and may be termed an indigenous syndrome in that it is recognized and named as a specific entity by the people themselves. Among the Zulu-speaking peoples it is referred to as *ufufunyane*, among the Xhosa and allied peoples as *ukuphosela*. Discussions of the social significance of this condition are to be found in Gluckman (1964), Hammond-Tooke (1962), Hunter (1961), Lee (1951), Loudon (1959) and elsewhere. In essence *ufufunyane* consists of attacks, usually recurrent over a period of days or weeks, of hysterical shouting and sobbing, sometimes accompanied by epileptiform convulsions and temporary loss of consciousness and by other symptoms such as transient blindness and abdominal pains. Typically those who suffer from these attacks are young unmarried women, but young men and married women are also sometimes affected. The people themselves attribute the condition to two principal agencies. In most instances it is believed to be due to the operation of magical love potions administered in various ways by rejected lovers. When a man has been discouraged or refused by a girl it is said that he may use "medicines" either to force her to accept him or to cause harm to her in revenge; it is also said that he may similarly use them to damage his more successful rivals in love. In addition, however, it is believed that *ufufunyane* may affect young men who fail to win the affections of a woman through the operation of a state of ritual impurity or *isinyama* (literally "blackness"). This is particularly the case in relation to institutionalized pre-marital liaisons; to fail to establish such a liaison successfully is to be a social failure, an *isishimane*. If a youth remains without a mistress while others of his age-group are accepted as lovers, and if he fails to win a girl after having been medically treated for his ritual impurity, it is said that he may become afflicted with *ufufunyane*.

The two principal agencies believed to be responsible for this condition are in fact special aspects of the two major sources of

most serious illnesses and misfortunes according to Zulu beliefs. One is the operation of magical powers, either through sorcery, the deliberate use of magical substances, rites, and spells to cause harm, or through the involuntary mobilization of witchcraft by feelings of jealousy and hatred; accusations of witchcraft are often made against those whom a person hates or envies rather than against those who hate or envy him. The second major source of illness or misfortune is ritual impurity, which arises either from the breach of certain taboos or from inborn bad blood or, in certain circumstances, from public loss of prestige.

In all cases of illness, but especially in those believed to be due to ritual impurity, appropriate treatment includes, as a first step, the use of emetics, purges and enemata; these are the most commonly used home remedies. It is only later, or in more serious and seemingly dangerous conditions, that sacrificial rituals are employed and indigenous practitioners, such as the *inyanga*, consulted. All these procedures involve the concept of power in one form or another—power residing in specific herbal remedies, or derived from ritual practices, or obtained from "doctors" who have acquired it through spirit-possession. Such concepts are widespread, of power as "a kind of psychic energy which is mysterious, ambivalent as potent for good or evil, and not clearly either personal or impersonal, natural or supernatural" (Emmet, 1958, p. 227). Among the Bantu, and especially among the Zulu, an important repository of power of this kind is found in the so-called "separatist" churches. As Sundkler (1961) has shown, these independent African religious bodies combine certain aspects of Christian teaching with the expression of nationalist aspirations, and build upon fundamental indigenous beliefs about the connexion between health and supernatural forces to create what he calls "institutes of healing". These contrast with the institutes of grace and of the word which are provided by the Roman Catholics and the Protestants respectively. It seems probable that a high proportion of the most dedicated members of the congregations of the "separatist" churches are sufferers from more or less

minor mental disorders; for the treatment of hysterical possession, including *ufufunyane*, plays a very large part in the healing activities of these organizations. Here there has come about an important modification of the general Zulu pattern of beliefs about the causation of disease: medicine is regarded not as the cure of disease but as the cause of demoniacal possession. As a result many if not most of these churches, but especially the powerful Zionist churches, are resolutely opposed to European doctors and to treatment in clinics and hospitals.

There is no doubt that hysterical illness of the *ufufunyane* type is common among the Zulu-speaking peoples; it also seems probable that these disorders are especially prevalent among those inhabiting rural areas and among the 35 per cent of the African population of South Africa who live on European-owned farms. Here it is tempting to look for some association between prevalence and certain social factors which might be regarded as pathogenic. In the Native Reserves, for example, some disruption of social life inevitably arises from the migrant labour system because of which a high proportion of the adult male population is away from home at any one time. Much of the work and many of the responsibilities of the men fall on the shoulders of their womenfolk, many of whom attribute the supposed increase in the prevalence of *ufufunyane* to this fact. On the farms, however, where for the most part men are unable to leave their homes to work elsewhere, the conditions of employment under which farm servants and their dependants are consigned to the land by the systematic control of influx to urban areas imposed by government legislation also leads to anxieties and uncertainties, particularly as the interpretation of labour laws are very much left to individual employers. There is no clear evidence, however, of any connexion between social circumstances in the Reserves, or on farms, and the amount and distribution of minor mental disorders, and without some kind of survey of the general population in such areas any impressions one way or the other must be treated with

great caution. What is clear is that relatively few Africans who do suffer from such conditions as *ufufunyane* ever consult European doctors about them; some of the reasons for this have been indicated. A number of other factors which influence choice of source and type of treatment are reflected in general practice consultation records.

As a means of investigating the prevalence of psychiatric illness the analysis of general practice records in any society is notoriously difficult and in many cases the usual problems of sampling and definition are further complicated by special local circumstances. Some of those involved in, but not peculiar to, South Africa are indicated by referring to one general practice based upon a small township and serving an otherwise largely rural area of about 1,200 square miles with a total population of about 25,000, of which some 21,000 were Africans, mostly living on farms owned and run by Europeans. The only industry of any importance in the area was a factory in the township which employed about 1,200 African men as unskilled labourers. Some of these men were more or less permanently settled with their families in the township or its immediate vicinity; very few had homes on European-owned farms in the district; most African workers were drawn from the large Native Reserves outside the district but within a radius of about fifty miles of the township. Many of these men lived in the town or its environs during the week and returned home at weekends as often as they could. Jobs at the factory were highly sought after and labour turnover was high; such other work so close to their homes as could be obtained by migrant labourers from the Reserves was in general less well paid and of lower prestige than that at the factory.

The general practice records show that during a period of twelve months more than 5,000 Africans received attention or treatment of some kind. Seventy per cent of these patients were men, over half of them factory workers with permanent homes in the

Native Reserves, the remainder including a substantial minority of men living and working on European-owned farms. About 15 per cent of the African patients were women and about the same proportion were young children, in both cases drawn largely from the families of farm workers. From this it will be seen that most African patients fell into one or other of two categories. The factory workers were mostly young men, many of them unmarried, suffering in the main from injuries and minor medical ailments; in any one week well over half such patients first attended the doctor on Mondays and Tuesdays, after the week-end break, in many cases because of the need to provide an explanation for absence from work on Monday mornings and thus ensure continued employment at the factory. These men were required to pay cash for the consultation and for any treatment received; apart from the relatively small number whose condition needed further treatment and who therefore attended for further free consultations, very few of these patients were seen again during the twelve months period under review.

The second main category of patient consisted of individuals who, unlike the factory workers, were more or less representative of the total population from which they were drawn. These were African farm workers and their dependents, persons of both sexes and of all ages suffering from a wide variety of conditions, many of whom were seriously ill when they first came to seek treatment. Most of the farms on which they lived and worked were some distance from the township in which medical care was available and virtually none of them was able to contemplate paying the fee for a visit by the doctor to a sick person in his home, since in most cases the sum involved would amount to more than the whole of an average household's wages for a month. The most important difference, for present purpose, between these people and the factory workers was neither in the distance at which they lived from medical facilities nor in the average wages they received; it lay rather in the way in which the farm worker's choice of action,

and the range of alternatives open to him, in illness as in many other situations, was limited by and dependent upon his position in a legal and economic system.

The bearing of this aspect of South African social structure upon problems of health and disease may be indicated in broad outline by a couple of examples. Some farmers readily provided transport to take sick employees and members of their households to the doctor and often paid the fees for medical consultations and treatment, as well as carrying out home nursing and after-care with varying degrees of skill and devotion. In such instances the employer took upon himself to provide medical care for all Africans living on his land, not just for those who happened to be working for him when they fell ill or were injured. In rare cases even the cost of domiciliary visits by the doctor were paid for by the farmer if he considered such a visit necessary; most such visits were in cases of difficult or prolonged obstetric labour. The crucial point in this and other instances where the employers saw their roles in paternalistic terms was that the critical decisions about whether to obtain medical help or advice, and when to do so, were taken by the farmers or their wives and not by the patients themselves or their own close relatives. Such decisions were of course taken in the light of a whole range of ideas and beliefs, not only about illness and treatment and doctors, but also about their employees and the nature and meaning of particular symptoms occurring among Africans. In many cases such employers, conscientious and devoted to the welfare of their labourer-tenants and their families though they might be, were resolutely opposed to any form of measures in the field of health, education, and religion which might alter the living standards and aspirations of Africans on European-owned farms: such opposition extended as much to the activities of the so-called African separatist churches as they did to those of other perhaps more orthodox agencies. At the other extreme were farmers who did not regard it as their responsibility to provide any kind of medical service for their

workers except perhaps in the case of men injured while actually carrying out farming operations. In all other instances of illness or injury among their tenants and families medical expenses and transport facilities to and from the doctor were a matter for the patients themselves. Time off from work was grudgingly given, especially in the case of a male head of household whose presence at a medical consultation was often essential if the doctor's advice or recommendations were to be accepted. Few mothers, for example, would ever give permission for the performance of a surgical operation on a child without the presence or prior consent of the child's father or of some other responsible adult male kin, a circumstance in which difficulties of communication were only one of the factors which sometimes led to serious delay in starting treatment. In such cases as this the influence of the master-servant relationship upon decision-making in illness, though relatively indirect, was just as important as in those instances where employers concerned themselves more closely in the affairs of their labour tenants.

Among almost all African patients a more or less clear division was made between symptoms or syndromes which were thought appropriate for, and likely to respond to, treatment by European doctors, and those symptoms regarded as indications of "Bantu diseases", conditions usually felt to be outside the realms of competence of any but what I have called indigenous practitioners employing basically traditional methods. Most psychiatric disorders, including *ufufunyane*, tended to fall into the second category; most patients suffering from them who came for treatment to European doctors did so partly as a result of the kind of socio-economic pressures already outlined. Many of them also and at the same time went to consult indigenous practitioners whose methods and approach were generally thought by Africans to be more suitable for dealing with illness where supernatural referents are overwhelmingly important. On the other hand, the fact that such referents are also of some importance in most non-

psychiatric conditions sometimes resulted in European doctors being consulted for psychiatric disorders by patients who had experienced or heard of some dramatic success in the use of western medical techniques of physical treatment. Inevitably this led to occasional disillusionment. The localization and subsequent drainage of a chest abscess led to a number of demands by other patients for similar procedures to be used in treating an assortment of complaints. In cases of mental disorder, for example, some misunderstanding followed the doctor's refusal to apply to the cranium the particular stethoscope widely credited with the power of bringing pus to the surface. Although ready to resort to placebos, most doctors say they are reluctant to charge high fees for the harmless use of what they regard as inappropriate techniques, especially in the treatment of conditions like *ufufunyane* which they feel themselves ill-equipped to handle. By contrast the indigenous practitioner is generally likely to charge higher fees in such cases, reflecting both his own and his patients' beliefs about his special skill in treating them.

One thing will be clear from this necessarily brief account: no very reliable estimate of the amount of psychiatric illness in a local African population can at present be made on the basis of consultation records. With this in mind, the figures for one practice show that an average of about ten African patients a month, over a period of twelve months, presented some kind of psychiatric symptom as their main complaint when visiting the doctor, that is to say, rather less than 3 per cent of all adult African patients seen. About half these patients in whom psychogenic disorders were diagnosed described themselves, either spontaneously or on enquiry, as suffering from *ufufunyane*, or were so described by accompanying relatives. In most cases they were unmarried and under the age of thirty, but otherwise they showed no special characteristics, patients of both sexes from Native Reserves and farms being represented.

TRISTAN DA CUNHA

I now want to turn to a discussion of a population where a somewhat similar form of hysteria seems to be endemic and about which it is much easier to be certain that a more or less true picture of the prevalence of symptoms can be obtained. This is also a population for which complete and accurate records of medical consultations over a period of about two and a half years, from early in 1961 until the end of 1963, are available. I refer to the Tristan da Cunha population for which some account of the epidemiology of mental disorder has recently appeared (Rawnsley and Loudon, 1964). The particular interest of this study for present purposes is the relation which appears to exist between social status, prevalence of psychiatric symptoms, and frequency of consulting the doctor.

The entire population of the island of Tristan da Cunha in the South Atlantic was evacuated in October 1961 and brought to England. At that time the population consisted of 264 islanders and 29 outsiders, mostly officials and their families, among whom was the island's doctor. Two years later, in October 1963, the majority of the islanders returned home, preceded by two advance parties. Most of them remained anxious to get home throughout their period of exile; only 14 individuals elected to remain permanently in England. The present population (1965) of Tristan consists of 270 islanders and about 20 outsiders, the latter being temporary residents. All islanders are descended from fifteen original settlers from overseas, including men and women from Britain, Holland, Italy, South Africa, St. Helena and the United States. There are seven surnames on the island and all islanders are related to one another by ties of blood and/or marriage. There is marked variation in physical racial characteristics, from those who are wholly Caucasian in appearance to those who show clear signs of Afro-Asian ancestry. The people themselves are eenly aware of these differences, placing high value on the possession of

fair hair and pale skin, and they are acutely aware of the importance of racial prejudice in the outside world, especially in the country nearest to them, South Africa, which is 1,500 miles away. Many of them share these prejudices and from time to time attribute behaviour and character to supposed racial origins. Throughout their history, which starts with the first settlement of the island by some of the ancestors of the present population in 1817, the Tristan people have been very conscious of their dependence on the outside world for all but the barest necessities of life. One of the most important sources of assistance has been the Church of England. In the last century a number of Anglican priests served as missionaries on the island, and since 1921 there has almost always been a resident clergyman. Most of the people are devout churchgoers and all but 25 of them are Anglicans; this small minority is Roman Catholic and the immediate descendants of one woman.

From time to time in the history of the community one or other member has achieved prominence as spokesman, often by virtue of literacy and consequent ability to maintain communication with the authorities and with charitable organizations in England, but there is no form of indigenous structure of authority. The islanders declare their antipathy to any leadership from among themselves, though there are certain clear contexts in which leadership is accepted and expected. Examples of acceptable leadership include the elementary family, within which the male head exercises authority over all members of the household, and the island long-boats, each of which has a crew led by a coxswain whose authority within the boat is absolute. In recent times high status has been achieved (and in some cases reinforced) by appointment to the permanent staff of a fishing company which opened a canning and freezing plant on the island about fifteen years ago. Finally, high status is associated with membership of the island council, a purely advisory body set up by the British Government. On the basis of these criteria of high status—headship of a household, coxswain in a boat, membership of the permanent staff of

the fishing company, and membership of the island council—it is possible to designate 28 men as "leaders" in the community, the remaining 66 adult males in the community failing to fulfil more than one of these criteria.

To the psychiatric epidemiologist the main interest of the Tristan population lies in the occurrence of two minor disorders, namely, epidemic hysteria and headaches associated with anxiety.

The principal outbreak of hysteria occurred in 1937–38, and was extremely well documented by the doctors of the Norwegian expedition (Henriksen and Oeding, 1940). It began a few weeks before the expedition arrived and was at its height during their stay of four months. The community did not know an expedition was due. Twenty-one islanders were affected. It is possible to identify these cases by name from the details provided in the Norwegian report; 19 of them came to this country following the evacuation and have been studied in the course of our survey. The following quotations from the Norwegian report illustrate the nature of these attacks.

. . . "From the end of August 1937 a series of peculiar nervous disorders occurred among the women like an epidemic. During a dance in the school-house one day in August, a young girl of 21 years fainted and afterwards got violent convulsions, while apparently in a state of unconsciousness. She had several attacks of such convulsions on the same day, and later had lots of spells for a couple of months, after which they gradually subsided to stop completely towards the end of January 1938.

A couple of days later another young girl of 20 got similar spells, and then the disease spread rapidly and in the course of September and October altogether 16 women got similar spells. . . ."

. . . "There were several types of spells, all of which had been given their special names by the people. One of the main types was the 'sleeping spells'. These fits were a sort of fainting. The

women swooned and lay unconscious for a considerable time—up to an hour or more . . ."

. . . "Another main type was called 'fighting spells'. These were much more dramatic. They usually started with fainting, often also with a cry, and then turned into convulsions—first a few jerking movements of the hands and feet and then more and more violent convulsions of a quite irregular type, of the arms and legs and the whole body. When the spells were fully developed the women hit with their arms in all directions, kicked with their legs and bent and twisted their bodies. They seemed to be in a state of violent fury or the utmost excitement. These young, slender girls demonstrated a surprising strength during the spells, and three or four strong men had to hold them lest they should get hurt . . ."

. . . "Finally, the third main type was the 'choking spell'. During these spells the women were conscious, or nearly so. They complained of a feeling of choking, and made violent movements, apparently in order to get air . . ."

. . . "The influence of the example was also obvious, as we observed on several occasions. When some of these young women were together in the same room, e.g. in the church or in the school, and one of them started with a spell, all the others usually went off in a spell immediately. During service in the church or during dances in the school-house it often happened that two or three, or more young girls got spells at the same time and had to be carried out. On the whole the girls got their spells by preference in large assemblies—a fact that fits well with the character of the disease . . ."

We have evidence from documents and from informants that "spells" have occurred from time to time in individual women since 1938. These attacks have not been confined to the original group of patients. As far as we know, the outbreaks have been sporadic, involving either one person or, at most, two or three people. There has been no repetition of the extensive epidemic

of 1937. Since coming to England there have been a number of manifestations of "spells" but no spectacular outbreak on the lines of the 1937 episode has occurred.

The 19 surviving cases with a history of "spells" in 1937 include 4 men and 15 women. Their mean age in 1962 was 47·6 with a range of from 36 to 65 years. The frequency with which they consulted doctors was compared with that of a control group matched for age and sex; the "spell" cases showed a statistically significantly higher mean number of consultations than the controls for a period of twenty-six months, from June 1961 to August 1963. This disparity may indicate a relatively high predisposition of the 1937 case group to a variety of disorders, but the nature of the conditions for which they consulted doctors, among them infective hepatitis, injuries, burns, and strains, suggests that it may really reflect a greater readiness on their part to seek medical advice.

There is an association between the occurrence of "spells" in 1937 and the patient's social position in 1962. Of the 23 women married to "leaders" among the men, ten had "spells" compared with only three from an age-matched control group.

From the 1937 survey the Norwegian doctors reported that six women suffered from typical migraine and six other persons had headaches of a less characteristic type. The population at this time was 188. We were not able to confirm the occurrence of typical migraine, but we were impressed early in the course of our own survey by the high frequency of headaches and by the remarkably stereotyped manner in which these were described. They were bifrontal in distribution, the position often being indicated by a characteristic gesture. They were common both on Tristan and in England and sufferers were accustomed to have them every week or two. Sometimes they were disabling, causing the patient to cease work for a while, but usually they were said not to interfere with life activities. They were not associated with eye symptoms or vomiting and were relieved by aspirin. The

commonest provoking factors were: exposure to strong winds or bright sunshine; menses; and worry. The term "psychogenic" is proposed for describing those headaches where respondents themselves said either spontaneously, or in answer to enquiry, that worry and anxiety were among the principal causes; "non-psychogenic" headaches are those occurring among individuals who specifically denied such an association.

Let us now consider the proportion of males and females with psychogenic and non-psychogenic headache. Fifty-nine per cent of the adult population reported headache of one kind or another, 66 per cent women compared with 52 per cent men. Within the headache group, women showed a higher proportion of psychogenic headache than did men. Twenty-four per cent of both sexes (14 per cent male, 34 per cent female) suffered from psychogenic headaches.

When we look at a comparison between the mean number of consultations of groups with psychogenic and non-psychogenic headache and those with no headaches, we find a markedly higher consultation rate for the psychogenic headache group and this remains significantly higher when compared with the rate for an age/sex-matched control group. Similarly, when we compare the proportion of leaders' wives with psychogenic headache with a control group, we find a significant association between leadership in husbands and psychogenic headache in wives. Finally we find a striking association between the occurrence of spells in 1937 and the prevalence of psychogenic headache in 1962. Fuller details of these findings may be found in the paper from which much of this material is drawn (Rawnsley and Loudon, 1964).

One of the most striking features of the Tristan community is its remarkable homogeneity. Another, particularly important from the point of view of the psychiatrist, is the degree to which this homogeneity, together with purely ecological factors, means that important life experiences are shared by members of the community. In addition, the community is noteworthy for an almost

total absence of overt hostility among its members; informal social control is by means of institutionalized teasing. One is tempted to suggest that these factors, together with certain tensions involved in pre-marital rivalries, may have been important aetiological agents in the 1937 outbreak of spells. What seems clear is that a close connexion can be established between social status, the presence of psychological symptoms, and individuals' readiness to seek treatment from doctors for a variety of complaints.

CHOICE OF TREATMENT: SOUTH AFRICA AND TRISTAN DA CUNHA

In the two examples so far discussed the choices open to people seeking treatment for mental illness have not included as a practical alternative the kind of specialist psychiatric services with which we are familiar elsewhere. This is not to say that such facilities, including mental hospitals and psychiatric out-patient clinics, are not available to members of all racial groups in South Africa, but it was extremely rare for any use to be made of them by African patients from any part of the rural area mentioned. The few exceptions encountered were all cases of major psychosis where the patient's behaviour had led to serious offences calling for intervention by the police or other government authority. On Tristan da Cunha it has never proved necessary for use to be made of the psychiatric services theoretically available in Cape Town, although over the last thirty years a few individual islanders have been admitted to general hospitals there for other specialist treatment such as major abdominal and eye surgery.

Among the African population studied, therefore, and among the Tristan da Cunha people, the choice of treatment for mental illness has broadly lain between that provided by general medical practitioners, who in neither case were themselves members of their patients' communities, and that available from the communities' own resources, including indigenous therapists. Among the Africans, as has been shown, the factors affecting choice are

most complex and include not only ideas about illness and about doctors but a whole range of other influences arising from the South African social system and from local socioeconomic circumstances. On the whole these factors have tended to bring to European doctors what is almost certainly no more than a very small fraction of those Africans who suffer from minor mental disorders. Among the Tristan da Cunha people, however, it appears that many such patients are those most often seen by doctors, although this is not only or even mainly because they seek treatment for symptoms which are predominantly psychiatric in nature.

Both these populations show such special features in their social structure, economy, and environment that it is easy to forget that the kind of influences which seem so dramatically related to patients' readiness or otherwise to consult doctors in South Africa or on Tristan da Cunha are also often of importance in other societies. In the United Kingdom, however, readiness to seek medical advice involves not only the patient's willingness to consult general practitioners but also, in the case of mental disorders, willingness to participate in the process of referral to psychiatrists. Public attitudes to doctors and illness in general, and to psychiatrists and mental illness in particular, are clearly of the greatest importance in this connexion.

SOUTH WALES

In the course of a series of investigations carried out by members of the Medical Research Council's Social Psychiatry Research Unit it also became evident that the attitudes and behaviour of particular persons occupying key roles in the referral process deserved detailed study (unpublished data). Among the most influential of these roles is that of the general practitioner himself.

When the general practitioner and his patients are members of the same community and there is substantial agreement between

them about the nature of mental illness, it is obvious that his attitudes to psychiatry not only influence public attitudes towards consulting psychiatrists but also tend to affect public views about the kind of symptoms about which patients may or may not be expected to consult doctors. It is also clear from one study that the attitudes of general practitioners to psychiatry are crucially important factors in the referral process and cannot be ignored in any analysis of psychiatric morbidity which is based on records of specialist treated cases (Rawnsley and Loudon, 1962*a*; Rawnsley and Loudon, 1962*b*). In spite of practice populations with almost identical socio-demographic characteristics, substantial differences between six general practices were found in one mining valley in South Wales in the rate of referral of patients to specialist mental health services over a period of nine years. These differences were unrelated to variations in clinical severity, major diagnostic category, age, civil state, or occupations of the patients referred; nor could they be accounted for, as seemed possible initially, by a process of selective recruitment by which particular patients, more or less prone to develop psychiatric symptoms, joined the lists of certain practices. The variations seemed rather to depend upon important differences between the general practitioners in the criteria which they commonly used in deciding whether or not to refer a patient for psychiatric advice or treatment. These criteria were largely social in character.

Other studies carried out in a rural area in South Wales, of which only a preliminary account has been published (Loudon, 1965), suggest that variations between socioeconomic sections of the population, both in general practitioners' estimates of the prevalence of neurosis and other psychiatric disorders and in admissions to mental health services, are as much the outcome of doctors' awareness of the social characteristics of individual patients as of purely clinical judgments. Some of the findings of these studies also seem to confirm for this population in South Wales the finding among the Tristan da Cunha people that psychiatric cases,

whether defined as such by the general practitioner or independently by a psychiatrist, have higher mean consultation rates with general practitioners than other members of a stratified random sample of the general population. There is no evidence, however, that members of different socioeconomic sections of the population, which in other respects constitute the important local subcultures, vary in their readiness to seek medical advice; but in all sections of the population women seem to have a significantly larger number of symptoms than men, and have them more severely. Nevertheless, although women are seemingly more sympathetic than men towards minor psychiatric symptoms occurring in others, they declare themselves less ready than men to seek treatment or advice from doctors for such symptoms.

CONCLUSIONS

In recent years a number of authorities have advocated intensive comparative investigations on a limited scale as the kind of study most likely to lead to initial results of value. As Evans-Pritchard (1963) has said, "circumstances where the societies compared have much in common structurally, culturally and environmentally would seem to offer the best opportunity for detailed and controlled comparative treatment, which would be an intensive as contrasted with a statistical study". Advances are most likely to be made when comparisons are not the implicit ones between the anthropologist's own society and the one he is investigating, but rather those explicitly made between two or more societies studied at first hand by the same investigator. There are, however, certain difficulties in trying to examine one aspect of a social process in such widely different *milieus* as those used for my material in this essay. Differences in the methods of investigation used in each case add to the problems of comparison. The difficulties of comparative studies, however, do not only lie in the complexities of the social facts and their correlations or in the shaky methodo-

logical foundations of over-ambitious constructions. Some of us still seem to be hamstrung by our concern with mathematical probability and with statements or assumptions about regularities which too often turn out to be tautologous. Others seem to me to be dazzled by laudable urges to apply comparative research to problems of preventive psychiatry, so that "if ways can be found to reduce the total load of psychiatric disorder, psychiatry can make a contribution toward solving some of the root problems of the conglomerate of socio-cultural systems which cover the world" (Leighton, 1960, p. 81).

In investigating the nature of social institutions we are concerned with the field of moral laws, not with that of natural laws. Within the limits established by principles of social organization the things we are dealing with are social affairs where men and women continually choose and make decisions. It is within this field that I have tried to follow the suggestions of Evans-Pritchard that it is in trying to solve small problems in relation to very limited hypotheses "and not by attempting sweeping generalizations that we shall make progress: piecemeal and little by little, it is true, but firmly grounded in ethnographic fact" (Evans-Pritchard, 1963, p. 24). There is still plenty of room for detailed and careful ethnography before we can make much headway in applying the results of transcultural study of people's ideas about seeking treatment.

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DISCUSSION

Margetts: Dr. Loudon's presentation has centred our attention on peculiar epidemic syndromes with hysterical symptoms. In Africa, for example, these epidemics are seen as possession syndromes and are very similar to what Dr. Loudon has described. I myself have studied one tribal group in central Kenya, the Teita, which showed a highly socialized form of possession syndrome. Grace G. Harris, a social anthropologist, has also studied this tribe (1957. *Amer. Anthrop.*, **59**, 1046). I have not published anything on the subject yet although there are some government documents which describe the syndrome (Margetts, E.L. [1956], [1957], [1958]. Mathari Hospital Reports to the Honourable Director of Medical Services, Nairobi. Unpublished data). I was particularly concerned with the possibility that in-breeding was an aetiological agent in the Teita. Dr. Loudon, do you think that there might be a genetic factor in the aetiology of the epidemic spells in the Tristan islanders, as well as the social factors that you have described?

Loudon: I think I can safely say that genetic factors do not seem to be important. These people were studied in great detail while they were in England and we have a large amount of information about the whole population of Tristan da Cunha, including genetic information. There is no suggestion that genetic factors have had anything to do with the aetiology of these attacks, and certainly not with individuals' susceptibility to spells.

Lambo: Another example of an epidemic of hysteria occurred a few years ago among the tribes on Lake Victoria in Tanganyika and Uganda. I think Professor Margetts witnessed this epidemic himself. Outbreaks of "laughing syndrome" occurred, between January 1962 and June 1964, in which about a thousand people were affected (Kagwa, B. H. [1964]. Paper presented at Conference of Association of Physicians of East Africa. Unpublished). This syndrome is very severe in young adolescents and those affected are nearly always between 18 and 30 years of age; the largest concentration was between 16 and 25 years. The illness at the first outbreak was confined almost exclusively to the pupils of Catholic convent schools (mainly girls). The epidemic started very suddenly and, in all, more than fourteen schools had to be closed. The remarkable way in which the epidemic spread was studied in three girls who were sent home to their villages from boarding school. Their mothers developed laughing disease, followed by their other female relatives, but not their fathers. The epidemic spread through villages like a prairie fire so that the affected villages has to be isolated from the unaffected ones. A team of psychiatrists, including Dr. Kagwa and myself, has been investigating this as comprehensively as possible and has found that the syndrome in some areas spread along tribal lines. Many investigations have already been done to exclude physical diseases such as encephalitis. The spread was in terms of tribal affiliation and the disease seemed confined almost entirely to young girls with a Christian education. It is significant that many of the people chose to take their sick relatives to native healers.

There are two things that interest me about the choice of physician in the African setting (I am now using that word in the widest sense to include traditional healers and therapists). These are, firstly, the effects of differences in cultural background between patient and physician, and, secondly, who actually makes this choice, the patient

himself or somebody else. When I speak of cultural background, I am not thinking now in terms of racial differences, such as exist between an English doctor in Nigeria or in a Bantu-speaking country in Africa and the traditional healers, but in terms of the differences between an African physician such as myself with a completely western medical education and the local therapists. A rural African coming from his village to consult me may say to himself: "Well, this man won't really have any sympathy with me, I will not come next time. I'm wasting my time with him".

The second point of interest is that often the actual decision of what measures are to be taken is not made by an individual patient but by the entire community or the entire family (including the extended segment). When a person becomes ill, whether he comes from a very sophisticated urban home or from the bush, the decision to see a doctor is usually made by the entire household or community, and therefore the patient's own sentiments really do not matter. This is particularly so when the patient comes from a rural background. In some cases where the patient has been forced by, for example, the public health authorities to go into a hospital, "treatment" is also carried on by his own people who consult traditional healers on behalf of their sick relative. I think that this demonstrates the fact that the concept of health and illness is an important part of the social life of the community. Dr. Loudon, what do you think about these two aspects of the problem?

Loudon: I would hate anybody to think that I have been talking only, or even principally, about the individual patient and his choice of physician. I hoped I had made it quite plain that I was concerned with much more than this. I am sure we would all agree that cultural differences between patient and physician are of great importance, particularly as they affect concepts and vocabulary. Here I would draw attention to the work of B. Bernstein (1964. *Brit. J. Sociol.*, **15**, 64) on communication between patient and therapist; this seems a big advance on the purely social class approach to subcultural differences in therapeutic response.

Lewis: To return to the origin of the epidemic in the convent schools in Tanganyika. This form of hysteria is a familiar phenomenon, as in the tragic epidemic at Loudun in France in the seventeenth

century. In many of these epidemics retention of urine was a favourite symptom. Professor Lambo, was this present in your group?

Lambo: No. The illness started with laughing alternating with crying. Some of the patients needed restraint because there was a great deal of motor agitation and restlessness, and finally they became completely exhausted.

Margetts: This syndrome was also referred to as "twisting" in the *East African Standard* (5 October 1952), which said: "Epidemic of twisting nears end".

Firth: I would like to describe an example of epidemic hysteria of which I learned the last time I was in the Western Pacific island of Tikopia, in 1958 (Firth, R. [1959]. Social Change in Tikopia, p. 328. London: Allen and Unwin). I was told of a series of cases of what, as a layman, I might call "frustration syndrome"; this affected adolescent girls who became mute, banded together in gangs and stole their own food. They were rather antipathetic to their parents and those in authority, but they were not actively aggressive unless they were actually interfered with or restrained. These girls went around as a kind of unit of their own, in an antisocial way, for some days. The interesting thing to me was that these were Christian girls, although they were not at boarding school or away from home. There were no doctors there to report on the condition but I enquired about the aetiology. No exact details could be given, though one good Tikopian informant advanced the view that only Christian girls were affected and that it was connected with Lent, for in Lent these Christian girls are not allowed to dance. The local view is that they should really be restrained in their behaviour and this is just too much for them. As a layman I would be most interested to hear what the medical people here think about this.

Loudon: I find the idea of a frustration syndrome very interesting because frustration was specifically mentioned by a number of Tristan people as one of the causes of their attacks. Some of my key informants in 1962–3 were individuals who had suffered from spells in 1937–8 and who themselves referred to frustrations of various kinds as aetiological factors. These people were, of course, talking about something that happened to them twenty-five years before; but according to the Norwegian report one of my principal informants, who was

also a very important source of information in 1937, said then, as she did to me, that she attributed her own attacks and those of some of her contemporaries to pre-marital frustrations. I suggest tentatively that some of the Tristan people, particularly some of the young women, found the social situation on the island extremely difficult. In some ways it is a very prison-like community from which it is hard for individuals to escape, especially if they are women. Perhaps the girls who suffered from these frustration syndrome attacks, if that is what they were, in 1937–8 turned out to be those most likely to marry men who later proved capable of holding positions of responsibility. There may be a selective process at work, involving links between intelligence, sensitivity to social situations and readiness to ask for treatment. One of the important factors which differentiates the Tristan people who readily seek treatment from those who do not might simply be that the treatment seekers are the people most ready to associate with outsiders who come to the island, such as the doctors and the priests.

Fortes: I think that sex must be an important factor in these syndromes. In all these cases, the convents, the Tikopia girls and so forth, the victims' way of life must have changed greatly when they became Christians; perhaps castration is also relevant.

Leighton: Professor Lambo's description sounds like a Beatles reaction without the Beatles!

DeVos: I saw the Rolling Stones arriving in the United States last year. It seemed to me that I was witnessing an hysterical reaction of their fans.

Leighton: I think that the behaviour-response to the Beatles is probably an example of something much more general than the Beatles themselves. It seems likely that they are just the occasion for something which we have seen before, for instance with Frank Sinatra some years ago. Dr. Loudon, do you think there is any connexion between all these different hysterical phenomena and the Beatles?

Loudon: Oh, I think there is, but surely this is a bit hard on Frank Sinatra as similar instances have been described for hundreds of years. For example, there have been some very interesting outbreaks of this kind from time to time in Wales over the past two centuries. One

particularly well-documented incident occurred in Anglesey in the seventeen-nineties, and sounds very like the Beatles phenomenon. It involved, apparently, frustrated girls who went round together in gangs, although I do not think the equivalent of pop musicians were involved.

Margetts: This phenomenon was known in Greek times and has been called *Chorea lasciva*, lascivious chorea. Paracelsus wrote about it in the sixteenth century, and said that the best cure, which rarely failed, was to throw the afflicted ones into cold water!

It is perhaps a mistake to group all these hysterical epidemics together. If we look carefully we will find many differences between the syndromes in different localities. I am not myself convinced that hysteria itself is a psychogenic disorder based on sex, and its cause is not certainly known. It is fashionable nowadays to call hysteria psychogenic, but it may, in fact, be a functional, organic or constitutional illness. Those of us who are particularly concerned with clinical medicine are almost invariably confounded by finding that hysterics fall into a well-defined diagnostic group; it is amazing to me that the symptomatology of this syndrome is so similar in people who have very different backgrounds and who have never observed some of the peculiar reactions that they exhibit. I am thinking particularly of the so-called hysterical fit. I have seen this many times in people who have never seen a fit of any kind, and yet their fits always have the same appearance as true epilepsy.

I do not believe that we should think that these syndromes only occur in the context of a religious environment, or that they are particularly common in young girls. In the Teita tribe, the people most likely to have the *saka* possession syndrome (*saka* means spirit in Kidabida) are the older married women. In the Teita culture, the possession syndrome takes the form of a very highly stylized dance in a circle. It is hard to pick out any girls less than twenty years old and most of the participants are probably over forty which, in such an isolated and unevolved tribe, is approaching senility. There is a great deal that we do not know about all this and the literature is unfortunately full of non-specific, inaccurate and misleading generalizations on the subject.

Loudon: Professor Margetts' point that these hysterical epidemics

are not entirely restricted to young girls is an important one. There is some evidence that the *ufufunyane* syndrome is becoming more common in older married women. It has been suggested that this may be related to the socioeconomic situation involved in labour migration, since this leads to a different kind of frustration among a different category of people.

I should like to try to get away from the discussion of detailed ethnography to a more general issue. I wonder—and I think Professor Margetts was implying this also—whether we are justified at present in thinking in terms of cross-cultural studies in these conditions where social factors are particularly important, pathoplastic influences. It seems to me that this is exactly the kind of situation where we need more studies of single communities before we become involved in premature and misleading comparisons between communities.

Shepherd: I think in this discussion we have rather lost sight of the point that Dr. Loudon made initially about epidemiological methods of investigating how and why patients go to see their doctors. Dr. Loudon has suggested that there is a connexion between this process and the social organization of the population of Tristan da Cunha. I would like to ask him whether this type of connexion has also been demonstrated in the work that he and Professor Rawnsley are doing in South Wales (Loudon, J. B. [1965]. The Social Anthropology of Complex Societies. London: Tavistock Publications, A.S.A. Monograph, No. 4). Here one finds a very different culture, but one which is perhaps more familiar than Tristan or Africa to most of the people who have been doing statistically sophisticated work on the subject. I wonder, too, whether other, quite different and possibly simpler factors may not complicate the picture. We found, for example, in a study of our own that the patient's opinion of his family doctor may be an important determinant of self-referral for minor psychiatric illnesses (Kessel, W. I. N., and Shepherd, M. [1965]. *Med. Care*, **3**, 6).

Loudon: Of course I agree with Dr. Shepherd. I did not have time to discuss our studies in South Wales (Loudon, 1965, *loc. cit.*) at all fully. I think there are a number of simple but very important variables involved in decisions of this kind including, for example, not only what people think of their doctors but what their doctors think of different sorts of illness.

Margetts: Or of their patients.

Loudon: Yes indeed. Professor Rawnsley and I have published one or two articles which mention these topics (1962. *In* Sociology and Medicine, p. 49, ed. Halmos, P. Keele: University of Keele. Sociological Review Monograph, No. 5; 1962. *Brit. J. prev. soc. Med.*, **16**, 174).

Transcultural Psychiatry
A.V.S. Deuck & Ruth Porter

CULTURAL ELEMENTS IN THE RESPONSE TO TREATMENT

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IN primitive society there is much less separation of the two categories of mental and physical illness than we are wont to make. Both forms of illness are recognized, and both are seen as an invasion of the integrity of the normal personality by a malign influence which is most commonly personalized, as god, demon, ghost, witch, or sorcerer. Primitive peoples are familiar with sickness and death, they see more of them at close quarters than we do. They are consequently well aware of the precariousness of the state of health. Any major departure from this state tends to be seen as evidence of supernatural intervention, and arouses much anxiety both in the patient and in his immediate kinsfolk. The primary role of tribal healers is to allay this anxiety by convincing their clients that the noxious agency has been identified, and the appropriate counter-measures taken.

This belief in the immanence of harmful and protective spiritual powers exists side by side with the recognition that certain drugs can also intervene to alter the balance of contending forces. In my own first field work, in the village of Sujarupa in northern India, the villagers' willingness to avail themselves of my stock of anti-malarial drugs, sulphonamides, penicillin and pink aspirin at first impeded my attempts to learn about their own concepts of sickness and healing. My patients would squat before me, as puzzled as I was, trying to reconcile my curious behaviour with their expectations of how a true healer should behave. They were especially disconcerted when I asked questions about their symptoms, or the

history of their illness; in their eyes, a healer who lacked the ability to *know* these things by virtue of his supernatural gifts, was scarcely likely to be able to contend with the spiritual cause of their complaint. It was indeed a village colleague, a man well versed in healing *mantars* or sacred charms, who first taught me that when my patients held up their wrists without my bidding, it was not to let me feel their pulse as I thought, but in the belief that by sensing the movements of the tendons at the wrist I could, like him, determine whether they were afflicted by a ghost, a demon or a witch.

In this village, and the next, I gradually learned the quite restricted range of harmful supernatural agencies and their respective counter-measures. It became plain that the great majority of remedies which I saw deployed were, physiologically speaking, quite irrelevant to the disease process itself. Instead, they were designed to bolster the morale of the patient and his family, and their effectiveness depended upon the authority and conviction which they imparted (Carstairs, 1955). In short, they relied on suggestion, and suggestion is a transaction which requires that healer and patient should each play a reciprocal role, in a familiar yet suitably awe-inspiring setting.

The element of suggestion has, of course, played a large, though not always recognized, part in medical practice in western society also. The family doctor too was an awe-inspiring figure whose role was to put an end to anxious uncertainties by uttering a diagnosis, prescribing a remedy, and giving the assurance that now he had matters properly in hand.

Most intelligent family doctors were well aware of the theatrical element in the treatment situation, and played their part with due ceremony. It should be noted that the theories about the causation of common diseases held by western physicians in the quite recent past have proved in many instances to be scarcely less irrelevant to the actual disease process than a village healer's diagnosis of witch or devil possession. What they had in common was their ability

to allay anxiety in their patients (and perhaps also in themselves) by professing to know exactly what was the cause and what the appropriate remedy for the patient's disorder. Our own medical vocabulary is still stocked with numerous terms which are designed to conceal the actual limitations of our knowledge; diagnoses such as "idiopathic migraine", "proctalgia fugax", "essential hypertension", or "fragilitas ossium" carry authority in the layman's ears because of their esoteric sound. They invoke the belief systems, very widespread in our culture, that all our practical problems are amenable to scientific control, and that most of them are already so controlled. Put very crudely, the physician's power to help the sick has depended, to a large extent, on his ability to persuade them of his omniscience and his omnipotence. The transition from the comforting certainties of magical thinking to the rational acceptance of the limitations of scientific knowledge can be a painful process for the members of developed as well as for those of developing societies.

Today, in Britain, there is a crisis of morale among our family doctors. They believe that in the last twenty years they have lost status in the community, and have lost prestige in the sight of their patients. Ironically, this decline in their prestige has come about precisely at the period when, for the first time in medical history, they have been able to employ a considerable array of really potent remedies. Submitting to the unrelenting pressure of pharmaceutical companies' salesmanship, doctors and patients alike are in danger of indulging their magical thinking nowadays in the constant pursuit of "wonder drugs", of antibiotics with ever broader spectra and psychotropic drugs which claim not only to relieve every known mental illness but also to protect the sensitive from "the heart-ache and the thousand natural shocks that flesh is heir to".†

†In case it should be thought that I am exaggerating, may I draw attention to a recent publication by the head of the Department of Psychological Medicine in a famous London hospital which concludes: "In fact, it seems that if an anxiety

No doubt the manifest ability of antibiotics and new chemotherapeutic agents to cure many formerly mortal infections has contributed to the renewed belief that there is a sovereign remedy for every ill. Unhappily for our general practitioners, the mystique of the new drugs has become associated with the mystique (also inspired by particular achievements of modern medicine) of investigation by laboratory methods in highly specialized hospital departments; as a result, status and prestige are now increasingly accorded to the hospital specialist.

In psychiatry, this trend in public attitudes may work at least temporarily to our advantage because psychiatry in Britain today identifies itself as much as possible with the practice of general medicine. Psychiatrists are happy to be regarded as members of the array of hospital-based specialists, equipped like their fellows with impressive investigations, and able to deploy treatment procedures, some of which are beyond the general practitioner's reach. To some extent, the mantle of the authoritative, reassuring physician has fallen upon the consultant psychiatrist; and yet both those who place their reliance on drugs, or on psychotherapy, tend to react with equal resentment against the imputation that suggestion still contributes largely to their therapeutic successes. Nevertheless, the evidence of controlled trials has shown indisputably that this is often the case for the former groups, and at

state or an 'atypical' depression, particularly of recent origin and occurring in a patient of good previous personality, does not respond within 10 days to one or other of the M.A.O.I. drugs and chlordiazepoxide, correctly given, both the diagnosis and the treatment should be reconsidered." The last phrase at least makes some sense; perhaps this reconsideration would permit an inquiry, however brief, into the psychological causes of the anxiety or depression; but the author intends only that other and more active physical treatments should be initiated in order to re-enliven the patient's mood. He illustrates his argument with the case of a 42-year-old married woman who suffered prolonged depression, accompanied by physical symptoms, after an illicit sexual relationship: "Psychotherapy enabled her to break off the affair, but failed to diminish her symptoms of severe anxiety. Electric shock only increased her tension, but iproniazid, and later phenoxypropazine, rapidly caused the symptoms to abate." (Sargant and Dally, 1962.)

least one sophisticated psychotherapist has pointed out that success with his methods is most likely to occur when the patient comes round to an acceptance of his therapist's interpretation of the cause of his symptoms (Kraüpl Taylor, 1961).

What is happening meanwhile to popular concepts of psychiatric illness and treatment in developing countries? My own experience here has been limited to brief visits to psychiatric hospitals in Colombia, Jamaica, Ceylon, Thailand and Formosa and a rather greater familiarity with what is going on in India. The former series of visits served only to bring home to me how very far most developing countries are from being able to offer a practical and acceptable alternative to the existing indigenous methods of treating the mentally ill. All over the world, at the present time, many more psychotics are being treated by exorcism than by electroshock therapy or tranquillizers. I found one thing in common in the traditional treatment of schizophrenics in each of these countries: they were likely to bear somewhere on their bodies the scar of a branding with red-hot metal, an apparently world-wide element in the procedures adopted to drive out this particular spiritual intruder.

On the other hand, in many Indian cities, psychiatry is beginning to play an increasing part both in clinical teaching in medical schools and in private practice. In Bombay, for example, a psychiatric out-patient clinic began to operate in the King Edward Memorial Hospital in 1949 and in its first year it dealt with two hundred cases. In 1964 the daily out-patient clinics of this hospital dealt with five thousand new psychiatric out-patients, while over two hundred patients passed through their psychiatric in-patient beds. Roughly similar numbers of psychiatric patients were seen in the other major teaching hospital in Bombay in the same year. Evidently, in this city at least, members of all classes have learned that mental illness can also be dealt with at the general hospital. Much of this is necessarily rough and ready practice, with an emphasis on physical treatment. In Bombay, as in London,

psychiatry is gaining acceptance by stressing its resemblance to general medicine.

There are, however, in India as in the West, very many minor manifestations of emotionally-determined ill-health which cannot be adequately dealt with by purely physical methods. On the other hand, the patients are much too unsophisticated to profit by an attempt to make them aware of the relationship between their emotional problems and their symptoms. Here the paradigm (implicit in so many expositions of psychodynamic theory) of an intelligent, educated patient being helped towards a clearer, fuller awareness of his own motives and feelings is wholly inappropriate. These patients are acutely aware of their symptoms. A generation ago, their only recourse would have been to indigenous healers who would at once have made their plight intelligible to them by formulating the disorder in terms of a familiar, communally shared system of beliefs. In my experience of Indian village life, the relevant beliefs were of two main varieties:

(A) An account of physiological processes in the body which might bear little or no relation to the realities of human anatomy, but which was clearly influenced by the values governing behaviour in their community. An example is the belief, reinforced by its universal acceptance, that human semen is produced by a long process of digestion, stored in a reservoir in the vertex of the skull, and preserved intact only if the individual faithfully observes the duties and taboos of his caste. Infringement of these obligations is held to lead to a deterioration and leaking-away of this store of pure semen, and hence to many vague symptoms of malaise and weakness (Carstairs, 1956). This could be understood, in our terms, as a metaphorical explanation of why guilt-laden actions could result in physical symptoms.

(B) Alternatively, and even more frequently, the ailment is explained in terms of one of the locally endemic supernatural agencies, and the necessary propitiatory acts prescribed. Here there may be an element of guilt, if the patient has himself offended

the spirit concerned, but more often he is regarded as the more or less innocent victim of misfortune. The healer, or the priest or *shaman* as the case may be, exercises his authority by virtue of his having a special relation to the world of spirits.

What happens to these expectations when psychiatrists enter the arena? In several psychiatric departments in India, I found colleagues wrestling with the problem of how to make psychotherapy intelligible to their relatively uneducated patients who obstinately persisted in adopting a helpless, dependent attitude and compelled their therapists to take the active role. This helped me to realize that the respective roles of patient and therapist in western culture have not simply been the creation of Freud and his successors in Europe and America: they have been created jointly by a series of therapists *and their patients*. Perhaps a similar joint undertaking is already under way in India, and in many other countries, as doctors and patients seek a common idiom in which to describe the processes underlying the subjective experience of neurotic illness, and agree upon a new paradigm for the relationship between the therapist and his client. I even ventured to suggest that perhaps the model for psychotherapy which will be widely acceptable to Indian patients could be found in the traditional concept of the relationship between a *guru* and his *chela*. This, however, is something which Indians, Africans and Chinese, indeed members of every separate cultural tradition, will have to work out for themselves if psychiatry is to become something other than an alien medical procedure, and play its full part in meeting each community's particular personal problems.

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DISCUSSION

MAGIC, FANTASY AND MYTH

Yap: I was very interested in Professor Carstairs' reference to Indian ideas about semen. From my limited experience of Western patients I think that a similar mythology may exist among some isolated groups in the West; it would be interesting to find out if this is in fact the case. Perhaps the semen mythology has spread from India to the rest of the world in the same way as astrology. One would like to know why an idea of this sort persists. I think that one of the reasons may be that a superstition persists when it serves to gratify certain needs and to provide a point for intellectual discussion about certain crucial problems for people. There will also be sociocultural reasons why this kind of myth is given more importance in some places than in others. In Chinese society, for example, we know that polygamy was the classic tradition and a lot of attention was paid to sexual hygiene so as to enable the husband to meet the needs of several wives in the same dwelling. The published letters of scholar officials to their sons (something like Lord Chesterfield's letters) often contain actual instructions on sexual hygiene.

It is not really surprising, in a race like the Chinese among whom there are queer superstitions about semen and about sexual functioning and prowess, that a clinical syndrome like Koro has appeared. It is a curious fact that the word "semen", in Chinese, contains the same character as the word "mind". This may have served to emphasize the relationship between semen and the psychological functioning of the individual and also, indirectly, the connexion between semen and mental illness.

Lewis: I think Professor Carstairs meant that you always find this superstition in Indians when you look for it, whereas in mentally ill Europeans it is common but not invariable. Dr. Yap, would you say that the Koro belief is invariable in the Chinese?

Yap: Oh, no, but it is more common in China than in the West; it is sufficiently common, of course, for people to give it a name and for us to be able to establish it as a syndrome.

Carstairs: I think Dr. Yap is really asking why a system of beliefs that is, to our way of thinking, demonstrably unsound, can persist for so long. Surely a scientific hypothesis will only persist for as long as it is verified, or at least not refuted, by experiment but a fantasy will persist for as long as it fulfills a psychological need. It, too, can be undermined by the advance of knowledge which refutes it, for then it will cease to fulfil the psychological need and a gap will be created which in turn will be filled with some other procedure.

Hes: I am particularly grateful to Professor Carstairs for his description of the shift of magic. Many of the Yemeni immigrants to Israel have high expectations about in-patient hospital treatment. They are referred and admitted to the mental hospitals as quickly as European patients but in hospital they become increasingly disappointed in their expectations; they see the doctor only once or twice a week and in between these infrequent sessions they are the victims of the occupational therapist and the nurses. Because of their disappointment in seeing the doctor so rarely, they consult the native healers, sometimes while they are still in hospital or on temporary leave from it. This is one example of the way in which the magic of healing has moved. Another change is the transfer of magic into injections. Outpatients do not believe in the power of pills or potions, they just want injections.

Fortes: This is happening in Africa, too.

DeVos: And also in Japan; medicine must be given by injection to be really efficacious.

Margetts: The Swahili word for needle is *sindano*, and treatment in actual practice for uneducated African patients is equated with this word. When we try to learn about treatment in Swahili terms we must think of the word *sindano* and realize that the

patients really believe treatment must be given by injection if it is to be effective. There is a great trade among East African native healers who give injections of water; coloured water is particularly in demand.

Lewis: Professor Carstairs, do you think that your or my activities with empirical forms of treatment are distinguishable from magic?

Carstairs: It is true that we like to think our treatments are rational, but as Sir Aubrey reminds us, this is a conviction which we probably share with practitioners of magic. Our claim can only be justified if, at the same time as using empirical remedies, we take the trouble to submit them to rigorous, critical evaluation.

Wolfenstein: I am glad to hear you comparing the medicine men of our own culture with those of other cultures. This reminds me of Freud's criticism of Adler that he blamed every kind of neurosis on an inferiority complex. Freud compared this to a native healer in Switzerland who would examine each patient carefully and then pronounced the same diagnosis for all of them: "bewitched". The term "inferiority complex" is one that has become widely diffused and all our adolescents discover at some point, when they suffer from self-consciousness, that they have an inferiority complex. This gives them a label but I do not know whether it is anxiety relieving, or anxiety aggravating.

Fortes: I noticed that Dr. Yap used the word mythology for what Professor Carstairs called fantasy. This terminology is very interesting because mythology is really a publicly acceptable form of fantasy. I think that this may be an important point, for if a person can transpose his fantasies into mythology, it may be healthier for him than if they remain as fantasies. This is one of the crucial problems in the relationship between the total outfit of culture and what goes on inside each person. How the culture comes to be incorporated in the individual and works

there is a difficult and crucial problem which I think relates to Dr. Yap's interesting scheme about grief. A person who suffers from grief may descend to depression or he may exteriorize his grief into mourning ceremonies or other socially approved and mythologically and religiously sanctioned techniques. It seems to me that in Africa mental health sometimes depends on whether a person is able to transform his grief into these religiously accepted forms of mourning and let it out naturally rather than keeping it inside himself. Here is a framework for observing the interactions between cultures (beliefs, values, customs and such arbitrary elements) and personality.

As a layman, I find that the views that the physicists put forward about, say, the atom and space are absolutely arbitrary: they are magical to me. I cannot understand them and I cannot connect them with any of my own experiences, I can only see that they are a demonstration of technological achievement. There is always this gap, this factor of the arbitrary, which the layman has to accept, and which occurs in science, medicine, economics, politics, and, indeed, in our whole social life. It is one of the signs of civilization that the gap is increasing. I suggest, therefore, that there is always an arbitrary element, in other words a mythological or fantasy element, in cultural experience. As Professor Wittkower has said, if one can accept the fact that cultural norms are arbitrary, and that there are publicly approved, community-shared devices for getting rid of stress, as described by Dr. Mead and Dr. DeVos, then these may be the kind of mechanisms needed for the advance in mental health towards which we are now clumsily striving.

Firth: I agree that the element of the non-empirical in a culture may be called the arbitrary. The arbitrary has to be accepted in many human relationships and human conceptions, particularly in a modern society; the complement to it is, perhaps, the element of confidence. Because we do not ourselves have the empirical experience, we must place our confidence in those who do

appear to us to have it. I think there is a kind of wide-ranging scale here; at one end there is the empirical which can be fairly easily checked by those who know and transmitted to the rest of the culture or society; at the other there is what we might call the non-empirical, or magical, which there is no possibility of checking. One of the difficulties here is that this field of the non-empirical embraces the religious as well as the magical. Is belief in the Grace of God, for example, magical or non-magical? It is certainly not empirical. This seems to me to link up with what Professor Carstairs was saying about fantasies. I liked very much his treatment of what he called the metaphorical or mythological aspect of fantasies. I would suggest an initial classification of fantasies into two groups: those which are not, in any easily acceptable sense, metaphorically or symbolically related to the situation which confronts the patient and his symptoms, and those which are. The first group contains fantasies of a general nature and the second fantasies which relate directly to the immediate aetiology of the particular case. I am really just trying to put into other words a concept of the aetiology of mental conditions and their surrounding circumstances.

COMMUNITY ATTITUDES TO MADNESS

Fortes: I would like to comment on the changing attitude of society to mental ill health. Many mental asylums today have a prison-like aspect. This is not altogether surprising because there was a time when a lunatic was put in the same social category as a criminal, extruded from society, and kept safe somewhere because he was a nuisance and a danger. I think that one of the greatest positive contributions that psychiatry has made to progress since Freud is the acclimatization of our culture to the idea that psychosis exists everywhere, in every person, and that we must learn to live with it and accept it as part of ourselves instead

of rejecting it. Society is gradually moving towards the same view of criminals and murderers. This acceptance of madness is the rule in simple societies where the people live with it and do not reject it. An intriguing puzzle to me is that this does not have a therapeutic effect. As civilization advances psychotics still exist and their numbers are actually increasing.

Mead: Psychiatric diagnosis is extremely important in the study of contagious, hysterical symptoms in groups of adolescent girls and people belonging to atavistic cults. At the start of these epidemics exceedingly psychotic people are affected but later the syndrome spreads contagiously by age groups or race or religious groups until finally the groups themselves are sufficient to contain the epidemic and it dies away. In Cincinnati we see a number of people from the mountains who are members of rather extreme cults, but cults that have been in existence for long enough to have evolved rules: for example, you should convert other people but not all the time, or you should try to convert most other people all the time, but not your boss. So many different sorts of behaviour are acceptable in these peoples' original habitat that it is possible for even those with rather extreme beliefs not to get into trouble with the surrounding environment. But when they come to Cincinnati, a wide open situation in many ways, we see in the hospitals the ones who try to break away from the rules. The other members of the cult are perfectly able to diagnose that there is something wrong with these people because they have broken the rules, and that they need help. This supports Professor Fortes' point that there are always psychotics, and that in these rather extreme and primitive groups the number of psychotics increases when the groups move away from the place where they were originally contained to another social environment.

Firth: I was reminded by what Professor Fortes and Dr. Mead have just said about the increase of psychotics in civilized conditions, of C. G. Seligman's paper "Temperament and Psychosis in

a Stone Age Population" (1929. *Brit. J. med. Psychol.*, **9**, 187). He in a sense foreshadowed this kind of idea.

RÔLE OF PSYCHIATRIST

Leighton: I would like to say something about the role in which the psychiatrist finds himself. It was suggested in defence of the psychiatrist that it is the patients who endow him with omnipotence and, being human, he likes to try to live up to this image. But I do not think we can escape altogether from our own responsibility in this, both as psychiatrists and as physicians. One of the viewpoints with which we have indoctrinated the public is that each problem is an individual, personal one, and that the only way it can be solved is by an individual psychotherapeutic approach. Any other method (and I think this is what underlies the scepticism about Alcoholics Anonymous) is regarded as superficial (see also this volume, p. 189). We label such methods as concerned only with epiphenomena, and claim that they do no more than sweep the symptomatic dirt under the carpet; that they take care of the symptoms with a temporary psychological aspirin, whereas what is really needed is a complete reconstruction of the personality. The idea that all emotional problems are individual is very general throughout medicine and not just confined to the psycho-analytically orientated. I think this orientation underlies a good deal of the indifference of the medical profession to preventive medicine. Perhaps it has something to do with the fact that the cured patient is far more grateful than the patient who never gets the disease.

Caudill: This idea of individualism is certainly deeply embedded in our values in the United States. I think it might be useful to discuss the relationship between what the psychiatrist often believes is the absolute truth and what is only a reflection of the values of the society in which he is living.

Mead: One of the problems that Professor Carstairs' presenta-

tion has opened up is the question of the expansion or contraction of the role of the psychiatrist. G. Engel (1961. *Psychosom. Med.*, **23**, 18) and others in the United States have suggested quite seriously that grief is an illness and belongs to psychiatry. Engel says that one sees in grief the same symptomatology that, in other circumstances, is labelled depression; therefore it belongs to medicine and to leave it in the realm of the church is obviously inappropriate. The clergy should not have to deal with grief, but the physician should. This is one extreme extension of the psychiatrist's role. Another, which I think is much more serious, is the tendency in the United States to substitute the psychiatrist for the friend. During the past twenty years one can see, in the middle and upper classes, the disappearance of the listening friend, the room-mate who stayed up all night with you in college. This was routine when I was an undergraduate. The stronger, healthier undergraduates, who needed less sleep, sat up and listened to the ones who had to talk. On the whole, the incidence of suicide was not greater then than it is now, when the first time you tell a friend that anything is wrong, he tells you to go to a psychiatrist. The inroads made by psychotherapy on simple friendship have been very great but in this case it is not the claims of the psychiatrist, but the expectations of human beings about what they are expected to do for other human beings, that have brought about this change.

Wolfenstein: I think that undergraduates still talk all night, but these talks mimic analysis more and more—these young people are analysing each other. I have an adolescent patient to whom I can tell hardly anything, because one of her friends has already given her appropriate interpretations.

Loudon: It might be relevant to mention here a recent publication on the use of psychotherapeutic lay assistants, the title of which refers to psychotherapy as the purchase of friendship (Schofield, W. [1964]. Psychotherapy, The Purchase of Friendship. Englewood Cliffs, New Jersey: Prentice-Hall). I think the

Samaritan organization in this country also calls its services "befriending". From this point of view friendship and psychotherapy may both be seen to involve reciprocal obligations, that is, payments and expectations of exchange.

TRANSCULTURAL PSYCHIATRY AS A METHOD OF RESEARCH

Murphy: I am going to try to relate Dr. Loudon's and Professor Carstairs' papers to the relevance of transcultural psychiatry as a method of research. It seems to me that what they have said has emphasized the need for cross-cultural, maybe transcultural, psychiatric methods as a means of searching out the roots from which we can predict how anxieties are allayed in any given society. Many of the transcultural studies that have been done in the past are not satisfactory and one of the reasons for this is that they are too discursive. I think that we should do much more to examine *subcultural* groups and seek out small differences within fairly standard conditions. Irving Zola's is an instructive study on these lines (1963. *Transcult. psychiat. Res.*, No. 14, 62). Zola investigated the motivation of Italian, Irish, and American-Protestant sections in Boston for in-patient and out-patient hospital treatment. He deduced that motivation varies considerably in these three groups and is related to basic value orientations.

Such studies of small groups, whether in the past or in the present, might help us with the difficulty, mentioned by Professor Leighton (this volume, p. 20), that we have in treating our Euro-American lower class patients. It might be helpful to find out why, historically, certain types of therapy succeeded with certain groups. Why did Janet succeed especially with the servant groups? Why was the moral psychiatry of the early nineteenth century mainly successful with working class males? I think that workers in our field should not just confine themselves to descriptive studies in different geographical localities but that they

should also analyse the situations they describe. I do not use the term analysis here to mean psycho-analysis but in a more general way. We should try to reach into the different cultures and see why in one particular culture there is a particular development of magical thinking, whereas in another magical beliefs take a quite different direction.

Firth: I would also like to comment on the usefulness or otherwise of cross-cultural studies. I think that the examination of one particular theme cross-culturally might be more fruitful than some of the gross community comparisons that have been made in the past. For example, one might examine in several societies the theme of community help in therapy. First, however, I think we should try to discover what we mean by the word "community". As an anthropologist I am struck by the differences in the concept as it has been used in the discussion here and its more precise connotation in much of the sociological and anthropological literature. There is a particular difficulty in the application of this word to the circumstances of a modern urban or metropolitan social environment. To an anthropologist a community usually means a system of shared knowledge of the individual circumstances of all the persons in the immediate environment. Because of this shared knowledge one does not need the elaborate psychotherapeutic dialogue mentioned by Professor Carstairs. In a sense this dialogue has been going on in a community day by day for years or even generations. I think that a cross-cultural, comparative study of the meaning of community relationships in different environments and societies (including our own) is needed in order to enable us to discuss fruitfully some aspects of therapy.

Another theme that could usefully be studied in this way is responsibility. In some societies, in the diagnosis and treatment of mental illness, an attempt is made to transfer responsibility away from the patient and his immediate surroundings to relieve him of feelings of guilt. In other societies, including our own, we

try to bring the responsibility home to the patient. It seems to me that this may reflect quite an important difference in methods of therapy. Of course this also has to be related to the kind of society with which we are dealing.

A third theme is the much more material one of exchange. In many of the societies known to anthropologists the relationship between therapist and patient is partly a relationship of material exchange. The patient expects and is expected to pay for treatment and help. It seems to me that Freud's perception of this revealed his genius. A cross-cultural study to find out just what the rôle of such exchange is, not only materially but also in terms of contributions such as services, status acknowledgement, and other less well-defined criteria, would be very interesting. Instead of gross situational comparison, we should try to take more manageable themes and dissect them out cross-culturally.

THE DOCTOR-PATIENT RELATIONSHIP

Caudill: I was interested in Professor Carstairs' suggestion that different models of the doctor-patient dialogue may be appropriate in different cultures. In Japan at the moment there is a lively dialogue going on between two very different schools of therapists. There is a small group who call themselves psycho-analysts, and another group, with more influence on the medical profession than the analysts, who are known as Morita psycho-therapists. Although adequate translations of psycho-analytical work have been available since the nineteen-twenties and a small group of people have been interested in it since about 1930, psycho-analysis has never been really popular in Japan. Dr. Takeo Doi and I have written about the problem of the apparent unsuitability of psycho-analysis for the Japanese patient (Caudill, W., and Takeo Doi, L. [1963]. *In* Man's Image in Medicine and Anthropology, p. 374, ed. Yaldston, I. New York: International Universities Press). One of our explanations for this is that the

Japanese patient comes asking to be taken care of, and he does this frequently by being *amal*. This is a Japanese word with the basic meaning of sweet, and in this context it means asking to be taken care of as a small child would be taken care of by its parents. Most doctors in Japan gratify this wish, not necessarily consciously, but simply because this is a customary way of interacting in the culture. Doi also suggests that the Japanese do not have a sense of *jibun* meaning a sense of "I" in the same way that Americans do. The Japanese person is much more a personality as part of a group than he is a separate entity by himself.

Morita psychotherapy helps the patient with a very specific type of difficulty—*shinkeishitsubyo* in Japanese—which is characterized by tense interpersonal relationships, phobias and bodily complaints. We would probably call this an anxiety reaction, but the syndrome has a very Japanese flavour. In Morita therapy, after a period of bed-rest, the patient is told that there is nothing so special about him, so he should get on with the business of living and learn to live with his symptoms.

I have used this Japanese material to indicate that there is a rich literature on the history of psychiatry in Japan which provides us with an opportunity to examine the problem of what the appropriate relationship between doctor and patient is in a particular culture.

Fortes: To return to Dr. Loudon's comments on choosing a doctor, it is not so much that people have to choose a doctor as that they have to make choices and to make them unguided. Any choice involves a gamble and even a simple choice such as whether to stay at home or go out and find a job can itself become a stress. I suggest that herein we may find a way of approaching the problem of how to integrate the role of the psychiatrist with the cultural background of mythology and beliefs for I do not see how one can establish any kind of guiding therapist-patient relationship except on the basis of these beliefs. Even a schoolteacher has to use magic to some extent. It is this aspect of the

integration of social, structural, and cultural belief systems which I find so interesting.

Wittkower: There are some factors additional to the ones that Professor Carstairs named which play a part in the therapeutic process. In my work in Haiti I noted various mechanisms, including projection of badness into the deities, displacement of badness into the scapegoat, displacement of the target (that is killing the animal instead of the enemy) and expiation by sacrifice.

COMMUNITY THERAPEUTIC ACTIVITY

Mead: In primitive societies therapeutic behaviour may be shown by the total community, not just by the specific healer. Eskimo women have hysterical attacks in which they dash out over the ice, the whole community goes after them, and this is a drama. The women's hysterical fit gets everybody out of bed and everybody helps to bring her back, with very definite therapeutic effects (Baashuus-Jensen, J. [1935]. *Vet. J.*, **91**, 339; Brill, A.A. [1913]. *J. nerv. ment. Dis.*, **40**, 514; Gussow, Z. [1960]. *Psychoanal. Stud. Soc.*, **1**, 218; Parker, S. [1962]. *Amer. Anthrop.*, **64**, 76). I have watched the same sort of thing in my own Manus community (Mead, M. [1956]. New Lives for Old, p. 379. New York: New American Library). A suicide attempt brings the whole community out, and afterwards everybody spends several days restoring the wounded self-esteem of the patient.

There are some recently developed institutions which are based on psychiatric understanding, but where a community is used as the therapeutic tool rather than the individual therapist-patient relationship. Examples of this are Alcoholics Anonymous and the Psychiatric Centre, North Ryde, Sydney, Australia, which really is a new type of therapy. From the psychiatric point of view this is a three-ring circus. If one member of the family is ill, everybody moves into the clinic—children, dogs, the car, everyone. The members of the family who are well enough

work or go to school from the clinic, everyone concerned is formed into almost continuous groups and the groups do most of the therapy. The whole thing is carefully controlled by a charismatic staff of a type which is probably necessary for this kind of procedure. This is quite an inexpensive form of therapy. If a patient goes home and breaks down again, one of the groups is sent out to bring him back to the clinic.

If we look at the therapeutic experience in all sorts of communities and think of psychiatry as a theory of human behaviour and of understanding mental disorder as well as a particular kind of practice, then I think we can call all these diffuse procedures forms of psychiatric treatment. They all attempt to restore faith, allay anxiety and cherish and protect individuals in regressive situations or moments of crisis. Professor Carstairs, do you agree that communities like the one at North Ryde can be regarded as extensions of therapy in the same way as the *guru* relationship can be included in the psychotherapeutic dialogue?

Carstairs: Undoubtedly our new interest in community care involves a reinvestment in community agencies and community participation in the rehabilitation of the mentally ill.

DeVos: Related to this is an issue which has been of interest from an ethnological standpoint in American society, that of professional organizations which take the place of the missing therapeutic community. In American society there has been a shift of numerous social functions, both from the individual and from the local community, into the hands of specially trained professionals. Social agencies have been set up to perform most of the therapeutic and sanctioning functions which were previously handled on an informal basis by the community itself. It is interesting to note that, when such a non-professional, therapeutic agency is set up, it may meet with tremendous resistance from professional bodies. A notable example of this phenomenon is the antagonistic reactions displayed towards a new movement in California. This is run for drug addicts by ex-addicts, is called

Synanon, and explicitly excludes outside professionals from participation. Synanon claims that cure can be achieved because individuals who have experienced the same problem can relate to one another in a way that is not possible for those who have no direct experience of addiction. This view specifically excludes the professional practitioner who feels left out and therefore will have nothing to do with the movement nor give it support of any kind.

It is an interesting observation that in many modern societies, and especially in America where the rate of social mobility is so great, there is rarely the opportunity for an established traditional community to apply informal sanctions or counsel to handle an individual's problem of deviancy, whether this be psychiatric or antisocial in nature. The police are one example of a specialized professional agency and the complaint is often made that middle-class youths, because they are taken care of somehow informally, do not get arrested as often as youths from the lower social groups. There is some truth in this because in communities in some established, middle-class areas there are still prestige individuals or groups who can take a youth and "straighten him out" within the informal context of community sanctions. In the more mobile and disorganized lower-class groups, however, this is very difficult and the official professional agency, the police, has to be called in to perform this function.

I think that the choice of professional individual versus nonprofessional community as therapeutic agent creates a certain amount of tension in psychiatrists. The psychiatric tradition is an individualistic one derived from that of the medical practitioner or medicine man. The idea that there may be community movements, such as Alcoholics Anonymous and Synanon, which are therapeutic by nature is not easy to accept and meets with considerable resistance. This makes it difficult to investigate the possibility that the organized community itself, if directed in a certain way, may perform a therapeutic function.

Lewis: I have never heard of antagonism, psychiatric or otherwise, to Alcoholics Anonymous. They present the same picture as Synanon of a group acting therapeutically who are not themselves professionals.

DeVos: There is a great deal of scepticism among psychiatrists in the United States, even though this is not vocal, about the value of Alcoholics Anonymous. I do not think that it is mainly the psychiatrists who are opposing Synanon in the States, but such bodies as probation departments and the professionals in the correctional field who believe that it is their professional function, since it is so defined by law, to take care of addicts. The psychiatrists, I think, are friendly to what Synanon is trying to do because they have a medical rather than a correctional approach to addiction. The social agencies involved believe that the programmes they have already instituted are the most advanced answers to addiction and so they resent Synanon.

Leighton: When Alcoholics Anonymous started there was plenty of snide comment by psychiatrists in Baltimore where I was working.

Lewis: In England, I know of no opposition; on the contrary. Dr. Shepherd, would you agree with that?

Shepherd: Yes.

Murphy: These comments on the differences in initial resistance to Alcoholics Anonymous in Britain and Baltimore are very good examples of the influence of culture on group reactions. In Montreal, where there is a mixture of European and American cultures, we see very clearly in the truly Canadian population that the idea of accepting help is resented and resisted both by patients and by clients of social agencies. The function of the nurse and of the social worker is regarded as that of a specialist and the patient sees himself as seeking specialist help and taking his own decision in the choice of a specialist to whom he pays a fee. This is individualism *par excellence* and even the client of the lower-class agency wants, and is expected, to pay for agency

guidance in his affairs. Social workers who come from Europe to work in Montreal run into difficulties with this sort of clientele because of their European attitude of community support and free services. This clashes with the North American emphasis on individual decision making and freedom of the individual to pay his way and be independent of society. I think this agrees with what has been said about the emphasis which is placed, in North America, on the patient carrying his own responsibility. I do not think that this idea has been entirely created by the psychiatrist, but rather that he has absorbed some of it from the values of the society and reprojected it into the patient.

Lewis: It is a reciprocal process.

Shepherd: I would like to ask Professor Wittkower, who has just returned from a visit to Eastern Europe, whether it is true that these countries, with a different political system from our own, tend to take more social responsibility for deviant behaviour and to exclude from medical care many of the kinds of behaviour which we would regard as constituting medical problems? Further, have they in fact, as they so often claim, succeeded in dealing satisfactorily with these difficulties?

Wittkower: I cannot answer your question adequately because I only spent ten days behind the Iron Curtain. It is true that the emphasis in these countries is on a collective approach rather than an individual one and our colleagues in East Germany and Poland, at least, claim good successes with their methods.

Lambo: Ghana is politically modelled on the Eastern European pattern and has been making this same claim, especially in the sphere of juvenile delinquency. When I was there in March 1964 they certainly seemed to have emptied some of their remand homes and put many of the young people from these homes into youth brigades. There is a great deal of community development, provisions for youth and organization of youth civic services in Ghana.

SUMMING UP

Carstairs: I am particularly indebted to Professors Firth and Fortes for redressing a false emphasis in my presentation by reminding us that there is an element of magic in our daily lives. We take on trust many things that we have not thought out or tested empirically. A socially shared and endorsed fantasy becomes a social reality. Implicit in this remark is recognition of the distinction, described by Professor Firth, between the kind of fantasy which is private to the patient and which separates him from the rest of his group and can give rise to distress, and the socially accepted fantasies.

I would like to comment on different attitudes towards responsibility. In some societies the remedy for sin, guilt, depression and malaise of various kinds is to put the blame outside, and in others, including our own, it is to persuade the patient to accept blame himself. My colleague Graham Foulds has been carrying out personality studies in neurotic patients in relation to the outcome of their illness (1965. Personality and Personal Illness. London: Tavistock Publications). He has found that one important measure is how the patient deals with his aggressive impulses. He has separated patients who are mostly intrapunitive from those who are extrapunitive and he finds that, in our society, to be extrapunitive is to get well while to stay in an intrapunitive position means that you stay sicker for longer. One can indulge to excess in this particular habit!

Dr. Mead mentioned that Engel would have us treat grief as an illness. The greatest extension of the psychiatrist's territory that I have heard was not advocated by a psychiatrist but by a social anthropologist. Jules Henry, provoked by me, was once rash enough to say that unhappiness was the psychiatrist's responsibility; this is an extreme position indeed!

Several speakers have asked the question: "What should we be trying to do in the psychotherapeutic relationship?" As

Professor Wittkower has said there are many ingredients in the healing process as well as the ones that I mentioned. I would like to ask the question: "Is insight necessary for cure?" We take for granted in psychotherapy that healing will only take place when the patient has insight, or, as Dr. F. Kraüpl Taylor puts it (1961. The Analysis of Therapeutic Groups. London: Oxford University Press), when he comes to agree with the psychiatrist about what has been going on. It strikes me that Alcoholics Anonymous give us an example of this element in the treatment process. One reason that psychiatrists have a certain reserve about this agency is that Alcoholics Anonymous are very dogmatic about alcoholism. They believe that the alcoholic is different from other people because he has a different biochemical make-up and that for him alcohol has a special metabolic significance. It is almost an act of faith to participate in this belief, which I would describe as a fantasy about a physiological process. Nevertheless, this seems to be a necessary ingredient for deriving benefit from this particular communally-shared form of therapy.

Dr. Caudill noted that the Japanese patient asks to be taken care of and this is precisely what my Indian colleagues complained of in their patients, who also insist on being dependent. The therapists, reluctantly, found themselves pushed into activity and forced to take over an active role in the face of a determinedly dependent patient. Here again, we may have to revise our preconceptions about what is the necessary transaction to give the patient relief. I certainly think we should look at other healing processes, as Dr. Murphy suggested, to see what, in them, are the essential factors for cure. It may be abreaction, or a scapegoating process as Professor Wittkower has said, or many other things. At the moment all we can say is that in one particular social context a particular factor is likely to be the most important therapeutic experience.

Transcultural Psychiatry
A.V.S. Deuck & Ruth Porter

CHANGING PATTERNS OF ADOLESCENCE

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THE biological process of growth from birth to maturity does not alone determine the demarcation of phases of development, nor the characteristic events or sequence of events within any particular phase. At any point in the growth process the young human being has a range of possibilities for learning, feeling and action. How these possibilities are realized varies widely from culture to culture, and within a given culture in different periods.

I should like to consider here changes in the pattern of phases of development by trying to analyse a particular instance, namely recent developments in adolescence in American middle-class and upper-middle-class families. The extent of diffusion of the phenomena which I shall discuss remains unspecified. They will be regarded as illustrative of significant changes in the patterning of the adolescent phase from the parent to the child generation, in relation to long-term trends in the present century, and against the larger background of traditions in western culture.

The general point which I wish to bring out is that, in a changing culture, we may get reversals of order in the sequence of development. Such reversals tend to be fraught with emotional complications and may entail various disturbances. I shall deal with two main reversals which may be observed in contemporary adolescent development: (1) the starting of a new family before material independence has been attained, (2) initiation of sexual relations before the emergence of tender romantic feelings. Both these sequences reverse what has been considered appropriate until now.

The values enforcing the older order persist to a considerable extent and make for emotional conflict in present practice.

We may take adolescence as the phase beginning with physical puberty and concluding with the attainment of a considerable array of attributes which, according to prevailing standards, qualify the individual as a mature member of the culture. Within the phase thus demarcated, the young individual in our culture has a large number of emotional and practical tasks to accomplish. Among these are the beginning of heterosexual relations and the attainment of financial independence from the family of origin. In the sector of American society which I am considering, the upper economic groups with high educational aspirations, there seem to be the following trends in these two crucial variables: attainment of economic independence is being more and more postponed; heterosexual relationships are beginning earlier and earlier. I should like to consider some of the conditions and consequences of these two trends.

Earlier in this century the attainment of economic independence was considered a prerequisite for a young man's marrying and starting a family. Higher education was generally less protracted and the moral atmosphere was one that enforced postponement of sexual relations with a girl of the young man's social class until he was capable of supporting a family. Much has happened in the course of the century to eliminate this moral restriction. Two world wars made young people of both sexes feel that they must live fully in the moment, in a world where tomorrow might never come. Long-term plans and cautious postponements tended to seem meaningless. The depression between the two wars had a similar effect, waiting and planning seemed without promise. The morale of the older generation was also shaken: how could they ask their young people to work and save to attain a life like their own, which was foundering in bankruptcy?

A major recent phenomenon has been that of the young family with one or two or three children, in which the husband is still

studying for his graduate (or in some cases undergraduate) degree. He is being supported by his parents, his wife's parents, his wife, university fellowships, or some combination of these. Parents do not dare to suggest that the young man break off his studies and go to work because he has a family to support. It would mean the abandonment of their own ambitions for the higher occupational status which their son can achieve with prolonged education. Margaret Mead has pointed out that where the wife supports her husband through graduate school, he may cast her off when he achieves financial independence. Where this happens, the wife is associated like the parents with a dependent phase now finally outgrown, and may have taken on incestuous connotations so that she must be renounced. The young man goes on to get a new girl, free from familiar associations, in relation to whom he can be a full-grown man and a provider.

Younger adolescents have been much affected by the model of older adolescents who claim full sexual prerogatives without attaining financial independence. The ideal to be striven for is not embodied in the adults of the parent generation but in the older adolescents whom their juniors are now eager to emulate. We have developed what has been called an "adolescent society" (Coleman, 1961) with its own standards, modes, and language, in relation to which adults are bewildered, and to a considerable extent despised, outsiders. The adults on their side tend to join in an idealization of youth, which is no longer seen as a transitional phase, but as the prime of life (Keniston, 1961).

There are indications that young adolescents are beginning to have coitus at an ever earlier age. This may be related in part to a biological trend for the increasingly early occurrence of puberty (Tanner, 1962). The biological factor alone is not decisive, since traditionally in our culture the attainment of puberty did not immediately precipitate sexual relations in adolescents of the social class which I am considering (Kinsey *et al.*, 1948, 1953). However, girls who attain puberty early look much older than their age.

Their sexual allure combined with the prospect of dependence on their families for many years to come creates tensions due to incestuous temptation for the father and jealousy for the mother. It is no accident that recently a novel by an esoteric high-brow novelist became a national best seller because it dealt with the sexual relations of a middle-aged man with his twelve-year-old step-daughter. I refer to Vladimir Nabokov's *Lolita*. I shall try to show that early adolescent sexual relations are in part an attempt to cope with these incestuous hazards.

The trend towards precipitous sexual activity runs counter to a very old tradition in western culture which has not lost its hold on our imagination, that of romantic love. Romantic love, from its inception in the time of the troubadours, gained its intensity from the unavailability of the loved one (de Rougement, 1940; Taylor, 1953). Typically, as in the legend of Tristan and Iseult, the beloved woman was married to another, and formidable barriers prevented a great passion from petering out in domestic routine. Dante and Petrarch gave poetic expression to love for women they could never attain. The intensity of feeling of Romeo and Juliet was maintained by obstacles which proved insurmountable. In a more playful variant, when Prospero summoned Ferdinand to his island as a husband for his daughter, he improvised all sorts of difficulties so that the young couple might fall in love. In more recent literature, Swann fell in love with Odette when she failed to appear at an expected time and place.

The absence of barriers, obstacles, and requirements for postponement in sexual relations eliminates romantic love in the traditional sense. Casual sexual relations between young adults of good social class became the mode in the twenties. In England the characters of Evelyn Waugh's early novels, with their laconic, almost vestigial style of talk, and meagre feelings, exemplified this. Partners in such casual relations could drift into marriage eventually. A cartoon in the *Humorist* from the early twenties shows a young man and woman lounging at opposite ends of a

couch. The legend reads: "He: 'I say, old top, let's get married.' She: 'But who'd have us, old bean?' " (Turner, 1954). The attitude here expressed approximates to what contemporary American adolescents call "cool", a term of great good repute among them.

The expectation that strong sentiment should precede the consummation of a sexual relationship is a matter of cultural tradition. In older Chinese culture, for example, a couple whose marriage had been arranged hardly saw each other before they came to the marriage bed. We are told that deep feelings of mutual affection and respect frequently developed subsequently between the partners. In our culture, however, courtship and sentimental intimacy have served as necessary sanctions for a sexual relation. Sex on brief acquaintance has been associated with low regard for the partner. It is an important question whether we can reverse the sequence of sentiment and sex in Anglo-American culture.

This question may be considered more generally: how many of the developments in the complicated phase of adolescence which we are accustomed to thinking of as occurring in a given order may be reversed? For example, we have thought that the intellectual spurt of adolescence precedes the young person's making his major life choices. Anna Freud has pointed out that the great questions with which adolescents typically occupy themselves represent their own as yet unsolved personal problems formulated in abstract and large-scale terms (A. Freud, 1946). Some observers have expressed doubts whether our adolescents, who marry so early, will have much motivation for intellectual development. There is also probably some assumption here about conditions for intensity of sublimation. However, I have had occasion to observe a young man in his early twenties who, as a graduate student, had a wife and child and had already chosen his career, and who only after all these things had been settled experienced an impressive phase of intellectual discovery.

We have been used to thinking of educated adolescents as

working through many life alternatives on the level of thought and fantasy, sharing their ideas and feelings with friends of the same sex, writing poetry and so on. We have also in the relatively recent past seen young people trying out their heterosexual roles on a level of trial action, as exemplified in the dating game (Gorer, 1948; Mead, 1949). This involved a semblance of pairing with a range of partners. The date was an occasion for public appearance with a prestigious partner. Sexual activity was not its main objective, and supposedly the more adept the partners were in the game, the less sexual intimacy occurred. We seem now to be in a phase in which adolescents tend to try out alternatives by living them out more completely. For example, a white adolescent girl of good family may dramatize her effort to break with her parents by taking as a lover a Negro dope addict. As she gradually comes to terms with her parents' standards, she may go through a whole series of love affairs with young men who progressively approximate to her own background (Hornick, 1965).

There seems to be an increasing intolerance of postponement of action, of working things out on the trial level of thought and fantasy. This need for action probably has many determinants, among them a deep distrust for any activity that can be carried on in solitude, such as day-dreaming. The drive to immediate action in adolescence may also be related to changes in the patterning of preceding phases. I would call attention particularly to the development in the recent past of our attitudes towards the latency period. Traditionally this phase, from the time of beginning school to pre-puberty, was utilized for the acquisition of many skills, often learned by rote, in which the intrinsic interest in what was learned was secondary to the child's gaining a high degree of automatic mastery. There was also a turning outward of interest to impersonal aspects of the external world. The potential of latency for this kind of learning has been for some time devalued by educators. There have been inroads on latency from both sides: the image of the pre-school child, creative and imaginative,

throwing colour on his easel-painting in great wide strokes, has intruded from the lower age level. The ideal of the adolescent who learns with intensity only what greatly interests him, has pressed in from the older age level. The possibilities in latency for cultivating a kind of learning remote from impulsive gratification have been played down to a considerable extent. The traditional latency-period character with its emphasis on sublimation, skill, control, and mastery of external reality has become less firmly established. When the young person who has not undergone the discipline of an orderly latency period emerges into adolescence he has fewer brakes to carrying into action his intensified impulses, and less facility for elaborating them on symbolic levels.

If there is a trend towards increasingly early sexual activity, with an abridgement of sentimental preliminaries, we would expect this to affect boys and girls quite differently. There are many customs which are better adapted to the needs of one sex than to those of the other. In the present instance, the custom seems more suited to the boy than to the girl. The drives of adolescent boys have a genital concentration which girls' do not. The longing of adolescent girls is to be loved, with the genital significance of the wish often remaining vague. Phallic pride for the boy is central to his self-esteem. The girl's feeling about herself is much more diffused, investing her whole person. Her strivings to perfect her appearance are often still in part defences against persisting confused images of her genitals as messy and formless (Deutsch, 1944). She wants to be admired and prized for her whole self and a too direct genital approach is likely to be deeply disappointing to her. Female sexuality does not find its end in the sexual act. It includes anticipations of pregnancy and child bearing. These anticipations are present on some level in the psychic repercussions of the sexual act for the girl. Thus a sexual relation involves more of a time perspective for her, expectations of persisting devotion and care, of shared life plans. Sexual relations with no anticipated sequel frustrate deep longings in the girl.

It is distinctive of American culture that we have never recognized the love affair (whether premarital or postmarital) as a regular part of life. Thus, while premarital sexual relations have been increasingly prevalent in the course of this century, they remain from the point of view of official standards, exceptional, lamentable, and a violation of the code. This, of course, leads to considerable emotional confusion in young people, and also to the rapidly increasing frequency of early marriages. It is often said that the majority of our teenage brides are pregnant at the time of marriage. Margaret Mead has pointed out the paradox that while we continue to disapprove of premarital sexual relations we are now rewarding premarital pregnancy. If the early initiation of sexual relations represents a triumph of the adolescent boy's sexual urgencies, early marriage may be seen as a hoped-for fulfilment of the girl's long-term expectations associated with sex.

I should like to return to the theme of sex and sentiment. The relation between these two psychic strands is a complicated one, and must be viewed in the context of development from early childhood. For the young child, bodily gratification and affectionate love are fused in the child's relation to the mother as they will be, ideally, re-fused much later in the adult relation to a chosen partner. But in the course of development between these two terms the two strands of feeling become radically dissociated. This is the sequel to the oedipus complex; feelings towards incestuous objects assume the form of what Freud called love with an inhibited aim. This is a tender, idealizing love from which sexual thoughts and expectations are excluded. The sexual impulses, split off from these tender feelings, are considerably repressed during the latency period. When biological maturation in adolescence brings sexual impulses to the surface again, the young person is initially confronted with the split previously enforced between sex and tender, idealizing sentiments. A more or less protracted further development is required to re-fuse the two strands. Freud has spoken of the disturbance in the love life of a

certain kind of man for whom these two tendencies remain dissociated. He can feel love and admiration only towards women who are sexually anaesthetic, on the model of his mother and sister as he came to regard them. On the other hand, the women to whom he turns for sexual gratification seem to him degraded; he cannot associate any of his more ideal feelings with them (Freud, S., 1912).

The re-fusion of sex and sentiment in adolescence has one major prerequisite: the detachment of a considerable sector of the young person's tender feelings from their first objects, the parents. Only then can the emotional reorganization occur in which tender feelings are re-fused with sexual ones, now to be directed toward extra-familial objects. The withdrawal of strong feelings from attachment to the parents is a long and difficult process, involving struggle and conflict, uncertainty and wavering. If sexual relations are begun early in adolescence, it seems doubtful that the process of detachment from the parents has advanced very far. We remember also that the life perspective of the young people we are talking about is one of economic dependence on their parents for many years to come. I would suggest that in the new mode of adolescence there may be a persisting split between sexual and tender feelings; these seemingly precocious young people remain bound, as far as their strongest feelings of love are concerned, to their parents.

A certain pseudo-sentiment, however, accompanies some of these precocious sexual relationships. Speaking of an older style of adolescence, Helene Deutsch drew attention to fanciful attachments to persons with whom there was little or no real contact; the contents of the amorous fantasies derive from persisting incestuous longings, and are masked by seeming devotion to a stranger (Deutsch, 1944). A similar fanciful romance, with negligible awareness of the real traits of the partner, may now be improvised in connexion with early, transient sexual relationships.

And what are the parents doing in all this? There is much to be

said about them, and I can only select a few points. American parents have for some time been averse to assuming a superordinate, authoritarian role with their young. In their anxiety to be loved by their children, they seem fearful of evoking any rebellion or opposition (Henry, 1963). They hesitate to assume any position opposed to the gratification of the child's wishes or impulses. A case in point is their readiness to supply teenagers with cars, despite the well-known high accident and mortality rate among teenage drivers. Parents are diffident about presenting their own life careers as precedents for their children to follow. The older view of youth as a preparatory phase in which sacrifices are to be made for later gains has become obsolescent. The Protestant ethic of virtue being attested by wealth laboriously gained seems meaningless in a society where a film star can earn $100,000 for making a one-minute television commercial.

I have spoken of the compelling power of the modes set in the adolescent group itself. Parents as well as children show deference to these modes. From the moment of the child's birth, when he is put into the hospital nursery with his age mates, the parents want above all for him to belong to his peer group. When the child says, "I have to do this or I won't be one of the group", there is no higher imperative that the parents can invoke. So the children model themselves on the leaders or idols of the adolescent group and the parents are in accord. Recently the *New York Times* reported the case of a teenage boy who had been suspended from his high school for wearing a Beatle haircut. The boy's father had hired a lawyer to fight the ruling of the school authorities and to defend the boy's right to conform not to old-fashioned adult standards but to the model of teenage idols.

Many novels and popular and high-brow articles and books tell us of the unrewardingness of adult life in American culture. A counterpart to this is the idealization of the phase of adolescence. Adults are fascinated by youth and parents yearn to live vicariously in the lives of their young sons and daughters. There is some

evidence to suggest that the early sexual activity of adolescents is far from being a secret to their parents, even that it is to a considerable extent shared with them. It was reported that parents whose daughter was attending a private high school in New York gave a "slumber party" for the girl and her friends, in which boys and girls shared sleeping quarters. College boys when they visit their girl friends' homes for the holidays may be assigned a room which they share with the girl (Harley, 1965). A 19-year-old girl patient of mine reports on all her sexual affairs to her mother. Another 19-year-old girl, whose parents I visited recently, was home from college for the holidays. She and her boy friend were there for dinner and an evening's conversation. As the hour grew late they put on their hats and coats and set off for the boy's apartment.

What seems relatively new in this is the openness, the publicity *vis-à-vis* the parents with which these adolescents are carrying on their affairs. In the past in our culture, and in many other cultures, adolescent sexual affairs have been conducted with a certain amount of secrecy, especially in relation to the parents. What seems to be the current American trend has been characterized by Rhoda Métraux (1964) and Margaret Mead (1964) as the primal scene in reverse: the parents become the spectators of the sexual activities of the children.

In speaking of the American custom of the double date, in which two young men who are close friends escort two girls who may be relative strangers to them, Geoffrey Gorer speculated that the heterosexual façade of the occasion served to reassure the young men that their mutual attachment was free from suspicion of homosexuality (Gorer, 1948). If we suppose that the adolescents of whom I have been speaking are still emotionally bound to their parents, we might consider that the exhibition of their sexual lives to the parents serves a similar purpose; the precocious sexual activity acts as a denial of the persisting emotional attachment to the parents. At the same time, making their parents aware

of their sexual activities has an ambiguous quality as they are in a sense including their parents in their sexual lives. The early sexual activity serves as an alibi for persisting incestuous longings at the same time that it gratifies these longings through the sharing of sexual secrets with the parents.

In a changing culture it is often difficult to sort out which departures from precedent may be new adaptations and which may give rise to psychopathology. In the trend towards earlier adolescent sexual activity there is a significant emotional hazard. Where sexual acts are performed in advance of emotional readiness, and where the tradition has been that they should be preceded by such readiness, there is the risk of loss of affect with the related disturbances of depersonalization and derealization. The ideal of "coolness" of contemporary American youth has been noted. Intensity and ardour of feelings and commitments are deprecated. The vaunted detachment may be a virtue made out of necessity, where there is an inhibition of strong emotion attended by covert feelings of uneasiness and unreality.

The typical neuroses of the late nineteenth century, notably the hysterias, were related to the strong sexual inhibitions and taboos of that period. One of the great expectations of the nineteenth century which was disappointed in the twentieth was that greater sexual freedom would greatly improve the human condition. There does not seem to be any lessening of neuroses, but they tend to take different forms. Affective disorders, particularly loss of affect, and disturbances of the sense of reality manifested, for instance, in uncertainty as to personal and sexual identity, have been noted increasingly in psycho-analytic literature, in novels, and in essays in highbrow journals. In the latter, "alienation" has for some time been a catch word. A novelist like Sartre, who apparently suffered all his life from feelings of depersonalization (Sartre, 1964), is regarded as an apt exponent of the state of soul prevailing in the present and recent past. Erikson (1959) has shifted the emphasis in dealing with the crisis of adolescence from sexual

conflicts to uncertainties about identity. While we do not know the frequency of such disturbances they have certainly been gaining increasing attention lately. In connecting such disturbances with changing sexual customs, I am offering only a very tentative speculation. There have been many alternations between periods of stringent sexual taboos and periods of sexual freedom since the beginnings of romantic love in the late middle ages (Taylor, 1953). The sexual freedom of the twentieth century developed out of the antecedent rigours of the Victorian age, in which also the ideal of romantic love which surmounted barriers, was again in the ascendancy. The expectation of intense emotion in circumstances where there are few remaining barriers leads to disappointment. Our persisting romantic expectations in sexual relations easily obtained entail feelings of uncertainty and unreality about what we have experienced. In our sentiments, in our ideals, we still tend to take sex very seriously, in practice less so. Some of the feelings of unreality and of expected emotion being missing may derive from this discrepancy.

It seems likely that our young adolescents, so long dependent on their parents materially and, as I have suggested, emotionally, experience difficulty in bringing strong feelings to their sexual relationships. The result of this would be uncertainty about where they really are emotionally, a sense of not being able to fit their feelings to their actions. However, we may be establishing a mode in which sex will precede the development of sentiment contrary to the order prescribed in traditional romantic love. And the gradual detachment of adolescents from their parents may be achieved through a series of sexual affairs which become progressively invested with warm and stable sentiments.

From the point of view of transcultural psychiatry, we may generalize to say that in changing cultures the sequence of events in the course of development is subject to reversal within certain limits. Observation of such reversals and the conflicts which they occasion can add to our understanding of trends in psychopathology.

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DISCUSSION

Fortes: As a teacher of adolescents I have tried recently to find out how the anthropological teaching affects the outlook on life of my first-year students and I generally set an examination question on how the students feel their anthropological studies have affected them. Last year this question evoked some interesting answers on the subjects of "mods and rockers" and initiation ceremonies. Many of these young

people felt that they needed an initiation ceremony. They said that they would even be willing to endure some of the rites of initiation ceremonies that they had heard about in their lectures. They wanted this ceremony because they thought it would help them to feel, through it, that society was taking them seriously and giving them their rightful place in the community. These undergraduates felt that they lacked the experience that reaching maturity was an actual moment in time, with a "before" and an "after", and they thought that this lack partly explained their need to dress up and behave aggressively. We know from comparative, if not transcultural, studies that a very important aspect of initiation ceremonies is to make a dividing line between non-serious and serious sex. In the parts of Africa that I know the people make a very clear distinction between what they call "play sex" and "real sex". Play sex is suitable for children, but real sex is for the serious purpose of procreation, for marriage, having a family and building your place in society. It seems to me that there is a certain group of adolescents in our society who realize, when they have a model presented to them, that they are missing something because they are not accepted as adolescents in society by a public or even a drastic ceremony.

Lewis: Professor Fortes, when your students tell you that they want to go through an initiation ceremony such as, for instance, knocking their front teeth out, do you think that they mean what they say?

Fortes: I don't ask them that! I am simply reporting that this idea—perhaps what Professor Carstairs would call a fantasy—has been evoked in some of these young people; they quite commonly make this particular connexion between themselves and the initiation rites that they have heard about in their lectures.

DeVos: We do not have any physical rituals in our society, but we have mental rituals, such as examinations, which define people as adults. School graduation ceremonies are often called "commencements".

Fortes: From the anthropological point of view a purely mental ritual is not enough. Psychologically I think that there needs to be something to show for it after the ritual, either on one's body, or clothes or somewhere; but this is a very complex subject.

Mead: In contrast to the lack of initiation ceremonies in western adult cultures, we have a great many initiations into the peer culture in the United States, for example, initiation into fraternities and delinquent gangs. In the gangs especially there are very peculiar initiatory ceremonies involving sets of quasi-criminal or violent activities. A boy may have to commit a rape, or even a murder, although this is probably not always achieved, or perform other antisocial acts such as robbery. In a high school students' group in Topeka, the necessary initiation achievement was to have witnessed parental intercourse but this is in a community which is rather heavily permeated with psychiatric theory! There is also an increasing number of reports of girls who have sought to be deflowered by a stranger, a rite that is well known in primitive societies. The girls pick someone who is either a stranger or of no importance to them, and use this intercourse as a ritual protection for what will happen later and mean more to them. I think this fits in well with Dr. Wolfenstein's remarks.

Yap: I have read Riesman's speculations on inner-directed and other-directed social character in different segments of the Eastern American population (Riesman, D., Glazer, N., and Denney, R. [1950]. The Lonely Crowd. New Haven: Yale University Press). These authors state that the other-directed personality-type is increasing at the expense of the inner-directed. I do not know how true this is, but Dr. Wolfenstein has left me with the impression that perhaps a kind of extended family is developing in the United States with the married children remaining in the parental home. This, I suppose, might relate to the emergence of the other-directed character.

Mead: These married children are not staying in the home; they are being supported by the parents at a distance and in many instances they have been very isolated. There are now large numbers of teenage couples who live in extreme isolation with no advice near at hand. A psychopathological condition which may develop in these very young and isolated wives is postpartum psychosis. This is now quite common in new suburbs where there are no female associates for these girls—no mothers, no sisters, no mothers-in-law and no female friends. A very carefully worked-out study has recently been made over the whole county of New Jersey in all its different types of suburbs

(Gordon, R.E., *et al.* [1961]. The Split Level Trap. New York: Random House). This showed that the likelihood of breakdown after the birth of a first child is related to the distance the mother lives from any female relative or confidante. The difference in the incidence of breakdown between the older, more stable suburbs, where friendships and ties exist across the generations, and the new ones with these isolated couples, was highly significant. These children are still economically dependent upon their parents but they lack the sustained relationships that would support them in a real extended family.

Lewis: I think Dr. Wolfenstein's account of the changing pattern of adolescence is incomplete until she tells us what happens now when an unmarried girl becomes pregnant. What is the social attitude, Dr. Wolfenstein, of parents, friends, and others in the community when they find that a girl of fifteen is going to have a baby?

Wolfenstein: I think the couple usually tell the parents and the parents arrange a big wedding.

Mead: We used to talk about a shot-gun wedding when the shot-gun was in the hands of the girl's father. Today we have an incredible number of shot-gun weddings with the shot-gun in the hands of the children. Pregnancy today is used as a way of persuading the parents to agree to a marriage. The parents do this willingly with the customary celebrations and then support the young couple financially after they are married.

Lewis: But the figures show that there is also a high proportion of illegitimate births.

Mead: This is probably because the age at which sexual activity starts is going down and down (Rele, J.R. [1965]. *Milbank mem. Fd. Quart.*, **43**, 219), consequently the girls use less judgment in selecting a mate and there are a number of miscalculations about whether a couple is stable enough to establish a marriage. This situation was dramatized in the case of two 13-year-old school boys who were selling pregnancy pills at 50 cents (U.S.A.) each. These pills were not to prevent pregnancy but to produce it. They were only aspirins, but they were accepted with great enthusiasm by the girls, who saw pregnancy as an immediate route to marriage.

Lewis: This recalls the Jude the Obscure theme. But national figures show a great number of illegitimate births; adoption societies know

this very well. Young unmarried parents present a social problem towards which there must be an attitude by society.

Mead: Part of the increase in illegitimate births occurs when minority groups, who do not have the same sort of marriage customs as we do, move into our cities. This applies particularly to the Caribbean immigrants. In the West Indies, periods of socially recognized trial relationships are understood and respected and there is naturally a high rate of what we call illegitimacy in these groups when they come to the States. Another cause of increasing illegitimacy, as Dr. Wolfenstein has said, is the lowered age of approved heterosexual association.

DeVos: Dr. Wolfenstein, would you comment on parental attitudes towards the use of contraceptives in adolescence?

Wolfenstein: This is a terribly confused subject and no one seems to be able to think clearly about it. The parents feel that if they give their girls contraceptive advice it is an encouragement to early extramarital intercourse, and they may feel this even when they know that their daughter is already indulging in promiscuous relationships.

Loudon: Since the parents apparently approve of some of these relationships, it is difficult to understand why they do not approve the use of contraceptives by their children.

Wolfenstein: This is something about which our thinking is not well co-ordinated.

Loudon: Dr. Mead, I think you said that marriages were frequently arranged for girls of 15 years of age if they became pregnant. Even so there are presumably a number of unmarried upper-class girls who in fact have illegitimate children.

Mead: The proportion of marriages due to pre-marital pregnancy is higher now than it would have been twenty-five years ago when, although there were plenty of hastily arranged marriages, this was not regarded as *the* way to get married. A large number of upper-middle-class and upper-class children today form heterosexual associations which are generally approved of, both socially and intellectually. These children become pregnant and then go and tell their parents that they are going to get married.

Loudon: Are the illegitimate children of young adolescents ever taken over by their maternal grandparents and brought up by them as their own children?

Mead: This does occur, especially when people move to a new neighbourhood.

I think there may be a new and hopeful sanction developing now which concerns prohibition of marriage if a girl becomes pregnant. The mother says to her teenage daughter: "If you get pregnant you can have your baby and I will help you to bring it up and support it, but you will not get married." This has a very salutary effect, because these youngsters want to get married and see pre-marital sex activity as a route to marriage. This gives them a kind of pseudo-maturity which has replaced the older, real maturity, when the girls could leave home because the boys were economically independent and could support their wives.

Loudon: Is there any evidence that this sanction has a salutary effect?

Mead: In communities where parents are beginning to take this attitude discussions among adolescent girls—and we have a good deal of information about this—show that the girls are worried when they hear that one of them is not allowed to get married although she is pregnant. In some communities nine out of ten of the senior girls in high school are pregnant when they marry; first one girl does it, and then the idea that this is the correct way to get married spreads to all her friends and they all get themselves pregnant. They all have lovely weddings and their taste in silver is registered at the local store; this shows, I think, that in the United States for the last ten years there has been tremendous approval of pre-marital pregnancy leading to marriage, by church, parents and the total society. This approval is present in the same way in our colleges. When a boy and a girl go to the Dean and say that the girl is pregnant and they are getting married, the Dean is delighted; the girl is not thrown out of college because she is pregnant and going to be married, she is only thrown out if she is pregnant and not going to be married. This is a definite reversal of what would have happened twenty-five years ago, and is true now even of the church-connected Protestant colleges.

Margetts: It seems to me that this is an area for some real social psychiatric research. Can Dr. Mead or Dr. Wolfenstein give us any specific figures to substantiate these remarkable observations?

Wolfenstein: I do not know how widespread these phenomena are and, as far as I know, there are no statistics on the subject. Dr. Pomeroy,

a collaborator of the late Dr. Kinsey in his work on sexual habits in the United States in the nineteen-forties, says that he has no recent figures on adolescents. The data I have presented are based on my own and colleagues' impressions.

Mead: We have statistics on girls who are pregnant when they marry, on high school marriages and on the degree of support given by the parents to teenage married couples, but the extent of the mechanisms involved has not yet been analysed. Many of these studies have been done in different colleges and surveys have been made by personnel and guidance officers. There are also statistics on the increase of illegitimacy and the decreasing age of the illegitimate mother, and there have been careful studies of the average income and the incidence of divorce of teenage married couples (Herman, R.O. [1965]. *J. Home Econ.*, **57**, 93).

If you doubt the need children have to exhibit their sexual activities to parents and parent substitutes, you should visit any girls' dormitory in the United States at 10 o'clock in the evening. You will see these young people, who have, by the way, unimpeded freedom to do as they like and go anywhere unchaperoned, standing in serried ranks locked in deep embrace, and all this just to impress the Dean of Women as she walks past. I do not think that you need statistics when you have this sort of evidence! I think that this is a situation which is potentially dangerous as it carries a heavy burden of maladjustment both for the young couples and for their children.

Hes: Dr. Wolfenstein and Dr. Mead have described changes in sexual habits in adolescence but not the psychiatric consequences of these changes. In Israel, unfortunately, we see many of these consequences. I remember a Moroccan family, where a mother of six children had brought up three of them in Morocco and had no difficulty whatever with them. The three youngest children, who were brought up in Israel, exhibited changed dating and sexual behaviour and were a great anxiety to their parents. In cases like these the parents become insecure, family tensions increase and the situation unpleasantly often ends in attempted suicide by the children. These changes in sexual habits may, in fact, lead to overt psychopathology.

Lin: I am interested in the hero-image of young people, especially of boys, because I think this may alter in changing societies. In Taiwan,

for instance, where society is changing rapidly, the objects of hero-worship are also changing. Chinese boys used to hero-worship an object related to the father figure, but I think that this is not so any more and I am trying to find out something about what constitutes the new hero-image. Dr. Wolfenstein, can you tell us if there have been any studies on this subject in the United States over the last few years?

Wolfenstein: Some years ago there was a study on the idols of young people and it was found that there had been a shift from heroes and heroines who sacrificed themselves for a cause—nurses and doctors and so on—to performers of various sorts—actors, film stars and athletes. Recently, of course, Mr. Kennedy became an idol of youth.

Murphy: Is development of the anti-hero in literature affecting youth?

Wolfenstein: I do not think that children today read "anti-novels". James Bond, of course, is different—he is a great hero!

Transcultural Psychiatry
A.V.S. Deuck & Ruth Porter

CULTURAL CHANGE AND PSYCHIATRIC DISORDER

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INTRODUCTION

WHILE it is commonly believed that cultural change may cause an increase in the prevalence of psychiatric disorder, the proposition is by no means firmly established. Furthermore, even if it be true, there remain many unanswered questions regarding causal factors and the sequential arrangement of social, cultural and psychological events. In this paper I propose to set forth a number of rather simple notions regarding these relationships. They may be regarded as a diagrammatic attempt to represent certain main aspects of cultural change and of psychiatric disorder, and the interaction between the two phenomena. I hope to be able to raise in your minds the possibility that the question of how cultural change affects mental health is equalled in importance by the question of how mental health affects cultural change.

The paper is not concerned with proof, nor does it pretend to contain new facts. It is rather a scheme, in the Lockian sense, in which old stuff is arranged in a somewhat novel way, and such merit as it may possess lies in the questions it raises. Although research results will be quoted, their intent is illustrative and explanatory.

CULTURAL CHANGE

As a starting point I would like to suggest that there is very little evidence to support the idea that cultural change *is of itself* destructive to societies. The introduction of the horse changed

the way of life of the plains Indians in the American West (Wissler, 1914), but there is little in Clark Wissler's beautifully written account to indicate that this was anything but a functional asset to the people concerned. The same can be said of the Navahos when they learned how to work silver from the Mexicans and how to rear sheep from the Anglos. On the basis of my own observations in a group of Eskimos, I can testify that the gun and the outboard motor had an enormous effect, but I cannot attribute any serious social disturbance or psychopathology directly to either one. Turning the matter around, I do not believe that the introduction of maize, the potato, rubber, lacrosse or the League of Nations into European culture has been really destructive at any point along the way, and if tobacco was a mistake, it has certainly taken us over three centuries to find this out.

At this juncture I anticipate objection. It may be asked, "what about Sharp's (1952) classic description of the introduction of steel axes to stone-age Australians? Did not this loose the bonds of the entire system of values and role relationships?" Many other similar examples could be added.

Such objection, however, would make my point: cultural change sometimes is and sometimes is not accompanied by extensive malfunction and even group extinction. It is a condition of risk, just as driving in a car on a super-highway is a condition of risk, but it does not inevitably result in damage and destruction.

SOCIAL DISINTEGRATION

In referring to the introduction of steel axes among the Australian aborigines I mentioned loosening the bonds of the entire system of values and role relationships. How this happened in this particular instance need not detain us here, but the fact that it did happen and that it can be abstracted and thought about separately from the cultural innovation itself suggests some points for consideration. Suppose we label this abstract notion "social

disintegration" (Hughes *et al.*, 1960) and let the words stand for such phenomena as role dissolution, loss and deformation of sentiments and values, confusion regarding the meaning of symbols and cues, failures of communication, the breaking apart of family structure and deterioration of capacity for leadership, followership and conjoint effort. This puts us in a position to analyse cultural change and social disintegration as separate processes that may or may not be interrelated on a given occasion. Thus, although it is possible to visualize the same set of acts as an expression of both, it is also possible to consider one as occurring without the other. Role patterns, for example, may alter without dissolving and losing their functional effectiveness.

Arguments may be raised about the meaning of the words "culture", "change" and "disintegration". Let me skirt this, however; and while recognizing frankly that these words have many meanings and that cultural change has a wide range of usages some of which incorporate what I am calling social disintegration, let me ask that for the purposes of this paper "cultural change" and "social disintegration" be allowed to represent a distinction. It is the distinction itself that is important, whatever words are used to describe it.

If this is permitted, then we can at once see a host of questions regarding intervening factors and circumstances that determine when cultural change will be accompanied or followed by disintegrative process and when it will not. Involved here are such matters as the type of culture, the state of equilibrium of the society in which change occurs, the extent and speed of the innovations, their affective meaning, their selective impact on the population and so on. In the present report I would like to direct your attention to only one of these variables.

PSYCHIATRIC DISORDER

This variable may be stated in terms of the following proposition: the capacity of a social system to undergo cultural change

without disintegration is dependent upon the mental health of the component individuals. That is to say, the higher the prevalence of psychiatric disorder, the greater the risk of social disintegration.

Like most propositions that are pressed into a single sentence, this one needs qualifications. For example, a population which already has a high frequency of psychiatric disorder could be visited by cultural changes that would immediately improve mental health. In such a case the risk of social disintegration would decrease instead of rise. This, however, is a special circumstance and not likely to be a common one. The proposition, therefore, is considered to apply except when modified by special circumstances.

If this is so, the question that immediately follows is whether the prevalence of psychiatric disorders is ever sufficient to make the proposition of any real interest. In this connexion, some figures may be quoted. In a rural district of North America one of our studies has shown that at least 29 per cent of the adult population have psychiatric symptoms and are impaired by these to a significant degree (D. C. Leighton *et al.*, 1963*a*, *b*).

A similar study in Manhattan (Srole *et al.*, 1962) yields a figure of 23 per cent. Another sampling study (not yet published) of a small commune in France shows 26 per cent affected. The investigation of the entire population of two rural parishes in Sweden (Essen-Möller, 1956; Hagnell, 1964) indicates that 25 per cent have psychiatric difficulties. Studies in other areas are comparable. Research work conducted by Professor Lambo and myself (A. H. Leighton *et al.*, 1963*a*, *b*) in the western region of Nigeria points to 16 per cent of the adults in rural villages and 17 per cent of the adults in a segment of a city having psychiatric symptoms to a significant degree. (Some data in this and the previous paragraphs were calculated for this paper.)

These, however, are overall estimates for populations encompassing many sorts of groups and all socioeconomic levels in the

areas investigated. As soon as one begins to look at subdivisions of these populations, marked differences on a considerable range appear. Thus, in some communities in the North American rural area, 62 per cent of the population were estimated to have a significant degree of disorder. In Nigeria, the range was from 6 to 50 per cent.

Many readers will wish to challenge the validity of figures such as these, and to question what is meant by "psychiatric disorder". Since it is not possible to review the evidence on reliability and validity in population estimates and still have time to cover the main points of this paper, I can do no more than draw attention to the fact that, as above, numbers of different workers have produced figures which are impressively large, and ask that judgment is reserved until the relevant reports have been examined. The strongest claim I wish to make at this time is that the figures are probably true in terms of first approximation and that it is therefore worthwhile considering the implications.

With regard to what is meant by "psychiatric disorder", it should be made clear that in the present report this term covers virtually all the conditions usually found in textbooks of psychiatry. The greater part of the percentages quoted, however, refer to people with personality and psychoneurotic difficulties, conditions which, although significant for the degree of impairment caused, are nevertheless low-grade and chronic rather than sweeping. Psychotic disorders as a rule constitute less than 1 per cent of the total. The mildly impairing disorders are, nevertheless, germane to the thesis of this paper. While the people who manifest them are not in need of hospital treatment, they are persons with attitudes, sentiments and behaviours that are capable of exerting a considerable influence on what happens when cultural innovations are attempted.

Examples of such individuals in an African setting (A. Leighton *et al.* 1963*a*), include a 63-year-old Yoruba man encountered in a small forest village in the western region of Nigeria. He has not

sought us out, but when we talk to him we find that he is a mass of difficulties and objections. He dislikes living in the village and complains about the people's ignorance, but he is apparently tied economically to this manner of life because he has a small pension. His symptoms include being often in despair, having tired feelings for the last three years, and being exceedingly preoccupied and worried over money and over criticisms which he feels people are always levelling at him. He is also concerned about witchcraft and malign supernatural forces.

A 48-year-old woman in another village is depressed to the point of hopelessness and despair, and wants to die. She has difficulty making decisions, does not like to be among people (although formerly she did), and worries to such an extent that she feels this of itself adds to her troubles and causes her to lose weight. Her appetite is poor, and she is tired.

Moving from the forest to one of the large African cities, we encounter a 35-year-old woman with "nervous trouble". She explains this as feeling "jumpy", having a tendency to weep, a light head, trembling hands, and cold sweats. She has trouble sleeping, worries a lot and suffers from frequent headaches. These troubles are associated in her mind with marital difficulties that have come about since her husband's return from England. Her problems have led her to seek psychiatric help and the psychiatrist reports that she is a difficult person, subject to moods, with rigid ideas of right and wrong. He believes that her marital situation contributes to her troubles. Her husband, he thinks, is a tactless man, who feels inferior to his wife and punishes her by withdrawing his affection. She is disturbed by any mention of her relationship with her husband.

Another Yoruba woman, 60 years old, who shows mildly impairing paranoid symptoms, has been divorced from two husbands. She spent about twelve years in England and claims that ever since her return she has been "like a fish out of water", and finds that relations with other people are becoming increas-

ingly difficult. During the interview, she is suspicious of the interviewer and the reasons for her being singled out for our interview. She thinks there is a conspiracy against her, namely that her sister and her niece are attempting to defraud her of her share of the family estate. Other people who know the family say this is without foundation. She feels that people try to take advantage of her and that they have used black magic against her on occasions.

Changing now from Africa to Alaska, in data collected by Murphy (1965), a 31-year-old Eskimo with anxiety symptoms is described. After the death of a fiancée from tuberculosis, he eventually married another woman, but this marriage ended in divorce. In talking about the past he mentions a year during his adolescence when he thought he was going to have a nervous breakdown. His father refers to this as a "shaking illness". The main symptoms now are palpitations and headaches.

A 23-year-old Eskimo woman appears to be an instance of personality disorder. Almost everyone in the village tells of episodes in which she beats up her boy friend with a leather belt. She gave her husband similar treatment on several occasions, and in one incident attacked her mother-in-law. She is well known in this male-dominant culture for wanting to be "the boss", and various evidences of her violent temper are cited.

I have chosen Africa and Alaska for these illustrations because of the wide difference in cultural setting.

To review the ground covered thus far, I have said that cultural change and social disintegration may with profit be treated as separate concepts, with cultural change constituting a condition of risk for the development of social disintegration; and further, that one of the sets of factors predisposing toward social disintegration is a high frequency of psychiatric disorders in a population. This latter proposition again implies a conceptual distinction, with social disintegration considered separately from psychiatric disorder. Not only is one a social and the other an individual

phenomenon, but it is possible for either to be present in a population without the other.

One would suppose that there must be a threshold of psychiatric disorder prevalence which, if exceeded, will produce social disintegration with or without cultural change. Below this threshold there may be varying degrees of vulnerability such that the greater the frequency of disorder, the more likely the social system is to react with disintegration when an additional stress such as cultural change is added. The point to emphasize about this formulation is that it is open to testing. Systematic studies with available techniques can demonstrate its significance or its falseness.

The prevalence of such affective states as anxiety, depression, apathy, hostility, and suspicion do not facilitate either a well-co-ordinated clinging to the old, nor a resilient absorption of the new. Taking deliberate innovation, that is planned cultural change, for purposes of illustration, it can be seen that the village woman who is depressed and in despair is not in very good shape for comprehending (much less adopting for her children) dietary changes such as those aimed at counteracting protein deficiency and kwashiorkor. The same can be said for other health measures that seek to control the prevalence of infectious diseases. The man who hates his fellow villagers and thinks they are plotting against him with black magic is not likely to be a constructive member, should a village co-operative be organized to attack agricultural and economic problems. The Eskimo man with the shakes would have difficulty in taking advantage of a programme for training mechanics. The young woman who beats people with a belt would not be an asset to a home-makers' study group.

These illustrations are not rare incidents, but rather the stuff that gives development programmes serious trouble. Psychiatric epidemiology suggests, further, that certain populations which are of critical importance in developing nations are particularly high in disorder risk. The work of Lambo (1958) and Prince (1960, 1961), for example, points to students, people on whom the future

of the nation especially depends, as being particularly vulnerable, and other groups with a high risk could also be identified.

The postulate is, then, that cultural change when combined with a high prevalence of psychiatric disorder, especially in certain key sectors of a population, leads to social disintegration.

At this point it is appropriate to ask about the reverse effect, the effect of disintegration on mental health. Has not the claim been made by my colleagues and myself that social disintegration produces psychiatric disorder? Am I now saying the opposite?

The answer is not "either", "or", but "both in a reciprocal relationship". The presence of a high prevalence of disorder in a population makes it vulnerable to disintegration, and disintegration in turn, through various mechanisms of stress and reaction to stress and deterioration of child rearing conditions, increases the number and severity of disorders. A downward malfunctional spiral is thus initiated and perpetuated.

In a number of communities we have studied (Hughes *et al.*, 1960; D. Leighton *et al.*, 1963*a*) the evidence suggests that disintegration has preceded an increase of disorder prevalence, and in at least one community a resurgence of integration has been accompanied by an apparent dropping away of impairing symptoms (Leighton, A., 1965).

It also seems probable that "disintegration-before-disorder-increase" is the commonest situation around the world, although it is possible to imagine situations in which a rise in disorder might come first and be followed by consequential sociocultural disintegration. The emphasis I wish to give in this paper, however, is on reciprocity and the malfunctional spiral which results from the interaction of disintegration and the prevalence of certain characteristic psychological sets and patterns of behaviour.

CONCLUSIONS

From an heuristic point of view, the scheme presented suggests that if cultural change, social disintegration, and psychiatric

disorder can be tagged and followed by means of independent indicators, hypotheses may be tested through prediction and thus lawful relationships uncovered. This draws attention to a considerable area in which conceptual clarification, theory development, and methodological advance are all in demand.

On the more immediate and practical side it may be noted that social disintegration is an exceedingly widespread phenomenon, and often constitutes a highly resistant barrier to the successful accomplishment of planned innovation, whether it be economic development, education, public health, or all three. The problem is essentially one of reversing the downward spiral, and this obviously involves co-ordinate and long range planning as well as various economic, educational and political emergency measures. If the picture of relationships drawn in this paper is correct, then it must be recognized that people caught up in a disintegrative social process exhibit a characteristic range of behaviour patterns and attitudes that call for specialized handling by the agents of innovation. These behaviours and attitudes do not constitute something new, for they have been well recognized, studied and clinically treated in various ways for years. What is new and generally unrecognized is the matter of their prevalence in populations undergoing rapid change.

There is, in fact, a mental health component in the complex that invites technical attention. The most appropriate actions to take, however, are not easy to visualize since to be effective they must yield results in a relatively short time. Unfortunately, most of the dictates likely to arise from clinical work and orientation are long-term, and apt to depend on the presence of those very levels of formal education, affluence, motivation, and organization that are notably lacking in situations of disintegration.

There are, nevertheless, some models, especially in community psychiatry, that can serve as portals of entry for dealing with the problem. One of these is the training of the agents of innovation so that they will be better equipped to anticipate, recognize, and

deal with the difficulties of personality function. Relevant here is the way the community psychiatry team, generally composed of psychiatrist, psychologist and social worker, has begun in numbers of places (Shapiro and Maholick 1963; Caplan 1964) to give orientation and instruction to teachers, clergy, personnel officers, and general practitioners. As well as imparting a point of view and information, this team also serves as a consultant resource to whom the other non-psychiatric personnel can turn for guidance in dealing with particular cases. Something along this line could be developed and tried out as part of innovation programmes.

Parallel with this is an additional step which might have its home in the public health sector of the development complex. One of the major pragmatic advances in modern psychiatry is the use of medication to modify emotional states such as depression, anxiety and apathy. Very often the use of this kind of treatment is an interim measure which plays a critical role by enabling the patient to make a start toward improvement and begin taking hold of and using the resources that exist within and without. When this has been accomplished successfully, the medication can often be dropped as it is no longer necessary.

It is not, I think, too far-fetched to imagine that part of a programme of innovation and development could include an appraisal of symptom prevalence and the provision of medication. We are not, obviously, in a position at present to recommend any such step as this on a large scale. What does seem worthwhile, however, is an experimental attempt in a few selected communities. The aim would be to see if medication, along with other short forms of treatment, could reduce symptom prevalence in the population and so play a significant part, together with public health measures, education, and economic development, in rescuing communities from disintegration or in promoting cultural change without disintegration.

This is not the place to elaborate such an idea into a research

design, but it is worth noting that the resources and technical means for such trials are all at hand. The problem is one of converting what is known into a responsible, adequately controlled programme of experimental action.

SUMMARY

This paper has considered cultural change and social disintegration in populations as separate concepts, with cultural change constituting one condition of risk for the development of social disistegration. Another condition for risk is a high frequency of psychiatric disorder, which can similarly be considered as distinct from disintegration. Each of these three variables can be studied separately and systematically.

They are related in this paper in the postulate that cultural change when combined with a high rate of psychiatric disorder, especially in key segments of a population, tends to produce social disintegration. Change in itself is not necessarily followed by disintegration. A high rate of psychiatric disorder, however, makes a population vulnerable to disintegration, and conversely, continued disintegration may foster high rates of psychiatric disorder, the two conditions reinforcing each other in a downward spiral of malfunctioning.

This view of cultural change suggests that innovators and development agents should include mental health among the variables with which they work. Psychiatric programmes and techniques could be adapted to this end.

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DISCUSSION

Fortes: Professor Leighton has suggested that it is possible to establish a baseline for the incidence of psychiatric disorder in a community and then study the changes in this incidence in changing situations. In other words, one can use incidence of psychiatric disorder as a variable and test it against other variables. Any number of communities can be studied in this way and given a numerical figure to represent the incidence of psychiatric disorder in them, and this figure is a kind of a prognostic index of the community's adaptability for accepting cultural and social change. I think that Professor Leighton has enunciated a very important principle here, for this means that every community has an index of psychiatric disorder built into it and that this affects the way this particular community fits into the

total spectrum of social structure and culture. I would like Professor Leighton to tell us if I have interpreted him correctly.

Another problem here is that we have extremely little information about psychiatric disorders in completely pure, untouched, uncontacted civilizations. I think that this is one of the many serious gaps in our understanding of transcultural psychiatry.

Leighton: It is sometimes difficult to fill in these gaps because people in different cultures vary in their degree of co-operativeness in surveys. I can visualize cultural situations where people will not answer your questions at all; indeed we have encountered such people in Manhattan, but they are not a very large or important group in comparison with the total population that we are studying. I do not know to what extent the kind of studies I have outlined are possible on a world-wide scale, but we have been able to do them in a number of different cultures now and I suspect that it will be possible to study communities in this way in more places than we had anticipated.

Fortes: Professor Leighton, you have not really answered my first question. I could put this the other way round and say that the examples you gave us from your Eskimo communities showed that many psychiatric disorders there were due to exposure of these primitive communities to twentieth century, western economics, to extensive air, road and rail communications, in fact to so-called culture contact with western civilization. Since the Eskimos you refer to are acculturated Eskimos, we might say that you are taking as a baseline a stage in a social process which is already in progress; your starting point is within the circle of modern changes in this society. Could you tell us if you think that one could take such an arbitrary starting point and establish an index for the incidence and severity of psychiatric disorder for every community? I think this is a most interesting idea. It should be possible to forecast, with this hypothesis, whether or not there will be resistance in a primitive community to innovations of the kind you have mentioned. We should have to take an arbitrary figure, let us say 10 per cent, as an index of psychiatric disorder and say that an incidence of 10 per cent or over in a given community indicated the probability of resistance to innovation. Is this what you meant?

Leighton: I did not answer this question of yours because I find it a difficult one about which to be brief; I apologize for this. I think that

it is now impossible to find any culture anywhere that is not already undergoing a process of change. If this change is not coming from the West, or the confluence of cultures, it is a change that is being generated within the group itself and which is having essentially the same effect on the culture as if it had come from outside. So the answer to your question is that I do think you can establish this index for a community even though you are not indexing a pure *a priori* situation. It seems to me that the relationship between mental health and social disintegration is a reciprocal one, the greater the social disintegration the worse the mental health, and you are involved here in a vicious spiral. Our attempts to reverse this spiral, therefore, should centre not only on social and cultural conditions, but also on mental health.

Rubin: I understood Professor Leighton to say that he thought it was possible to pin-point a segment of the population in which set psychopathology may be found. This has been done in at least two American epidemiological studies, which found that the bulk of mental illness seemed to cluster in the lower socioeconomic groups. (Hollingshead, A. de B., and Redlich, F. C. [1958]. Social Class and Mental Illness. New York: Wiley; Srole, L., *et al.* [1962]. Mental Health in the Metropolis. New York: McGraw-Hill). One of the problems about this, as shown by various workers (Gans, H. J. [1962]. The Urban Villagers: Group and Class in the Life of Italian-Americans. New York: Free Press of Glencoe; Lewis, O. [1959]. Five Families: Mexican Case Studies in the Culture of Poverty. New York: Basic Books; Miller W. B. [1958]. *J. soc. Issues.*, **14**, p. 5; Miller, W. B., Geertz, H., and Cutter, S. G. [1961]. *J. Stud. interpersonal Processes*, **24**, p. 283), is that in the culture of poverty there is a pervasive apathy as well as hostility and resistance to change, and that these feelings are part of a kind of social pathology and an adaptation to a real situation rather than a psychiatric disorder. Professor Leighton, can one distinguish between these two, the social pathology and the true psychiatric disorder?

Leighton: To some extent, but not entirely, we are looking at the same thing from two different points of view. My colleagues and I have been studying these communities since about 1948 (Hughes, C. C., *et al.* [1960]. People of Cove and Woodlot: Communities from the Viewpoint of Social Psychiatry, vol. 2. New York: Basic Books;

Leighton, A. H. [1959]. My Name is Legion: Foundation for a Theory of Man in Relation to Culture, vol. 1. New York: Basic Books; Leighton, D. C., *et al.* [1963]. The Character of Danger: Psychiatric Symptoms in Selected Communities, vol. 3. New York: Basic Books) Such communities can be classified as poor, uneducated and disintegrated through a variety of criteria that have nothing to do with the amount of depression or psychosomatic and other psychiatric disorders in them. My theoretical position on this at the moment is that what you see in a community of poverty is the end product in sociocultural disintegration and its psychological effects, and that this is parallel to the social disintegration and its consequences which arise from cultural change. The original social causes in the two situations may be very different, but the end product, both from the point of view of social disintegration and of psychological states and attitudes, is very similar in the culture of poverty and in the disintegration of primitive cultures which has been precipitated by cultural change.

It is difficult to be clear about this without specific examples. I have had the opportunity of watching a community of poverty become reintegrated over a period of twelve years. This community changed from a profoundly disintegrated, rural slum with shacks, children sleeping in the garret and no inside plumbing as well as the full gamut of apathy, depression and paranoid and hostile attitudes, to a comfortable, secure and physically attractive place. Associations—formal and informal—developed, with leadership, followership and a reduction in the prevalence of psychiatric disorder.

Rubin: How did this come about?

Leighton: It happened through a combination of events: the first of these involved a deliberate effort on the part of two educators to develop the economic resources of the community and raise the educational level. To do this they recognized that they must both change attitudes and form a new social structure, and they set out to do these two things simultaneously. The first survey of this community showed that there were the beginnings of some informal organization among the women; there was also a feeling that education might be a way out. These two factors were exploited, and the rudimentary women's social organization was developed into a fully active group. After

the initial catalysis of the community by outsiders it began to carry on its own development.

The second event which changed this community was an entirely adventitious but rather interesting one. A power company built a dam near by which for three years offered constant labouring employment throughout the whole region; we were studying three other poverty stricken communities in the area and these also had this opportunity for full employment. The man recruiting labour for the power company was also a salesman for electric appliances. He encouraged the men to whom he gave jobs to buy washing-machines, television sets and similar machines from him. For the first time in their lives these people discovered credit. Once they owned washing machines or television sets they began to use them, and this changed their habits, their ideas, their clothes and the interiors of the houses. Whatever fantasies television may foster, one thing it undoubtedly does is to demonstrate the manners and speech desirable for social intercourse. As this community was originally quite isolated this meant that it gradually became easier for these people to talk to people outside their own circle. The breadwinners discovered something else about the television sets; if they did not retain their jobs and pay the premiums on the sets somebody came along and took them away. They were thus trapped into working rather steadily. One result of this was that the men from this community, who had previously had a reputation for being poor employees, gradually developed the opposite kind of reputation. These gradual changes involved a whole series of activities and attitudes and produced a kind of upward spiral in the community. There were a number of other major factors involved, but the ones that I have mentioned serve to illustrate my thesis.

Murphy: Your psychiatric assessment of this community must have given it a poor prognosis and yet, without psychiatric action, the outcome here was favourable. Surely this is contrary to your own theory.

Leighton: That hardly follows. I think Professor Fortes was right when he suggested that this is a circular (I used the term spiral) relationship. I have believed for many years that the hypothesis that social disintegration produces psychiatric disorder should be investigated,

and in this particular community we found that the evidence supported this hypothesis. We should be able to use both psychiatric and social lines of attack to influence the effect of change on a community. I would suggest that if we had used drugs and psychotherapy in this community the changes I have described might have taken only five years instead of twelve.

Rubin: What about the influence of the fortuitous events, the dam and the television sets?

Leighton: These were tremendously important, of course, What I was advocating in my presentation was not that by giving pills to people you can completely change a community, but that if the mental health of a community, as well as its education, public health and economic development, is given attention improvement might occur faster and more smoothly than when the mental health component is ignored.

Yap: Social change may be disintegrative or integrative, depending on the time perspective and also on the values by which we are going to judge the situation, which may also, of course, be related to time. I would like to put forward the perhaps unscientific suggestion that we may gain insight into some of the mental health consequences of social change by biographical study of historical figures who held unique and significant positions in particular social movements. Some years ago I investigated the story of Hung Hsiu-Ch'uan, the founder of the Tai-ping rebellion in China in the latter part of the last century (Yap, P.M. [1954]. *J. Asian Stud.*, **13**, 287). Hung Hsiu-Ch'uan was a schoolmaster, a village intellectual, who was very distressed by the state of the country at that time when China was faced with the first waves of expanding economy from the West. It is recorded that he became ill and remained, for forty days, in a semi-stuporose condition. In his illness he thought that he had been received by the Christian God, who was blond with a blond beard, and that this God had commanded him to go down to earth and slay all the evil demons there. He recovered from his illness and remained apparently normal for three or four years until one day, in Canton, he saw a Protestant missionary with a blond beard. This rekindled his old feelings, for it seems that he had not completely recovered insight into his past illness and still felt that his experiences might have been real. He organized

the people and started the Tai-ping revolution which eventually succeeded in creating the kingdom of Tai-ping. This kingdom adopted certain Christian ideas and introduced social reforms such as the prohibition of foot-binding, the equal distribution of land and universal education. At that time, the Ching dynasty persecuted this man as a bandit and a rebel, but now, because of his reforms, the Chinese government see him as a hero. Was he a genius or was he a madman? I do not know. I have told this story of a Messianic movement as an illustration of the relationship of social changes to mental health.

Loudon: Many personality attributes, such as the dissatisfactions, mild paranoia and fear of the supernatural described by Professor Leighton in his paper, may be present in people who do not consider themselves to be ill and who are not necessarily regarded as mentally disabled by the psychiatrists. How can we be sure, in comparative studies, that the people we describe as normal in one section of a society are being assessed by the same criteria as normal persons in another section? Professor Leighton, can you throw any light on this problem?

Leighton: There is no real answer to this except to say that I share Dr. Loudon's anxiety about it. I am also very sceptical about fairly small differences in percentages given for normals and abnormals in various cultures really representing absolute differences. For example, what significance should we place on the observation that 42 per cent of men in Nigerian villages have psychiatric symptoms compared to 47 per cent of men in Stirling County? On the other hand, I would be more inclined to believe that results are significant if one finds that the ratio of psychiatric disorders in men and women is different in Nigeria, from the ratio in Sweden or in North America. I also think that comparisons between villages in the same culture are more likely to be valid than comparisons of villages across cultures. In our Nigerian study (Leighton, A. H., *et al.* [1963]. Psychiatric Disorder among the Yoruba. New York: Cornell University Press), we used a systematic questionnaire which had been translated into Yoruba, tried out in pilot studies and then re-translated with the aid of Professor Lambo, Dr. Asuni, two psychiatric nurses who were themselves Yoruba speakers and other assistants. In our attempt to develop an instrument which would be appropriate to the culture we also had native healers helping with

the terminology in each rural area. Further, to try and ensure that the methods we were using really were eliciting the information that we needed, the interviewing with the questionnaire was done (through interpreters) by psychiatrists. The results were evaluated independently by two psychiatrists and then a joint rating was made. Professor Lambo and Dr. Asuni sat in during some of the interviews and listened to what was being said in Yoruba as well as to the translation in English. They were also present for most of the sessions when the joint rating was made from the two independent evaluations. It seemed to us that in this way we took into account the meaning of cultural variations as much as we could.

We attempted to check the validity of our results by using these same techniques on about 60 in-patients and some of the patients of native healers who were severely ill.

In a North American study (unpublished) we have recently taken a sub-sample of people whose mental health has been rated by a questionnaire, and had them re-examined by a psychiatrist. Each subject was seen by one psychiatrist only, but a total of five psychiatrists took part in the study. The psychiatrist first made his clinical evaluation without knowledge of the results of the survey, he then read the survey and after this, if he wished, he could have a second interview with the respondent to try to clarify any discrepancies between his own findings and those of the survey. He could also interview the local practitioners and make a second rating with data from them. The results of this study are now being prepared for publication, but in general one can say that there was a satisfactory level of agreement between the psychiatrists and the survey.

Loudon: It is perhaps worth mentioning that we have used this kind of technique for validation in our South Wales studies, with one psychiatrist working blind on a stratified random sample which had previously been assessed by the use of a questionnaire. It looks as if our results are likely to be similar to Professor Leighton's.

Transcultural Psychiatry
A.V.S. Deuck & Ruth Porter

ADULT ROLES

MARGARET MEAD

American Museum of Natural History, New York

EXPLORATION of the relationship between the experiences of infancy and early childhood and the functioning of the individual in adult life has led to the most fruitful interaction between psychiatry and anthropology. It has also been the most frequent source of misunderstanding.

Far too often a comparison of apparent similarities between the regularities in culture worked out by the anthropologist and the aetiology of specific types of neurotic functioning worked out by the psychiatrist has had as one outcome a premature consensus on points of theory (Kluckhohn, 1956; Mead, 1961*b*; Roheim, 1950). In such a comparison the anthropologist's detailed description of the circumstances under which infants in a given society are indulged or disciplined, are fed frequently or infrequently, and are protected or exposed to hardship is placed beside the psychiatrist's account of the interrelationship of events arrived at by the laborious process of tracing back in time a series of experiences that have left an indelible mark on a patient, deeply affecting the way he meets life, and the two sets of data appear to be comparable. It then seems an easy step to a consideration of the adult character of a whole people, all of whom have been reared in ways that approximate the distorting experiences of the individual patient (Du Bois, 1944; Kardiner, 1939; Spitz, 1935). As a result, the theorist in the field of culture and personality is tempted to treat child-rearing practices as part of a simple causal sequence in which certain experiences in childhood, narrowly defined in terms of the child's relationship to parental figures and to its own developing

impulses, have their outcome in a certain type of adult character that is coterminous with the culture. Acceptance of this view leads to the equation of the regularities of child rearing in a culture with the regularities of certain types of distorting experience in the explored personalities of neurotics (or occasionally of psychotics), as well as the equation of the life history of the psychiatric patient in our own society with characteristic life histories of members of other cultures. But when this is done, serious difficulties immediately arise.

When an anthropologist studies the cultural character of a given people, he includes in his account *all* members of the society, not merely those who break down or show obvious personality distortions. Childhood deprivation or childhood indulgence is, in his account, a cultural regularity; that is, it applies to all children reared in that society. And it is regularities of this kind, not occasional deviations in practice, that are compared with regularities in a particular individual's developmental history in our own society. For example, one can build a sequence that leads from an early childhood experience of extreme indulgence—permissive breast-feeding, continuous carrying, low demands on muscular control, on individual autonomy, and on a cognitive grasp of experience, and also slow weaning and parental toleration of tantrums arising from frustration, to an adult character that is essentially receptive and responsive, in which the capacity for organizing experience is poorly developed, sex and cherishing behaviour are equated, the positive emotions of passion and anger are feared, and the approach to others is a dependent and passive-aggressive one (Mead, 1935). In the anthropologist's account of a culture, these are pervasive experiences and characteristics common to all members of a group, with allowance for individual constitutional and experiential variation. But as the same sequence and combination of characteristics are seen by the psychiatrist, they occur in an individual who reaches his consulting room because of some recognized failure in his adjustment to ongoing life. Unless

one is prepared to accept the generalization that all members of one society—or all members of all societies—are neurotic and in need of treatment, it is obvious that there is a discrepancy of circumstance; for while the anthropologist's subjects are functioning, reproducing members of a living society, the psychiatrist's patients are deviant individuals who are suffering or causing others to suffer, in ways that their own society recognizes as deviant. And if this is so, some other reconciliation of differences must be found.

One approach to such a reconciliation of theory lies, I think, in a better understanding of the process of development in societies that are undergoing rapid, rather than slow, culture change. The psychiatrist's model for the developmental treatment of character is based on the study of individuals in rapidly changing societies, where the child's life differs from the life experienced by his parents and the supports offered by his society are, in the case of the individual who becomes a patient, inadequate to his needs. The psychiatrist, focusing on the idiosyncratic experiences of his individual patient, traces the events of a particular life history. His model is the single human being, who is born of a particular pair of parents and whose development takes place in response to the vicissitudes of life in a particular family and a specific interpersonal setting. When the individual, so defined, is observed, the particular succession of events—weaning, habit training, the birth of siblings, separation from the immediate family circle and so on—is seen as causative, and the culture is taken into account only in very general statements, such as, "his parents were strict Catholics", or, "he came from a hardworking peasant family", or, "his parents were circus performers and the family lived an itinerant existence". In effect, inclusive case history statements of this kind obscure the wealth of information about the tradition in which the patient was reared, especially when the psychiatrist and those with whom he wishes to communicate share a common culture, in which the words "Catholic", "peasant", and "circus" carry the same rich load of reference for all of them.

The anthropologist's model is a very different one. It is the small, self-contained, very slowly changing society. In this context, he views the individual life history as part of the life cycle of a group that includes, at any given time, members of both sexes as infants, children, adolescents, and adults of all ages, whose lives are interrelated in such ways that the acts of any one member of either sex or any age are linked to the behaviour of all others. In clinical practice the psychiatrist may have a patient who retains, as an encapsulated, crippling memory, a picture of a black-shawled mother wailing over a corpse, though he himself now lives in a society from which black shawls and formal mourning have long since been banished. But, in the kind of society on which the anthropologist draws for his model, the shawl and the formal mourning are present-day events as well as childhood memories. At every funeral a child's mother, aunts, and grandmother and a man's wife, sisters, and daughters will dress alike and mourn in congruent tones. The particular events in the life history of an individual are also the continuing events affecting everyone's life. They do not occur in a single, individually significant sequence, each event triggering or underlying the next. Instead, all events affecting the individual enter into an intermeshing pattern of repetition, reinforcement, and recapitulation, so that the life of one's children evokes one's own actual childhood and one's own childhood pictures of the old age toward which one is, in fact, moving. No stage of living is single or isolated. Instead, at every stage, the individual is continually reinforced by the presence of others living out other stages in a congruent way.

When a description of a retrospective life history of an individual in trouble and a description of the experiences and interrelationships of a biologically complete group are set side by side, and we focus our attention on their salient characteristics, two things become apparent. First, the configurations with which we are dealing are discrepant. And second, the discrepancies are related

to the different models on which the two kinds of description are based. Moreover, a comparison of these two models highlights the fact that the contemporary psychiatric model has been shaped by experience in a society in which individuals are not given support by the repetition of congruent forms of behaviour, but, on the contrary, are continually confronted by situations that have no systematic precursors in their own experience or that of their associates. Within this context the psychiatrist's concentration on the sequence of events in the individual life history assumes a new significance.

A parallel can be found in the use of a complex projective technique, such as the Rorschach test, in contrasting cultural situations. It is our experience that where we are concerned with the cultural character of members of a stable primitive society, the use of such techniques adds very little to our understanding that cannot be obtained through meticulous work with interviews, sequences of observed social behaviour, and the analysis of myths, folk tales, and rituals. However, as soon as a society becomes involved in the problems of change and individuals who have been reared to respond to a systematically reliable environment are confronted by the uncertainties of change, exploration with the Rorschach test—which was itself designed to delineate individual psychological functioning in a changing, differentiated society—becomes a relevant method of distinguishing among individuals who are responding differently to change (Mead, 1954*a*).

In dynamic terms, every culture, whether or not it is undergoing rapid change, must contain ways of rearing and sustaining individuals who also, in effect, sustain the society. Otherwise the society cannot survive. Where individuals are reared without the ability, in their turn, to rear a new generation that is able to carry on an effective life, the society itself will disappear. This does not mean that there must be a perfect fit between the needs of individuals and the needs of a society, but only that where the discrepancy

between these two sets of needs is very great the society will disintegrate. Its members will be taken into captivity, or they will be scattered, or they will die off, or, even though they may live on, they can do so only as dependents, because their own sustaining institutions have collapsed. It can be said that, from an evolutionary viewpoint, human existence has been shaped by the long struggle between more and less efficient forms of culture, and that each cultural system can be defined, at any point in time, in terms of its internal possibilities of success and failure in the face of particular forms of challenge. The capacity for success is not the same in different periods and in the face of different forms of challenge. The societies and cultural systems we are able to study at any given period are those that have survived up to that time, but already, by the time a given primitive society is exposed to the sophistication of the social scientist, change is setting in. The image of the completely primitive, self-contained, self-reinforcing society, like the image of the single, completely self-consistent individual life, is a construct from less complete scientific experience.

In any ongoing society, the cultural ways of dealing with individual impulse life are a significant aspect of the survival system. And these regularities in the handling of impulses and modes of thought, which were appropriate to much earlier stages in hominid life, as projected by Professor Hallowell (1965), provide the nexis between the anthropological study of the culture through a study of all its members and the psychiatric study of individual members of a culture who break down.

Our present evidence appears to indicate that the basic mechanisms by which individuals develop and maintain an ego structure do not differ from one society to another, but that there are cultural differences in the relative importance of the various mechanisms as well as in the intensity of focus on certain types of nuclear conflict. For example, cultures differ in the proportional use made of such mechanisms as projection and denial (Wolfen-

stein and Leites, 1950), in the extent to which they develop intra-punitive and extra-punitive forms of behaviour, in their focus on weaning or sphincter control or the oedipal situation, and in the ways in which they pattern development so that childhood, adolescence, or maturity may be a period of maximum or minimum stress (Erikson, 1964). These cultural variations are within the definable bounds of our understanding of how the human psyche varies when individuals are studied intensively in depth.

It has been extremely fruitful, therefore, to make cross-disciplinary comparisons between the characteristic forms of these mechanisms and their combinations in different cultures, on the one hand, and the forms and combinations (configurations or types) of these mechanisms as they have been identified in individual dynamic psychology, on the other. The psychiatrist can approach the study of culture by treating its character formation processes as individual character formation processes writ large and given historical form. And the anthropologist, in turn, can gain valuable understanding both of the psychological processes characteristic of the culture he is studying and, following the original insights of Freud, of evolutionary processes of human development before the emergence of *Homo sapiens* (Freud, 1918; Mead, 1963). Psychiatric insights may lead to exaggerations of the individual life history and anthropological insights may exaggerate the role of culture. Yet the interchange is a very fruitful one as long as it remains on the level of theory.

The situation changes, however, when attention is focused on the problem of transcultural psychiatry not as a body of theory derived from an intensive examination of individuals, but as a body of practice designed to heal. For then the discrepancies between insights based on individual case histories in changing societies and insights based on studies of individuals within an intricate, multi-generational social network become more apparent. The existence of these discrepancies and their importance is pointed up, I feel, in statements of doubt as to whether psychi-

atric practice, based on the scientific study of thought processes, is more effective than are the therapies provided in primitive, traditional, and modern societies by healers whose methods are based on religious, magical, or other social practices. Yet where such methods conform in some respect to our knowledge of psychotherapy, they do so only in the sense that psychotherapy itself is based on the recognition of universal situations. (A parallel can be found, for example, in the correspondence between traditional methods of cookery and methods based on an analysis of the processes involved by means of modern chemistry.) Actually, when we compare the therapies that are deeply based in a culture with the individual therapies that proceed from the modern scientific study of individual psychodynamics, we are making a comparison only by analogy. For the traditional curing methods provided by religious ritual, magical practice, and social participation (as, for example, when the members of an Eskimo group are able to rescue a young woman who has fled onto an ice floe during a fit of hysteria) are available only in those societies in which one finds the wholeness, and only to those individuals who still retain the sense of wholeness, that characterizes the intact, not very rapidly changing culture. In modern urban situations, the patient who turns to, or is referred to, the psychiatrist is typically someone who has been alienated in the course of rapid culture change, that is, he is a kind of person to whom the older traditional methods of curing are not available. In fact, the kinds of patients who are treated by the older methods and those who are treated by modern scientifically based methods are essentially incomparable, and these different modes of treatment also are incomparable.

In saying this, however, one should not lose sight of the question of whether methods of therapy that are deeply grounded in the culture may not, even now, have possibilities for success under certain circumstances. If they do, it may be worth while for preventive purposes to encourage the development of new

culturally congruent forms of therapy. In our own society parallels may be found in the use of various forms of group therapy, in the types of mutual support given by individuals who have common problems, in sheltered workshops, and in other self-conscious attempts to reproduce in a complex, partly disintegrated society the forms of help, reassurance, and support that were available in the past in a more integrated society.

Two possibilities may be envisaged. One would be the development of new forms of therapy that would be congruent with the very rapid cultural change that underlies modern individual psychotherapy. The other would be the incorporation into modern cultural forms of an expectation that individual psychotherapy is desirable under various circumstances and at various stages in the life cycle. It might be feasible, for example, to institutionalize the use of brief, exploratory, therapeutic sessions before the oedipal crisis, in situations with traumatic implications, such as the divorce of parents or the death of a parent, and during transitional periods as, for example, at graduation, before marriage and parenthood, and at the point of undertaking a new career. That is, it may be possible to develop in modern societies new institutional forms that reflect our psychodynamic knowledge and that are grounded in the culture out of which this knowledge came.

As new institutions of this kind were created and tested in at least one rapidly changing culture, they would become a source of models for new forms that might be transplanted, under appropriate psychiatric and anthropological guidance, to other changing cultures and there developed in styles that were internally consistent. The end result might well be culturally regular forms of therapeutic intervention that would be more reliably effective than the ceremonies of magical cure and restitution that served primitive peoples so well in the past.

These remarks are by way of comment on the central topic of this symposium.

2.

Turning now to my assigned subject, I shall consider the problem of adult roles under two headings: first, the relationship between childhood preparation and adult roles in relatively stable, slowly changing societies; and second, the relationship between childhood expectations and adult roles in rapidly changing societies. Here it is necessary to point out that our major sources of information in those two categories are different, as the main material on slowly changing societies is derived from anthropology, while the main material on rapidly changing societies is derived from psychiatry.

Almost as soon as anthropologists had built up a corpus of information about the details of childhood in various primitive societies, it became clear that the sequence of childhood experiences and the cultural handling of these experiences could be very differently related to the adult roles imposed by particular societies (Benedict, 1959; Erikson, 1939; Linton, 1939; Mead, 1937).

In some societies the whole of childhood was a preparation for a series of smooth transitions to adulthood; in others, on the contrary, transitions were marked by various types of discontinuity. Among the Bathonga of South Africa (Goldman, 1961; Junod, 1927) periods of indulgence were followed by periods of sharp discipline and one period of life was seen as opposed to another. The Manus of the Admiralty Islands (Mead, 1930) presented a particularly striking example of a childhood that was carefree and unconcerned with property, competition, or gain and that was followed by a young manhood of humiliating exploitation by others and an adulthood of intense preoccupation with competitive gain. More recent studies have made available additional materials on continuity and discontinuity in the cultural handling of transitions, as well as contrasting ways of handling discontinuities. So one finds, for example, in Japanese culture, the indulgence accorded the young child and the harshness of discipline

subsequently meted out to the schoolboy (Benedict, 1946) or, in French culture, the discipline of childhood and the freedom in adulthood to enjoy the pleasures of living (Dolto, 1955; Métraux and Mead, 1954).

By now enough material on different cultures has been analysed to demonstrate that there are many variations in the path taken to adulthood. Childhood may be treated as a simple precursor of adulthood. Or adulthood may be treated as a reversal of a childhood role. Or adults may remain in a subordinate position as long as their parents remain alive. Or childhood experiences and the experiences of adulthood may be treated as analogues, as when the bride's removal from her parents' to her husband's home is treated as a death and a rebirth. Or the analogies may be muted or ignored altogether. Under all these varying circumstances children born into a culture do finally become adult members of their culture, but it is pertinent to ask whether certain types of discontinuity may not lead to strain that may, or may not, be met by appropriate ameliorative ceremonies.

Such differences may be significant for the entrance of psychiatry when a society becomes an open and changing one. Where the discrepancies, discontinuities, and tensions between the form of upbringing and the expected adult roles are great, cultural breakdown and disorganization may produce a great many marred and disoriented individuals who lack the protection of the special traditional initiation ceremony, ritual rebellion, or ceremonial expression of some deep, culturally fostered tension. In particular, the destruction of traditional *rites de passage*, whether these are primitive tribal initiation rites (Van Gennep, 1960) or Victorian forms of mourning (Gorer, 1965), may leave a very large number of individuals without the protections that formerly would have been available to them, or, indeed, that would have been enjoined upon them.

Such changes are especially likely to occur when people shift from a rural to an urban mode of life, and their occurrence should

be anticipated in mental health planning for any country in which rapid urbanization is taking place. Where the shift involves migration from one country to another, the adult migrant may sustain the move without serious impairment of his own mental health (Mead, 1953). But the next generation may suffer from very great difficulties of adaptation, for the infant and early childhood upbringing of the children of immigrants may be quite incompatible with the institutions of the wider society. For example, in the parental culture, childhood may be seen as appropriately dependent and indulged, but in the wider society there may be an expectation of early training in autonomy and self-reliance (Martin, 1943). Then children who are reared within a continuingly coherent immigrant setting and later are exposed, through their schooling and other experiences outside the home, to a different though coherent wider culture may reflect the absence of the childhood experiences that would have been appropriate to the adult culture or, alternatively, the absence of the sustaining rituals that were appropriate to their early childhood training. Such strains may occur even in the absence of rapid culture change wherever there are population interchanges between countries.

Still using the model of the small, relatively stable society, one may ask how the delineation of adult roles prepares the child for participation in the adult society, and also how the existence of this delineation can serve as a formative influence in the child's development. In many societies the range of adult occupations open to the individual carry with them, explicitly or implicitly, specifications as to the personality characteristics that are believed to be appropriate to each occupation or role. The hunter should be swift, the fighter brave, the fisherman patient, the singer versatile, the magician dour and silent, the witch ugly. As particular traits are identified in children, the fact of this identification may destine them for one occupational choice rather than another, and those who make an occupational choice will be encouraged to develop and emphasize the appropriate traits. And,

generation after generation, hunters will be swift, fighters brave, and fishermen patient. The diagnostic eye, assisted by the formative effects of the diagnosis, will fit the child for his future role.

In a tightly organized society where there are hierarchical arrangements of caste and class, occupation and power, the mechanisms of selection and designation may work well or poorly in fitting the particular child for his expected role. For example, an institution like primogeniture carries with it disproportionately different role expectations for the first-born son and later born sons. Therefore, where primogeniture occurs, the role of the eldest son must be slowly tempered by lower expectations, or extreme differences in training must be instituted, or ruses must be devised to deal with discrepancies in character and role. Among the Maori of New Zealand, for example, when the inheriting first-born son did not dare bind his father's corpse, a younger son might perform the act that made him heir to his father's position. Elsewhere the processes of education may so well succeed that, as the Samoans of Manu'a phrased it in 1925, a man of low rank could not be brave because he would not dare to go ahead.

In situations of this kind, negative sanctions work more effectively than positive sanctions. The fact that a man is born a prince or a nobleman cannot assure nobility of character and behaviour, but the status of slave, serf, or commoner is an effective deterrent to the play of ambition and aspiration in most societies. More complex situations arise in a multi-caste system like that in India. There, as Carstairs (1957) has shown, each member of the society introjects not only the image of the behaviour that is appropriate for himself as a member of a caste but also the image of the behaviour of each other caste that would be inappropriate for him.

Sex membership is, of course, the source of the simplest form of negative introjection of other roles. In most societies children are taught how they should behave as members of their own sex group and what behaviour they should avoid in order not to be

classed with the opposite sex. Present evidence suggests that the more completely a single adult sex role is defined as being *not* the role of the other sex, the less flexible and the more vulnerable will be those individuals who, for any reason of temperament or trauma, are attracted to any aspect of the role of the opposite sex. Very clear, mutually exclusive definitions of sex, caste, or occupational roles that include a counterpointing both of the suitable and the unavailable roles may provide quite high personality stability, with only occasional extreme deviance. But in situations of change they lead to conditions in which individual personality breakdown is likely to occur. It is illuminating to compare the effect on individuals who come from a society with very clear status definitions (as, for example, Germany) when, under conditions of social disintegration, emigration, or exile, they are confronted with the necessity of accepting a downward redefinition of their status, with what happens under like conditions to those who come from cultures where individuality is relatively more important than social status (as, for example, Greece or the rural United States). In the one case personality breakdowns may follow on the inability to accept such a redefinition, while in the other case individuals are able to accept marked changes in status with, relatively, far less disturbance.

In considering the relationship of particular adult roles to character formation and so to mental health, it is important to recognize the significance of those roles that are special and rare and yet enter into the fantasy of all members of a culture. At one extreme these include such figures as the king and the queen, the young man who is chosen to play the role of a god who will be sacrificed after a year of royal living, and the athlete who performs extraordinary feats; at the other extreme they include such figures as the hangman and the polluted person who performs necessary but unspeakable tasks. The suicide may fall into the category either of the heroic or of the repellent and the disapproved. All these roles, although they are physically played by only a few individuals,

are models for all—types to be approximated or shunned—both in self-evaluation and in a recognition of how the individual is evaluated by others. In general it can be said that the range of self-perception—the range of possible roles in relation to which an individual views his present and his future roles—is a function of the total number and kind of roles existing in his society.

This means that one must include not only political, economic, and social roles, but also those with religious and artistic relevance. Although a capacity for mysticism or trance may be presumed to be present in some—possibly in all—human beings, expressions of mysticism and forms of trance behaviour occur normally in adults only in those societies where fasting, asceticism, trance, and ecstasy are recognized states, presented to growing children in actuality and in symbol and story. The limits within which one must stay and outside which one may not go are culturally determined and provide the framework within which judgments are made as to the sanity or madness of those individuals who, whether because of constitution or upbringing, are least able to conform to cultural expectations.

This is the sense in which Ruth Benedict (1938) discussed culture and abnormality. Her discussion of the different roles assigned by different cultures to various personality types does not conflict, as some critics, for example Wegrocki (1948), have suggested, with the view that certain kinds of mental instability are human and are to be found in some individuals in all societies, however different and contrasting may be the roles assigned to them. Present evidence suggests that certain forms of behaviour that can be diagnosed as psychotic occur in all cultures. The psychotic individual who becomes a *shaman* or an inspirational leader is nonetheless psychotic; the difference lies in the fact that he may be better "adjusted", more socially useful, or more destructive in one culture than in another (Devereux, 1957).

From the standpoint of transcultural psychiatry, a cultural change that eliminates roles within which, in a certain society,

psychotic tendencies can be expressed or sheltered, as, for example, among the wandering religious mendicants of India (Soddy, 1961), brings about one of the kinds of situation within which new forms of care are required. In mental hospitals in the United States there are individuals whose psychosis takes the form of self-identification with religious figures and also individuals who once played a permitted role in some narrow and extremist cult without any perceptible breakdown, but who later, under changing conditions (connected, perhaps, with a move to the city), went beyond the limits of toleration even of other sect members and, after they had been repudiated by their followers, sought or were sent to the new institutional shelter, the mental hospital.

In considering the effects of culturally recognized adult roles on personality and mental health, it is important to take note of perceived forms of character as well as institutionalized roles. For children reared to have certain expectations will respond, within the limits of their constitution and their experience, to the influence of the whole range of recognized character possibilities, that is, not only to expectations about positively valued qualities, such as bravery or generosity, but also to expectations about negatively valued qualities, such as cowardice, greed, lust, uncontrollable temper, or psychological martyrdom. Among many peoples the fear that their male children may become passive homosexuals is a potent formative influence on the children who are intently watched and tested. But among peoples who do not conceive of or who do not fear passive homosexuality, the same difficulties do not arise. The image of a bisexual, inclusive character like that of the Navaho *berdache* (Hill, 1935) gives quite a different form to potential or learned bisexuality. And where the angry man who cannot control his temper is a known and a named type, the capacity of every child to control his or her temper is thereby limited and compromised.

In some societies a very early identification is made of expected types. A baby bangs his silver bracelet and in doing so hurts his

cousin's hand. A bystander immediately comments, "I Bawa is cruel!" His comment immediately establishes an expectation that will be verified and reinforced each time some act, intentional or unintentional, appears to validate the original judgment. The expectation of others that a particular child will fit one of the recognized character types both shapes the child's behaviour and increases the visibility of certain kinds of behaviour. In Bali there is a general division of people, both men and women, into two categories, the serious and the mischievously frivolous. The distinction is made early and is enforced throughout life. Where diagnoses of this kind are in keeping with the individual's temperamental possibilities, the early enforcement of adult psychological roles may serve either to reinforce or to intensify too greatly specific constitutional tendencies. But where the diagnosis incorrectly matches perceived temperament or is not accompanied by adequately enforcible sanctions, the discrepancy between actual personality and assigned psychological role may introduce special forms of strain and maladjustment. Patience and humility may be expected of the hunchback who is seething with hostility and resentment; or sorcery may be attributed to a recent widower, a little man covered with ringworm who is assumed to be angry when he is only relieved. For those who have been subjected to misdiagnoses that deeply affect their sense of themselves, emigration may not lead to disorientation and disturbance; instead, it may provide them with a new environment in which they may be cast in roles they can play with greater ease and a greater sense of appropriateness. One analogue of such cases is the psychiatric patient who cannot be helped until he can express the anger that neither he nor his associates believe he harbours.

Another way in which adult roles may shape psychological role choice was exemplified in the stories by pre-revolutionary Russian writers, who described the household or *mir* in which a child was exposed to a large number of extreme types—the drunkard, the spendthrift, the anarchist, the ascetic, the miser

and so on—and, exposed to all of them, the child eventually chose one of these extreme roles himself. Where extremities in contrast are the only type of roles presented, choice is limited to some form of extremity.

3.

Cultural variations such as these are found in every society. In stable cultures they are simpler and more sharply defined; in complex, urbanized, rapidly changing cultures they are more complicated and less clear. In such situations the complications may also take a variety of forms.

The child who grows up as a member of an ethnic group living in an enclave is given a picture of adult roles in his home and community, but when he moves out into the larger world he is lost. The American child of very devout, orthodox Protestant parents, who has been taught that drinking, smoking, dancing and card playing are wrong, faces confusion, doubt, and loss of religious faith when he discovers, in the larger heterodox community, that those who otherwise meet his standards of good and ethical behaviour nevertheless do dance, drink, smoke, and play cards. But some minority groups so successfully shape their children that they find the different standards of the world outside their community too distasteful, repugnant, and frightening to be endured and, as a result, they are unable to break the psychological bonds linking them to their childhood community. There are, for example, the children of the kibbutz who cling together and either return to the kibbutz or, at most, form a new one (Spiro, 1958); the orthodox Jews who shrink from a world where they are in danger of eating *treyf*; the Hutterites who return to their settlements where they are cut off from news of the outside world (Eaton and Weil, 1955); and the young people who have apparently adapted themselves to the ways of the larger society but who return at marriage to the behaviour of their natal group (Stern, 1930).

Especially in complex societies, the crucial problem for the transcultural psychiatrist is that of understanding the traditional adult roles and forms of role preparation. Without such understanding he cannot judge, allow for, or deal with the kinds of breakdown that are characteristic of the individual who has been reared in the expectation of filling one role or set of roles but who, in a changed cultural situation, finds himself attempting to fill some very different role or, on the contrary, attempting to fit a familiar role into an inappropriate setting. Nor can the psychiatrist understand how, in his relationship to his child, such an individual may attempt to present a picture of parental behaviour that is keyed in part to his memory of his own parents, from whom he differs, and in part to his child, who differs from himself as a child and has no known adult sequential patterns. Such an individual cannot refer to his past for it will betray the man who stands in a transitional position; nor can he, as he might in a more stable society, count on his children's ability to provide him with the cues of behaviour learned from kin and age mates. His expectations, based on a sequence of prefiguration, co-figuration, and post-figuration (Mead, 1961*a*), in which each event reinforces a consistency of character, are radically disturbed. Past, present, and future have lost their coherence. Or, reared by parents whose expectations have failed them, he may have no coherent expectations of his own about the interrelationships of stages of living.

Under these circumstances neither patient nor therapist can turn with any certainty to an older cultural model. Each event in the patient's life, instead of being part of a familiar way of life shared by his predecessors, his peers, and his successors, appears to be only individually significant and traumatic. Where this is so, specific events—premature weaning, severe toilet training, or unusual primal scene experiences—are cited by patients and treated by psychiatrists as directly related to the maladjustments and suffering that brought the patient to the consulting room or clinic. Where in the past individual suffering and madness could

be referred to a recognizable belief system, in accordance with which a sin or a broken taboo, an accidental encounter with an evil spirit or some evidence of special selection by the gods placed the sufferer in a known context, today patient and psychiatrist face an apparently idiosyncratic sequence for which they must find some cause—toxic, genetic, traumatic, or experiential.

Exploration of this kind, carried out under the conditions of rapid change and cultural ferment of the late nineteenth and early twentieth century, led to our new knowledge of individual psychodynamics, with its important consequences for our understanding of cultural psychodynamics. Yet it is still very easy to make the mistake of treating as identical the source of our knowledge and the cause of the condition that provides us with our cues.†

Everywhere in the world today there are individuals, sometimes numbered in millions and sometimes limited to the few young, modern-educated leaders of a newly emerging country, on whom the pressures of change weigh with extraordinary force. Such individuals may be the masses of children who for the first time in the history of their society are gathered into schools for the purpose of a generalized formal education; or they may be the few young men who were trained to be rural school teachers and medical assistants, but who are now charged with responsibility for the education and health of millions. Their protection, and when they break down their treatment, is of the utmost urgent concern to the modern world. In order to support and care for them, whether as enormous groups of recently urbanized and disoriented persons or as the small number of individuals who have been singled out by reason of the valuable contributions they have made or may make, the therapist will need a knowledge both of

† This point is well illustrated in Gorer's discussion of how Russian swaddling practices provided him with a clue to the understanding of Russian character structure and of how readers, mistaking his acknowledgment of a source of knowledge for a statement of cause and effect, reinterpreted what he actually said to mean that Russian swaddling "caused" Russian character (Gorer, 1962; see also Mead, 1954*b*).

the more stable traditional culture within which they were reared and of the consequences of rapid change within the individual personality.

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DISCUSSION

Fortes: I remember talking to one of my oldest informants in North Ghana when I returned there in 1963. We were discussing, in general terms, schooling for the new generations and he said very solemnly to me, "Well, you see, things have changed, I don't know what sort of world my grandchildren are going to live in. We used to know, we don't know any more." This spontaneous comment underlines what Dr. Mead has said about the awareness of change: people in primitive communities may be quite conscious of these problems of the future and able to formulate and talk about them. I would like to put forward an idea about the life history of any personality, what T. Parsons and E. A. Shils call the personality factor (Parsons, T., and Shils, E.A. [1951]. Towards a General Theory of Action. Cambridge,

Mass.: Harvard University Press). I like to think of this as an input/output model. For every personality there is a continuous input which begins with the parents' influence; when there are cultural changes in a community the output and input sides get out of phase and it is this out-of-phasedness which creates some of the problems. The inputs are not all family-channelled, and there may also be complications because of a lack of homogeneity. When we talk about change in this rather global sense, the time in which a person lives and grows up must be taken into account, as well as all the segments of his total sociocultural background. An individual is influenced by a number of overlapping *milieus* and these will not necessarily be in step. Professor Leighton's comment on the development of the steel axe (this volume, p. 217) was an example of a change taking place in one possible dimension of existence with no corresponding changes in other dimensions.

I suggest that Dr. Mead has concentrated on one particular part of this model, that is the family-channelled part of the input and the differences between successive generations. She noted that these differences may be counterbalanced to some extent by feedback from the children to parents and grandparents. I think we could call this the educative function of children. This is a rather complicated way of describing what we see, for example, in immigrant families. In the developing African tribes there is often a gay acceptance of the idea that the older generations must learn from the children. The children are moving into a complex world which contains literacy, a language the parents do not understand, an economy they cannot fully grasp and new medical ideas. A possible way of counterbalancing the discrepancies between the generations is by the assimilation of this complex input along a generation layer (the children) and a feedback through the children to their older relatives. Where there is severance of the nuclear family, for instance, when the children leave home to work or go to school, this feedback cannot occur and the problems of change in a community may be accentuated.

Lewis: Parin and his colleagues (Parin, P., Morgenthaler, F., and Parin-Matthey, G. [1963]. Die Wiessen deuzen zuviel. Zurich: Atlantis), in their study of the Dogon in Mali, did not report the same awareness of differences between generations that you have described. The Dogon were much more alive to the difference between those

who stayed at home and those who had gone abroad for a time—to Ghana or the Ivory coast or on military service—and then returned, to be given fuller responsibility and prestige, in keeping with the respect paid to their added experience.

Mead: There may be a reactive hardening in the older generation against change if the change is catastrophic or incomplete or disappoints them, but I think that Professor Fortes' old man was rejoicing that the children were learning. My Manus people still feel that they are developing with the children, the whole village takes an interest in the grades the children are making at school, and the older people look forward to the examinations with great excitement. This can happen whenever there is complete forward movement throughout the community.

DeVos: Clyde Kluckhohn suggested that when we talk about the problems of mental health in a changing society we are really talking about two sets of problems, those of social adaptation and those which are concerned with internal adjustment. When we speak of internal maladjustment, we mean that there are defective internal organizational structures in the individual which are prone to breakdown *or* rigidified defence structures which make adaptation to change difficult. Such intrapsychic disturbances or defects are the traditional concerns of psychiatry. We are now, in this modern age, talking about something different, namely, the problems of adaptation in a complex, changing, social environment which produces special forms of stress for the individual. I think that we should try to think about these two variables, adjustment and adaptation, separately and realize that two types of processes may be at work in an individual or a community at the same time. On the one hand, there may be maladjustment to developmental experiences, or even physiological incapacities, which induce a type of structural derangement within the personality that makes it difficult or impossible to deal with the ordinary environment. On the other hand, a change or disturbance in the environment may induce certain types of problems of adaptation for individuals who, under ordinary circumstances, would not show disturbance. There are degrees of adjustive potential in individuals which make them more or less able to deal with social change and the stresses that go with it. The individual who is too rigidly structured cannot, be-

cause of defects in his inner organization, deal with radical disturbances in the external environment. Conversely, different cultures conduce toward certain forms of social adaptation and make other forms more difficult.

Hallowell: An old concept which seems relevant here is the concept of definition of situation. To bridge the inner and the outer changes a mediating concept is needed and the way in which the new situation is perceived and defined depends on this. We could say that in Dr. Mead's Manus community the people have defined the situation in a way which means something very positive to them in relation to their own values and progress. I am suggesting that for a satisfactory adjustment to change what is involved is not just a vague, internal psychological process; there has to be somebody who is actually thinking and communicating to the rest of the tribe what has now become so important to them. This leads directly to a very positive motivation for accepting the external changes.

Mead: Just as long as we include in your definition of the situation the reality of the situation. I think this confirms what Professor Leighton said, that you need an economic change, something really happening, for these changes to be accepted without mental trauma.

There is still the problem of the social apathy that can produce a self-defeating environment for a group. In Professor Leighton's community of poverty people seemed able to overcome their own apathy in a way which is quite unusual. In the Apache American Indian group described by Alan Coult (1965. Doctoral Thesis. University of California, Faculty of Anthropology), the secret of overcoming this apathy has not been learned and the reaction of many people to change is to take on degraded or depressed roles without any clear expectation, at least in the adult Indians, of where they are going. No government agency or outside force has been able to do anything about this. Even if there are few reported cases of classically defined neuroses or psychoses in these communities there are certainly many forms of social pathology, such as alcoholism. Professor Leighton, could you comment on the differences in these two situations?

Leighton: I have only some unscientific impressions to offer here. I visited the Ramah area in New Mexico a short time ago. This was about fifteen years after I had done field work there and I noticed a

striking change in these Navahos. The general post-war surge of the Navaho Indians towards education, the development of industries of their own and co-operation with other industries, the influence of Navahos who had found work in the cities and the resulting traffic back and forth, all seemed to have made a great difference to the people of the Ramah area. They appeared socially and psychologically better off than they were before the war.

Fortes: I think there is some confusion over the word stable. Would Dr. Mead and others be willing to designate these communities of poverty described by Professor Leighton as stagnant rather than stable? It seems to me that they are pools of stagnation, there is nothing stable about them. A suitable psychiatric description of them might be depersonalized communities because they are depersonalized in contrast to the personalized society which rejects them.

The intriguing problem here, as in Dr. Yap's story of the Tai-ping rebellion (this volume, p. 233), is to find out what recuperative effort can personalize this sort of society and give it the sense of community which it lacks. Is a Messianic revival needed, or leadership from a political rebellion, or an injection of wealth, as among the Navahos when vanadium was discovered?

Leighton: Two things which I think are equally disintegrating and psychologically devastating are abject poverty and sudden wealth.

Loudon: Brunei received an injection of wealth from the discovery of oil without disintegrating.

Fortes: But the Brunei people were not abjectly poor before oil was discovered. It has been said that poverty in an affluent society can be defined by the feeling that, although you are inside a society to which you know you belong as a right, there are barriers which make it impossible for you to become established in it; it is this situation that produces stagnation.

Mead: I am quite willing to drop the word stable in this context. Stagnant is a perfectly adequate description, and these societies certainly have to be distinguished from healthy, isolated, primitive societies which, I agree with Professor Fortes, should be called stable. I also agree with Dr. DeVos that there are a range of internal vulnerabilities that are not a function of the structural conditions in the society. Some of these are probably congenital and genetically

determined and some are due to a series of traumatic situations in the past life of the individual. There is no society, as far as I know, where people are equally healthy, happy, and wise, and where there are no distinctions among members of the group.

This reminds me of a study on peptic ulcer in civilians and naval personnel done by J. Ruesch and co-workers some years ago (1948. *Psychol. Monogr.*, **62**, 1). These workers found, quite accidentally during this study, that the ulcers in civilians occurred in men who had highly competitive sisters and these ulcers were deep, intractable, chronic and resistant to treatment. The ulcers in naval personnel, on the other hand, were found in men who had brothers with whom they had competed successfully. If these men had stayed in civilian life they would not have become ill, but in the navy half a million brothers were more than they could stand! These ulcers, however, were quite superficial and easily cured. The first group of men, the civilians, had been severely traumatized by the sibling situation, whereas the second group had been able to meet their life situation satisfactorily until this particular competitive stress was too much for them.

Lambo: I was interested to hear Dr. Mead say that she thought old people have to make a greater adjustment to change than the young. In the last decade or so we have seen a unique picture of the effect of acute change on the mental health of some African leaders. An example of this is what I call the "sliding change" which takes place when an African rural school-teacher, with little or no educational background or intellectual resources, finds himself a political leader overnight; in addition to this he may find himself in any capital city of the world as ambassador for his country. It is usual for him to enjoy these facilities and the prestige for a limited period of time but he may lose his position at any time and have to return to his humble profession as a school-teacher. It is at this last stage that his mental health may be threatened. In fact we have seen a number of people in this catastrophic situation.

Wolfenstein: I would like to describe some research that I have been doing on the vulnerability of the nuclear family after the death of a parent (unpublished data). The nuclear family is flexible and adaptable to changes in society in general, but it is utterly unprepared for changes in its own personnel. It is assumed that the young parents are the only

people to bring up their children and that they will last as long as they are needed but, of course, this is not always so. The best adaptations that children make to the death of a parent is where there is some approximation to the lost parent in the extended family, for instance, grandparents, or uncles and aunts to whom the children are already attached and can transfer some of their feelings.

Murphy: Dr. Mead has described the function of roles presented in childhood, but there is another influence on the child's choice of a role for adult life, and this is the reward system that he is taught. Children learn from their parents possible roles for the future, but they also learn to seek from life a particular set of rewards which are related to the values of the culture. I think the reason that some societies are able successfully to assist a new generation growing up and taking on new roles and others are so much disturbed by change, may be related to how much the rewards offered by modern life are of the same general character as those which the society had taught its children to value. If they are similar, acquisition of new roles is seen as gratifying; if they are dissimilar, the new roles are seen as frustrating, since they provide rewards which are not recognized as such instead of the rewards which were previously valued.

Shepherd: Dr. Mead has discussed the differences between psychiatric and anthropological models in the study of the roles adopted by individuals within a single culture. I wonder whether it is possible to link together these two models by the concept of anthropological psychiatry, so-called, which is popular in Holland and some of the German-speaking countries. It has been used recently, for example, by Professor von Baeyer and his colleagues in their study of some of the victims of the Nazi regime (von Baeyer, W., Hafner, H., and Kisker, K. P. [1964]. Psychiatrie der Verfolgten. Hamburg: Springer-Verlag). Von Baeyer and his colleagues analysed the mental health of people who had suffered in concentration camps and who applied for compensation from the German authorities some years after the war. They claim that it is possible to categorize the psychopathology of these people not only according to their psychiatric symptoms but also in an anthropological way which, in this context, is closely related to existentialist theory. Dr. Mead, have the factors, such as choice of role, that you have been talking about ever been tested in this way in

direct relationship to mental health? Has the psychopathology of people who have been placed in stressful situations ever been analysed and the subjects' subsequent mental health related to these early influences?

Mead: There have been several studies of concentration camp victims of different periods such as the one by D. P. Boder (1949. I did not Interview the Dead. Urbana, Ill.: University of Illinois Press). G. W. Allport and co-workers (1941. *Character Person.*, **10**, 1) investigated the extent to which the personalities of early refugees remained intact through extremely traumatic situations.

Comparative studies have also been done with groups of delinquent and non-delinquent young people. Some adolescents with horrendous situations in their past react by becoming delinquents, but some, with extremely similar past histories, seem to withstand these traumas and do not break down into delinquency. From these and other studies on the response to a variety of types of stress, it is becoming possible to select individuals who can respond constructively to stress and those who cannot (Roe, A. [1961]. The Making of a Scientist. New York: Dodd, Mead; Roe, A. [1964]. The Origin of Interests. Washington: American Personnel and Guidance Association).

I think there is a close connexion between this sort of anthropological study and psychiatry, because one of the things that preventive psychiatry must look at is the structural situation of individuals in a changing society. Psychodynamic psychiatry attempts to unravel the consequences of the vicissitudes in the development of the emerging personality in its whole relationship to the parents, dealing with the oedipal situation and so on, although the resolution of these conflicts is never complete in any culture. There is an interesting book by W. R. F. Collis which attempts to approach the subject in this way (1953. The Lost and the Found. New York: Woman's Press). Collis, a paediatrician who led the first team into Belsen, adopted two very badly traumatized children from that camp. They ultimately recovered their mental health, and Collis studied their early childhood to try to find out the situations that had given them sufficient strength to survive their terrible experiences. Are these the sort of studies that you mean?

Shepherd: Yes, except that it is not possible to generalize from two

subjects. In von Baeyer's study (1964, *loc. cit.*) several hundred people were involved so that the results could be analysed theoretically in terms of various factors, including early choice of roles, to try and discover which elements were related to outcome. The authors of this study did attempt to do this but they used a very different system of classification from ours, so it is difficult to interpret their results.

As a psychiatrist I would like to have clear in my mind exactly what concepts we can share with the anthropologists. We are, of course, familiar with the psychodynamic approach that Dr. Mead has mentioned, and Professor Firth has a particular view of social organization which offers a different approach. Dr. Mead, is there anything else that both anthropologists and psychiatrists can share as a model?

Mead: Von Baeyer's study sounds to me much more like a sociological and situational, and possibly an anatomical and physiological, study than a psychiatric or anthropological one.

A meeting point between anthropologists and psychiatrists is the study of constitutional types. In small isolated communities where there is a great deal of inbreeding we see exaggerations of constitutional types which may be linked with culture. In these situations it is possible to study the relationship between constitution, culture, and mental illness or stability. Scholastic ability and intelligence may be another factor that anthropologists and psychiatrists can share. I now have records of my Manus people which start thirty-six years ago and from these I can trace the origins and ancestry of the children who are now doing best at school (Mead. M. [1956]. New Lives for Old. New York: New American Library). The history of this tribe is that in the aboriginal situation there were families of high rank and prestige, an intermediate group and a very low level group. In the transitional period, 1946–9, during a nativistic cult phase, a lunatic fringe took over some of the leadership (Schwartz, T. [1962]. *Anthrop. Pap. Am. Mus. nat. Hist.*, **49**, 211). The children who are doing best at school now come in about equal numbers from the old high prestige, high status families or from the lunatic fringe, cult group but not from the intermediate group. In twenty years time, when these children will be asked to carry the burden of leadership in the emerging nations of

Papua and New Guinea, we may find a different outcome in the two groups because of their different lineage.

The people that I call the lunatic fringe among the Manus tribes were the ones who came forward as prophets. They had the most seizures and the most dogmatic, fanatical, systematized delusional ideas at the time of the original nativistic outbreak. They seem to have a kind of drive for their children which parallels, as far as mobility is concerned, the drive of the high status families; but the underlying genetic constitution in the two groups of children may be very different.

DeVos: Another aspect of this question is what might be described as the demeaning effect of minority social status and position on adaptation and adjustment to society. I would like to quote an example to illustrate what I mean. A study was made of the I.Q.'s of school children in an area on the north coast of Japan where the people believe in fox possession (Nobukiyo Nomura. [1956]. *In* Shukyoto Shinko no Shinrigaka [Psychological Studies of Religion and Beliefs], p. 247, ed. Oguchi, I. Tokyo: Kawade). There were here some children whose families were said to have made a pact with a fox (as was supposedly done in New England where one made a pact with the devil for financial advancement), and other children whose families and houses were thought to be pure. Incidentally, in a rather symbolic way, the pure houses were called white and houses with reported fox possession were called black. These two groups of children were compared with a third group, who came from outcaste villages. All these children went to the same school. It was found that the children from the "white" houses had higher I.Q.'s and did significantly better in school than either the children from "black" houses or the children of former outcasts who lived in the same area. It seems that there is a socialization factor here that can be measured by intelligence or achievement tests and which is related to identity and social self-identity. Social attitudes must be incorporated in the individual very early in life to be manifest influences on measures of intelligence or personality.

Margetts: Perhaps I might, very briefly, clarify Dr. Shepherd's comments about anthropological psychiatry. Anthropology is derived from the Greek *anthropos*, meaning man, but this term was rarely

employed in classical times. In the sixteenth century it was revived and was used, for example by Magnus Hundt in his *Antropologium de Hominis Dignitate*, but in a restricted way to mean man's anatomy and physiology. The word psychiatry was used much later, by Johann Christian Reil in 1808. (See also Editorial. [1951]. *Amer. J. Psychiat.*, **107**, 868.)

Transcultural Psychiatry
A.V.S. Deuck & Ruth Porter

METHODS OF PSYCHIATRIC RESEARCH IN AFRICA

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SOME GENERAL DEFINITIONS

PSYCHIATRY (Gr. *psyche* mind, soul; *iatria* healing) is the branch of medicine which specializes in the investigation and treatment of illness of the mind. The mind, a function of the brain, determines ideation (thinking), feeling (emotion and mood or affect), and action or behaviour as a result of the pressures of the instinctual drives and of biochemical–biophysical reactions. The brain may be moved to specific function from stimuli originating from other sites within, or from outside the body, that is from the world at large. The human body is unbelievably complicated; it does not act in the textbook manner of stimulation leading to response, but it always functions as a resonating *continuum* or interplay with the environment, in a global biological, or organismic, manner. Since man's primary contact with his environment is with other people, this total manner of action is also called biosocial, or psychosocial, less complete terms to define the same function. Man's functioning has been described as the organism-as-a-whole, since man relates to things other than society—to temperature, weather, landscape and food, and to mechanical materialistic things. In ideal functioning, the body acts in homoeostasis or equilibrium within itself and with the world outside it. This conception of man allows an eclectic and practical attitude, and is satisfying to most students of human phenomena. When homoeostasis of body function becomes disrupted the human organism strives to readjust itself biologically,

or socially, or materially in some way. If it cannot do so, it is somehow incapacitated, and if this leads to impairment of function and to signs and symptoms, as clinical abnormalities are called in medicine, the person is said to be "sick".

Normality may be identified with homoeostasis, in which the human organism functions at a level of adjustment with itself, with society, and with the world at large, without sickness. Abnormality results when the organism, by some stress, is thrown temporarily or permanently out of adjustment. The mind is but one functioning part of the total organism. When homoeostasis is disrupted, the mind always reflects this in some way, by manifest disorder or abnormality, in one or all of the triad of thinking, feeling and behaviour.

Psychology must be differentiated from psychiatry, though because of the shading of normality into abnormality there is also a shading of psychology into psychiatry. Psychology is the study of normal, and psychiatry of abnormal, mind function and the treatment of it. Personality (Lat. *persona* mask) is the psychological make-up of a person, his total thinking, feeling and behaviour pattern, his usual self, or "what he is like". We think of the study of the "normal" personality as an area for the psychologist, the "abnormal" personality for the psychiatrist, and there is, again, a shading between the two.

RESEARCH AND METHODOLOGY

In general terms, research (Lat. *re* again, back, over again; *circare* to look through, to seek, to examine, to go around—*circus* circle, ring) may be defined as (A) primary: the seeking and discovery of facts previously unknown to anyone and/or the assessment, interpretation, and utilization of these facts in some meaningful way, and (B) secondary: the re-evaluation and recombination of human facts to make order in existing knowledge. Research is different from learning, which is the seeking and appreciation of already known facts. Method (Gr. *meta* after;

hodos a way), or methodology, or technique of research consists of the planning of how research is to be done, of the procedures to be employed and indeed of the procedures actually utilized. Chance procedures and unplanned procedures are just as important to consider as planned rational action or method in research. Many discoveries have been accidental and not the result of conscious planned reasoning—one could name examples of discoveries by subconscious association, by dream, by revelation, by blind trial, by casual observation, and by error of technique and judgment.

PSYCHIATRIC RESEARCH: GENERAL CONSIDERATIONS

Medical research: In medicine, research sets out to find out about the causes, courses, effects, and cures of specific illness syndromes. General medical research has become highly developed and technical over the centuries. The French physiologist Claude Bernard (1813–78) developed the philosophy and requirements of medical research, which were established in his classical monograph *Introduction à l'étude de la médecine experimentale* (Bernard, 1865). The methodology of medical research is now accurately defined (Fisher, 1951; Hamilton, 1961; Mainland, 1954).

Psychiatric research: Psychiatry has not kept pace with the rest of medical science; it is still much less a science than an art, and stresses appreciation of illness observed, sympathy and compassion, based on interpersonal attachments, and on empiric treatment. It has a sort of pseudo-understanding of emotional and mental illness, not a true knowledge based on scientific facts. In psychiatry, more and more is written about less and less, and called "research", than in most other medical specialities. The reasons seem clear—psychiatry deals with distorted ideas and feelings, relies much on the patient's subjective statements to arrive at a diagnostic formulation and utilizes interpersonal feelings and conversation in treatment procedures. Psychiatry emphasizes *language* and other *communication.* The scientific psychiatry of the future will undoubtedly

depend much less on words and will be effective by technological methods and not empathy and interpersonal communication.

Because psychiatry is as yet such an undeveloped body of knowledge, and still subjective rather than objective in emphasis, research in psychiatry is more diffuse and nebulous, and less standardized, than in other branches of medicine and pure science. The multidisciplinary compilation of Kruse (1957) is an attempt at integration of the various approaches to psychiatry, and comprehensively illustrates the diffuse spread of psychiatry through many other social and pure sciences. A shift to the more materialistic is in process at the present time, advances are being made in neurochemistry and neurophysiology, in the application of epidemiological and social statistical methods, and in physical and drug treatments, all of which are more easily subjected to experimental control than subjective reports and psychotherapeutic methods. From these advances, systems of investigation and experimentation in psychiatry are now being developed (Brosin, 1961; Cameron, 1941; Gregory, 1961; Maxwell, 1958; Richter *et al.*, 1962; Sainsbury and Kreitman, 1963; Tanner, 1953). It is to be expected that it will not be too long a time before the socio-psycho-physiological gaps are bridged, and we will gain factual understanding of the chemistry and physiology of thinking (memory, association, concept formation, symbolism), of feeling (emotion, affect), and of instinct as they relate to learning, conditioning, behaviour and interpersonal relationships.

A sequence of research: The most orderly sequence of research, including psychiatric research, consists of three steps; firstly observation or fact-finding to produce data, secondly explanation (theory) about organization of the data, and thirdly proof of the explanation. This sequence may be applied in three areas which overlap to a considerable degree; firstly clinical, secondly social, and thirdly experimental. Moreover, this sequence and these areas of research may be considered from both the qualitative and the quantitative points of view. For example, if it is established

as a qualitative fact in the clinical area that schizophrenia occurs in a given population, it is possible by further research to determine how much schizophrenia exists in that population.

PSYCHIATRIC RESEARCH IN AFRICA

The sequence: The sequence of research and the areas of research described for psychiatry in general can be applied unmodified to psychiatric research in Africa. For example, in the clinical area, signs and symptoms— the clinical facts— observed in one Kikuyu patient led to the explanation and theory that the syndrome could be Huntington's chorea, and this was tentatively proved to be the case when the illness was discovered in some blood relatives. The proof would be more closely approached if the characteristic clinical and hereditary picture could be supplemented by post-mortem gross and microscopic examination of the brains of the afflicted ones. In the social area, these facts, qualitative to date, could be quantified by making a more detailed statistical survey of the family and preferably of the whole Kikuyu tribe.

The need: The need for psychiatric research in Africa has been stressed in the literature, particularly in the Bukavu and Brazzaville meetings of 1958 (C.C.T.A./C.S.A., W.F.M.H./W.H.O., 1960; W.H.O./C.C.T.A., 1959) and the Abeokuta conference of 1961 (Lambo, 1962; Margetts, 1962).

Methodology: The methodology for psychiatric research in Africa, while the same in principle as in the rest of the world, is in some ways more difficult than in most places because of several factors. One is the unusual and multiple culture sets with which one has to deal, and another is lack of interest and administrative knowledge and judgment to make money available for studying them.

Guides for anthropology and sociology: Anthropology and sociology, with which psychology and psychiatry should be more closely integrated, have in the past developed reasonable

guides and methods for their research activities in primitive and unevolved societies, and have guides, manuals and handbooks available for researchers (Balandier, 1960; Bartlett *et al.*, 1939; Boas, 1938; De Pedrals, 1949; Editorial, 1949; Festinger and Katz, 1953; Greenwood, 1945; Griaule, 1957; Hall, 1938; International African Institute, 1950; Kluckhohn, 1944; Kroeber, 1953; Le Vine, 1961; Mensh and Henry, 1953; Murdock, 1957; Murdock *et al.*, 1950; Nicaise, 1960; Radcliffe-Brown, 1958; Radin, 1933; Royal Anthropological Institute, 1951; Schapera, 1935; Selltiz *et al.*, 1959).

Guides for medicine and psychiatry: The data and procedures of tropical medicine and surgery in Africa have become well standardized (Bowesman, 1960; Gelfand, 1957, 1961; Trowell, 1956, 1960; Trowell and Jelliffe, 1958), and in fact Trowell preferred to think of "medicine in the tropics" rather than "tropical medicine", making the point that already the tropics are little different from anywhere else so far as medical practice is concerned. One could disagree with this general view, and certainly psychiatry is very much complicated in tropical Africa by the peculiarities of cultural differences.

Psychiatry has not yet a satisfactory text or guide for psychiatrists working in Africa, though a start has been made (Margetts, 1958) which is in process of being extended and improved.

Interdisciplinary co-operation: Modern psychiatry has stressed a social frame of reference, that is that social and cultural forces may cause or at least precipitate mental and emotional dysequilibrium and illness in the individual. It is therefore reasonable to seek closer co-operation and collaboration with sociologists, anthropologists, and other society-oriented specialists in African psychiatric research. There is a huge literature from isolated specialities but this is not integrated into a concise concept of "man in his environment" (Margetts, 1962; Miroglio, 1958). Interdisciplinary integration and co-operation could be in the form of a "team" approach to the study of the psychology and psychiatry of the

African peoples. The Cornell-Aro survey recently published utilized a team approach (Leighton *et al.*, 1963, reviewed by Gutkind, 1964; Margetts, 1964*a*; Prince, 1964*a*). The need for general physicians, and particularly psychiatrists, to be versed in the principles of anthropology and sociology has been stressed at the three meetings of psychiatry and mental health in Africa held so far, at Bukavu, Brazzaville and Abeokuta (C.C.T.A./C.S.A., W.F.M.H./W.H.O., 1960; W.H.O./C.C.T.A., 1959; Lambo, 1962).

Planning for psychiatric research: Research activity should be on-going and permanent. Most research in African psychiatry so far has been casual and short-lived, done mostly by clinicians very much overworked with their service jobs in understaffed and inadequate mental hospitals. The situation has not been helped by the considerable exodus from Africa of European specialists in recent years, for political, economic and other reasons. Improved allocation of national and foundation monies, enticement of foreign experts back to Africa, and the setting up of *long-term* research programmes are essential if much more is to be learned about psychiatry in Africa than is already known. The future lies in more available money and a more enlightened concern about how best to use it.

Psychiatric research in Africa requires quite an elaborate physical set-up. Whether urban or rural investigations are being carried out, centres to work in or from are essential both for examining cases and for processing the data. In the past, mental hospitals have served as such centres. In a few places, general hospitals are now set up to do the same. This is possible because of the decentralization away from mental hospitals into general hospitals, which has been recommended for Kenya since 1956 (Margetts, 1960*a*; Margetts, 1962). Psychiatric wards in general hospitals are, in 1965, a reality in Kenya, at Nakuru, Nyeri and Machakos, and working very well. These, of course, are service units for treatment, but would make excellent bases for research

activity. The universities in Africa are away behind, so far as undergraduate psychiatry teaching is concerned, let alone research. Notable exceptions have been at Dakar and Algiers. Dakar is still an active centre for psychiatric research (Collomb *et al.*, in Margetts, 1962; bibliography p. 303). It is anticipated that the department of psychiatry at Ibadan, started in 1963, will develop psychiatric research in a progressive manner. Makerere has not yet developed a department of psychiatry, and what little psychiatric research there is in East Africa is still centred in the mental hospitals. University departments of psychiatry at Cape Town and Johannesburg are encouraging research, and reports from the mental hospitals in South Africa show that the standards of research are high though funds are limited, as elsewhere in Africa. In basic science research, the Belgians were doing fine work in neuropathology, and in sociology, clinical psychology, and psychiatry (Margetts, 1962), but since the Congo trouble most of the European experts have left the country. In basic and applied psychology research, the National Institute for Personnel Research in Johannesburg has completed many fundamental projects. The South African Institute of Medical Research at Johannesburg is perhaps the most important unit in Africa for basic medical science research, including neuropathology. The sociologists of the East African Institute of Social Research in Kampala are collaborating with psychiatrists and other physicians to produce reviews of topics relating to psychiatry and sociology.

Bush and field psychiatry: "Bush" psychiatry is a relatively undeveloped research area in Africa. It is a fascinating subject because it is exotic, strange, and exciting (Ackerknecht, 1943, 1945; Aubin, 1952; Drobec, 1956). It is psychology and psychiatry combined with rural anthropology. It includes observation and understanding of abnormal and unusual bizarre behaviour in the context of the culture concerned, case finding in rural areas, and treatment of illness away from the mental hospital setting. Very few psychiatrists have specialized in this so far (Field, 1937, 1958,

1960; Margetts, 1960*a*, 1964*c*; Prince, 1960, 1962, 1964*b*). There is much work yet to be done in this subject, so far as observation, documentation and explanation are concerned. Psychiatry out in the country, away from centres, involves major topics for research—the study of the general psychology, psychopathology and sociopathy of cultures (normality and abnormality), the observation of mentally deviated people other than "patients", the observation and treatment of patients, the study of medicine men and religious healers and their management of the sick, and surveys of the statistics of psychiatric illness and other pathology in discrete populations. Basic science methods have to be drawn into bush psychiatry to explain many of the observed phenomena such as sickness and death by magic (Barber, 1961; Cannon, 1942, 1957), basic psychology and philosophy (Aubin, 1952), trance, abreaction, medicines and poisons (Margetts, 1958, 1960*a*, 1962, 1964*b*, *c*; Prince, 1960, 1964*b*; Watt and Breyer-Brandwijk, 1962).

CLINICAL RESEARCH IN AFRICA

Hospital studies: Clinical research investigates the description, aetiology, and treatment of mental, emotional, affective and behavioural illness. Doctors in Africa for the last fifty and more years have been doing this, though many of them have not written anything about their work. Most clinical psychiatry has been done in urban hospitals and clinics, and two universities, Dakar and Algiers. A select bibliography of some of the principal publications in this field has been attempted elsewhere (Margetts, 1962; bibliography p. 303). These might be called "urban" studies in the sense that they deal with patients in hospitals, in a town or near one, but they include patients from all round the country, so that they are not completely urban.

Field studies: Rural or field studies in African psychiatry have not been stressed, except by very few investigators (Field, 1937, 1960; Margetts, 1958, 1960*a*, 1964*b*; Prince, 1960, 1964*b*).

Contributions by non-psychiatrists: Much of the research of interest and importance to psychiatry has been done by psychologists (see also Margetts, 1962; bibliography p. 303). Psycho-analytic studies have been very few to date and are perhaps too sophisticated for the African subject (Ly, 1948; Morgenthaler and Parin, 1964; Parin, 1964; Parin and Morgenthaler, 1956/7; Parin, Morgenthaler and Parin-Matthey, 1963).

Problems: The description of the patient and taking the past history is much the same in Africa as anywhere else, the particular shortcomings of the doctor being an ignorance of the physical and cultural anthropology of the patient's background and a difficult language barrier (Margetts, 1958, 1962; Lewis, 1962). Any psychiatrist dealing with a country-wide population has to deal with the language problem somehow. In Africa, the only practical solution at the present time is to accept, and to use correctly, interpreters. One psychiatrist dealing with seven million people speaking forty languages, as in Kenya, cannot hope to do anything else. He would waste his time learning tribal languages, or at least any more than one in which he is particularly interested or with which he deals mostly. Photography and tape recording are invaluable adjuncts to the gathering of anthropological and clinical facts. Griaule, as long ago as 1938, even published a monograph with a gramophone record in an envelope in the back of the book. An abbreviated guide for the examination of native African patients, commenting on identification of the patient, history taking, objective appearance, symptoms, subjective mental content and so on has been published already (Margetts, 1958) and is in process of being revised and enlarged. The diagnostic categories into which African patients can be placed would appear to be the same as those in common use in standard European and American practice, for example the American Psychiatric Association (1952) nomenclature and the World Health Organization (1957) international list. The simpler a classification is, the better. The W.H.O. list seems to be the most

appropriate one; it is simple, accurate, and has the advantage of an international orientation which allows ease in comparison of one country's statistics with another's. A skilled clinician who understands the culture values involved seems to have no more difficulty in making a diagnosis in African patients than in patients of other races and cultures. The primary illnesses are the same and any differences are of pathoplastic significance and not pathogenic. Cultural and social factors determine the forms or patterns of illness (Margetts, 1958, 1960*a*, 1962). There is one aspect of the psychiatric examination of African patients which has not received any attention so far in the literature, the factor of *observer error*. It is common knowledge that psychiatrists anywhere will frequently disagree about diagnoses. While this is sometimes quite understandable, and acceptable, there seems to be more than necessary disagreement in these days. The reasons, perhaps, is the result of inadequate training of the modern psychiatrist in observation, or objective evaluation, of the patient. This may be a result of faulty education, which, in America certainly, is inclined nowadays to encourage excessive preoccupation with the subjective symptoms of the patient and attempts at psychodynamic formulations. Improved training of psychiatrists in observation is important and would minimize mistakes in diagnosis. This is the main source of observer error and it is more important in African patients because of the confusing overlay of secondary, culture-determined mental content that one sees in Africa and because of language limitations. Another source of observer error is in the personality of the observer himself. Some doctors are better at diagnosis than others, they have an "intuitive skill", which is part of them and cannot be learned by everyone. They have an empathy for the patient and can get more information out of him, and an acuteness of perception which allows them to pick up obscure signs and subliminal cues. A psychiatrist with such a skill will often be able to re-diagnose correctly a case labelled differently by another psychiatrist.

Causation: Causation, in mental as in any other illness, is the aetiology and pathogenesis of that illness. In psychiatry, pathogenesis in psychodynamic language is usually described in terms of mental mechanisms, psychic traumas and interpersonal relationships. In general medicine, a great deal is known about causation but in psychiatry very little, and this applies particularly to aetiology. The most disconcerting thing a patient or a relative can ask a psychiatrist is what "caused" a mental illness.

Aetiology is sometimes considered as primary or secondary. Primary (pathogenic) aetiology is the basic, specific agent which produces an illness. An example in psychiatry would be the *Treponema pallidum*, which produces syphilis of the brain parenchyma, or general paresis of the insane. The illness cannot result without the presence of the primary cause. Secondary (pathoplastic) aetiology is the non-specific stress which adds to an illness. An example would be a culture attitude, value or stress which colours the basic clinical picture of a patient's illness. It should not really be called aetiology at all, because the illness can result without it. The consideration of secondary or pathoplastic factors (a better term for the purpose than "aetiology") has led to the idea of "multiple causation", that is many aetiologies contributing towards the production of a disease. This may be referred to as "colligation" (Lat. *colligare*, to bind together), a binding or connecting together, or summary, of a number of isolated factors and stresses all acting together to produce a disease syndrome (Kretschmer, 1934). The concept of colligation is a reasonable construct provided one differentiates the isolated factors and stresses into primary aetiologies, and secondary ones which, added to the primary ones, produce the illness by the process of pathogenesis—or in psychiatry, "psychopathogenesis".

The concept of multiple causation can be a hindrance to psychiatric research, though favoured very much by the proponents of social medicine and sociology (sociogenic causation). The ultimate unit to be considered in the causation of psychiatric

illness is the individual human being, particularly his brain, and the final causes (aetiologies) of mental sickness, at some time in the future, will probably be found there (biogenic causation).

Causation of mental and emotional illness in Africans will undoubtedly prove to be the same as in anyone else, because such illnesses are apparently universal throughout the world and do not seem to depend primarily on cultural and racial factors. Research on pathogenesis in Africans must clearly differentiate the pathoplastic factors of culture values, attitudes and stresses from pathogenic aetiologies peculiar to Africa and well known in tropical medicine, hygiene and public health (bacterial, viral, protozoan, parasitic diseases, genetic, nutritional, metabolic deficiencies and so on).

Diagnostic aids: In clinical research, a number of aids are essential. These adjuncts to diagnosis include clinical laboratory tests, radiography, electroencephalography, somatotyping, psychology testing of intelligence, aptitude and personality, photography, and sound recording. The first-named laboratory tests are the only aids which are more or less available in all major centres in Africa, the reason being that they are essential for all types of medical work in Africa, including psychiatry. The other techniques are not available everywhere, because of lack of allocated money and of trained personnel to offer them.

Differences: Treatment of Africans with mental illness, will allow for research to see if methods used elsewhere work as well in Africans as in other peoples of the world. There appear to be differences in responses to psychotherapy, to drugs and to physical treatments, but no substantial research has yet been carried out to investigate the details of, and to measure, such differences.

SOCIAL RESEARCH IN AFRICA

Epidemiology/endemiology and ecology: Epidemiology is the branch of public health which studies, firstly, *morbidity*, or the amount (prevalence) of illness existing in a given population, and

the occurrence (incidence) of new cases appearing in a specific period of time and, secondly, the factors influencing morbidity. The term epidemic (Gr. *epi* upon; *demos* people) is generally taken to imply an outbreak of *unusual* illness in a population, for example an epidemic of typhoid, and epidemiology would refer to the study of such an illness.

Endemic means an illness prevalent *at all times* within a population, for example malaria in certains parts of Africa. Psychiatric illnesses could be called endemic since they are chronically with the population and do not ordinarily occur in outbreaks. Endemiology would be a more accurate term than epidemiology to cover the study of the distribution of such illnesses (Margetts, 1962). Ecology is a word used in zoology and now in public health, anthropology (Bates, 1953) and social psychiatry. It is a poor term from the etymological point of view (Gr. *oikos* house; *logos* discourse) and has come to mean simply the study of organisms in relationship to the environment. This definition covers just about everything. Several more terms of interest have been derived from *oikos*, including economy, which, according to the dictionary, means system (money system) and in biology it means the body physiology as an aggregate of functioning organs. From *oikos* is also derived ecomania, an obsolete psychiatric term to define a current endemiological entity, that is mental derangement characterized by perversity or uncontrolled ill temper in one's domestic relations!

In epidemiological research, since morbidity statistics are an essential part of it, it is essential to be precise and standard about definitions and other criteria so that reliable and valid comparisons can be made between one group and another (Doll, 1959; Lin and Standley, 1962; W.H.O., 1960). Epidemiological studies of pure races and cultures may in some ways in fact be easier in Africa than in other places where more extensive racial and culture mixing is present. In Africa, the races and cultures are more likely to be discrete and isolated and therefore easily available to examine and

control. Language barrier, race prejudice and inadequate knowledge of the culture would invalidate statistical research unless very carefully corrected.

Definition of a case: Epidemiology deals with the distribution of cases and the factors affecting this distribution. What is a case? May a case in one culture not be one in another? A case implies abnormality and, except in biogenically caused illness, abnormality is determined very much by interpersonal and culture-determined factors. Any survey must define case as used in the specific survey. What is called a case in one study may not be so considered in another (Lin and Standley, 1962). A committee of the World Health Organization has defined a case as "a manifest disturbance of mental functioning, specific enough in clinical character to be consistently recognizable as conforming to a clearly defined standard pattern and severe enough to cause loss of working or social capacity, or both, of a degree which can be specified in terms of absence from work or of the taking of legal or other social action" (W.H.O., 1960).

Case finding: After a case is defined, the next step in epidemiological research is to *find* cases in a given population. The most accurate way to do this would be to assess every member of the population, and even this would not produce a one hundred per cent accurate survey. Careful examination and questioning about individual and family histories of samples of the population give a much less complete idea of types of illness and of morbidity statistics. In African countries, epidemiological research is complicated by inaccuracies of census, lack of birth and death registration, by migration and displacement, and inadequate knowledge of culture values and attitudes, which lead to unusual observer error and misleading of investigators by informants. The questionnaire method, so popular in social research, is not a useful tool for gaining information about psychological attitudes and mental illness in illiterate populations. A thorough knowledge of the culture is essential and this can only be gained as a "participant

observer", living in the culture and learning about it by "ego-involvement", understanding it on terms of both inside and outside culture values. Cultures showing rapid changes (acculturation, culture contact, deculturation, adculturation, culture mixing) may possibly contribute to the manifestations if not the actual prevalence of mental illness. A. Lewis (1962) pointed out that "stagnant societies breed troubles too". Acculturation has been studied quite extensively by psychologists and sociologists (Beals, 1953; Margetts, 1962; Richelle, 1960; Schapera, 1935; Spindler and Goldschmidt, 1952; Wilson, 1934), but very little as yet by social psychiatrists (Daumezon, Champion and Champion-Basset, 1957; Leighton, 1963; Mertens de Wilmars and Niveau, 1961).

Mental and general hospital statistics are known to be unreliable indicators of morbidity. Not all "cases" reach hospital. Nevertheless, hospital figures do mean something when properly analysed according to the social *milieu* of the population.

Homicide and suicide rates are certainly meaningful as indicators of possible mental sickness, but again have to be assessed in relationship to the culture of the people. *Attempted* homicide and suicide are just as important morbidity indicators as successful homicide and suicide rates, and figures for them are not at all reliable even in civilized countries, let alone in Africa. Attempted homicide is lost in police files as assault or even more trivial charges, and homicide may be quite frequent in some parts of Africa but not recorded. Attempted suicide and suicide figures are similarly inaccurate, if available at all. Both homicide and suicide, real and attempted, may in some cases be quite justifiable and "normal" in certain African cultures (Bohannan, 1960; Margetts, 1958, 1960). Local concepts of "normality" and "abnormality" must be taken into account in epidemiological and in clinical research, because what is "normal" in one culture may not be in another and vice versa, and what is "abnormal" to the researcher may not be in the culture concerned, and would therefore not be defined as a case. Definitions of everyday words, such

as honesty, love, punishment, and so on, vary greatly from culture to culture and must be specifically defined for the culture being investigated.

The ratio of male to female hospital cases in Europe and America is about one or one plus male to one female, whereas in central Africa it is two male to one female in native patients. The reasons for this are not clear and further research is necessary here.

Planning: It seems essential to set up *continuing* epidemiological research in Africa. Proper planning and statistical methods should be introduced in order to plan for control and treatment needs (American Public Health Association, 1962; Daumezon, Champion and Champion-Basset, 1957; Gear, Biraud, and Swaroop, 1961; Gordon, 1952; Gregory, 1961; Josie, 1963; Lin and Standley, 1962; Manuwa, 1962, 1963; Pemberton and Willard, 1958; Reid, 1960).

Cultural or social anthropology: There is a long list of topics generally studied by anthropologists which are of much interest to the psychologist who likes to find out about what normal people think and feel and how they behave. Anthropologists and psychologists have, for a long time, integrated their methods in the study of primitive culture (Bartlett, 1937, 1939, 1946; Henry and Spiro, 1953; Henry *et al.*, 1955; Kluckhohn, 1944; Margetts, 1962; Nadel, 1937, 1939, 1951; Seligman, 1932). Psychiatrists also are concerned with these things because when values and stresses in societies change, that is, when social homoeostasis is upset, the conception of a "case" and morbidity also changes. In the present state of our knowledge it is almost impossible to separate psychology, and psychiatry, from cultural and social anthropology and there is much to be said for integrating them in some way, or at least for pooling knowledge and ideas in a better way perhaps by a multidisciplinary or team approach in research.

The long list of topics which seem to be common ground for research may for the sake of completion be reproduced as follows, without further discussion:

Culture, pure culture, culture contact, culture change (acculturation, adculturation, deculturation, culture mixing),
Rapidity and extent of culture change,
Learning new values,
Technology,
Demography/population control,
Migration and displacement,
Urbanization, urban/rural standards and shifts,
Industrialization, labour,
Economy,
Language,
Art,
Race, colour, miscegenation, racism and antiracism (both are examples of prejudice, and probably "abnormal"), non-African immigrant groups,
Social, or psychosocial, "stress",
Social behaviour: family, child/parent relationships, clan, tribe, leadership, consciously (controlled) and subconsciously determined group action (mobs, cults, sects, secret societies, spirit possession, splinter religions, dancing, politics), sex, prostitution, alcohol/drugs, criminality, vagrancy, delinquency/crime, all extreme social psychopathy.

Field ethnic and cultural psychiatry: Apart from epidemiology, though less apart from anthropology, there are a number of subjects which are of research interest to the psychiatrist who works out in the field, away from the hospital and the clinical values of civilized medicine. In time, as acculturation in Africa proceeds, this field, or "bush" psychiatry will become less exotic and be a part of clinical and social psychiatry as we understand these specialties in modern medicine.

Field psychiatry may be considered in two distinct frames of reference. The first of these is the service one of carrying modern treatment methods out into the bush, perhaps by means of a

travelling team or clinic. This was suggested for Africa at the Brazzaville conference (W.H.O./C.C.T.A., 1959), but the suggestion was never put into practice anywhere in Africa, because of lack of money and personnel, and perhaps of incentive. This would be worth while to do, as a research project, to see if it would be of any use. The second frame of reference of field psychiatry is one which is at present being applied in several areas of Africa. This is the research study of traditional magic and medicine, areas which previously have been mostly the concern of anthropologists and missionaries. It is not true to say these areas have not been the concern of doctors—they have, but doctors have not written much about them. The customs in native African tribes which are of interest to students of normal and abnormal psychology may be listed without elaboration. Research methodology at the present time consists primarily of observation and documentation (Field, 1937, 1958, 1960; Margetts, 1958, 1959, 1960*a*, *b*, *c*, 1962, 1964*b*, *c*; Prince, 1960, 1962, 1964*b*). Explanation of the reasons for these customs, some of them very unusual, has not been satisfactory to date and should in the future be subjected to the methods and controls of experimental basic science when applicable.

Traditional magic and medicine:

Magic, religion, witchcraft (magical experts, deception, procedures, rituals, magical images, sacrifice, oaths, ordeals, dreams, spirit possession, amulets, shrines, cannibalism, medicine murder),

Materia medica (specific and empiric drugs, poisons),

Native methods of medical and magical treatment,

Native methods of surgical treatment (childbirth, bonesetting, incisions, trepanning, circumcision, excision, infibulation, sub-incision),

Decoration (tattooing, cicatrization, genital stretching),

Native nomenclature: syndromes, causation, theories of mental function and behaviour.

EXPERIMENTAL RESEARCH IN AFRICA

In general medicine, it is ideal to be able to prove the validity of theories based on observed facts and accumulated data. This proof is by the experimental method utilizing, for instance, the well known criteria known as "Koch's postulates". A simplified example of this method would be in such a disease as typhoid fever. Observed facts in typhoid include lesions in the intestine, from which bacteria can be isolated. The theory would be that the bacteria cause the lesions, which produce the clinical picture of typhoid. The theory that the bacteria are the cause of the typhoid can be "proved" by injecting them into a healthy person and producing the lesions and the clinical illness.

The rigid criteria required by experimental method in medicine are, with the present state of our knowledge, impossible to meet in psychiatry. This is because of the multiplicity of stress factors operating in human relationships which are nowadays proposed as contributing causes of psychiatric illness, and because basic scientific research has not yet come up with facts and theories adequate to explain psychiatric illness. However, it is expected that psychiatric illness will be explained ultimately by the neurochemists and neurophysicists. The basic medical sciences include genetics, anatomy, physiology, biochemistry, biophysics, pharmacology, and pathology including bacteriology.

These basic sciences have not been applied much in African research so far as psychiatry is concerned, except brain anatomy and pathology to a limited extent. Whether the healthy native African is different from other races has not been established by basic science research, let alone whether the unhealthy African is any different. Some research has been done on the gross and histological anatomy of the African brain, but it has been very little, and not in any way definitive.

Measurement of brain function by electrophysical means has so far not given results which would indicate essential differences

in the brain-wave recordings of healthy Africans and those of other races. Pathology research in Africa is active in tropical neurological disease. This research has in the past been done in Equatorial and West Africa by the French, in the Congo by the Belgians and continues at a high level of achievement in South Africa.

CONCLUSIONS

There is no doubt much to be gained from a multidisciplinary approach to psychiatry in Africa, or anywhere else for that matter. However, there is also a need to realize the limits of this sort of frame of reference and to narrow down the emphasis by drawing psychiatry back more into classical medicine and basic science. Only in this way will individual and group behaviour be finally understood and the individual-society barrier (or continuum?) be scientifically explained. One must plead for better delineation of research problems and the use of more strict methods in solving them. Research scientists trained and experienced in the fundamentals of anatomy, physiology, genetics, pathology, and pharmacology are required for this. At the present time, there are hardly any at work on psychiatric problems in Africa.

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DISCUSSION

Caudill: I appreciate Professor Margetts' hard-headed approach to this subject but I disagree with his comment that, since the general types of psychiatric illness are found everywhere in the world, they are not cultural. There are two ways of considering culture in anthropology; the more usual of these is to compare similarities and differences in two groups of people and their ways of life. The second way, which has been less adequately handled, is based on the belief that culture is a property of being human. I agree with Professor Hallowell when he says that a way of defining culture is to describe it as the state of the human condition in the generic sense, and I think that there is a relationship between the existence of psychiatric illness throughout the world and the state of the human condition. I do not agree with Professor Margetts when he implies that all psychiatric illness can be explained by abnormalities in biochemical and physiological processes, because I think there is an element in these illnesses that can only be accounted for in cultural terms.

Margetts: I think that we should try to avoid using terms like "being human" and "state of the human condition". These are vague words which do not really describe or explain anything. I think what Dr Caudill means by these terms is "being alive" and "state of taking part in living". We cannot, of course, explain all psychiatric illness by abnormalities in biochemical and physiological processes yet, but I foresee the time when we will be able to do so. I think that no reaction by the human organism, whether inside the skin or outside it, can possibly take place without a biochemical change. "Inside" includes what Dr. Caudill calls physiological and "outside" includes all cultural factors.

Mead: I agree that in one sense we might say that mental illness can be explained in biological terms but, in the framework used by Professor Margetts, parts of biology have been missed out altogether. Professor Margetts passed from language and the study of interpersonal relationships, without any intervening steps, to neurophysiology and chemistry. This treatment contrasts with Professor Hallowell's approach in which man is treated as a human being and as a part of the biological world where interpersonal relationships are an essential element in his sociocultural, biological evolution.

Margetts: I anticipate that one day we will be able to be more exact and scientific about the "chemistry of interpersonal relationships". Language, other factors of culture and interpersonal relationships all take place as a result of chemical action in the human organism concerned, and one day it will be possible to evaluate these elements scientifically. The gap in knowledge between what is observed in culture and interpersonal relationships, and what goes on in the participating human body, is at present very great, but it is narrowing. Further advances to determine the details of biological action and reaction within the organism can only be made through the basic sciences and not through study of the secondary effects—for example interpersonal relationships and cultural impacts—of such action. The organism reacts to other organisms, and to the environment, as a result of chemical processes activated by stimuli both inside and outside the body. These processes invariably involve chemical reactions with consumption of outside energy and they depend on individual constitutional make-up.

Wittkower: Professor Margetts, what do you think can be learned from psychiatric research in Africa that cannot be learned from other countries?

Margetts: I think that we can learn a great deal from African descriptive psychiatry, the secondary symptomatology of which is based on local culture and on epidemiological data peculiar to Africa, and from basic scientific data, which may vary according to the different human types and different environments being studied.

Lambo: We have no data at all on genetic problems in Africa. I think that this is an obvious gap in our knowledge and I would like to have Professor Margetts' views about this.

Margetts: Data about genetic factors in the causation of illness is incomplete even in many highly civilized parts of the world. To elicit this sort of information accurate records of census-taking, birth, marriage and death registration, case finding and history-taking should be kept. This is, of course, especially difficult in Africa, where we have to sift out more errors due to the covering up of information, downright lying and ignorance than in the more highly evolved societies (Margetts, E. L. (1958). *Med. Proc.*, **4**, 679). It seems to me that many psychiatrists today deny the possibility of a genetic causation of mental disorders because of their fixed belief in so-called psychogenic origin. Very little specifically genetically-orientated research has been carried out in Africa. Professor L. A. Hurst, in Johannesburg, is particularly interested in this subject (1962. *In* Expanding Goals in Genetics, p. 235, ed. Kallman, F. New York: Grune and Stratton).

Leighton: It can often be as confusing and misleading to look for a single, primary cause of illness in psychiatry as it is in general medicine. This idea that there is a single cause for mental disorder is illusory. We say that the pneumococcus is essential for a diagnosis of pneumococcal pneumonia because we have defined pneumonia in this way; it is part of our definition and not really part of nature. Pulmonary lesions produced by other organisms may be indistinguishable from pneumococcal pneumonic lesions but, by definition, they are not called by the same name. This approach by-passes the problem of why, under some conditions, a person develops the illness and under other conditions, when the body is equally invaded by the pneumococcus, he does not. I think that in general medicine as well as in psychiatry it is better to accept multiple causes without trying to divide these into primary, secondary and tertiary. However, it is important to recognize that some parts of the causal complex are more open than others to investigation and, what is even more significant, to control and modification. Surely, what we are really trying to understand is the complicated interrelationship of the factors of causation of mental disorder, and what we should really be trying to identify are the points at which our intervention will make the most difference in the system.

When psychiatrists restrict themselves to the description and identification of patterns of behaviour, and here I include such matters as verbal expressions and life stories, they find that they agree reasonably

well. It is when they are concerned with interpreting these phenomena in terms of theories of aetiology that they argue and contradict one another. In other words, psychiatrists may disagree considerably on interpretation and yet be in good agreement on the type of disorder that is phenomenally being manifested. I think it is not inappropriate to point out here that the very concrete branches of medicine, such as radiology or electrocardiology, do not have any better record for agreement than psychiatry.

Margetts: Causation is a very difficult problem in psychiatry and in medicine in general. I find it useful to consider causation as primary and secondary, and to try to define how these two aspects operate according to the "quantity" of each which may be present. I agree with Professor Leighton that some parts of the causal complex are more open to investigation, control, and modification than others. But I do think that we should spend more effort on trying to elucidate the chemical parts of the problem. The chemist Berzelius said more than a hundred years ago: "In science, nothing may be built upon uncertain possibilities; science may not be a tissue of conjectures: it must consist, as far as possible, of a system of demonstrated realities."

Rubin: Professor Margetts, from your uncompromising organic platform, do you think that biological factors can be subject to environmental modification?

Margetts: Yes, I do. I expect that we shall eventually be able to modify all biological functions of thinking, feeling and behaviour by the environment. If you administer a pill, such as a psychotropic drug, to someone, the drug acts within the organism but it came originally from the environment. If one person frightens another, a chemical action takes place in both persons. If a person is isolated and his senses are artificially altered his body chemistry changes, just as when a person living in a mountain area moves to the plain his chemistry changes. By analogy, if a person has to live in extreme poverty, this is bound to affect his bodily equilibrium possibly because of inadequate nutrition. I think that there is a continuum between environment and organism which, in the final analysis, depends on biochemistry.

I would like to stress again the difference between functional and organic. Functional imbalance means that an organism does not work in ideal homoeostasis; there may be no visible structural abnormalities,

but the organism is deficient in some way. Organic malfunction, on the other hand, is defined as a structural abnormality and sometimes also as a known metabolic or toxic dysfunction. I anticipate that functional dysequilibrium will become more explicable when additional facts about its causation are known. Perhaps it would be more rational to use the words "cryptogenic" (concealed cause) and "phanerogenic" (manifest cause) for functional and organic respectively. I certainly think that the adjective psychogenic would be better replaced by cryptogenic. Since we do not really know what the psyche is, we cannot say that it is the cause of anything.

Loudon: I think that Professor Margetts may have over-emphasized the importance of errors in training and differences in intuitive skills in the production of observer errors. Such work as that of Professor Cochrane and his colleagues (Cochrane, A. L., Chapman, P. J., and Oldham, P.D. [1951]. *Lancet*, **1**, 1007) seems to indicate that lack of skill is not as important as other factors in producing observer errors in, for example, reading X-rays and recording blood pressures.

On a quite separate issue, may I beg Professor Margetts to consider dropping the word "bush" and using the word "field" instead; bush has overtones which in twenty years' time will be difficult to justify.

Margetts: Perhaps the explanation of the discrepancy between my statements and Dr. Loudon's criticism is due to our different conceptions of what is meant by observer error. I believe that factors other than errors of training and differences in intuitive skills are involved in the production of observer error. Intelligence, learning ability, breadth of knowledge, powers of observation, attitude and motivation are additional factors to be considered in evaluating diagnostic acumen, and there may be still other factors.

I do not see much wrong with the word "bush." In Africa, by common usage, it means the country outside the towns. A lot of Africa *is* bush and there will be plenty still left in twenty years. Margaret Field has used the word bush both in correspondence with me and in a paper entitled "Bush Psychiatry in Africa" (1963. *Transcult. psychiat. Res.*, **14**, 66).

Lin: Psychiatric research in Africa is beset with difficulties and yet, because of the many ethnic groups there, it may eventually provide us with an unique opportunity. Because of its small, well-defined,

ethnic groups Africa may be regarded as an ideal laboratory. If the sociocultural research of the anthropologists and social psychologists can be combined with the studies of psychiatrists and psychiatric geneticists, observations on the susceptibility of certain populations to certain specific mental disorders might result in an increase of our overall understanding of mental illness.

I think that operational research into the evaluation of the psychiatric services, as described by Professor Margetts, is another very important area of investigation. We should not confine our research to understanding the aetiological factors in mental illness; we should also try to understand how these factors can be influenced by intervention. There is a shortage of psychiatric personnel in Africa and therefore we are not yet able to conduct elaborate research into the elucidation of aetiological factors in mental disorders there, but we can help our colleagues, in a practical application of research, to improve their mental health services.

Yap: The dichotomy between pathogenic and pathoplastic is usually expressed in terms of aetiology; we talk about these two types of aetiological factor, but surely investigations which attempt to enable us to distinguish between the two are premature at the moment. We do not really know how to diagnose and identify our cases in aetiological terms, so surely the most important starting problem is to learn to recognize *pathogenic* (*or process*) symptoms and *pathoplastic* symptoms. It is very easy to distinguish between the two theoretically, but in practice it is difficult to tell what is a pathogenic symptom arising from a basic pathological process, and what is a pathoplastic symptom produced merely by cultural moulding.

Without knowing what a disease is in terms of basic pathological processes or aetiology, we just do not know what we are talking about and so other people cannot make use of our observations. Professor Lambo has assumed that textbook descriptions of disease in the West, for example schizophrenia, will provide the process symptoms for identification. I realize that in such preliminary methodological discussions there must be an arbitrary element, but I think a more scientific approach would be to acknowledge that these western textbook definitions are not themselves complete aetiological disease definitions, and are capable of refinement. We should take a rough definition

of a given disease and study what is denoted by it in as many different cultures as possible. In this way we shall discover what is common and what is not, what is universal and what is variable. I think that it is from cross-cultural studies such as these that useful, refined definitions of psychiatric disorders, related closely to basic pathological processes if not to aetiology, will evolve.

Lewis: Does this apply to the neuroses as well as the psychoses?

Yap: I think so, but this kind of study of the neuroses would be difficult because of their intimate relationship to the cultural background.

DeVos: It seems to me that one of the tasks of psychiatry is to arrive at considered definitions of the limited spectra of psychopathological human reaction-syndromes regardless of cultural variability. In medicine, definitions of syndromes are related to a particular combination of bodily reactions to disease. Fever, for example, has many aetiologies and it usually appears combined with other bodily reactions; it is this combination of fever with other clinical features which enables us to say that a specific, definable syndrome is present. In psychiatry, we do not yet have agreement as to what particular mental or emotional reactions also form part of specific syndromes. Examples of such reactions might be breakdown of thought processes, certain types of affective responsiveness and different types of dissociative reactions. I think that this is where the problem lies: we should first attempt to define the types of reaction processes regardless of aetiology, and go on from there to a systematic attempt to define syndromes which include these processes, whether they are related to a specific aetiology or not. There are syndromes of mental pathology that differ in different cultures although the specific reactions of which they are comprised are found universally. In cross-cultural material we find only a limited number of reactions whatever the culture; for instance, the typical physical posture associated with schizophrenia can be seen in Illinois or Nagoya or Japan. We do not have to travel to Africa, say, to find a recognizable schizophrenic stance, and when we find the same clinical features of disease in Africa and in California we realize that this is all part of what Professor Hallowell has called the human potential. This human potential comprises how one breaks down as well as how one learns. It is the function of transcultural psychiatry, as I see it, to

search through the world's cultures, to investigate first the spectra of mental and emotional breakdown and then to define consistencies or inconsistencies in patterned syndromes.

Mead: I am very much in favour of Professor Margetts' advocacy of the use of photography, filming, tape-recording, somatotyping and genetic and other measurements in our research. I think, though, that it is unfortunately quite possible to obtain extraordinarily confusing results with high-powered methodology based on physical measurements and modern technology.

In a recent study of Kuru (Gajdusek, D. C., and Gibbs, C. J. [1964]. *Nature* [*Lond.*], **204**, 257), the investigators used photography and were able to do post-mortems, and anthropologists in the area did much careful work on the genetic background. It was possible, in this study, to delimit the language area within which Kuru occurs and also to treat the patients with a variety of drugs. In spite of all this, no one has yet been able to make up his mind exactly what Kuru is or whether or not it is caused by a virus. Kuru is just one small, exceedingly individual disease which has only recently been identified and occurs in a very small geographical area, so we probably had exaggerated expectations about the immediate results of our high-powered methods. In spite of this disappointment, genetic data are very important and we can at least collect material that we now know has some genetic usefulness whenever we are studying a small, culturally delimited and isolated group. We need a combination of all methods, and somatotyping, testing for blood types and adequate genealogies are just as important as careful clinical descriptions, verbal and behavioural, of the aberrations, deviations, and characteristic syndromes in all areas.

Margetts: I certainly agree that a combination of all methods is needed.

Fortes: Professor Margetts mentioned the W.H.O. definition of mental illness, which I think is extraordinarily interesting from the sociocultural point of view. The criteria used in this definition are inability to work according to the standards of society or inability to carry on a normal social life. The Hadza, a surviving hunting and gathering tribe in Tanganyika and the most perfect example of an isolated society which remains in Africa, have the same sort of criteria

for mental illness—what they call being "on the edge"—as the W.H.O. In Hadza communities you are given the label of madness if you are unable to work in the way that is appropriate to your age, sex and status, unable to fulfil the adult role of partner in marriage or unable to obey the laws of the community. We may use these three criteria, employed by the Hadza and W.H.O., as a starting point from which to ask the question: "Are these the basic universal elements which identify madness; and if so, what does this mean?"

Shepherd: Professor Fortes, are there any societies where the norm is the individual who does *not* work?

Fortes: Not so far as I know. We must now ask ourselves exactly what we mean by work. In Africa it can be anything from working on a farm or looking after cattle, to fulfilling your role as a Fulani elder. When the Fulani elder's career is finished, he hands over everything to his sons but his "work"—and the word is used to mean work—consists in sitting under a tree outside his cattle encampment and keeping an eye on everything!

Transcultural Psychiatry
A.V.S. Deuck & Ruth Porter

THE EPIDEMIOLOGICAL APPROACH TO TRANSCULTURAL PSYCHIATRIC RESEARCH

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TRANSCULTURAL psychiatry calls, almost by definition, for co-operation between disciplines and yields relatively little to any single approach. For this reason I would have preferred to discuss the results which obtain when different approaches combine, rather than a single technique. However, I also feel that my allotted subject, the epidemiological approach, has been considerably misapplied and misunderstood in the past, and that a clarification of both its limitations and its potentialities is called for. Rather than survey the general field, therefore, I propose to touch on the reasons for its past failures, and then to illustrate some of what I regard as its potentialities. The latter I will do by reference to work currently going on at McGill, not because I believe this necessarily to be the most sound or most advanced, but so that you can learn a little of what we are doing there. There are many other sources that could have been tapped for illustrations of such potentialities, of course, but some are too well known to require comment and others are represented here and will, I hope, be referred to in our later discussions.

The past failure of psychiatric epidemiology is quite obvious if one considers how much has been done and how rarely the findings have contributed to psychiatric theory or even to the application of that theory and the planning of services. It has never been demonstrated that a survey, for instance, provides a good predic-

tion of the psychiatric services that a community will need, and it is only in sociology, through such workers as Dürkheim and Faris, that a real contribution to theory can be detected. Yet despite this, the flood of statistical reports and epidemiological surveys continues, as I know to my pain, since it is often my task to draw some meaning from them for the *Transcultural Psychiatric Research* abstracts. Why is it that such reports continue to roll forth and that they manage to tell us so little? The answer is that we too often ask the wrong questions.

The continuance of epidemiological work in our own transcultural field seems natural, for if one wants to explore the relation between social variables and mental disorder one wants to start with hard facts about the latter, and epidemiology seems to be one of the obvious ways of reaching these. It is recognized, of course, that in transcultural work the data may not be just lying there to be brought together, but it still seems to be assumed by many people that, if one takes the trouble to go out and hunt for the data, these can then be processed to yield hard results similar to those which epidemiology supplies in other branches of medicine.

This is a false assumption. Psychiatric epidemiology differs from the classical variety in several important ways. The traditional goals of the epidemiologist were to map out trends in the distribution of discrete disease entities, to track down prime causal agents, to prepare the way for the mass application of established preventive or curative steps, and to measure the effects of such steps. Through confining himself to clear, accepted indicators of pathology and to simple, quantifiable, antecedent variables the epidemiologist achieved a reputation for reliability, and his assessment of a situation was usually taken as representing sound objective fact. The modern epidemiologist strays in less secure areas, but the soundness of his facts is still believed in and is an object of envy for many psychiatrists and social scientists, involved as they are with the ambiguities and complexities of human person-

ality. Yet the epidemiologist's findings can never be more objective or sound than the data from which he operates. It is fruitless to look to any survey technique, however refined, to measure the true prevalence of mental disorder when the definition and indicators of such disorder are still very undecided. Moreover, even if the epidemiologist's global assessment of mental disorder in a population were as sound as his assessment of, say, tuberculosis, the bare result would still be of limited value. It will not enable us to predict the nature and amount of psychiatric services required by the population as long as we are still in considerable confusion regarding what such services can be expected to achieve. It will not, either, add much to our theoretical knowledge as long as mental disorder seems likely to be the outcome of a multiplicity of factors all present in varying degree in the same population and none having such overwhelming influence that the others can be disregarded. Complex aetiologies need complex analyses to unravel them. To ask for simple answers is to misunderstand the situation.

What, then, is the use of psychiatric epidemiology? In particular, what justification is there for adding it to the more basic clinical and anthropological approaches of our transcultural field? Compared to the latter it is insensitive, destroying the subtlety of observations by its procrustean pigeon-holing; it is voracious, demanding much larger numbers of observations than would suffice for a clinical approach; it is crude, usually unable to handle the concepts in which we see most promise, concepts like the ego defence mechanism, *Eigenwelt* and *anomie*. Why add this approach to the more sensitive clinical or anthropological ones? The answer is that, although the clinician and anthropologist can with experience infer probable associations between psychiatric manifestations and antecedent sociocultural experiences, they cannot usually identify broad, low-relief patterns of association in whole peoples, and they find it extremely difficult to assess the probable weight of different antecedent factors. This the epidemiological approach,

if given suitable data, can do. It can search for general patterns underlying the welter of presenting data, it can provide estimates of the relative probability of different causal associations, and it can specify within whole peoples which particular social situations or experiences are likely to be associated with disease and which with health. Its answers, given thus, are not going to be "reliable" in the sense that some classical epidemiological estimates were, but they will be "probable" in the sense that a psychiatric diagnosis is probable, permitting provisional decisions to be taken and indicating further lines to follow. If one is dealing with global estimates of psychiatric vulnerability in whole cultures the margins of error which are involved make it very difficult for the social scientist, or anyone else, to develop and test responsible theories; but if the same techniques are used to provide a range or pattern of information, then this will usually be something that can be built on, regardless of the imprecision of any particular part.

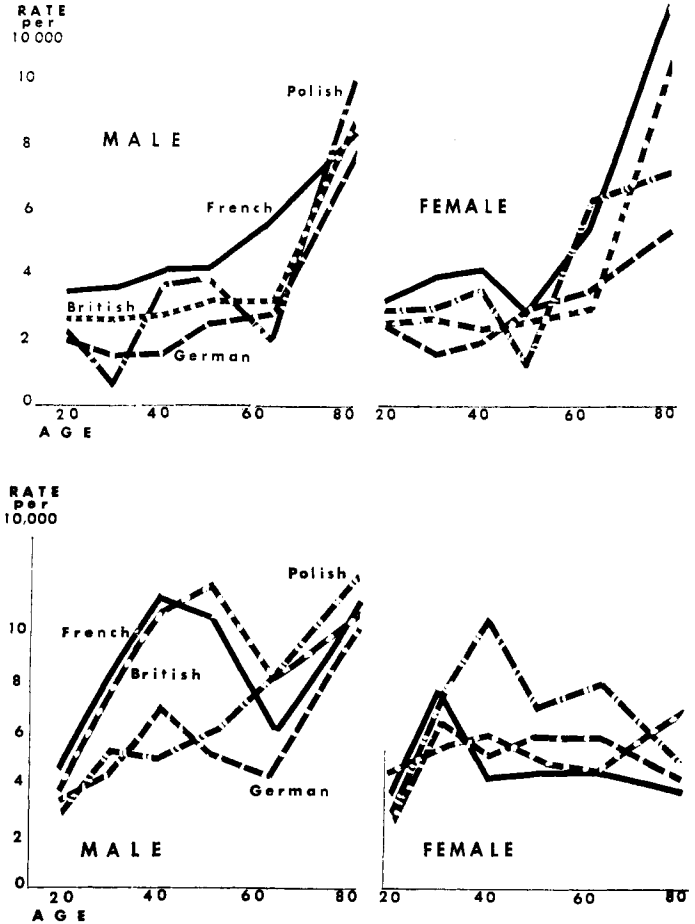
Before turning to the illustrations of this thesis, the question of what are suitable data needs to be faced, since transcultural psychiatry is often fortunate to be in possession of any data whatever, and must usually make do with what is admittedly incomplete or biased. Can the goals I have mentioned be pursued with the sort of information we usually possess? My own belief is that suitable data can be found much more often than is usually recognized, and that the common weakness of epidemiological research in our field is not the unavailability of information but the inappropriateness of the questions asked and the techniques applied. One must be modest and imaginative in one's approach. Unquestionably there are some indicators of mental disturbance that are sharper than others, or more sensitive, or more embracing, but almost any indicator can be constructive when used properly. Bias or distortion will be present regardless of the indicator chosen, but there are ways of circumventing or neutralizing this and it is often possible to select questions to which the major biases present will not apply. Epidemiology, I have said, is voracious, but it can work

with quite small population samples if enough is recorded about them. It works best on a comparative basis, with two or more cultural groups being studied with the same criteria and under the same conditions; but if one's information relates to a single people only, useful conclusions can still be drawn if attention is focused on the relationships and patterns within the data rather than on the total rates they provide. Finally, if we believe ourselves short of data let us remember that any society that has distinct cultural traits is grist for our mill, not only those in psychiatrically underdeveloped lands. Europe is rich in cultures and subcultures, and not poor in psychiatric information. Much could be done there by comparing, for instance, the Calvinist Friesians and Catholic Brabanders, the Bretons and the Lyonnaise, and the different Swiss cantons.

But epidemiology should not move alone in such directions. No matter what source of data one uses, the epidemiologist's findings are of little value in transcultural psychiatry if we do not have the social and anthropological information to match. Our field must remain a co-operative one.

CANADIAN SUBCULTURES

I can illustrate the last two points by using Canadian national statistics. The Canadian census and mental hospitals record cultural "origin", and this is a reasonable indicator for the tradition, usually European, most likely to possess a vestigial influence underlying the more obvious general Canadian culture. Here are some findings for four such "origin" groups, by no means the most extreme. Fig. 1 shows chronic hospitalization: the French origin group has the highest rates at nearly all ages, and the German usually the lowest. Fig. 2 shows brief hospitalizations: on the male side the two older-settled peoples (French and British) show a very different picture from the two others, but on the female side what is striking is that the Polish women show a



FIGS. 1 and 2. Age-specific curves of first admission to Canadian mental hospitals, 1961, by sex, for patients of French, British, Polish and German origin respectively. Fig. 1. Patients who remained in hospital for more than 18 months before initial discharge; Fig. 2. Patients who remained in hospital for less than one month before initial discharge. Quebec Province data are excluded from the analysis since mental hospitals in that province do not record "origin". Curves for each of the other provinces were calculated separately and it was found that the same broad relationships between the origin groups applied in each. Therefore, adjustment for provincial distribution has not been thought necessary in this presentation.

pattern quite different both from that of the other women and from that of the Polish men.

Fig. 3 shows hospitalization according to occupational class. Following with the eye the broad trend from highest social class on the left to lowest on the right, one sees, among other things,

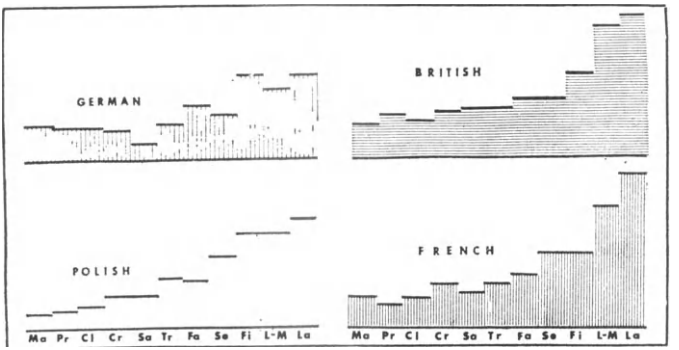


FIG. 3. Relative occupation-specific rates of first admission to Canadian mental hospitals, 1961, for male patients of French, British, Polish and German origin, corrected for provincial distribution but not for age. The occupational categories correspond to broad divisions used in the Census of Canada, 1961, and are ranked in order of their overall hospitalization rates. They are: *Ma* Managerial; *Pr* Professional; *Cl* Clerical; *Cr* Craftsman and skilled manual workers; *Sa* Salesmen; *Tr* Transport and Communications workers; *Fa* Farmers and farm workers; *Se* Service occupations; *Fi* Fishers and hunters; *L–M* Logging and Mining workers, excluding skilled mining technicians; *La* Labourers and unskilled workers generally. A number of small categories of occupational group have been omitted for the sake of clarity.

that the French origin group shows the sharpest rise with declining social class, whereas the German shows a much milder and more irregular gradient. If one contrasts the two predominantly Protestant groups with the two predominantly Catholic, one sees that, although in the lowest class the former have lower rates than the latter, the position is reversed in the upper-class managerial and professional sectors, the German group even showing a rising trend towards that end of the chart.

Similar differences between the four groups can be found if one looks at the diagnostic distribution, at marital status, at nativity status, and at probably whatever other variable one likes to choose.

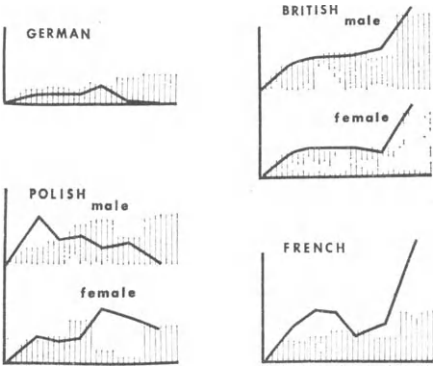


FIG. 4. Age-specific curves of first admission to mental hospital for patients of French, British, Polish and German origin residing respectively in Metropolitan Toronto (curves) and in the rest of Ontario Province (shaded). As some Ontario hospitals treating the lighter types of disorder do not record "origin" the above figures should be taken as referring mainly to admissions to the government mental hospitals. Allowance for the non-reporting hospitals could change these pictures slightly and, as numbers are less than in previous figures, chance variation from year to year is likely to be greater than that illustrated in Figs. 1–3.

Fig. 4, as a last illustration, shows the marked differences to be found when one contrasts the age curves yielded by the metropolitan sections of these four subcultures, and the non-metropolitan, from the same province. These latter curves may change somewhat when corrections are made for incomplete reporting from some hospitals, and I find the German origin curve for Toronto suspiciously low, but the main differences are likely to

remain and to contribute to the psychiatric profiles which can slowly be built up for each subculture.

One would naturally like to know the probable explanations for these differences but, for two reasons, I am not prepared to suggest any. The first is that more work still needs to be done to reduce distortions in the patterns shown and to explore the relation of one social variable to another. We are, for instance, allowing for the different hospitalization patterns of Canada's ten provinces in most of our work but not yet for the different hospitals within each province or for the age structure of different occupational groups. The second reason is that information from the social sciences regarding the traditions of the four subcultures as they persist in Canada today is still lacking. We have, as I will indicate shortly, information regarding a few rural communities belonging to the subcultures, but that is very different from knowing about the picture in Canada as a whole.

SINGAPORE

However, in case you think that this absence of available interpretations must be the rule when one deals with gross data of this type, let me give you an illustration from Singapore, where the cultures were better documented. Fig. 5 is a comparison of the mental hospitalization rates of the three main peoples in Singapore, analysed by broad age group and by social class (males only). The Chinese show, quite consistently, the double trend of an increase in rate with declining social class and a decrease in rate, regardless of social class, with increasing age. The Malaysians, however, show no increase in rate with declining social class and no consistent age pattern. The Indians, finally, show a peculiar pattern of a declining rate with age in the upper two social classes, and a rising rate with age in the lower two classes. The numbers of patients involved are large enough for me to assure you that these differences in pattern are not accidental. How then do they arise?

It is easiest to suggest an explanation for the Chinese picture. It derives, I believe, from the emphasis on striving for the sake of the family which was a very clear tradition in the Singapore Chinese, and from the respect and tolerance shown to the old, even in the labouring class. The Malaysian pattern, more complex,

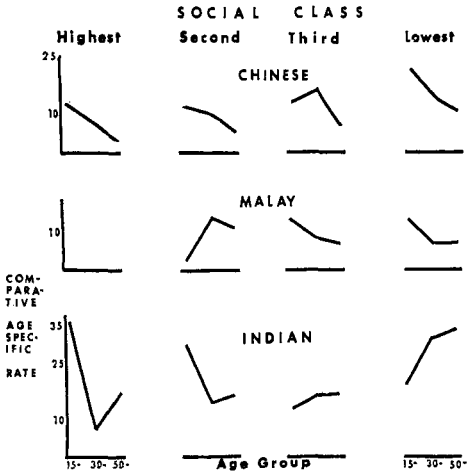


FIG. 5. Incidence rates of first admission to mental hospital, Singapore, 1950–4, for three ethnic groups, by broad social and age class, males only. There were insufficient Malay patients from Social Class I to permit separate calculation of rates for each age group; for this ethnic group, therefore, Social Classes I and II have been combined.

derives, in my opinion, from the lack of cultural emphasis on material, and a preferential emphasis on emotional, values. At that epoch in Singapore, a livelihood of a non-competitive kind was available to all Malays, but any attempt at entering and remaining within the modern urban middle class called for the Malay to compete with the Chinese and Indians on their own ground, and to adopt a future-oriented, possession-oriented way of life which clashed with the present- and emotion-oriented

emphasis of the general Singapore Malay population. Hence the poor mental health of the middle-aged, middle-class Malay trader and minor professional which the figure suggests. The Indian pattern, however, is the most interesting. It is related, in my opinion, to the removal of restraints on the use of power which the migration from South India to Singapore occasioned. From my student patients it was clear that age *per se* was not respected, but that the power and the purse of the older generation was. Accordingly, I suggest that the picture in Fig. 5 represents the frustrated resentment of the young white-collar Indian towards his elders, and the stress imposed on the aging labourer unrespected by his more physically powerful juniors. In their home villages there were various traditions to restrain such use and fear of power, but my belief is that in Singapore at this time these restraints had temporarily disappeared.

These suggested explanations are absurdly condensed and they may be untrue, but they indicate, I hope, the type of new theorizing which becomes possible when one adds to the appropriate anthropological information the appropriate epidemiological technique, and the medical data need be no more refined than that provided by hospital or court records.

CANADIAN VILLAGES

If one's data are more refined, of course, then explanations can be offered with greater confidence, especially if social, clinical and epidemiological approaches have gone hand in hand. Even here, however, results can be missed if the analysis is insufficiently flexible. For instance, Fig. 6 gives some data from a recent field prevalence survey involving the same Canadian subcultures mentioned above. This was a key-informant type survey of the major mental disorders only, and hence much less sensitive than the census type which Professor Leighton has made familiar to us, but better suited to the question of patient-community relations

which was our main focus. In order to be brief, I am confining my illustrations to a single disease, schizophrenia, and to a single main aspect of it.

The seven communities cited in the figure are the most culturally distinct of the thirteen rural populations we studied. St. Gildas and St. Xavier are traditional, wholly French-speaking communities from the heart of Quebec, very similar in history,

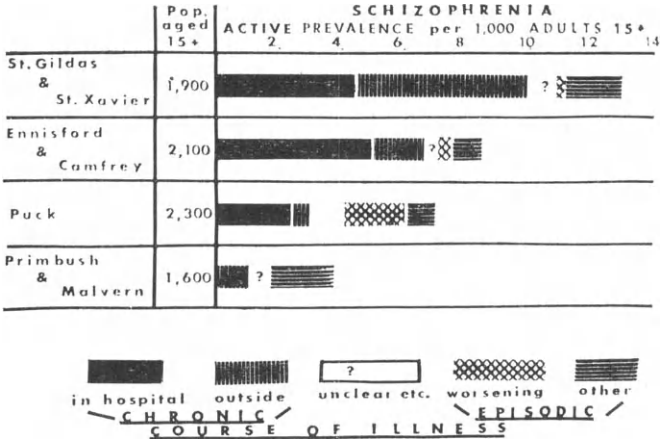


FIG. 6. Active prevalence rates for schizophrenia in seven rural communities in the provinces of Quebec and Ontario, divided according to course of illness. For meaning of "active prevalence" and for general explanation, see text.

attitudes and values, and in mental disorder pattern. Ennisford and Camfrey are mixed but predominantly Roman Catholic areas of mainly Irish origin, less culturally distinct than the others but the most strongly Irish we could find. Puck is almost wholly of Polish origin, and Primbush and Malvern are English-speaking Protestant enclaves in the Quebec countryside. The rates indicated by the total length of the bars refer to all individuals reportedly showing signs of active schizophrenia within a one-year period, whether hospitalized or not, and exclude those with evidence of

apparent previous or later schizophrenia if they showed no appropriate abnormality and were not taking an ataractic drug during the year. No German origin community is included since none with sufficient schizophrenic patients for this type of analysis was covered.

The spread of rates is very striking, but the relative positions are the same as in the Canadian hospitalization rates given in Fig. 1, and the same as are obtained when taking the total members of each origin group in the 30,000 population surveyed, as shown in Fig. 7.

Comparing the prevalence with the incidence data, one could infer that there must be a difference not only in the incidence of schizophrenia but in the chronicity of the active process, and one could have started to search for explanations at that point. The picture becomes more informative and theorizing more soundly based, however, if instead of taking only prevalence rates, perhaps divided by sex and age group, we analyse them by the course the illnesses have taken. First, if we divide the patients into the relatively chronic, slowly deteriorating group, regardless of symptomatology and formal type, and the episodic, intermittently functioning group, then it can be seen that the former predominate in the French and Irish origin communities but not in the others. Next, if we divide the chronic patients into those fairly steadily in hospital and those remaining completely or almost completely out of hospital, we can see that the non-hospitalized predominate among the French-Canadians, but not in the other Roman Catholic communities. Finally, if we divide the episodic into those whose episodes seem to be more severe each time and those without this tendency, there is a suggestion that the former are especially to be found in Polish-tradition Puck.

These differences, with the possible exception of the last, do not appear to arise by accident, since a study of former patients' histories yields similar patterns. Instead, there are two cultural variables to which our sociological and clinical materials point

as significantly contributing to the divergence of patterns. The first is the contrast between an emphasis on acceptance of God's will, which we found in most of the Roman Catholic communi-

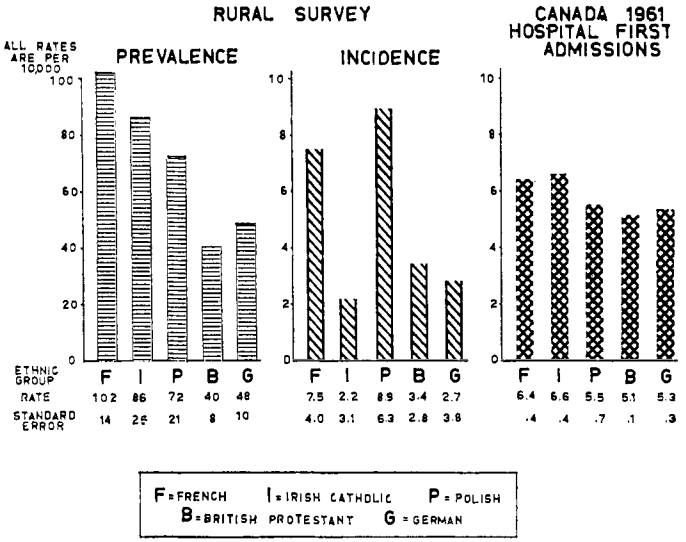


FIG. 7. Comparative levels of active prevalence, of onset incidence and of hospitalization incidence for schizophrenia in five Canadian subcultures. "Active prevalence" and "onset incidence" data derive from the rural survey described in the text, but from the total thirteen communities, not only from the seven discussed in relation to Fig. 6. "Active prevalence" measures cases showing signs of active pathology at any time within a one-year period; "onset incidence" refers to cases reported to have shown first signs of schizophrenia within a three-year period, the rates being reduced to a per annum basis. The hospitalization incidence rates refer to first admissions only, and derive from the same source as Figs. 1–3.

ties, and an emphasis on personal work and striving which we found especially in the two Anglo-Protestant enclaves, but also in Protestants generally. The second relates to community definitions of the sick-role and the degree to which social integration clarifies and enforces these definitions. The description and discussion of these influences need, and will receive elsewhere

much fuller attention than there is time for now. Let me say briefly, however, that in the two Protestant communities I have been struck by (*a*) the community's tolerance of deviance, provided only that the patient can earn his own living, (*b*) its promptness to seek treatment when someone ceases to function thus, and (*c*) the effort which patients seem to make to reoccupy a working role, even when still hallucinated or delusional. This picture is not found in any other of the communities cited. Dr. Marcel Lemieux and myself have been impressed by the clarity with which St. Gildas and St. Xavier indicate permissible and unpermissible sick-role behaviour, supporting the patient who conforms but expelling, usually permanently, the one who does not. This prescription and support of a chronic sick-role contributes substantially, in my opinion, to the presence of so many non-hospitalized, non-self-supporting schizophrenics in these, the two most highly integrated communities in this study. The Irish and Polish origin communities, as well as some less traditional French ones, share a relative resignation towards mental illness, but do not provide the same clarity of sick-role definition and support, so that their patients' positions are more unstable and lead more often to death, relative recovery, or expulsion from the community to chronic hospitalization.

A fuller exposition of our observations and ideas regarding these communities and their schizophrenic members can be found in a recent paper (Murphy, 1964). They were cited here to illustrate how more stimulating lines of thought may sometimes be opened by straying slightly from orthodox epidemiological categories.

TWO-STUDY COMPARISONS

In both the studies just cited we have been fortunate enough to have different subcultures living in the same area and sharing the same occupational possibilities, psychiatric services, and so on, but this will not always be so and it may be objected that they provide

a model only rarely applicable. What does one do when for conceptual or operational reasons one has data only on a single culture, and perhaps relatively scant data at that? Such material can be used, and the results compared with results collected elsewhere, using different approaches and criteria, as the following illustration shows.

Dr. Norman Chance, of our unit, has been studying the impact of modern life on a small Eskimo community in Alaska. Among other approaches, he administered a modification of the Cornell Medical Index to 53 subjects, 90 per cent of the adult population, and looked for some association between cultural change and this index of mental health. Such an association did not appear initially, and with such small numbers there seemed little manoeuvring open to him, but he persisted and divided his cultural change indicator into two parts, one reflecting a presumed inclination for change and the other the opportunity for it provided by culture contact. With each alone, there was no significant variation in the Cornell Medical Index scores, but when the two parts were recombined, a clear result appeared.

This result came to the attention of Dr. Hsien Rin of Professor Lin's department in Taiwan, who visited our unit and brought with him material from a much larger field survey of psychophysiological syndromes in a Taiwan town. We soon realized that Dr. Chance's approach could be applied to the Taiwan material, both to enlarge the analyses possible and to act as a check on the general validity of Dr. Chance's conclusions (Chance, 1965; Rin, Chu and Lin, 1965). Fig. 8 shows the two sets of results. Since different measures were used both for mental health and for cultural change it is clearly not permissible to compare the results directly, but it is permissible to compare the patterns which each exhibits, and this is instructive. First, one sees that both for the Eskimo and for the Chinese mainland sample, an association exists between the height of the mental ill-health rating and the degree to which inclination for change seems to outstrip contact with it.

Cultural differences presumably account for the fact that among the Eskimos there is a marked sex difference in rating but little age difference, whereas with the Chinese there is a marked age difference but little difference between the sexes; the general

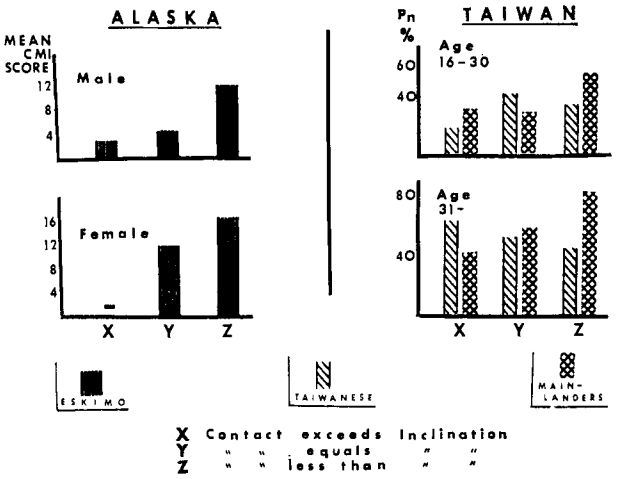


FIG. 8. Comparative measures of mental disturbance in Eskimo and Chinese subjects with different degrees of inclination for, and exposure to, culture change. Alaskan data refer to scores from a modified and shortened translation of the Cornell Medical Index. Taiwan data refer to percentage of a population sample showing signs of psychophysiological disturbance at psychiatric interview. The Taiwan Chinese are mostly non-migrant whereas the Mainlander Chinese had all been displaced from their homes on the Chinese mainland.

association, nevertheless, is clear. Yet the association cannot be assumed to be universal, since it does not appear in the Taiwan-born Chinese, who can be presumed to be less socially disturbed than the migrated mainlanders or the Eskimos. If Dr. Rin's analysis is confirmed, a still more sophisticated model seems called for.

While these questions need further exploration, the two methodological points should be clear. Dr. Chance's work

yielded useful results even though it applied to only 53 people, all of the same culture. Dr. Rin's material could be used to enlarge on Dr. Chance's even though different measures of mental health, different survey techniques, and different measures of cultural change were used. Comparisons must be carefully undertaken, of course, but in so far as they can take place between two such independent sources, they can clearly be made with equal validity among many sources.

INTERNATIONAL SURVEYS

I would like finally to discuss the possibilities and limitations of general surveys in a number of countries using unstandardized criteria, settings, and sources. In the past collections of psychiatric data from different countries, although regularly published, have served more to confuse than to enlighten. The use of such a collection in the otherwise admirable Hutterite study of Eaton and Weil (1955) is an obvious example. In contrast, international surveys of anthropological observations, through the rich resources of the Human Relation Area Files, have contributed substantially to our field. Is there any possibility of psychiatry providing something like the Area File material? I believe that there is. At McGill we have been attempting this, using only the impressions of voluntary collaborators. These efforts have yielded some success (Murphy *et al.*, 1963; Murphy, Wittkower and Chance, 1964), but even more usefully, they have also provided pointers to the potentialities and problems likely to be uncovered if the work were done more systematically. The potentialities lie in two directions: the first is the relation of psychiatric characteristics to transcultural social variables; Dr. Chance (1964) has uncovered a probable association between the reported frequency of endogenous depression and the degree of social cohesion, and delusions of grandeur have been shown to be associated more with rural than with urban life (Murphy *et al.*, 1963). The second is the preparation

of psychiatric profiles for single cultures; an attempt to do this for depressive symptomatology is shown in Fig. 9. The findings themselves, especially those in Fig. 9, are still of quite doubtful significance, but the possibilities indicated by them are impressive enough to justify a more systematic collection of uniform data on comparable series of patients in different countries.

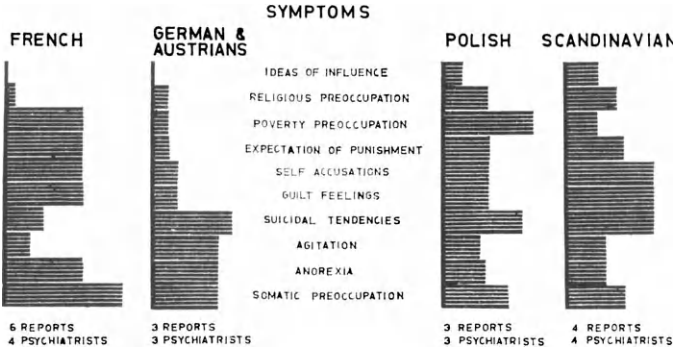


Fig. 9. Comparative frequency of auxiliary symptoms reported by psychiatric respondents to an international questionnaire, as respects French, German, Polish and Scandinavian patient samples suffering from endogenous depression. For details of approach, see Murphy (1964).

Yet the problems are also impressive. One is the training of psychiatrists to classify patients socially and culturally; others are the organization of anthropological information on the cultures and subcultures in which psychiatric information is most readily available; the development of equivalent terms in different languages and the avoidance of those which have similar face meanings but very different overtones; the assessment and allowance for conceptual and operational differences between various sources of data; the choice of appropriate epidemiological techniques with which to handle the data once they are obtained.

At the beginning of this paper I tried to indicate how transcultural psychiatry is in danger of misusing the epidemiological

approach. At the end of it I would stress the danger of the epidemiologist misusing or misunderstanding transcultural psychiatry. There are several centres where attempts are being made to devise means of making international psychiatric comparisons. Too often these programmes seem to be underrating the problems I have mentioned and especially the anthropologist's contribution. Although there are great potentialities in the proper collection and comparison of international data these will not be realized unless a fully co-operative approach is used.

SUMMARY

Past contributions of epidemiology to the field of transcultural psychiatric research have been relatively slight, despite the frequent use of this approach. This is largely because there is a tendency to regard epidemiological findings as more precise than the diagnoses, symptomatology, and so on, on which they must be based. The correct contribution of epidemiology to this field should be, not the production of spuriously precise rates, but the elucidation of probable patterns and relationships between sociocultural and psychiatric variables when these relationships are difficult to perceive clinically. Illustrations of such patterns and of the theorizing to which they can give rise are presented, using sources ranging from national hospitalization statistics to psychological test results from a small Eskimo village. It is stressed, however, that such findings cannot lead far in transcultural psychiatry unless they are supplemented by clinical and anthropological contributions.

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DISCUSSION

Lewis: Dr. Murphy, do you think that you are taking a rather narrow view of epidemiology when you regard it as preoccupied with head-counting?

Murphy: In the international surveys which we are doing at McGill University we are not counting heads at all but trying to explore the distribution of symptoms in different cultures (Murphy, H. B. M. [1965]. *Int. J. soc. Psychiat.*, in, press; Murphy, H. B. M., and Chance, N. A. [1964]. *Transcult. psychiat. Res.*, **1**, 5; Murphy, H. B. M., *et al.* [1963]. *Int. J. soc. Psychiat.*, **9**, 237; Wittkower, E. D., *et al.* [1960]. *Transcult. psychiat. Res.*, special issue no. 9). In the last published paper on this subject I suggest a possible distinction between the primary process symptoms of endogenous depression and the secondary symptoms (Murphy, 1965. *Loc. cit.*). I certainly do not think that counting heads is always necessary in epidemiological surveys.

Lewis: You have shown that there is an antithesis between the treatment of male and female elderly, Polish schizophrenics, as the male goes into hospital whereas the female does not. Would you make this observation the basis of a hypothesis, which you can than test by more headcounting in other areas and by a study of the social structure of the Polish community as a whole?

Murphy: This observation on the elderly, female, schizophrenic Pole was made only in Puck. It is rather puzzling and we hope to take the investigation further. The older generation in Puck is very male-dominated. The men, when they retire, form a club which meets in the summer on the Post Office steps; they rule the village as a committee of elders and enjoy considerable power over the younger generations presumably as a reward (or compensation) for their old age.

The wives stay at home and lead a very isolated life. From these superficial observations one would think that the women were under a greater stress than the men and I expected, before I elicited my data, to find breakdowns in the menopausal and senile phases in the women, but, rather surprisingly, I found no evidence of this.

Loudon: The crucial element which needs to be looked at here is the relationship between formal religious organization, ethnic background and the strength of kin ties and kinship obligations. Dr. Murphy, you suggested one kind of expulsion from the community, that is, expulsion to a mental hospital; but it seems that there may be other kinds of expulsion which you hinted at when you said that chronic psychotics among the Protestant British may either migrate or die. Is it possible that expulsion to a mental hospital, which was so striking among the French Canadians in comparison with the other sections of the population, is a different kind of expulsion from migration or death because of the greater possibility of maintaining kin ties with patients in hospital through visiting?

Murphy: No, because the cases sent into hospital from the French-Canadian and Irish groups tended to be forgotten.

Loudon: How did you get your data about them?

Murphy: We traced them because we worked in every institution and through every priest in the area as well as in the communities themselves.

Loudon: What I am suggesting is that perhaps the Protestant British patients are lost; when they first become sick they are more likely than the Catholic French to escape the epidemiological net because their kin ties are of a very different order.

Murphy: I agree. I am not claiming that this picture necessarily means that there is a higher incidence of expulsion amongst the Protestants than the Catholics. I merely mentioned the matter as one of the possibilities which we have to take into account, and to counterbalance the premature inference from our data that the Protestants are much more healthy than the Catholics.

Loudon: Our own work in South Wales has made it very clear to us that one of the most difficult problems in this sort of study is the migration of early cases, which probably depends on the structure of the subcultures concerned.

Shepherd: Dr. Murphy, could you clarify one of your remarks for me? You said that even if our methods for psychiatric diagnosis were as accurate as our methods for diagnosing, say, syphilis we might still be no further forward because there is no rational therapy for most psychiatric diseases.

Murphy: I had two reasons for making this statement. Classical epidemiology serves two main purposes: the first is to prepare the way for a therapeutic or preventive programme and then to assess its effect, and the second is to contribute to theory. In psychiatry, the first purpose is premature since we do not yet have the means of producing the type of programme whose assessment would call for accurate epidemiological data. The second purpose will also remain premature in psychiatry, even in the presence of accurate indicators of what we at present use for diagnoses, for as long as these diagnoses do not imply any clear aetiology. I view some of our terms—schizophrenia for instance—as little more precise than, say, the terms "fever" or "nephritis" in their eighteenth and nineteenth century meanings. Invention of the clinical thermometer gave a precise means of measuring fever, and the coagulation of albumin became a reasonably accurate means of assessing the presence of nephritis as it was then understood. But knowledge of the precise rates of occurrence of fever or of albuminuria in a population would not have advanced the medicine of those days until one had the means of associating this knowledge with a host of other variables. Similarly, the acquisition of knowledge of precise rates of incidence of schizophrenia by itself would not, in my opinion, advance psychiatry much. But, of course, if schizophrenia had a single prime cause, like the spirochaete, and if we knew the relation between that cause and the indicator, I agree that precise epidemiological data on simple distributions of the disease would be most valuable.

Shepherd: I still do not see why you should be so cautious. You are suggesting that the type of aetiological knowledge provided by the Wassermann reaction would, by itself, be inadequate for psychiatric research because, even with such knowledge, we would still be unable to do more than make an aetiological diagnosis; I do not agree with you about this.

I must also disagree with Professor Leighton when he says that

agreement amongst psychiatrists is modestly accurate. A study of case findings by Rosemary Brooke and Professor K. Rawnsley (unpublished) in this country recently has shown the extent of differences of opinion about fairly clear-cut psychiatric disorders. In itself, of course, this need not occasion alarm providing we try to measure the reasons for our disagreement. If we had an aetiological method of classification, as Dr. Murphy was suggesting, the sort of information that Professor Lambo has given us would enable us to ask questions of considerable clinical significance, such as why, for example, do 10 per cent of his schizophrenics show symptoms of confusion? If we had a Wassermann reaction or Koplik's spots or some other objective test on which to base our psychiatric diagnoses we would be in a stronger position to make comparative statements. I would suggest that outcome alone would be of considerable importance without the need for a therapeutic parameter.

Caudill: Dr. Murphy has shown us how much can be done in a creative way by studying variables within subcultures. These subcultural surveys may not be so important from the transcultural, epidemiological point of view but one can still learn a great deal from them.

Another example of this sort of study in small populations is the birth-order material I have from Japan (Caudill, W. [1964]. *Psychiat. neurol. Jap.*, suppl. no. 7, 35). From data on all admissions to four hospitals in Tokyo we found that eldest sons and youngest daughters were over-represented among psychotic patients. Further examination showed that this result was only true for patients coming from families engaged in small independent businesses, and was not true where the principal bread-earner of the family was working in a large organization for salary or wages. From these data we can now try to discover what it is that may be stressful for eldest sons and youngest daughters in small business families.

This sort of problem can also be approached experimentally. A psychologist colleague of mine has studied chronic female schizophrenics in St. Elizabeth's Hospital, Washington, in connexion with the relationship of birth order to psychopathology (Schooler, C., and Scarr, S. [1962]. *J. Personality*, **30**, 178). This experiment involved two small waiting rooms both leading out of a common entrance. A

female confederate sat in one room, and the patient was brought to the entrance and asked to wait; which room the patient chose to wait in was noted and, if she chose the room with the confederate in it, whether she talked to her spontaneously or responded to her conversation was also recorded. This study showed that, even after ten to fifteen years in hospital, chronic female schizophrenics born in the first half of their sibships were more affiliative than those born in the last half. A great deal can be learned from an experimental situation like this as well as from wider ranging sociological-anthropological studies.

Transcultural Psychiatry
A.V.S. Deuck & Ruth Porter

TRANSCULTURAL DIAGNOSIS OF MENTAL HEALTH BY MEANS OF PSYCHOLOGICAL TESTS

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RESEARCH on problems of mental health transculturally cannot be accomplished without some form of controlled comparison of individuals and groups. For standardized assessments of mental functioning within Western culture the psychiatrist or psychologist usually relies on a battery of several personality tests which are used to measure a number of dimensions of mental competence, intellectual capacities, and personality controls operative in cognitive functioning and emotional expression. The question arises what can one do with these helpful, if not completely satisfactory, present methods when one leaves the normative cultural base on which they were originally validated.

There is considerable controversy today among those who have concerned themselves with the study of mental health transculturally, as to whether one can presume validity for results obtained by the administration of identical psychological tests in highly divergent cultural settings. Examples can be found to bear out the feasibility of such comparisons, given due consideration for cultural differences in the content of particular items. The use of the socialization scale of the California Personality Inventory has produced comparable results in India, Italy, and Japan (Gough, 1960; Gough and Sandhu, 1964; Mizushima, DeVos and Gough, 1965). In most instances, however, one finds that insurmountable difficulties are involved in any attempt at translating individual items of objective questionnaires or structured tests for comparative

use between literate cultures. As a consequence, those who are working in the field of psychodiagnosis have learned to delimit the use of such questionnaire or objective methods very carefully in instances where individuals come from divergent cultural backgrounds.

The use of tests transculturally is even more severely circumscribed when comparisons are made across the barrier of literacy. The cultural experience necessary to respond perceptually to unfamiliar written patterns has no doubt profound influences on the general organization of perception which affects radically the validity of standardized tests with non-literate groups.

Unresolved in the minds of many is whether one can properly use even the so-called projective psychological techniques eliciting responses to relatively unstructured materials with validity on any given culture. Using a perceptual test such as the Rorschach, can conclusions be made as to the comparative quality of thinking, or the nature of ego mechanisms involved in the control of impulse or affect, of subjects residing within such divergent literate cultures as Switzerland and Japan; semi-literate cultures such as Algeria and India; or even such non-literate cultures as Truk and Alore?

Specifically in reference to transcultural comparisons of aspects of mental health, can we distinguish variabilities among individuals in these unlike culture groups that may be considered in relation to degrees of intrapsychic adjustment? Further, can we come to some considered over-all judgment concerning the relative prevalence in particular populations of such variables related to mental health as flexibility or rigidity in meeting unstructured situations; maturity or primitiveness in cognitive structure; or the relative proneness to resort to particular defence mechanisms? Finally, do these variables, usually associated, in Western culture at least, with neurotic or psychotic states, have the same diagnostic significance from one culture to the next as far as the status of mental health in any group is concerned?

In short, is it feasible, first, to attempt by means of tests any

valid assessment of prevailing modalities in factors presumed to be associated with the mental health of a particular population or culture; and secondly, can we discriminate among individuals in any particular population or culture, regardless of literacy or other such dimensions, about qualitative differences in their comparative state of mental health?

From the standpoint of scientific research this field of inquiry has obvious defects. For those aware of the difficulties of well-controlled research, the standards of work with projective psychodiagnostic instruments within Western culture, let alone the application that has been made with them cross-culturally, suffer by objective comparison with what has come to be expected in other fields of psychology (Lindesmith and Strauss, 1950; Lindzey, 1961).

Training to higher standards of research in this area is lacking. After an initial period of uncritical enthusiasm it has become almost fashionable, in present-day psychologically oriented social anthropology, for individuals who have been at one time or another identified with the study of personality in culture to turn elsewhere for scholarly security, rather than to increase their technical competence in this field. In the universities, more intensive training than now exists will be necessary and there are no indications that such an increase in training will come about in the near future.

In surveying the present status of training programmes in psychology or anthropology in the United States, a country that has been in the forefront in studies of personality in culture, I have found very little provided in the graduate schools to improve the standards and thus lead future social scientists to more definitive transcultural studies using controlled psychological methods. There is obvious need not only for the refinement or modification of present test methods but also for the development of new techniques to be administered collaboratively in large-scale testing programmes.

One would hope that the present hunting and gathering stage

of gaining relevant materials will eventually be supplanted by a planned economy of scientific effort. The evidence now relevant to a general cross-cultural definition of mental health derived from efforts at psychological test assessment is at best comparable to the poorly related fragments and clues available to the archaeologist by which he must reconstruct cultural sequences. The evidence is suggestive rather than conclusive.

In what follows I will attempt to illustrate the potential validity of transcultural diagnosis in mental health almost solely in terms of the Rorschach test. This is not only because it offers the best potential means one can envision for comparative work, but because, in my judgment, it is the only method that has yet been applied in a sufficient number of cultural settings to permit comparative assessment on a sufficiently complex number of variables. In the future I hope that other means will be found to deal more intensively or more satisfactorily with particular variables that can be considered only incompletely by use of this method. The Rorschach, whatever its limitations, has proved useful since it is one of the few tests which economically, in approximately one hour, permits inferences to be made concerning a complex interplay of intellectual and emotional factors considered central in personality functioning. Discussion of the potential validity of some tests is only possible after clarifying a number of confusions and ambiguities that occur frequently in the published literature dealing with transcultural definitions of mental health. We will present some personal opinions about the limitations and possibilities of the test.

A NON-RELATIVISTIC TRANSCULTURAL DEFINITION OF MENTAL HEALTH

Any transcultural definition of mental health is based implicitly on some theory of ideal standards of psychological functioning potentially realizable in human maturation, given facilitative socialization experiences.

UNIVERSALS IN INTELLECTUAL AND PSYCHOSEXUAL MATURATION

All psychodynamic theories today are based on the assumption that universals of psychosexual maturation are inherent in individuals rather than in culture. This assumption of universals in the sequences of maturation seems to be borne out by the little controlled evidence which has become available from perceptual and cognitive studies so far attempted in diverse cultural settings.

The delineation of the parameters of human potential for intellectual maturation has received grossly insufficient study outside Western culture. Additional work on the maturation of cognition, for example, is essential since one parameter of any universalistic definition of mental health must be some definition of optimal maturation of conceptual-cognitive abilities in the individual. These abilities allow for optimal adaptation to the environment, natural and social, as well as a capacity to modify the surroundings to the advantage of the individual and his group. There is some evidence concerning comparative maturation of cognitive processes by techniques other than those used in clinical testing which hold promise, although little more than a beginning has been made.

Some studies suggest that the same stages of maturation can make their appearance in other cultural settings at about the same physiological age period as they do in Western civilization. Price-Williams (1961) used the Piaget method of testing concepts of conservation of quantity among children of the West African Tiv tribe. He found no essential differences in the maturation of the concept of continuous and discontinuous quantities when his results were compared with those obtained by Piaget in Switzerland. Price-Williams (1961, p. 305) concludes:

> "Concerning the adjacent thesis which was incidentally put to the test, namely Levy-Bruhl's ideas of 'native' concepts of number, it is quite clear that this is an excessive over-generalization. At least by the age of eight, the kind of thinking that

> Levy-Bruhl envisaged, has tended to disappear. . . . It may of course be true that what goes under the heading of 'pre-logical mentality' may be more evident in other so-called primitive societies. If this is found to be so, then the direction of theory would be steered towards the relevant varying experiences which children undergo. As regards Tiv children, in the particular fields explored, there seems little difference to the sequence which has been found in European children."

Other evidence suggests that culture patterns can impede or retard the appearance of these seemingly physiologically determined maturational stages. In a comparative study of children by Laurendeau and Pinard (1962) in Canada, and Dubreuil and Boisclair (1960) in Canada and Martinique, there was found a retardation of approximately four years in Martinique subjects compared with French Canadians in the appearance of similar maturational stages. The stages appeared sequentially in the same order, but culturally induced impediments or the lack of a facilitative cultural atmosphere seemed to prevent the appearance of expected behaviour at the anticipated time.

More work like this in the field of cognitive development is necessary. This evidence points to a generalization that, in any indigenous cultural group of sufficient size (although one must not rule out the possible dysgenic in-breeding of mental defects or deficiency in a relatively small isolated group), one would find the biological capacities for the appearance of maturational potentials in cognitive development to be the same throughout *Homo sapiens*. The evidence also indicates that specific cultures may inhibit or impede the appearance of these maturational stage patterns in a significant number of their members.

MENTAL HEALTH AS AN IDEAL NOT A NORMATIVE CONCEPT

Some adherents to relativistic theories of human adaptation believe that mental health can be defined by recourse to the normative standards of mental functioning general to a group. They

fail to realize that implicit in, and necessary to, the psychiatric transcultural approach is some universalistic concept of optimal levels of psychological functioning constituting mental health that would remain constant regardless of cultural variation. It is necessary to stress this since it is only on this basis that one can presume to assess mental health transculturally by any controlled means. Clinical inferences derived from psychodiagnostic tests are made with reference to a physiologically determined range of possible functioning which varies from grossly deficient to optimal along several parameters of mental functioning. This point has already been made very cogently by Hallowell in his perceptive article on the use of Rorschach cross-culturally (1957, p. 72):

> ". . . For our present-day knowledge, imperfect as it may be in some respects, makes it necessary to differentiate a positive, higher, or optimum level of psychological functioning or psychodynamic adjustment . . . from a lower or less positive one. . . . mental health, or an equivalent concept, is not culture-bound but has universal significance both as a concept and a value. If this be so, then it may be possible to emerge from the chrysalis of an elementary cultural relativism."

The pertinence of a relativistic position in regard to subjective values cannot be gainsaid, however. Rating one culture over another in many instances becomes a matter of personal preference. Cultures in one sense can be viewed as patterns which selectively impede or foster various forms of imperfect maturation in their members.

From the standpoint of scientific description, one must not impose one's own moral judgment but simply record what one finds. Nevertheless, only through the assumption that mental health is based on some absolute standards of psychosexual maturation, and not on a normative distribution of personality traits, is it possible for us to measure human functioning relatively.

Using optimal standards of emotional and intellectual coping as criteria, it does become possible, in limited instances, to arrive at some objective consensus concerning the relative efficiency of some cultures in the moulding of personalities more or less capable of functioning at various levels of mental health. Although all cultures in one sense or other are imperfect compromises, some are manifestly less facilitative of cognitive or affective development in their members than others. Just as it is possible to differentiate stable, mature individuals from those who have severe problems of intrapsychic adjustment, so, too, it is possible in specific cultures to differentiate between settings that are relatively conducive to the realization of human potentials and those producing severe internal conflicts or maladjustments among many of their members.

MENTAL HEALTH RELATED TO SOCIAL MALADAPTATION AS WELL AS MALADJUSTMENT

Social psychiatry, in assessing cultural patterns, is not limited to a consideration of the relative maturation of intrapsychic processes, but it must also examine the continual interaction of individual and environment throughout the life cycle. A clear-cut definition of mental health in terms of inner adjustive considerations alone is impossible. In addition, varying socialization patterns which conduce to patterns of inner adjustment are potentially more or less capable of meeting environmental stresses faced as adults. Cultures also differ in the manner in which they provide a relatively satisfactory environment for various sex, age, and occupational segments of the population.

A further necessary differentiation, therefore, is to be made in clarifying the role of psychodiagnostic testing cross-culturally. It generally relates to intrapsychic patterns rather than to patterns of social adaptation. Social psychiatry must differentiate between maladaptation from the standpoint of social functioning within a culture, and disturbed behaviour resulting from intrapsychic

difficulties. Such difficulties (the traditional province of psychiatry) may take the form of physiological defects, rigid, maladjustive, defensive structures, or some other failure in the complete maturation of ego functions which makes ordinary social life impossible. This distinction between adaptation and adjustment is discussed by Kluckhohn (1962).

Psychodiagnosis, by both psychiatrists and psychologists in Western culture, tends to rely heavily on the fact that there seems to be a great deal of congruence between some forms of obviously maladaptive behaviour from the standpoint of society, and specific forms of maladjustment definable from the standpoint of intrapsychic processes. There are some superficial definitions of mental health that are derivative from this apparent congruency. The usual clinical psychodiagnosis is basically a differential diagnosis in which certain quantitatively defined cues are considered to reveal a propensity in the individual to manifest overtly some signs of his inner maladjustment. However, when a particular psychological test is given cross-culturally, or, for that matter, when it is used in normative studies within Western cultures, one frequently finds a large number of individuals showing signs of maladjustment on tests whose manifest behaviour does not readily distinguish them from those whose test protocols reveal more adequate forms of mental functioning. The converse can also be true. There has been sufficient experience now on the part of those who work with normative populations, such as delinquents, to require caution in any assumption that one can judge some forms of socially deviant behaviour as reflecting critical inner maladjustment.

To illustrate the complexities involved in one study using the Rorschach I refer to an unpublished study reported to me by Dr. Jean MacFarlane (1965). This was part of a large-scale longitudinal study of human development conducted at the University of California. In this research, psychological tests including the Rorschach have been given, and detailed interviews conducted

at specified times over a period of years, so that the results obtained in childhood and adolescence could be compared with the results on the same individuals, now over thirty years of age. In one study specifically dealing with Rorschach results, a quartile division was made of extreme cases dividing those with inconsistent ratings from those rated similarly on test results and behavioural evidence. There were records of cases given a prognostic rating of excellent in regard to social adaptation on the basis of behavioural and interview criteria, but at the same time given a poor rating on the basis of the Rorschach. In addition to those excellent or poor on both measures there was a group rated excellent on the Rorschach but receiving poor prognostic ratings on the basis of behavioural evidence. It was found, in examining their subsequent social and occupational patterns over a period of years, that many of the individuals who had been given excellent ratings on the Rorschach but poor ratings on the basis of their behaviour as adolescents, have in many instances made productive and creative adjustments, but in unconventional modes of adaptation. In many instances they came from families in which affective displays of both a positive and negative nature were overtly a part of family interaction. At the same time, those with good interview ratings but relatively poor Rorschach findings that demonstrated various forms of constriction, immaturity and peculiarly morbid content, tended to come from families where interpersonal patterns were maintained on a pleasant but "proper" basis. These results suggest, in these latter cases, less capacity for a creative open use of fantasy. Instead, considerable expenditure of energy was necessary to repress or inhibit any overt expression of what would be considered disruptive behaviour. It was among those cases given poor prognostic ratings on both the Rorschach and the behavioural-interview evidence that one now finds the most problems appearing in subsequent occupational and marital career patterns.

The use of psychodiagnostic tests on normative populations anywhere amply demonstrates the relatively rare appearance of

individuals who show optimal psychological balance characteristic of full mental health defined by ideal standards. Most individual personality formations are characterized by some form or other of incomplete maturation. It is a characteristic of the Rorschach to reveal the relative pressure on behaviour of unresolved unconscious processes. The limited view of an individual seen through the perspective of the Rorschach sometimes tells us more about potential malfunctioning than about effective resources in the person and his immediate social environment. Such resources as integrative beliefs, or patterns of successful dependency, that help an individual to meet any form of stressful situation may not be shown by the Rorschach.

The usual, clinical use of the Rorschach test depends on certain critical definitions of imbalances which, if exceeded, suggest some serious form of intrapsychic malfunctioning. Individuals who use the test only in the clinic, where by definition the patient's behaviour is disturbed, are sometimes unaware of the frequency of signs of malfunctioning in records of individuals who are not referred as patients. This leads me to caution the users of psychodiagnostic instruments to differentiate between weaknesses, defects, or distortions of personality structure, and the actual appearance of overt, psychiatrically designated symptoms.

In a study of schizophrenics in remission, Samuel J. Beck (1952, 1964) found that after five years of remission in former schizophrenic patients Rorschach patterns did not, for the most part, differ significantly from those obtained on the same individuals when they were brought to the hospital acutely ill. Yet many of these individuals, at the time they were retested, had made some form of social adjustment, in a sufficiently benign, non-stressful, social environment and were able to function without further psychiatric guidance.

One must recognize that individuals with neurotic or psychotic propensities may only show symptoms under conditions which have become too stressful for them to cope with, without drawing

critical attention to themselves. If it were otherwise there would be no social psychiatry; the social psychiatrist must take into account the systematic social or cultural stresses that contribute to the selective appearance of manifest patterns of intrapsychic disturbance in particular groups or cultures.

Even within our own culture it may be very difficult to recognize the potential source of stress from outside. Such difficulties in understanding situational provocations often lead to the assumption on the part of some psychiatrists that particular manifest symptoms in the neurotic or psychotic can occur without any environmental provocation whatsoever. Yet many individuals who, at some time in their lives, have manifested psychotic symptoms may never do so again. One must assume some relationship of clinical patterns to environmental conditions and not simply to organic or biological deficiencies of integrative capacities.

This lack of congruence between the personality structure revealed by the Rorschach test and the actual presence of symptoms should not lead us to dismiss as invalid what the test reveals concerning patterns of intrapsychic malfunctioning. The test does not measure the relative stress in the social environment in which an individual is functioning or his reactions to it.

What we have said so far concerns psychodiagnosis and psychiatric symptomatology within Western culture itself. These problems are made even more complicated when we attempt to assess mental health in a non-Western culture when the incongruities between a psychodiagnostic picture on the Rorschach and the social adaptation of the individual may become even greater. One finds numerous examples of cultural patterns which, in effect, provide for types of expressive behaviour which, if they occurred within our culture, would be defined as symptomatic of intrapsychic maladjustment. For example, trance behaviour, indicating loss of control, can be socially expressed while the individual remains essentially conforming and within the bounds of prescribed behaviour defined by his culture. Cultural patterns may

disguise patterns of personality rigidification which become apparent only if the requirements or modes of social adaptation are suddenly disrupted by changes in the social environment. Social change is much more apt to occur today because of the greatly increased *tempo* of communication throughout the world. Cultures whose members are unduly rigidified both intrapsychically as well as in terms of culturally prescribed behaviour may manifest numerous signs of social maladaptation.

Many cultures flexibly provide for deviant patterns for those individuals within the culture who have particular aberrant tendencies resulting from intrapsychic personality structures that make conformity impossible. Such persons can often take on some form of traditional, unconventional role, which in effect is one form of tolerated or even highly valued adaptation within the society, without incurring total ostracism. For instance, assuming the role of a holy man within Arab or Indian culture provides for the possibility of a type of withdrawal from society which receives positive sanction and support as long as the role is at least minimally played according to prescribed expectations. The degree to which certain forms of idiosyncratic religion or minor deviant social cults within American culture are peopled by individuals who are thereby attempting to socialize intrapsychic tensions is an issue worthy of further systematic exploration.†

THE VALIDITY OF RORSCHACH RESPONSES IN TRANSCULTURAL STUDIES

Even assuming that there are universals in human nature and carefully observing the distinctions to be made between social

†Dr. Hedda Bolgar reported to me (personal communication) the general presence of paranoid ideation and other suggestions of definable psychotic traits among "true believers" in a number of minor religious cults appearing in the Chicago area which drew to themselves new members from traditional Protestant religious backgrounds that no longer satisfied their particular needs.

adaptation and internal adjustment, before one can hope to use the Rorschach or any other test cross-culturally, one must make some further assumptions. These cover the feasibility of eliciting samples of responses to identical stimuli which can be interpreted with equal validity irrespective of culture. Quantitative testing of validity is very difficult with the Rorschach because the material elicited evades mechanical systems of scoring. Scoring a Rorschach properly demands an experienced interpreter. Both quantitative and qualitative comparisons are possible but only when certain explicit reservations are kept in mind.

Cross-culturally there are extreme variations in the type of responses elicited which gainsay ordinary clinical interpretations based on the usual quantitative scoring methods now in vogue. Nevertheless the Rorschach responses, as Hallowell (1957) justly points out, elicit samples of standardized behaviour that can be interpreted in the context of the culture producing it as well as in terms of an individual's own patterns of thought and perception. Once the subject's sincere co-operation in meeting the task has been gained, the forthcoming responses, whatever their limitations or peculiarities, are valid examples of his perceptual operation indirectly communicated by verbal means. For example, the manner in which responses are communicated, the particular location on the blot evoking them, and the manner in which they are elaborated can be interpreted to follow lawful forms of mental association, regardless of attitude, that may signify the pressing presence of definable pre-logical modalities in thought. One cannot assume, however, that such modalities always relate directly to identifiable types of breakdowns in social expected behaviour.

I have discussed this subject in some detail elsewhere in the context of interpreting thought patterns revealed in the Rorschach records of Algerian Arabs (DeVos and Miner, 1959). When we compare the thought patterns prevalent in one group with those in another, we may infer that we can differentiate the relative ease with which they meet a situation demanding new types of decision

making. But, as was obvious in the case of the Arabs tested—including some with what can be termed highly paranoid ideation—when an individual continued to reside within a culture pattern familiar from childhood, he could remain socially adaptive as long as he was not exposed to any situation that would unduly challenge his potentially defective, integrative capacities.

Unfortunately, in many of the studies done with the Rorschach cross-culturally, the level of competence manifested by the investigator in the use of quantitative methods is not satisfactory. Often no attention is paid to qualitative features of the record that are not scorable within the limits of present generally accepted methods.

Gardner Lindzey (1961) has made an excellent dispassionate survey of the use of projective techniques in cross-cultural research, and points out the severe limitations found in many of the published studies. He states (p.299), for example:

> "A defect of many of these studies . . . is the tendency toward *mechanical application of scoring systems and interpretative generalizations* developed in connection with the study of educated European and American subjects. To most observers, it has appeared that the users of projective techniques have been the strongest resisters to actuarial methods and psychometric procedures. If such were the case, one would expect the interpretation of projective-technique protocols in anthropological research to rely upon various complex and elusive cues, or clinical judgments, entailing the testing context, the individual subject, and a variety of other information. The typical report gives quite the contrary impression, for we find a willingness to equate a particular type of projective-technique response regularly to a particular personality attribute. Actually, if we remove from this literature all interpretative statements dependent upon Klopfer's specific generalizations, we would probably eliminate three-quarters of the results we have examined." (Author's italics.)

The point that Lindzey is stressing is that most of the applications of projective techniques cross-culturally have appeared too ready to adapt slavishly those conventional scoring practices at present used clinically for the test. There is obvious neglect of an intensive examination of the specific modalities of thought which may possibly be revealed in these records that are not captured by a quantification of the use of specific locations on the blots nor by simply scoring for form, colour, movement and shading. Another of the many difficulties in several studies noted by Lindzey is lack of concern with individual variation in particular cultures. He reports (1961, p. 300):

> "[one finds a] *tendency to take group averages and treat them as descriptive of the group as a whole.* This procedure is particularly prevalent in studies employing the Rorschach test, where many investigators have secured averages from the various Rorschach scores or ratios and then have proceeded to develop a psychogram from these group averages and to interpret this average psychogram as representative of the personality of the group. In actual fact the group may include no single member whose psychogram closely resembles the average profile. The essential error in this procedure is its failure to take into consideration the likelihood of wide differences in test performance among the members of a single culture." (Author's italics.)

This criticisms of Lindzey's is well taken. Such presentation of results obviously occurs because the individuals involved do not know that one must consider Rorschach protocols in respect to some theory of specifying possible types of total personality configuration. The simple expression of means and standard deviations becomes a mechanical listing of results in an elementalistic fashion which throws very little light on "personality" as an integrative system.

In understanding the limitations of the Rorschach test one cannot overlook the fact that, in a number of instances, the material

elicited from a particular culture group is not very revealing. Some groups are characterized by the type of constrictive ego defences which seriously limit the material available for interpretation. When one has to deal with a great deal of constriction present in the perceptual apparatus one must employ other means to gain access to what may be the dynamics behind such constriction. Such problems of interpretation do not only occur in cross-cultural studies but are a relatively frequent occurrence in psychiatric settings within our own culture where one is periodically faced with a guarded, constricted record difficult to define as to possible pathology.

In spite of such obvious limitations it is my considered judgment that the material elicited when an individual genuinely attempts to cope with the assigned task is a valid sample of behaviour related to mental functioning regardless of culture. To gain valid inferences from this elicited behaviour demands more from the interpreter than the mechanical application of a scoring system. One must not forget that any scoring system of itself is simply a mnemonic device used to summarize the complex variables found in given responses. One cannot simply reify scoring symbols and extract mechanical interpretations since it is always necessary to pass a considered judgment as to what any particular response implies, after consideration of other qualitative features found in the protocols which are not subjected to quantified scoring. The process of interpretation demands considerable experience, both with normative and clinical populations, of individuals who have exhibited some demonstrable forms of mental illness.

THE DISTINCTION BETWEEN DIAGNOSIS OF MENTAL HEALTH AND SOCIALLY ACCEPTABLE TREATMENT OR INTERVENTION

There is confusion in the minds of some that a definition of mental ill-health is necessarily related to institutionalization of the individual or some form of psychiatric intervention which serves to designate the individual as "mentally ill". This confusion can

best be illustrated by a recent paper presented at the First International Congress of Social Psychiatry (Guttentag and Denmark, 1964). The authors, in a careful analysis of Rorschach tests given to 30 institutionalized adult female southern Negroes, in-migrants to New York, and a control sample of 30 Negro women living in Nassau County and Queens County, New York, matched for age and socioeconomic status, found, in effect, no significant differences in the standard scoring between the experimental and the control groups on the Rorschach. Both groups varied considerably below what are considered minimal standards on the Rorschach, obtaining for example a very poor level of form quality in their responses. Both groups produced a great deal of sado-masochistic material similar to that reported by Golfarb in the clinical study of American Negroes conducted by Kardiner and Ovesey (1951). To illustrate, among some of the content responses mentioned by Guttentag were: "A butterfly, someone tore the wings and it's still alive" —"I see two bugs standing and looking at some kind of dead meat at their feet"—"A vagina all red, raw, acid eating it away like when you get cancer, it spreads all over." These researchers conclude:

> "Since the two groups are not significantly different from each other the institutionalized population may be assumed to be a 'normal' segment of the Southern Negro in-migrant group. . . . The clinical interpretation of the Rorschach rests on normative assumptions. In view of the findings of this study, the clinical interpretations and diagnostic labels typically made in clinical settings of groups which are not part of the standardization population, such as the Southern Negro, are thrown into serious doubt. In the case of the particular sub-culture investigated in this study, there are only two possible evaluations of the Rorschach results. It is necessary to assume either that the entire sub-culture is 'abnormal' and therefore in need of institutionalization, or that the Rorschach performance of an individual

> who comes from a racial, ethnic or sub-cultural group for which there are no norms, cannot now be evaluated, nor can any normative diagnostic label be applied to their Rorschach performance. . . . The diagnostic label of 'psychotic' naturally carries with it the connotative implications of 'potentially dangerous', 'unable to care for oneself', etc. and it implicitly sanctions the institutionalization of the Southern Negro."

These particular conclusions illustrate three of the apparent confusions concerning psychodiagnosis and a concept of mental health. There is, first, the misconception already discussed that the Rorschach, or perhaps even concepts of mental health, rest on normative assumptions. As I have indicated, regardless of the prevalence of certain responses within any cultural or sub-cultural group, if these are indicative of intrapsychic disturbance, they are so interpreted. Their frequency makes them typical or prevalent, statistically "normal" rather than "normal" in a mental health sense of the term. The prevalence of these responses, therefore, is an index of the prevalence of mental illness in the group in question. One must avoid semantic confusion in the uses of the word "normal".

Secondly, the conclusions of this study oppose any possible assessment that a serious defect could be so prevalent in a population experiencing debilitative socialization experiences. Opposition to such a conclusion stems from the misconception that the only treatment solution envisioned by psychiatrists for people showing problems of this nature is some form of institutionalization. The authors suggest: "Alternative solutions [to unthinking institutionalization] might include job training, non-psychiatric housing and out-patient social services. These could substitute for society's present solution of institutionalization, and at the same time result in more constructive changes in the life of the individual Southern Negro in-migrant."

The authors' misconception concerning the current concepts of treatment in social psychiatry is readily apparent. Recent thinking

in social psychiatry, especially in England, has gone far forward toward the realization that the creation of a therapeutic environment for individuals with severe personality instabilities or deficiencies need not involve some form of institutionalization. Working out satisfactory modes of protected adaptation within the total community may be feasible for a good proportion of the mentally ill. Such forms of environmental manipulation do not, however, remove mental abnormalities. In effect, these authors are arguing that if some form of illness or mental incapacity becomes normative to a population, it can no longer be diagnosed nor considered a mental health problem.

The third confusion present is somewhat less apparent but illustrates the problem of an overlap between social definitions of maladaptation and definitions of severe maladjustment based solely on an assessment of structural characteristics of individual personality. As we have indicated, the limitations of tests of intrapsychic organization should be explicity recognized; they should not pretend to be direct measures of manifest social behaviour in every given environment. Nevertheless, these data from Rorschach tests, if properly interpreted, provide supportive evidence for why the particular Negro population examined would potentially manifest a very high rate of psychiatric disturbance, given an experience of urban in-migration. The fact that not everyone in the particular sample gave similar overt concrete expression of difficulty does not counter-indicate the validity of the Rorschach results, which suggest some form of mental debilitation to be rather general for Negroes migrating north from rural southern Negro culture.

ACCULTURATION, MINORITY STATUS AND PROBLEMS OF MENTAL HEALTH: SOME RESEARCH EVALUATIONS BY MEANS OF THE RORSCHACH TEST

It is precisely in such stressful situations as social mobility or social change that assessments of intrapsychic adjustment are

helpful in assessing mental health problems. One may accept the proposition that a group showing serious potential problems on measures of adjustment may not reveal any overt maladaptation as long as the individual remains within his own traditionally stable social group. Problems in inner adjustments are obviously more apt to reveal themselves under conditions of unusual environmental stress.

Some form of social stress may occur in situations where an individual must change his pattern of life in the face of changing demands placed upon him by an increased level of occupational specialization necessary when his entire culture is being affected by social and technological change. Well-habituated behavioural patterns are no longer adaptive in such circumstances. The individual may have to adapt himself to a more complex technology which demands of him capacities to integrate and comprehend new forms of learning or new modes of interpersonal relationships not found in his traditional culture.

The geographically or socially mobile individual who leaves his own society to enter into a host culture faces even more extreme difficulties, for not only does he face a change in cultural patterns but he finds himself in a new status as a member of an ethnic or cultural minority within a new host culture. Some specific results obtained with the Rorschach test in a study of acculturation situations affecting Japanese Americans and Algerian Arabs, previously reported by the author, may be cited to indicate how the Rorschach may serve to throw light on the issues involved.

I began a study in 1947 of two generations of fellow Americans of Japanese origin, the immigrant generation and their American-born children. I obtained a normative sample from the population in the Chicago area shortly after their relocation from internment camps. They had been displaced from their West Coast homes on the basis of supposed exigencies of security during World War II. The results obtained by quantitative comparisons demonstrated dramatically that the Japanese who had gone through an

American school experience in grade school and high school produced perceptive patterns remarkably similar to a normative control group of Americans of various ancestry (DeVos, 1954). The immigrant population produced patterns which we later found to be highly similar to those found in a normative sample of close to 800 Rorschach protocols obtained from three villages and two urban centres in Japan (Murakami, 1962). In addition to those American-born Japanese who had been totally reared in the United States and gone through the American public school system, I obtained an additional sample of American-born Japanese who had been sent back to Japan for some period of time, during childhood in most instances, to obtain some Japanese education. The resulting protocols in this group were highly varied, in general showing considerably more signs of maladjustment than those who had not been put in a position of cultural marginality. There was one systematic difference in the Japanese records generally when compared with both the American and the Japanese comparison samples. The Japanese American records had many more anatomical responses than the records obtained in Japan. Some of this anatomical material had a sado-masochistic flavour to it. Subsequently, in another study with Horace Miner (1959), I found systematic differences in affective symbolic material in Arabs living in a rather encapsulated environment on an oasis and individuals from the same oasis who had relocated to the casbah of Algiers. Of principal interest again was the fact that in the urbanized sample one found a type of sado-masochistic material relatively absent in the records taken from individuals in the oasis. In other structural aspects of personality the records are highly dissimilar from those of the Japanese (DeVos, 1961).

These two samples of records share at least one similarity with a sample of American Negroes obtained by Goldfarb as part of the study by Kardiner and Ovesey (1951) on the debilitating effects of minority status on personality structure. These Negro

records were characterized by the presence of considerable sado-masochistic content, similar to the recent study by Guttentag and Denmark reported above. The obvious inference that can be made on these comparative findings is that individuals finding themselves in minority status in what they may perceive as a hostile host culture may be subject to a type of chronic social stress which shows up symbolically in the content given to the Rorschach. It must be stressed that there is a vast difference in the perceptual adequacy revealed in the Japanese Rorschachs by American standards from those reported by American Negroes in studies such as that of Guttentag. The family based socialization experiences of the Japanese Americans equip them to meet the challenges of American middle-class social expectations in a far more integrated way than can the American Negro. At present we can only speculate whether the Rorschach would reveal differences among other minority groups directly related to their capacity to cope with the exigencies of acculturation.

The elaborate studies of Malzberg and Lee (1956) on the relationship of mental illness and geographical mobility point to the complexities involved in tracing out the influence of the simple factor of migration as related to the statistics of first admission to hospital for psychosis. Malzberg's statistics separate the minority non-white from the white population of the state of New York. They reveal that minority status itself, in the stable non-migrating part of the population, shows a rate of admissions for both men and women almost twice that of the white majority population. In those with less than five years residence, the incidence of schizophrenia *per se* in those under 30 is three times as great in the non-white as in the white population. The type of Rorschach records reported for Negro subjects is certainly in line with expectations of a much higher rate of obvious mental malfunctioning in this minority group.

In some unfinished research dealing with total families of Northern Athabascan Indian communities in Canada which we

are doing in collaboration with June Helm, the anthropologist, we have obtained radically different Rorschach materials from two small groups whose culture is very similar (DeVos, Helm and Carterette, 1960). One may speculate on the reasons for this. Is it due to specific familial hereditary factors, or simply to idiosyncratic modes of socialization, of a non-integrative sort as far as mental processes are concerned, taking place in families in one community but not in the other. The anthropologist could observe no apparent differences in observable child-rearing practices. The issue obviously cannot be resolved by the present data available. What we do know is that the Rorschach tests obtained in the Slavey Indian kin community, which as a group has shown some unusual economic enterprise in a co-operative saw-mill operation, also demonstrate as a total group more adequate personality patterns not far removed from what would be considered normal expectations in non-clinical testing in the United States. In contrast, the records obtained from a Dogrib settlement manifest types of associative debilitation usually found only in records of individuals of sub-normal mental capacity or with organic brain damage. These records suggest a level of functioning which would be insufficient for independent maintenance in the modern American culture. One would, therefore, question the present capacity of many of this group to meet any challenge of a more complex form of social or economic life should it be forced upon them. This type of evidence relates to the findings already mentioned concerning minority status American Negroes.

In Hallowell's research with the Ojibway in various stages of acculturation he notes significant differences in the norms of his groups (1957). In his samples, the more acculturated Lac de Flambeau people gave evidence of poorer mental health by Rorschach criteria than the less acculturated groups. The question arises whether these differences are a function of the stresses inherent in their marginal cultural position or of special factors related to socialization within the specific families of this settlement.

Our speculation would be in the direction of attributing these differences to acculturation, but we cannot be certain.

It is not within the scope of this paper to arrive at any conclusions. We simply reiterate that at the present state of our knowledge the evidence is fragmentary. We hope merely to illustrate that while psychological tests such as the Rorschach cannot answer problems that involve complex social or cultural barriers affecting mental health, they do provide a valid form of behavioural evidence that pertains to assessments of mental health.

CONCLUSIONS

The problem of assessing mental health transculturally in its broadest compass involves assumptions as to the universal nature of human maturational potentials. Judgments concerning mental health relate to ideal rather than to normative standards of maturation and must distinguish between forms of manifest social adaptation, degrees of intensity of social stress in the environment related to age and status, and relative strengths and weaknesses of intrapsychic personality structures.

Psychological tests can only with considerable difficulty be applied comparatively in different cultures. Of the methods of testing now in general practice the Rorschach, as a relatively unstructured and economically efficient technique, has been put to widest use. This test, a valid though imperfect means of obtaining perceptual responses related to a number of intrapsychic structures, has not always been adequately interpreted nor flexibly adapted to the task when applied cross-culturally. There remains considerable controversy and misunderstanding as to its merits and deficiencies.

As a means of eliciting standardized evidence of mental functioning and psychological defences or controls, the test supplies valuable evidence that cannot be readily obtained by other methods. It must, however, not be interpreted out of context,

but used in conjunction with other data about the social environment. Of itself it cannot uniformly predict the overt appearance of what is socially perceived as disturbed behaviour. These contentions concerning the usefulness but limitations of this test are illustrated by brief reference to some specific studies of acculturation or minority status in which evidence from the Rorschach test played a central role.

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DISCUSSION

Lewis: Dr. DeVos, do you really think that we can assess maladjustment independently from the social life in which the individual is embedded?

DeVos: I think that maladjustment is a concept related to the integrative pattern of the individual, that is, how and with what capacity he can respond to his environment. Individuals respond to the environment with specific mechanisms and these internal mechanisms are usually what we describe when we make a psychiatric diagnosis. We may say that a person is an obsessive-compulsive or an hysteric or that he has these or those propensities within his reactive patterns to the environment. Social adaptation, as distinct from internal psychological structuring, is the way in which an individual relates selectively to particular environments. For example, in a college environment to be obsessive-compulsive is more adaptive than to be hysteric, for when one employs obsessive-compulsive defence mechanisms one resorts to a pattern in which cognitive control and the acquisition of knowledge are very important characteristics. If the individual, as part of his obsessive-compulsive defence structure, makes continual use of abstract and intellectual concepts he can find himself doing well in a school environment where these traits are consonant with meeting intellectual requirements. On the other hand, the hysteric, who blots out parts of experience which have a possible associative relationship to his intrapsychic problems and has failures of memory related to tabooed thoughts and inhibitions of the free play of imagination, can do very poorly in school.

I would now like to give another example of the problem of

cultural change in relation to patterns of adjustment. When an individual finds himself in a situation of cultural change, his inner adjustments become crucial, whereas if he remains in his traditional culture, especially if this is stable, there is an easier adaptation to tradition. In a traditional culture the individual is living within an environment which, in a sense, is already adapted to the socialization experiences that occur within a particular culture and which tends to produce types of individuals that easily adapt to patterns they have experienced from childhood. There are less apt to be what Ruth Benedict termed cultural discontinuities in socialization (1938. *Psychiatry*, **1**, 161). The crucial point here is that, as a culture begins to change or become more complex, there are increased possibilities of alternatives, decisions and personal choice. I think that this is why people in modern cultures are so aware of individual neurosis. They have so many alternatives to choose from that they find rigidities within themselves or others which prevent the freer choice of behaviour that would permit them to do what they want. They are discontented with their own inhibitions and rigidities and begin to feel that there is something wrong within themselves because they are not as flexible as they would like to be. People become more conscious of this inner constraint in a complex culture than in a culture with few alternatives.

Rubin: I would like to discuss some of the problems of psychological testing in a primitive community by describing a programme of research carried out in Haiti by Dr. Emerson Douyon (1964. *Transcult. psychiat. Res.*, **1**, 157). Dr. Douyon is a Haitian psychologist who had worked abroad and went back to Haiti to carry out a programme of psychological testing in the rural areas. He ran into some very severe problems in the field research. He was unable to assign roles to the population but *they* decided to assign certain roles to *him*. The first of these was that of an *houngan*, the Vodun priest, because the test objects he was using were similar to the paraphernalia which these priests use. However, in the priest-client relationship, it is the priest who does the talking and not the client, and the test of the priest's ability is whether he can divine what is in the client's mind. Douyon may have been able to do this but it was not his primary interest, and this created so much confusion that he was forced to give up the whole programme. He next tried to test members of the Vodun cult in trance, and now,

for a series of complicated reasons, he was assigned the role of *loup-garou* or werewolf, which didn't help his investigations much either. Douyon finally decided that these subjects were untestable because their thinking was pre-logical and they had a completely different sequence of concepts from his. I think that the problem here was not so much the impossibility of the subjects relating to the tester, but the unsuitability of the psychological testing techniques, which could not relate to the subjects.

Caudill: I find it necessary in my own work to think about personality in adults as a separate, but open system, in interrelation with small group systems (such as the family) and also in interrelation with the broader systems of society and culture. Each of these systems can be studied profitably by itself, but there are many occasions when it is necessary to study the interrelations between several of the systems. Much of the literature on "culture and personality" addresses itself to problems of the interrelations between several systems, but the results are usually stated in broad general terms that obscure the meaningful variations occurring within the data. I feel it is possible to make orderly statements which account for most of the variation in a body of data, and still arrive at useful insights as to the patterning of interrelations among systems. This is a complex topic of method and theory, but I think we need to develop such topics in order to arrive at more sophisticated statements about the issues involved in making cross-cultural comparisons in social psychiatry.

Hallowell: I would like to defend the use of the Rorschach test in psychology. There has been criticism of this test but I think that this is partly because there is a great deal of confusion about what it actually does. Professor B. Klopfer, under whom I studied the Rorschach test a long, long time ago, repeatedly emphasized that this technique gave you an intrapsychic picture of an individual, but that to try to interpret Rorschach directly, in terms of actual behaviour, needed extreme care and could be misleading. I find it very interesting that Dr. DeVos has stressed this particular point today in a different context. The projective tests represent a special category of testing instrument and you have to know what this instrument is capable of before you can use it in a constructive way. If the nature and purpose of the instrument are not clear then of course you get confusing results.

GROUP DISCUSSION

INTRODUCTION

Murphy: Ciba Foundation symposia are traditionally intended to organize knowledge in esoteric or highly specialized branches of medicine, but I do not think that this meeting has fitted the normal pattern because we have not presented very much in the way of concrete knowledge. This is partly because there is no firm anatomy and physiology of culture comparable to the anatomy and physiology of the body, so that it is difficult for workers from different parts of the globe to communicate with each other about the same observations. It is also partly because our field is developing rapidly and has an enormous mass of "half-knowledge". By this I mean that any one of us could offer a relevant observation on almost any topic raised at this meeting, yet almost none could submit a really rounded body of fact and theory.

I think that there may be another reason for the lack of concrete substance at our meeting and this is that we have not yet come close together from our varied approaches. On the one hand, we have heard from the anthropologists that examination of almost any society with sophisticated eyes reveals clear sources of strain, or foci of pressure, which can be expected, or actually observed, to have deleterious effects on mental health. These foci need preventive psychiatric measures, but naturally the anthropologists have not been prepared to instruct the psychiatrists on how to carry out such measures. The psychiatrists in our group recognized this theme and clearly responded to it, and common ground was found in the area of epidemic hysteria. I think, however, that the psychiatrists here are mainly concerned with the universality or the transcultural nature of basic mental illness processes. They are trying to dispose of the confusion

which culture contributes to the picture of mental disorders and to dispose, also, of the problems which culture contributes to the doctor-patient relationship. For instance, Professor Lambo stressed the essential nature of schizophrenia and distinguished between pathogenic and pathoplastic factors; Dr. Yap stressed the essential nature of manic-depressive disorders; and Dr. Margetts stressed what he believes to be the essential nature of most forms of mental disorder. Even when less emphasis is placed on the assumption of the universality of one clear prime cause or one clear basic picture, there still seems to be an inclination to look to our field to clarify the picture of psychopathology rather than anything else.

It is my impression that the psychiatrists do not find it easy to visualize the problem before they see the patient, just as the anthropologists do not find it easy to become involved in problems of pathogenic and pathoplastic elements in mental disorders. There is a gap, then, between the sections of our group; each side has certain expectations about what the other side can do for it but each finds it difficult to identify with the other side's goals. Professor Leighton, with his dual background, has bridged this gap with a most constructive attempt to demonstrate both that the psychiatrist can get concrete aid from the anthropologist and that the anthropologist or social engineer (if you will permit that term) can get assistance from the psychiatrist. The anthropologist can help the psychiatrist by improving mental health through social integration, and the psychiatrist helps the anthropologist through his ability to assess how well the key individuals in a community will handle social change. Unfortunately, Professor Leighton's principal illustration of this theme was not one in which either the psychiatrist or the anthropologist played a key role, as this was played by the community itself.

Bridging the gap also have been those of us who have talked about techniques of study—Dr. Caudill, Dr. DeVos and myself—but my impression is that our efforts are thin strands of

communication across the gap rather than any real filling-in of the middle ground.

I have exaggerated what I have been saying about this gap for two reasons: the first is to remind us that our field is best envisaged as having a still largely unknown centre which is gradually achieving delimitation from shots round the margin. We can see, from the nature of our own evidence as we approach each other from different sides, that there obviously is something in the middle but it is not yet clear what this is.

My second reason for emphasizing this gap between the sections of our group is that I wanted to recall Professor Wittkower's complaint about the infrequency of joint psychiatric-anthropological studies in the literature of our subject. In his opinion, and also in my own, too many contributions to the field of transcultural psychiatry come from one side only. I think that, if possible, we should discuss this again before the end of the meeting, just as I think we should return to Dr. Shepherd's question about how we propose to distinguish transcultural psychiatry from the wider field of social psychiatry.

I would also like to mention some areas where I think we have made penetration of the middle ground. The main one, I believe, relates to the potential vulnerability of individuals undergoing rapid sociocultural change and the need to understand their strengths and weaknesses. This is a topic which impresses both the anthropologists, who view the problem from the social angle, and the psychiatrists, who see it through their patients. It is also both a transcultural and a cross-cultural problem, and one which has certain features common to all situations. It is closely tied to the question of what cultural base the individual is moving from and, if this is known, what sort of cultural situation he is moving to. Finally, it is a topic which seems to be equally relevant both to the overdeveloped and to the underdeveloped countries. Dr. Mead's comments on the Manus and on families in the United States is an obvious illustration of this.

The second area where I think we are on common ground, and which is a real central point in our work, concerns the doctor-patient relationship. Here we agree that in all cultures there are similarities in the basic processes employed, namely the concept of the alleviation of anxiety through the use of what we have loosely been calling magic and magical beliefs. We have agreed that it is necessary to contemplate this problem in relation to specific cultural situations, and we find that this is a matter of concern from both social and medical angles, since neither psychiatrists nor anthropologists can be satisfied if the relationship between doctor and patient is poor. We believe that if the general characteristics of the doctor-patient relationship could be better defined they would apply everywhere; but we also agree that cultural conditions and expectations about this relationship affect it so strongly that the problem must be separately studied in many different localities. Again, this is a topic which we have found is of equal importance in overdeveloped and underdeveloped areas.

Finally, there is a less well-defined area where we can come together, implicitly if not explicitly. This is in our common belief that the study of mental abnormality and its associations in different cultures can eventually be a potent tool for the exploration of human nature with all that this term implies. This exploration extends, as Professor Hallowell emphasized, from the biological, through the psychic, to the social, and in both directions. This belief, appropriately, was most forcefully expressed by Dr. DeVos, the psychologist, but I think that the idea was behind remarks from many sources, and especially from those of us who are concerned with academic research techniques.

The idea for our group discussion is that we should consider future lines of research. What suggestions for these have come up so far? Almost every subject we have mentioned has been described as an area which needs more research, which does not help us very much. I did not myself feel that there were particular loci where we could say: "If this were tackled it would illuminate

many other areas." Nevertheless, I have two suggestions I would like to make. Our discussions have swung to and fro between the applied and the theoretical, and between transcultural and cross-cultural psychiatry, with the emphasis, on the one hand, on what needs to be done in particular situations and, on the other, on purely theoretical knowledge. On the applied side, references were made by Dr. Mead and Professor Leighton to the need to help individuals and societies in the process of change. I would suggest first, therefore, than an important line of research which should be carried out soon is a small pilot project, with adequate time-span and with agreement on how outcome should be measured, to explore the validity of our belief in our ability to modify social processes and to achieve the results we expect, rather than finding that unforeseen results supervene because of the pressure of unenvisaged, social factors. If we are able to show that we can find enough information to predict outcome, then large-scale studies on the same line could be started.

Secondly, on the theoretical side, I would pick out Dr. Loudon's and my own suggestion that we need more situations where a large number of social variables are standard, or capable of being standardized in the epidemiological sense, so that we can explore the interrelationships between a few manageable variables and the psychiatric picture which combinations of these variables present. I think that this is the most appropriate step at the moment towards an advance on the theoretical side.

COLLABORATION BETWEEN ANTHROPOLOGISTS AND PSYCHIATRISTS

Lewis: I agree with most of what Dr. Murphy has said, although I doubt if such a wide gap exists between anthropologists and psychiatrists as he has suggested. It is true that Professor Wittkower's analysis revealed a preponderance of articles by one or other group which showed some unawareness that the two disciplines might be complementary. I think, however, that if

these papers had been rearranged according to his own system of evaluation, he would have found that the best studies, from our point of view, were those on which people had collaborated, and that such reports of joint inquiries were increasing.

I would also question the emphasis that Dr. Murphy put on the middle ground being partly made up of the doctor-patient relationship. It is not only the reciprocal relation between doctor and patient which is important here, but it is also the relationship of the patient to many other people who are fulfilling roles of great consequence to him as a sick person.

Mead: I think one of the reasons that more concrete material is not available is that our subject is so very wide. It is extremely difficult to organize sets of concrete data in a field of such tremendous width as transcultural psychiatry. So I do not think that we should be discouraged when the papers have ranged so widely. We had no means of knowing in advance what different people were going to discuss so a good many different shots in the dark did come in from the outside, but no more than one would expect from such a large field.

I disagree with Dr. Murphy when he says that we do not have an anatomy of culture and that this makes it difficult for us to communicate with each other. We have been invoking, implicitly at least, a fairly large number of reasonably well-agreed cross-cultural concepts. We have been primarily concerned not with building up a picture of either cross-cultural or transcultural culture, but with defining an area of theory and practice. There is always the problem that anthropologists on the whole act solely as representatives of research, whereas psychiatrists act as representatives both of research and of practice. Professor Leighton's work, in a sense, bridges this gap since he speaks for applied social psychiatry and anthropology, but his presentation was primarily based on research. This is a problem we cannot avoid because psychiatry is an applied discipline that has responsibility for human lives and human welfare in a way that anthropo-

logy has not; every time we try to combine the two we run into difficulties. It might have been useful to have a few educators present at a meeting such as this, if there were any with a sufficient interest in cross-cultural problems. If we try to imagine what might have happened to dynamic psychology had Freud been a teacher instead of a doctor, I think we can picture what the balance might be if we were concerned with the exploration both of mental disorder and of human learning and strength. For as long as this is not the case, the anthropologist is forced to maintain his position as an advocate of the whole of society and the whole of learning, and this, because of the practice to which he is committed, makes the psychiatrist seem more specialized than he actually is in his research, intent and understanding.

Hes: Collaboration between anthropologists and psychiatrists has certainly taken place in the past. Dr. Caudill, for instance, published a partly anthropological, partly clinical study on second generation Japanese in America (Caudill, W., and Babcok, C. G. [1958]. Personal and Cultural Factors in Treating a Nisei Man. New York: Ronald Press Company). Another type of collaboration which may be fruitful is between psycho-analysts and psychiatrists. M. B. Cohen and co-workers (1954. *Psychiatry*, **17**, 103) published an intensive case study of twelve manic-depressives. Although the conclusions from this study are not universally accepted I think this kind of collaboration, between analysts and psychiatrists and based on intensive case study, could formulate hypotheses which could then be tested on a larger scale. Similarly, results from more general surveys could be tested out by intensive case studies; this should be a reciprocal arrangement.

LINES OF APPROACH FOR FUTURE STUDIES

CONTROL OF VARIABLES

Carstairs: I would like to endorse Dr. Murphy's warning about comparative studies with too many uncontrolled variables. I

agree emphatically with him that ideally many of the social variables should be the same, and only a few different, in the two compared groups. It is only in this way that valid conclusions can be drawn from our studies. My own feelings, influenced by those of my colleague Dr. W. I. N. Kessel (1965. *Milbank mem. Fd. Quart.*, **43**, in press), are that it is extremely difficult in the present state of our knowledge to hope to learn much from a really international study; we simply find that we are investigating two very different situations with many different ingredients. I believe, however, that comparative studies of groups within roughly the same cultural setting could be very instructive. I am impressed by the fact that, when I read Professor Leighton's work, I learn more lessons from internal comparisons within Stirling County and from internal comparisons within Nigeria, than I do from the direct comparison of these two very discrepant environments.

I would like now to make some constructive suggestions for the sort of study I have in mind. Professor Leighton has told us that in Nigeria he was able to elicit the symptoms of depression, although the concept was unknown there. In India, also, there is an absence of the complaint of depression, but my colleagues in private practice there say that they see plenty of depressive cases among the evolved Indians who pay for private care. I feel that there is an opportunity in a society such as this, which is evolving the concept of depression, to study this process in these two groups of people—those who seem to appreciate the concept of depression and those who do not—which are otherwise not too unlike and share many cultural experiences.

Fortes: I do not agree with Professor Carstairs about this. I think that Dr. Murphy was right when he described (this volume, p. 318) the useful information that may be obtained from comparative trials from completely different settings—the Eskimos and the Taiwanese in this particular case (Chance, N.A. [1965]. *Amer. Anthrop.*, **67**, 372). I also agree with Professor Margetts that

transcultural research is really rather a clumsy way of doing something that could be done more accurately by the right kind of internal investigation. The study of internal structure, as Dr. Murphy has said, is very important to an anthropologist such as I, who has been brought up on the intensive method. I think we may be entertaining some illusions about the possibility of controlling variables; you have to identify a variable before you can even start to know if you are controlling it. There are many very simple examples of problems with variables in anthropology. A group of people who all speak the same language, such as the Tswana tribes, seems to be a case where one variable at least, the linguistic one, is controlled, but when you study this rather more deeply, you find that there are many minor dialect differences in this group which are highly significant. Another example of the difficulties of controlling variables is in the use of a conventional term such as "patrilineal". It is better not to use this word at all, because we now know that it may be being used to mean two different things; so here is another conceptual problem. You soon begin to realize, when you work in the transcultural field, that the idea that you can keep some variables static is an illusion. We have adopted this idea from the natural sciences but even in them it is not always true.

THE ANTHROPOLOGICAL APPROACH

Another problem seems to me to concern the fact that psychiatrists have tacitly accepted the view that they can recognize psychiatric illness anywhere; they know what psychoses and neuroses are and can always identify them. In certain areas, however, this apparently clear picture becomes confused by the unfortunate phenomenon of culture, and so the psychiatrists have brought in the anthropologists to help to dispel this cloud which obscures vision and understanding. In a way I sympathize with the psychiatrists; I think that they do know what a schizophrenic is but—and this was implicit most of all in Professor

Leighton's argument—it is an oversimplification to think that if this confusing element of culture and social organization were eliminated we would immediately have a clear, definite, quantifiable picture.

There is another side to all this. If we could understand the connexion between what we have been calling culture and social integration on the one side and psychosis on the other, then perhaps we would be able to help to improve therapy. I think that culture may itself be therapeutic. This has not been very well explored but it was implicit in what Professor Wittkower said about the sacrifice of a scapegoat. Perhaps, if we knew more about this particular aspect of the culture of a group and its connexion with the roles exercised by its members, we might see how the community itself works as a system of defences which enable its members to operate adequately in social life. If we anthropologists could then reflect back to the psychiatrists our understanding of this aspect of the problem we could, perhaps, be of use in unravelling the therapeutic implications of culture.

There was a very important conceptual problem, mentioned by Dr. DeVos, which I would rephrase like this: we accept the traditional point of view, which of course commends itself to us from our own experience, and which is summarized in the notion of the personality system as being inside a body which grows and matures. Suppose we put this aside and consider the situation we have got here at this moment. A process of linguistic communication is going on which is being recorded on a tape recorder. Anybody, anywhere, can listen to these reels being played, and understand what we are talking about; he does not have to visualize the speakers or their personalities to do this. This is a very simple example to illustrate what I am trying to say: if we abstract the action-system from the actors we have identified the meeting ground where culture meets personality. But this action-system can only be realized by actors acting. In other words, the system is both inside the actor and outside the actor,

and one of our difficulties is in visualizing these interchanging phenomena which are both inside and outside ourselves. Perhaps we might say that one of the problems of mental illness is that it represents an action-system that has got too far inside the personality, and one of the ways of curing it might be to try to get it outside again. I have mentioned this to illustrate what I mean when I say that it is not one of our problems as anthropologists to contribute to the psychiatrist's task of identifying his cases, as he well knows how to do this himself. But the anthropologist could help the psychiatrist by explaining and investigating how culture itself is therapeutic.

Lewis: We have to remember that culture may be therapeutic but nevertheless quite undesirable. Neurosis is widespread and serious and yet we know that in the German internment camps, under appalling conditions, many neuroses became very much less than they were in ordinary civil society. It seems that therapeutic effect is not the only criterion for assessing whether changes are favourable or desirable (Kral, V. A. [1951]. *Amer. J. Psychiat.*, **108**, 185).

Fortes: I was not really talking about therapeutic effectiveness at that level. There are plenty of cultures, such as that of Alor described by Cora Du Bois (1944. The People of Alor. Minneapolis, Minnesota: University of Minnesota Press), which really sounds rather like a lunatic asylum, where highly undesirable features seem to work therapeutically. I agree entirely with the neat way in which Professor Lewis has put the problem.

THE EPIDEMIOLOGICAL APPROACH

Leighton: I agree emphatically with Dr. Murphy's suggestion that one of the main functions of epidemiological work is to set up hypotheses which can then be investigated to find out what the relationships between groups and cultures really are. Correlations do not, of course, say anything about causal relationships but they do propose targets for investigation. I think that one

line for future research might be to look at all the questions which have been raised by past epidemiological studies and see what investigations could profitably be pursued from them.

It is important, however, to recognize that epidemiology has at times been able to adduce crucial evidence of a causal sort, and I suppose the most spectacular work in this line was Goldberger's investigation of pellagra (Goldberger, J., and Wheeler, G. A. [1915]. *Publ. Hlth. Rep.*, **30,** 3336); the recent work on lung cancer is another example of this. It seems to me incontrovertible that, if you can combine experimental, predictive work with epidemiological studies, you have a powerful tool for investigating critical factors in the causal complex.

Several people have objected that our knowledge has not been sufficiently concrete and specific. I suspect that if we had been presented with a number of papers containing definite results, we would probably have spent a great deal of time trying to understand what the actual figures meant and struggling with the details of methodology. I would like to suggest, therefore, that it would be profitable to have, first another conference which would be directly concerned with method and its conceptual implications and, after that, a conference focused on specific results.

Caudill: I do not agree with Professor Carstairs' remarks about the lack of value of epidemiological surveys comparing extremely different groups. I think, as Dr. Murphy does, that widely different cultures can be compared, as in the study he described of the Eskimos and the Taiwanese. It seems to me that two communities may be very different but one can still form a reasonable idea of their differences and make a useful internal analysis of the data within each group for comparison. Professor Carstairs, why do you suggest that we discard this sort of broad, transcultural study?

Carstairs: The answers to all questions like this are to be found in Professor Leighton's studies (Leighton, A. H., *et al.* [1963]. Psychiatric Disorder among the Yoruba. New York: Cornell

University Press). He has done the hard work and he has told us, in considerable detail, how he set about it and what difficulties he encountered. Because his report is so candid we can see the almost insuperable difficulties of taking verbal descriptions of a disease from one cultural context and translating them into the context of a completely different cultural setting. You are never quite sure, even after all this effort, that you are really counting the same things. Professor Leighton asks this question repeatedly in his report and he does suggest that, for individual symptoms, he is fairly confident that the observations were comparable in his two groups. When it comes to disease entities he does not even suggest that he is counting the same thing in the two situations. This means that we have only a very imperfect approximation of the amount of a given disease in two very different situations. For example, if you try to compare the number of schizophrenics in Nigeria and Stirling County I do not think that your two sets of figures would be comparable because there must be so many uncontrollable variables influencing the ascertainment of cases in these two widely different situations. The problem is to find the quickest way to answer our questions about aetiology, and I think that it is better to look at different parameters in sub-populations, as Dr. Murphy has done in the villages near the St. Lawrence, than to do these vast studies.

Caudill: I am not so much interested in finding out if there are more schizophrenics in Nigeria or Stirling County, but, assuming that these two places are comparable, my interest would be in the distribution of schizophrenia in different social classes, in males and females, and in the use of other variables of this kind.

Carstairs: I think there are altogether too many variables to make these comparisons valid.

Caudill: I do not agree. If we take Professor Leighton's Nigerian data and study them internally, even with a great number of variables we can produce some useful information. We can then do the same for Stirling County internally and produce some

more information, and I think that we can then compare our findings in these two areas. I have been doing this sort of work in Japan and the United States, as, for example, on problems of birth-order among schizophrenics, and I think that my results are meaningful although, of course, there are many problems of comparison still to be worked out.

Leighton: I feel very flattered at being trampled over in this way! Of course there are these difficulties, but they can only be solved if somebody tackles them and I would feel sorry if everybody here gave up trying. Different people with different prejudices, talents and myopias are needed for this work, and out of many approaches useful results may come. I am immensely impressed myself that, in spite of our best endeavour, we did not find anything we could define as symptom-patterns which are peculiar to the Yoruba cultural group.

If you regard a person as a black box containing a certain sort of behaviour, including verbal behaviour, it is a striking and I think not an inconsequential finding that symptoms in Nigeria are very similar to symptoms in Stirling County. We seem to have accepted, at this meeting, that psychiatric disorder is much the same all over the world, but I think that it is presuming too much to say this—we really do not know at the moment. It was not so long ago that people were asserting the impossibility of doing *any* work in other cultures because of the great differences between them; I think the pendulum has now swung the other way, which is equally dangerous. More work is certainly needed to map out the areas that different cultures have in common, but the interesting possibility remains that we shall also uncover *differences* that are both valid and significant.

DeVos: It sounds as if we are concluding that we have not accomplished anything in this field over the years but I think we have accomplished a great deal. Forty years ago there was still controversy about such things as how primitive peoples think, and it was concluded that there were essential differences in the

thinking processes of natives and Western people (Levy-Brühl [1926]. How Natives Think. London: Allen and Unwin). Although it was never stated explicitly in the literature, it was generally implied that these differences were biological. We are finding, on the contrary, that not only does an individual in one culture have the same human potentials as an individual in another, but he also has the doubtful privilege of succumbing to the same problems; problems in both intrapsychic adjustment and social adaptation seem to be universal. There remain, however, certain cultural differences. These introduce differences in the incidence of particular symptomatology and psychiatric syndromes. I do not think that we can dismiss altogether the idea that there are differences in disease which are related to cultural experience. One of our tasks is to define and to delimit these differences.

Leighton: In our North American studies (Hughes, C.C., *et al.* [1960]. People of Cove and Woodlot. New York: Basic Books; Leighton, A.H. *et al.* [1963]. The Character of Danger, vol. 3. New York: Basic Books) we found, fairly consistently, that women had more symptoms than men, but in the rural villages in Nigeria symptoms were about equal in the two sexes and, in some of the better integrated villages, the women actually had better mental health than the men. This narrows the field a little, but it also raises a number of new questions. One of several theoretical possibilities is that of a biologically determined, constitutional difference between men and women with regard to predisposition to psychiatric symptoms. The findings in Nigeria cast considerable doubt on this biological interpretation and point more toward the importance of the stressful role. In our Nigerian sample, where the villages were most traditional and well-established the women had the least number of psychiatric symptoms, and where the villages were most shaken up by transition they had the most. We returned to our Stirling County data with the benefit of this observation and looked at the picture in our most traditional community,

a French-speaking area in which the women were given a very different role from the men—they were very secure and clearly assigned to the position of mother, child-rearer and so on. This community, although in a completely different culture, approximates to the stability of the Yoruba village. We analysed the male/female ratio for mental ill-health and found that in this one community the women had better mental health than the men. This does not prove anything but it suggests a different focus—that of role—for future studies.

Lewis: Could you say some more about what you called the other side of the ledger, the social features of your studies? You have emphasized the force of disintegration in cultural change. Could you tell us if, for example, this force is different in the Yoruba and in Lapland?

Leighton: The only places where we have attempted to make this kind of comparison so far have been Stirling County and Nigeria because the populations in these two areas are sufficiently similar to make comparisons reasonably valid. It was possible to take a concept like disintegration, translate it into corresponding concrete criteria for observation and still feel that we were dealing with the same general process. For instance, if you are working with broken families, you have to decide, first, what a family is, and, second, how such families differ in Nigeria and America and what differences there must be in the criteria for "broken". Leadership must be defined differently when it is tied up with kinship and when it is not and so on. But even when these approximately soluble problems have been dealt with you are still left with difficulties over direct comparisons.

DeVos: Another dimension in this field is the behaviour of a group in its own traditional environment and culture compared to its behaviour when it is transplanted to other cultural settings. Professor Hallowell (Hallowell, A. I. [1957]. Culture and Experience. Philadelphia: University of Pennsylvania Press) has studied this with the Ojibwa, examining them in three stages

of acculturation. Dr. Caudill and I (Caudill, W., and DeVos, G. [1961]. *In* Social Structure and Personality: A Casebook, p. 391, ed. Cohen, Y. A. New York: Holt) have compared Japanese Americans in the United States with Japanese in Japan. Among other things I have studied the use of content symbolism (DeVos, G. [1961]. *In* Studying Personality Cross Culturally, p. 599, ed. Kaplan, B. Evanston: Row Peterson) on Rorschach testing and I have found systematic differences between people in an acculturative situation and those who are living within the traditional culture. These findings have direct implications for problems of mental health. It has also been found that the rates of breakdown in New York City, in Negroes and Whites, relate to the time and age of migration (Malzburg, B., and Lee, E. S. [1956]. Migration and Mental Disease. New York: Social Science Research Council). The average number of hospitalized cases is three times as high in migrants as in those who were born in the specific area, regardless of colour and other cultural features. I conclude, therefore, that there are a number of variables which relate to migration and acculturation and that their relationship to the incidence of mental illness between populations coming from the same cultural origin can be assessed.

Wolfenstein: I think a developmental approach in this field might be rewarding. We have talked about schizophrenia as a disease of adults, and Professor Fortes has discussed not being able to work as a criterion of madness. I think it would be interesting to examine, in different cultures, the age at which this malfunctioning is recognized by the community. In our own culture we often find that children are not brought for treatment until they start school. At this time something has to be done about them because they are starting to work, whereas until then a wide range of idiosyncrasy is tolerated. The nosology of child psychiatry is too muddled at the present time for us to count child schizophrenics, even if this seemed desirable, but I think it would be interesting to find out at which point in the age scale

different cultures regard children as malfunctioning and this might also be highly relevant to preventive psychiatry.

THE PSYCHIATRIST'S ROLE: PATIENTS AND NON-PATIENTS

Margetts: It seems to me that there has been a certain amount of confusion about "patients" and "people" during our discussions and I think that we should try to clarify this. Psychiatrists, I suppose, are inclined to view the whole population as patients and I think that perhaps we overreach our specialty authority in doing this. We tend to annex some areas that would be better explained phenomenologically by the anthropologists rather than phenomenologically and biologically by the psychiatrists.

Lewis: Could you give an example of the trouble that you think is brought about in this way?

Margetts: I think what happens is that the field of psychiatry is so much enlarged that we have difficulty in seeing the wood for the trees. Maybe psychiatrists want to treat everybody. Maybe everybody deep down inside wants to be treated. The more "cases" we seek, the more we will find.

Shepherd: I think Professor Margetts is taking us back to the old distinction between psychiatric illness and personality disorder. It was, and in some quarters still is, assumed that illness must imply some form of structural or functional cerebral disorder. This would open up the possibility of a physical distinction between people and patients. Professor Margetts, is this what you meant?

Margetts: Not exactly, because I think that we must clearly differentiate functional, biological and dyshomoeostatic syndromes from organic illness; that is the difference that I meant.

Rubin: I don't understand; are you saying that patients are not people?

Margetts: No. I am saying that we sometimes think that all people are potential patients.

Loudon: Is Professor Margetts trying to differentiate between what some of us might call individuals and persons?

Margetts: I am talking about something that has developed in the last few decades, along with what we call social psychiatry, the treatment of society rather than the individual. There is today a tendency to regard social forces as causative. It is my opinion that social forces may contribute to mental disorder, as stresses, but they are not direct causes. Some of us have become more preoccupied with total societies than with individual patients.

Mead: I think we could link this with what Professor Fortes was saying about mentally ill people being those who had gone too far into themselves, whereas delinquents and criminals are people who have moved too far out and away from the normal values of their culture; for almost opposite reasons these two groups have difficulty in communicating with their own kind. It is possible that there is one group of practitioners whose patients are the people who get into individual difficulties and another group which deals with people who get into social difficulties. But once you accept the position that every human being has both sorts of vulnerabilities, the question is: should the psychiatrist pay any attention to them until they have selected themselves as patients, or should he also take responsibility for the conditions which make them patients? I think this formulates one of the problems about people and patients.

Psychiatry seems to have two possible applications: the first is its extension to our common humanity and vulnerability, and to an undoubted junction between organic and functional diseases of every sort; the second concerns the feeling that psychiatry is a lively, applied, behavioural science that is good for anybody, anywhere.

Yap: Professor Margetts discussed the possibility that psychiatrists may have taken on too much and should leave the understanding of the individual person to the anthropologists, who perhaps are more suited to this job than we are. He mentioned

the term phenomenology, and I assume that by this he means psychological understanding. I think that it is absolutely necessary for psychiatrists to attempt the psychological understanding of their patients right from the beginning, in order to help them with the fundamental problem of conceiving what particular kind of mental disorder is present. In affective disorders this is relatively easy because we can understand directly the patient's reactions since his abnormality is only one of degree—we all experience the emotions of sadness and euphoria and can, therefore, formulate a psychologically meaningful concept of his illness. Because of our direct understanding of the affective disorders we can, in cross-cultural studies, appreciate more readily the variables that influence the particular manifestations of the illness that we are studying.

Leighton: The psychiatrist, partly because he allows himself and is forced by others to assume the role of omnipotence, is in constant danger of viewing everybody as patients and thinking that everybody could be helped by psychiatric treatment. This, of course, is a common view, but it is a mistake. On the other hand, I cannot visualize as desirable the idea that the psychiatrist restricts his attention to people who are psychiatrically ill, whether they are defined as patients or not, because this is like the Zen Buddhist advice to practise clapping hands with only one hand. We cannot understand the pathological without also knowing the mentally healthy. Our notions of what is normal are too much based on the dubious assumption that we ourselves, and most of our friends, are normal. One of the great needs of psychiatry is to develop a more realistic idea of what normal or "well-functioning" is in different cultures. We should study how non-patients behave, and what kinds of personalities they have, in the various cultural settings. I think that, although the psychiatrist should be very modest about telling people how they ought to behave outside the narrow confines of his clinical capacity (and probably inside it too) he should also be avid for knowledge

and understanding of how and why people who are not patients manage themselves socially and psychologically.

Lewis: This is for his own needs rather than society's.

Leighton: Yes.

Margetts: I hope Professor Leighton did not misinterpret what I said, because I have always emphasized that one has to understand a person in the context of his culture before one can deal with him as a patient.

Leighton: I was not opposing what you said.

Margetts: However, I still think that a person's or a patient's behaviour will eventually be explicable on a biological, biophysical or biochemical basis. This is the burden of most of my remarks.

Wittkower: My own view of the role of psychiatrists is that they do not take on enough. My reasons for this provocative statement are these: psychiatrists and anthropologists have in common an interest in human subjective experiences and in human behaviour. The psychiatrist concentrates on these qualities in human beings who are mentally disturbed, but he cannot understand patients without understanding persons and so he must enlarge his field of study to include the so-called normal individual. The level of analysis that we are discussing here is a bi-disciplinary approach, involving the individual's interpersonal relationships and his relationship to his culture. There are, however, many other levels at which these problems can be analysed. Genetics and biology have been mentioned here and to understand mental illness one should encompass many other disciplines as well as these.

GENETICS AND TRANSCULTURAL PSYCHIATRY

Margetts: There is some confusion in this area because a psychiatric geneticist is called, by the psychiatrists, a geneticist and, by the geneticists, a psychiatrist.

Lewis: Professor Penrose is a striking example of the successful combination of psychiatrist and geneticist.

Margetts: —and Franz Kallmann.

Loudon: I think that one of the difficulties today which has led to some confusion is that we are swinging from subject to subject all the time without stopping to define our terms and concepts. When Professor Fortes said that the anthropologist is expected to come along and get rid of the cultural elements—to pare the thing down—I found myself wondering whether, in Professor Margetts' terms, if we eliminate the sociocultural clothing Professor Fortes really expects to find underneath any of the things we have been talking about. Without his fine clothes, perhaps, the Emperor is not only naked but has no body at all; in fact, without the sociocultural clothing he has no real naked existence except in purely genetic terms.

Mead: P. Sivadon (1958. *In* Growing up in a Changing World. Papers presented at 10th Annual Meeting of World Federation for Mental Health, p. 45. London: World Federation for Mental Health) tried to determine how many generations of peculiar behaviour it takes to produce a genuine psychosis; he studied four generations. In the first generation he described a minor deviancy —a rather small, miserable man married a tall, dominant woman. There is no need to involve the psychiatrist in this situation. But if one of the children of this marriage is also male and miserable, and again marries a tall, dominant woman, you have a little trouble. When in time you accumulate a series of reactions involving contrasts in somatotypes and confusion of roles in society, you may in the end produce an exceedingly disturbed personality who looks as if his disturbance is totally and deeply organic, although in fact, genetic, familial, cultural and emotional factors have all contributed to it.

THE SIGNIFICANCE OF SYMPTOM-THEMES IN DIFFERENT CULTURES

Lewis: This sounds like an echo or modern version of Morel's idea that degeneration will show itself in increasingly damaging form in four successive generations with, as the result, the ex-

tirpation of the affected line (Morel, B.A. [1857]. Traité des Dégénérescences Physiques, Intellectuelles et Morales de l'Espèce Humaine. Paris.)

Fortes: I think that sometimes the anthropologist can provide the psychiatrist with some straightforward information which tells him whether an expressed belief or behaviour pattern is a symptom or not. For instance, if an Ashanti has a stomach-ache and tells me that he is bewitched and is taking precautions, he is behaving in an extremely appropriate way; if he behaved in any other way he would not be normal. But if an undergraduate comes and tells me that he is bewitched, I would send him to a psychiatrist because I know that he is probably psychologically ill. In two different cultures the same idea and the same behaviour can have two quite different meanings. Perhaps we could compile a catalogue of such meanings for different communities.

Murphy: Another area where psychiatrists and anthropologists can come together concerns terminology and the meaning of symptom-terms in different cultures. I am thinking of L. Takeo Doi's discussion of *Amae* and other Japanese terms (1962. *Psychologia*, **5**, 1), and of Andrée Benoist and her group's work on the very interesting differences in connotation of the word "depression" in French Canada and English Canada (1965. *Anthropologica*, in press). There is considerable misunderstanding in this area which needs to be cleared away and Charles E. Osgood's "semantic differential" approach to medical terminology could be very useful here (1964. *Amer. Anthrop.*, **66**, 3).

Loudon: Confusion also arises when we make too rigid a distinction between verbal and non-verbal behaviour. When we started working among the Tristan people and explored their vocabulary in relation to depression, for example, we found that there was no commonly used word which expressed what we mean by feeling "low", "blue" or "miserable"; indeed I think depression is generally expressed among Tristan people by headaches.

Lewis: Did the Tristan people not know the word "unhappy"?

Loudon: Yes, but I think for them unhappiness is something different, as it relates to an obvious cause, whereas depression seems to be a feeling of undue anxiety without obvious cause.

Fortes: There are some English people who have never heard the word "depression".

Margetts: In Swahili, a depressed patient will say he is *mbaya*, which means bad.

Pfeiffer: I think it is very important for us as psychiatrists that the anthropologists should tell us much more about the value systems of different peoples, as these play a large role in our psychiatric concepts. Kurt Schneider (1950. *Nervenarzt*, **21**, 193) has described three forms of depressive ideas which he calls *die Urängste der Mensezzeit*, three basic sorts of anxiety which can be revealed by pathological depression. These are: firstly, anxiety for life (hypochondria), secondly, anxiety for property (delusions of impoverishment), and thirdly, anxiety for moral existence (feelings of guilt and sinfulness). Schneider thinks that these three anxieties are universal, but I feel that they may be related to culture and are perhaps specific for our culture in Central Europe. For instance, in the rural population in Indonesia, I have seen practically no patients with hypochondriacal ideas. It seems that to develop hypochondriasis the patient must have a special concept of health and a specific relationship with his doctor. The rural population in Indonesia is frightened by many other dangers, such as gangs and magic, but does not feel threatened by ill-health. Anxiety for property presumes a certain relationship with material possessions and, in spite of the actual danger of famine, such an attitude is unusual in the native population. The feeling of sinfulness and guilt is also infrequent and I have never heard a patient accuse himself of a specific action about which he feels guilty. When there were expressions of guilt they were vague and general for example: "a sin against the parents". There were other forms of anxiety in Indonesia such as fear that a husband

would look for other wives, anxiety about the fate of the family, about a good reputation in society and about sexuality. It would be interesting to hear comments about the main themes in the depressions in different cultures.

COMBINED PSYCHIATRIC-ANTHROPOLOGICAL TRAINING

Mead: The best collaboration between two disciplines is to contain them within one skin, even though there is some doubt about the effectiveness of the skin as a barrier. Our experience has shown that for inter-disciplinary work to be valuable either you must have double training as a psychiatrist and as an anthropologist, or you must have one and one-half training, that is, full training as an anthropologist and then exposure to the psychiatric *milieu* for the other half, or vice versa. There are people with this sort of training, but at the moment it can only be achieved by individual effort—scholarships, fellowships, grants and so on; there is no official way of doing it. If our field is to be developed fruitfully we shall need more orderly methods of training and clearer career lines for individuals who take the trouble to learn two disciplines. Professor Carstairs is an example of a psychiatrist who stepped aside for three or four years to do anthropological work and then returned to his original career personally enriched. Unfortunately there are no devices for encouraging young people to do the same sort of thing.

Fortes: I absolutely agree that there should be some official way of training in both anthropology and psychiatry. At the moment, if anyone has this dual training it is either through pure accident or because he has taken a considerable risk with his career. The only thing to do is to evolve a routine double training of this sort, so that it is no longer regarded as exceptional.

Margetts: I think we could make a start in a less ambitious way than this. If even two or three lectures on anthropology were given during the medical course it would be something. The

psychiatric conferences in Africa recommended this, but I know of no medical schools where it is done (1958. Mental Disorders and Mental Health in South Africa, C.C.T.A. Conference, p. 5. London: C.C.T.A./C.S.A. publ. no. 35; 1962. 1st Pan-African Psychiatric Conference, ed. Lambo, T. A. Ibadan: Government Printers; 1959. Seminar on Mental Health in Africa South of the Sahara, W.H.O. Conference, p. 8. Brazzaville: World Health Organization).

Lin: The need for training more research scientists in cross-cultural psychiatry—psychiatrists in social psychiatric research, social scientists in mental health or, preferably, scientists of dual training in psychiatry and social science—cannot be too strongly emphasized if this field is to develop further with vigour and increasing quality. At present the number of well-trained personnel is pitifully small. In looking round this room one will find that more than half of us have met almost annually in different parts of the world, discussing this very same topic for the last decade. How to draw good brains and new blood into this field and how to provide them with the best possible training are questions that deserve our immediate and careful consideration.

I am not sure if we are not wasting too much time on semantics. For instance, I was very confused by Dr. DeVos' differentiation between adjustment and adaptation. It is most important for us to ensure that we are "speaking the same language" in our future exchange of scientific information and ideas. In this connexion I should like to say something about psychiatric diagnosis and classification. Although I appreciate that, at this stage of development of psychiatry as a science, we may have to use symptomatology as an index of impairment of mental function as Professor Leighton did in his study of two cultures, we should not forget that the great majority of psychiatrists all over the world still use such diagnostic criteria as schizophrenia and manic-depressive psychosis in their daily work and also in their teaching to medical students and psychiatrists in training. The

research workers in this field, I think, should try to use the same technical terms, definitions and diagnostic criteria as psychiatrists in general do, while making every possible effort to create an internationally acceptable system of classification of mental disorders. Otherwise we shall not be able to communicate with these wholly clinical psychiatrists or draw on the wealth of their clinical data for our research purposes.

* * *

Lewis: We arrived at some hopeful conclusions. Professor Leighton's suggestion for a future meeting here which woul dconcentrate on methods is most valuable. The theme would be illustrated, of course, by studies that have been carried out successfully and by others that have come to grief because of the difficulties of method. It is also clear that there must be closer collaboration between the two disciplines, psychiatry and anthropology. We are agreed that some of the obstacles to progress could be removed if there were better facilities for training, a useful common language, and a career structure that would attract young people into this field. The needs are plain, and our purpose can be summed up as "Let me not to the marriage of true minds admit impediments".

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ADDENDUM

The bibliographical references to Professor Lambo's paper omitted from p. 75 are printed below:

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Numbers in bold type indicate a contribution in the form of a paper; numbers in plain type refer to contributions to the discussions.

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