Vienna Circle Institute Yearbook

B.F. McGuinness *Editor*

# Friedrich Waismann – Causality and Logical Positivism

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Springer

FRIEDRICH WAISMANN –
CAUSALITY AND LOGICAL POSITIVISM

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15

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## Institut Wiener Kreis
*Society for the Advancement of the Scientific World Conception*




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# Friedrich Waismann – Causality and Logical Positivism

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# Editorial

Friedrich Waismann (1896–1959) was one of the most gifted students and collaborators of Moritz Schlick. Accepted as a discussion partner by Wittgenstein from 1927 on, he functioned as spokesman for the latter's ideas in the Schlick Circle, until Wittgenstein's contact with this most faithful interpreter was broken off in 1935 and not renewed when exile took Waismann to Cambridge. Nonetheless, at Oxford, where he went in 1939, and eventually became Reader in Philosophy of Mathematics (changing later to Philosophy of Science), Waismann made important and independent contributions to analytic philosophy and philosophy of science (for example in relation to probability, causality and linguistic analysis). The full extent of these only became evident later when the larger (unpublished) part of his writings could be studied. His first posthumous work *The Principles of Linguistic Philosophy* (1965, 2nd edn.1997; German 1976) and his earlier *Einführung in das mathematische Denken* (1936) have recently proved of fresh interest to the scientific community. This late flowering and new understanding of Waismann's position is connected with the fact that he somewhat unfairly fell under the shadow of Wittgenstein, his mentor and predecessor.

Central to this Yearbook about a life and work familiar to few are unpublished and unknown works on causality and probability. These are commented on in the volume of a conference in Vienna which took place at the beginning of October 2010, which will also include a publication of new or previously scattered material and an overview of Waismann's life. In this regard I am grateful to Brian McGuinness who proposed this volume and served as chairman of the related conference.

The general part of this volume contains a publication of Otto Neurath's unpublished programme of an Encyclopedia of the World War in German and English which was found in Moscow archives only recently. In addition, three review essays on the history of philosophy of science, on Ernst Mach literature, and on French studies in the Vienna Circle complement this part together with several reviews and an obituary on Stephen Toulmin by his colleague and collaborator Allan Janik.

Vienna, February 2011

*Friedrich Stadler*
(University of Vienna and
Institute Vienna Circle)

Brian McGuinness

# Waismann: the Wandering Scholar

Stuart Hampshire's excellent memoir of Waismann in the *Proceedings of the British Academy* and Anthony Quinton's introduction to *Philosophical Papers* give (among other things) an affectionate portrait of an unworldly scholar alternately seeking to conform to British ways and then shunning them. The idioms and the pronunciation both slightly wrong his English itself witnessed to a profounder alienation. But it was an alienation much more seated in his character and life than that of most of the refugees that so illuminated British university life in the 30's and 40's, of whom some indeed became remarkably assimilated.

Waismann was even born a foreigner, in the Vienna of 1896, his father, a hardware manufacturer in a small way, being of Russian nationality. The boy's later schooldays fell into the period of the First World War and it is natural to suppose that this is what led to his leaving the Gymnasium and studying at home, thus avoiding also the higher fees that a foreigner had to pay. It was only after the collapse of Russia that he took his *Matura,* as an external applicant and, at the age of 21, entered the University of Vienna, where he counted as a *Nationaler* or foreigner, coming, in theory, from Odessa, his father's birthplace, and once again paying higher fees.

Waismann completed the obligatory courses of lectures in reasonable time, with some philosophy (Reininger), but mostly mathematics (Hahn among others) and physics (Thirring). (Karl Menger, a contemporary, was to comment on what a good grasp of mathematics Waismann had.) In 1922, at just this point, Schlick was called to Vienna, and struck by his teaching Waismann decided to devote himself to problems of logic and theory of knowledge. There can be no doubt that the fascination of Schlick's personality, the personal modesty and politeness, allied to an effortless clarity and self-assuredness in his judgements, which many felt and which Menger describes, had their influence on Waismann, whose later career shows that his penchant for admiration was exceptionally well developed. Curiously or significantly something similar was true of Schlick himself, who had a succession of idols—Max Planck, Einstein, Hilbert, Russell and finally Wittgenstein. In respect of the last-named Waismann was to follow in his footsteps.

Perhaps unwisely Waismann deferred the writing of his dissertation and engaged himself instead to write a major work. So he described it in 1937, but no trace of it remains, except a publisher's letter of 1925 referring to a contract for a book on Phenomenology and Space. The theme is related to parts of Schlick's *Allgemeine Erkenntnislehre,* a second edition of which appeared in that same year. But Schlick, in this edition, avoids polemic, whereas Waismann seems to have

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revelled in it. Menger describes a good talk by him in a seminar of Schlick's where he sarcastically criticized a paper on geometry by Oscar Becker, a pupil of Husserl's. Why the work from which this talk no doubt came would not serve as a dissertation is not clear, and this suggests that the underlying motive was that reluctance of Waismann's to face examinations which his friends and mentors noticed and tried unsuccessfully to overcome.

Without a doctorate Waismann was unable to obtain even a modest academic position (we shall see shortly what solution Schlick found for him). He did not come from a moneyed family like Feigl; nor could he obtain a position in commerce like Felix Kaufmann, nor fall back on religious teaching in the Jewish community like Josef Schächter.

He was thus preparing for himself the life that he lived for 12 or 15 years, that of the penniless scholar, dependent on private tuition or part-time jobs, but all the more proud of his scientific work for that. Intellectual rigour and theoretical truth were the chief motives of such a life. There were a number who lived it in the Vienna of his day or in central Europe generally: you would find them also in emigration in Tel-Aviv. In England something similar might be true of writers, not so much of men of ideas, and so the discussion of ideas, and specifically of philosophy had perhaps less of passion about it there.

There is no need to recount here how Schlick already an admirer of the *Tractatus,* came to know Wittgenstein and introduced him to the *Tafelrunde,* as even Wittgenstein called it, meaning of course not the Schlick circle as a whole but a smaller group consisting of Carnap, Feigl, Feigl's future wife, known as *die Kasperle,* and Waismann. It was in this group that Wittgenstein dictated to Schlick his letter to Ramsey about identity (Carnap typed it and Waismann kept a carbon copy): to them too, rather than talking about philosophy, he preferred to read from Rabindranath Tagore. Wittgenstein typically had a different relation with each member of this little circle. He soon fell out with Carnap, probably not solely because of the latter's taking parapsychology seriously (after all Schlick did the same). Wittgenstein showed perhaps too much interest in *die Kasperle,* and in time Feigl too dropped out, though not before he and Waismann had induced Wittgenstein to attend a lecture or lectures by Brouwer. Waismann remained the faithful discussion partner and amanuensis for both Schlick and Wittgenstein.

This relationship was to dominate Waismann's life for at least ten years. He made his own every position of the master's and would defend even the indefensible (it was thought) at meetings of the Schlick Circle. Private pupils would even hear Wittgenstein's tones in his voice. He wrote for circulation a summary of the philosophy of the *Tractatus* (as modified prior to 1931) called *Thesen* and he gave a whole series of expositions of Wittgenstein's philosophy at meetings of the Circle. (It was the predominant theme of their discussions between 1929 and 1931, a fact more evident in minute books than in later memoirs by the participants.)

Waismann had intended to present his account of Wittgenstein's philosophy in book form as the first volume of *Schriften zur wissenschaftlichen Weltauffassung* due to appear as early as 1929 with the title *Logik, Sprache, Philosophie.* Schlick indeed wrote a preface for it, now published in the Reclam edition of a later version of the book. It had indeed many versions, for Waismann worked on it throughout the first half of the thirties, at first on the basis of notes and conversations, then in collaboration with Wittgenstein, and then again on his own with complete discretion as to the use of material supplied by Wittgenstein. Family members recall that when Wittgenstein was in Vienna, Waismann was lost to his family. On one occasion, to be sure, Schlick got the impression that Wittgenstein had changed his mind and now meant to write the book himself, but in general down to 1937 the plan remained, as far as Waismann knew, that the task of presenting to the world this body of thought would be Waismann's, though once or twice a practically complete version was withdrawn for correction or amplification by Wittgenstein.

In the first half of the 30's Waismann wrote a number of articles, reprinted in *Philosophical Papers,* and composed his *Einführung in das mathematische Denken.* In all of these the influence of Wittgenstein is strong: nonetheless they are substantial works and it is once again puzzling that they were not presented for a doctorate until after Schlick's death.

Waismann was also extremely active didactically during these years; conducting Schlick's pro-seminar entirely on his own to the general satisfaction. His formal position was that of librarian, a very poorly paid post, so that he was obliged to give private lessons to earn a living. He himself spoke of giving assistance to foreign scholar and students attracted to Vienna by Schlick. We know also of tuition given to a nephew of Wittgenstein's, and a private seminar organized by the wife of a wealthy businessman.

In 1935 there came a threat to this position as librarian—it was a fictitious employment and Waismann had held already held it for longer than the period allowed. In 1936 the ministry excluded him for any form of employment in the faculty. How far this was due to bureaucratic impatience with a flagrant irregularity, and how far to ideological hostility to the Vienna Circle and its rejection of metaphysics is hard to say. We know that the authoritarian government had closed down the Ernst Mach Society: on the other hand Waismann when he came to England mentioned only formal grounds for the termination of his employment.

Schlick, concerned for Waismann and affronted for himself, protested that the limitations on how he might use this librarian's position were unacceptable. But a far worse blow was to follow, with the assassination of Schlick by a deluded former student on 22 June 1936. The depth of Waismann's feelings is apparent in his moving preface to the collected papers of Schlick, edited by him in the following year. During that year he conducted his pro-seminar as before and assisted the other professors in guiding the orphaned pupils of Schlick.

It was clear however that this could not long continue. His position in the faculty was gone in any case and the demand for private teaching would hardly continue in the absence of Schlick. Public comment on the death of Schlick had shown in any case that public feeling was not on the side of his style of philosophy.

The year did however see Waismann finally promoted Doctor of Philosophy on the basis of the works already mentioned. Robert Reininger managed in six months what Schlick had not contrived to bring about in many years.

It now fell to Waismann to complete and publish his work as the only testimony now possible to "our philosophy", as he called it, that of Schlick, Wittgenstein and himself. He judged, rightly, that Wittgenstein himself would never publish and only overlooked the possibility of posthumous publication by others. (It is ironical to reflect that those editors are the successors of Waismann.) The German text was given to Springer and then, for political reasons, to a Dutch publishing house. In the end proofs and manuscript were lost in the war. Carnap advised him to get it set up in type straightaway. (Carnap feared, though he did not say as much to Waismann, a further intervention by Wittgenstcin. It is perhaps also significant that he thought the contribution of the rest of the Circle other than Schlick and Wittgenstein - understated in the book.)

By this time (mid-1937) Waismann clearly had to look abroad for employment and in the autumn he went to Cambridge to give talks for a term on the theory that this would serve as a launching pad, enabling him with good luck to find a post somewhere outside Austria (or of course Germany). The invitation in 1937 was perhaps indirectly connected with the fact that Wittgenstein had gone away. The little money that he had received for his university lectures until 1936 was now freed and the application of it to refugees seemed a natural one. Karl Popper in fact was the first intended recipient, but he was offered a post in New Zealand—the launching pad was not needed for him. Various well wishers, including Hayek suggested the diversion to Waismann. The small university contribution was supplemented, as in so many cases, by the Society for the Protection of Science and Learning.

Waismann made his bow in Cambridge (Wittgenstein was not there) to a mixed reception, since his thoughts (being much based on Wittgenstein's) seemed familiar and his English was not as fluent as it later became (though always with engaging idiolexemes). The manuscripts of his lectures are even touching: they contain corrections of the English in G. E. Moore's hand and (only approximate) indications of English pronunciation in Waismann's. Here as elsewhere we see that in Waismann's reception in England there was much not to complain about.
The worsening situation in Austria made him want to remain, and prolongation for a term was approved. With the annexation of Austria during that term, Waismann became in truth a refugee. His mind was bent on remaining where he was and bringing over his wife and child. Again with help from the SPSL they eventually came, under the condition (it was of a type necessary at the time) that his wife would help Mrs Braithwaite with an expected child. With all good will it was not a situation to which she, or Mrs Braithwaite, were well-suited.

Shortly after Waismann's arrival in Cambridge Wittgenstein returned. He had been in England from 1929 to 1936, but not, except in an extended sense, as a refugee: if anything he was fleeing himself and his family. Then at the end of his

Fellowship, his seven years up, as it were, he went, like some Flying Dutchman, to Norway and had, for all we know, no intention of coming back to England at all. His return at this point seems to have been motivated by the need to make some disposition as to his papers. His thoughts alternated between placing them in Trinity Library for future generations or alternatively digesting them into a book and published it. He had indeed at the end of 1936 and 1937 respectively dictated drafts of the first two parts—separated by later editorial policy. Then, as he had in fact done after the completion of the *Tractatus*, Wittgenstein meant to take up some other occupation.

With the annexation of Austria his liberty of action was considerably abridged: he too had become perforce a refugee, though he passionately rejected the name. He had been visiting Ireland and thought of staying there, but it became clear that in reality he had best hopes for employment (academic employment at that) and citizenship in England. Even the publication of his book became now advisable rather than a mere option. The idea of returning to Austria and suffering with his family only engaged him briefly: he could help them better from outside. (In fact they in the end escaped relatively intact, though this was in advance far from evident.) Previously, when lectureships had been advertised in Cambridge, he had not applied. Now he offered his services gratis, thus releasing for Waismann—and this is a point crucial for judging his attitude—the few funds available. In the event, funds were found for both of them—for the academic year 1938-9 only. In the Easter Term of 1938 Wittgenstein gave a small class, unpaid.

He met with Waismann already in the Lent Term of that year and even showed him some of his work, but both men were desperate and preoccupied, and the relation did not flourish. In the past they had worked together, Wittgenstein had found tutorial work for Waismann, Waismann had checked the safe custody of Wittgenstein's manuscripts in Vienna, but now, "The man became fat", said Wittgenstein in his dismissive way, no doubt with a grain of truth, for Waismann at this juncture did have too high expectations—for want perhaps of anything else. (The SPSL had many similar cases.) Braithwaite, who saw the egoism and lack of sense of reality, tried to persuade Waismann to go to Oxford, where his views would be new, but Waismann insisted on the possibilities at Cambridge—some lecturer might be appointed to succeed Moore and Waismann could apply for the post thus left vacant, and so on. Actually the professorship went to Wittgenstein himself (no one foresaw this before the beginning of 1939) and so no lectureship was vacated, even supposing Waismann might have obtained one.

Waismann thought his relative lack of success at Cambridge was due to Wittgenstein's influence—that Wittgenstein had forbidden his pupils to go to Waismann, and had cut him in the street: Braithwaite thought there was some paranoia here. In the relevant period Wittgenstein was in no position to help Waismann—both of them were at risk of drowning together and as far as friends or foundations in England were concerned Wittgenstein was plying them with requests for his old Professor in Berlin and for other cases that his family or connexions brought him.

He was also constantly travelling in the campaign to save his family. Of an actual disservice to Waismann or any influence on appointing bodies there can have been no question, for Wittgenstein was not on any of them.

It is natural to suppose that the issue of the publication of Waismann's *summa* created a difficult atmosphere between the two men. For many reasons, including the necessity to establish his name in the English-speaking world, Waismann wished to publish this in translation and C. K. Ogden accepted it for his series (the very series in which the *Tractatus* had appeared). In these years 1937-9 Waismann perhaps too much cried up the value of this book (now called *Principles of Linguistic Philosophy*—the new edition is to be preferred) perhaps not so much from vanity as from a need to present himself in the most favourable light in letters applying for posts or for help towards obtaining one. In the preface, however, which escaped the book's posthumous editors, he speaks very explicitly of the book's dependence on ideas and material that Wittgenstein had put at his disposal.

This of course Wittgenstein was not to know. The book and its translation was being discussed in Cambridge at the time and perhaps helped to provoke Wittgenstein's bitter reference in his preface to mangled accounts of his ideas that were in circulation. (This remark was written in autumn 1938.) Waismann several times told Ogden that the book was nearly ready but in the middle of the War he abandoned it: he had long been complaining about the inadequacy of the translation and Neurath for one supposed this to be the reason. Wittgenstein's easily inferred hostility and a growing aversion from some (though some only) of Wittgenstein's ideas may also have played a role. At all events its publication was posthumous and, though some of the corrections to the galleys (a stage it reached in 1939) are considerably later, there is no indication that Waismann himself seriously contemplated reviving the book.

Waismann met the general Cambridge situation with some petulance—lie would lecture only to mathematicians (this is strangely misrepresented by Turing's biographer) or he would not lecture at all (which made things difficult for his sponsors). In the end he decided to accept an offer from Oxford, and he went there as the war broke out. It was risky for an enemy alien to travel in those very days, but for once Waismann's lack of a sense of reality stood him in good stead. At exactly the same time Wittgenstein took up the chair at Cambridge, a post lie probably would not have sought but for the political situation.

The war was a time of deep unhappiness for Wittgenstein—his family at risk, the death of his friend Skinner, his total rejection of the British wartime attitude. There was a breakthrough only in January 1944 when he went to Swansea (the Welsh were more tolerable than the English!) and again began writing. Waismann fared no better—his lectures were a success at Oxford, but he thought his colleagues fell below his intellectual standards and wrote bitter little epigrams about them. Yet he was handled with the utmost consideration (at one time colleagues there—probably in fact just Henry Price—arranged for him to be supported for a while by private subscription, given anonymously though the SPSL). What Wittgenstein said of himself at the beginning of the First World War was perhaps true of both these very different characters, “I feel profoundly German”. The seriousness of England, the point of it, escaped them, though to be sure there were some excuses for this. In Waismann’s case there was even internment.

One difference between the two men was, as we have seen, on the issue of the parentage of ideas: Wittgenstein all the more fiercely defensive of his priority because he was reluctant to publish, Waismann more concerned to publish to the world an ideal philosophy which he never considered his own. The one was interested only in originality, the other only in the truth: philosophy is a subject that favours just such a tension, a subject where the Kuhnian model functions. Yet in another respect the two men were alike—in passionate affirmation of intellectual standards (even if these differed). Wittgenstein never thought of accommodating or reserving his judgements on such matters to allow for external factors. Nor did Waismann—the present writer has seen him welcome an eminent Polish logician to Oxford by castigating the errors of the Polish school. The two *were* Germans, after all, and we should form our expectations of them in that light: there is indeed perhaps something to be learnt from it. (That they were also Austrians, of different kinds, does not detract from this fact.).

Waismann had considerable success after the war, developing themes like the open texture of concepts, language strata, alternative logics, where he (as Ryle said of himself) had “learned much from Wittgenstein”—but could develop it independently. It is heartening to think of Herbert Hart bicycling out to Keble to hear lectures that have left their mark on the philosophy of law, and there were other examples. But Ryle, Berlin, Hampshire—Waismann’s later literary executors—when they came back from the war had little success in overcoming his sense of isolation from institutional Oxford. There was a positive side to this, since his need for human contact led him to befriend individuals young or old who also stood a little outside it. But against many—Carnap, Polish logicians, above all Wittgenstein—a resentment remained: as the day closed he must gird himself to carry on alone the battle for “our philosophy”. In a notable meeting of an Oxford essay society (the Socratic Club) in 1947 he renounced positivism and developed the theme that clarity was riot enough. It was the supposed positivism of the *Tractatus* and the early Vienna Circle that he was rejecting. Actually his own position was not so different from Wittgenstein’s at the time. Gordon Baker has shown that when Waismann came to sum up his thoughts in “How I see Philosophy” (1956), he was using unselfconsciously, perhaps unconsciously, notes taken down in conversations with Wittgenstein twenty years before.

*Ein tief Gemüt bestimmt sich selbst zum Leid:* perhaps these two men were of a temperament to suffer in any case, but Wittgenstein was additionally scarred by two wars, one spent in the dangers and grim choices of combat, the second in anxiety; Waismann by an accumulation of personal tragedy which finally left practically no member of his family alive—or even dead in the course of nature. In early 1938 all of this, or the threat of it, hung over their meetings, in a country and amongst people with no such preoccupations, and it makes prickliness and a degree of *Teilnahmslosigkeit* (the characteristic Wittgensteinian term for lack of concern for another's problems) more than intelligible.

Contingent factors operate: others—younger men—made better exiles. Popper is an example: from his first salary in Canterbury he sent subscriptions to SPSL for his wife and himself. A fine gesture, and already English in style (for those times): one sees him on his way to becoming Sir Karl. Wittgenstein gave too, of course, but with careful consideration of how it was to be used, of what was best for the intended beneficiary in all his awkward particularity. In general he insisted, as ever, on preserving and following his own individual judgement. Waismann, who in fact contributed much by his very singularity, still thought at the end of finding some country better suited to him, and the only one that seemed serious enough (though he could not in fact go there) was Germany.

*The above account is based, apart from slight personal acquaintance, on useful talks with friends of Waismann's, the late Karl Menger for the Vienna period, Stuart Hampshire and the late Isaiah Berlin for Oxford and others, but also on researches in the university archives of Vienna, Oxford, and Cambridge, in the records of the Society for the Protection of Science and Learning, kept in the Bodleian Library, Oxford, as are Waismann's own papers, and in Carnap's correspondence with Waismann, Schlick and Neurath, held in the library of the University of Pittsburgh.*

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# Tributes to and Impressions of Friedrich Waismann

## Waismann's Big Book

Brian McGuinness

As late as 1948, when he was making his report to the Literae Humaniores Faculty Board on the work he had done as University Lecturer since 1945, Friedrich Waismann listed three text books that he had ready for publication (one of them being the essays on causality printed in the present volume) and one book—as it might be "real book"—which he referred to as "Philosophy and Grammar". That was his final title for a work he had been preparing since 1929 and which was originally to be called "Logik Sprache Philosophie". In 1948 that work will have consisted physically in a heavily corrected set of galley proofs, printed for Routledge and Kegan Paul (the publishers of Wittgenstein's *Tractatus Logico-Philosophicus*). The print was set up in the unhappy year of 1939, the copy reaching the publisher on 25 July of that year. At that time (precisely in December 1938) he gave the work what he called the new title "Philosophy and Language". A German typescript of the whole had been sent to Holland and was meant to be published there under the title "Sprache und Philosophie". The English version was set up in metal type, as the galleys show and one set of galleys seems to have been returned to the publisher for corrections to be made, but the project was abandoned at that point, presumably because the author thought better of it. To be sure the difficulty of obtaining paper and the uncertainty of sales in wartime may also have played a role. The relevant correspondence has still to be found in the publisher's records. Once the metal was broken up the work would of course have to be re-set anew if it was to appear.

The two versions did eventually reach publication after Waismann's death, the English one as *The Principles of Linguistic Philosophy* edited by Rom Harrré in 1965 (2nd edition by Gordon Baker in 1997), the German typescript was reconstructed from Waismann's *Nachlass* and published under the old title *Logik Sprache Philosophie* by Gordon Baker and the present writer in 1976. That German title can now be seen to be a mistake: Waismann in 1938 (and later) wished the reference to logic to be dropped. It was from the study of ordinary language, not formalized languages, that enlightenment was to be sought. Here he diverged from former colleagues such as Carnap, whom indeed he criticized fiercely during his Oxford teaching. The English title is also open to criticism. The book is not systematic enough to give principles, but, more important, it does not concern itself with one form of philosophy, namely linguistic philosophy. Its message is

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precisely that a correct study of language will give us the clue to all philosophy, not just to one part or interpretation of it.

The descriptive note that Waismann sent to Routledge and Kegan Paul to appear in their catalogue, and no doubt in some form in the book itself was as follows:

Philosophy and Language
By Friedrich Waismann

The subject of this book is the connection between philosophy and language. Starting from certain problems of classical philosophy we come to recognize that these puzzles arise out of a confusion as to the use of language, and disappear as soon as we get clear about the meaning of the words in which they are expressed. This, however, is a merely negative result. In the positive part of the book an attempt is mad to give a rough draft of the logical grammar of our ordinary language. This problem involves difficulties which do not occur in formalized languages. The method adopted in this book is to illuminate the use of everyday language by the help of artificially constructed models which are clear-cut, transparent and free from the confusing mental mist which enshrouds out ordinary language. As a result of the construction of such models and their systematic variation our ordinary language is set off against a background of languages of different structures; and some light is thrown upon the circumstances which incline our grammar to follow certain paths.

Among the subjects dealt with are: Problems of Communication. "Public" and "private" languages. Is only structure communicable? ~~Meaning and Verification~~. Incorrigible knowledge. The problem of non-Aristotelian logic. An inquiry into the logical structure of problems and discoveries. ~~The position of metaphysics.~~

The author was allowed to use unpublished ideas of Dr. L. Wittgenstein and the whole of this book is deeply influenced by these ideas.

Waismann later deleted in pencil two of the items, as if he meant to abandon the verification principle and the attack on metaphysics as central points. This would be of a piece with his avowed reluctance, from 1946 on, to be identified as a logical positivist. Probably nothing in the text we have was in fact changed.

Otto Neurath, to whom this notice or *Anzeige* was also sent, did not conceal his surprise at the respect and gratitude shown to Wittgenstein. Had not Waismann complained bitterly about the *Eigendünkel*—self-satisfiedness—and the hostility to science that had marred his stay in Cambridge? But the notice is enough to acquit Waismann from the charge of intended plagiarism, which some of Wittgenstein's fiercer friends brought against him. At the same time there was bound to be an awkwardness inherent in thanking for assistance once willingly given but now to all appearance begrudged. This unresolved problem may be what perpetually delayed publication.

## The Exile and His Family

*Max and Hedi Lieberman of Orinda, California, kindly answered a letter of mine (BMcG) when this book was being planned. Max writes on behalf of them both. Hedi, as will appear was a niece of Waismann's wife.*

Since we received your letter in September of this year, a number of developments occurred which caused us to be side-tracked and defer our reply until now … My wife and I are most anxious to do what we can to facilitate your publication of any additional material about the life and work of Friedrich Waismann (FW).

In preparation for this reply, we recently attempted to re-read, after so many years, the personal letters which FW. over the years. had addressed to my wife. Because the letters were so heartrending, we stopped after reading part of the correspondence, with the intention, however, of completing, as soon as possible, the remaining correspondence. In what we have reread so far, there does not appear to be too much biographical material which would be helpful to your inquiry. For example, in the correspondence that we have re-read—and, according to our recollection, this is also true of the remaining correspondence—FW makes no mention of his relationship to LW. Nevertheless, we thought it might be useful if we responded to your questions as numbered by you.

1. We know that FW was born in Vienna, but that his father came from Odessa. I do not know whether the term "nationality," as you use it, means or includes citizenship. I presume that the Soviet Union did not extend Russian citizenship to émigrés. I also question whether the Waismann family ever acquired Austrian citizenship. If my assumptions are correct, the Waismann family was "stateless" in post-war Austria, but that should not have prevented FW from completing his studies at the gymnasium and taking his Matura. My own conclusion is therefore that his lack of Austrian citizenship was not the reason for his leaving the gymnasium. It is, however, possible that, since tuition fees for foreign citizens (or non-citizens) were higher, the continued study at the gymnasium was too costly for the Waismann family. Also, as you pointed out, FW, inherently, resisted the "bureaucratic aspect" of the public education system and may well have preferred the "externist" route to the Matura. We do not know what sort of home tuition, if any, was available for him.

2. *Waismann's university career*. Again, we cannot throw any light on the questions you raised in this paragraph. Incidentally, I did not enter the University until the 1932 Fall semester and did not start attending the MS lectures, seminars and proseminars, until the summer semester of 1933.

After MS's assassination, Professor Reininger accommodated a colleague of mine and me in accepting our respective dissertations and guiding us through the final examinations. We always respected Professor Reininger who, though operating in a very conventional frame, took on the students of MS after MS's death. I was even more impressed when I learned from your letter how generous Reininger was to FW).

3. *Waismann's position in the faculty* You are, no doubt, aware that, during the 1935–6 period, Austria was under a so-called "authoritarian government" with Schuschnigg, instead of Dollfuss, being then in office. That regime was hostile to the thinking which MS and FW represented. I believe that *both* this ideological hostility and bureaucratic fury at FW's conduct in ignoring academic procedures accounted for the action of the Ministry of Education. The ideological hostility of the regime was also manifested, among other things, by the Government's action in closing down the Ernst Mach Verein. If my assumption that FW lacked Austrian citizenship is correct, it would be surprising that the Government permitted FW's employment—and a fictitious employment at that—as a University librarian.

4. *Waismann after Schlick's death.* FW was married long before MS's assassination. You are, however, correct in your observation that, after MS's assassination, FW had a hard struggle in earning a living for his family. His wife had the benefit of a small pension from a bank which was granted to her by way of compensation for severance when she retired from the bank several years earlier. There was also Dr. Steinhart, the wife of a wealthy businessman, who arranged for FW to conduct a private seminar for a fee which was raised by set fees payable by those attending the seminar. The first seminar—which my colleague and I attended for a while—was devoted to a discussion of LW's Tractatus. I do not recall how long these seminars continued.

5. *Waismann and Wittgenstein* My wife, who was Mrs. Waismann's niece, recalls that FW's life virtually revolved around his meetings with LW. Whenever LW visited Vienna, FW, she recalls, was lost to his family during LW's stay in Vienna. being fully absorbed in his meetings with LW. While in Vienna, neither I nor my wife was aware of the break between FW and LW, but there is no question in our minds that the break must have been a devastating blow to FW. It was only after she arrived at the Waismann household in February of 1939 that my wife learned of the break in the relationship between FW and LW.

My wife and I, not yet married, emigrated independently, and on different dates, to England and, thereafter, to the United States. As you may know, FW and his wife had arranged for my wife to obtain a temporary entry visa to England on the ground that she would stay there only a short time to await the issuance of her immigration visa to the U. S. This temporary visa enabled my wife to emigrate to England in February 1939 and join the FW household where she assisted her aunt in taking care of the Braithwaite child and her own child (incidentally, I was able to emigrate to England a month earlier on the same type of temporary visa).

On the several occasions when I saw my wife in London—as indicated above, we were not then married—she mentioned some of the conversations that took place in the FW household. Based upon my impressions from these reported conversations, Professor Braithwaite's observation that FW was not taking a realistic view of things may have been quite correct. Granted that, in those years, life for a refugee in England was very difficult, especially in view of the tight restrictions on employment and the lack of employment opportunities, I was nevertheless struck by the fact that, after having experienced the deterioration of the political and intellectual environment in Austria under the authoritarian Government, FW did not appreciate the British scene, including its intellectual freedom and vibrancy or the British political system, including the debates in Parliament.

Unfortunately, we cannot provide any factual input on what lay behind the break between FW and LW. We believe that LW was rather petty in his sensitivity on the point as to whether FW had given LW adequate recognition of his authorship of certain ideas. We also believe that your hypothesis, according to which LW opposed FW's plan to act in opposition to LW, must be correct. My wife recalls, however, that even after her arrival in Cambridge, FW actively worked with two graduate students on the English translation of his book. One of these students was well versed in translating German publications into English while the other was very knowledgeable in the field of logic and philosophy. My wife also recalls that the three men had considerable difficulty in attempting to translate the book, especially in areas where FW used an example from German literature in illustrating a point and it became necessary to match the point illustrated by the use of a comparable example in English. In any event, the break between the two men is, in itself, a tragedy which, in the long run, probably hurt LW as well—although not to the extent of the devastating blow suffered by FW

The atmosphere between the FW household and the Braithwaite household was rather uncomfortable. Mrs. Waismann also resented Mrs. Braithwaite's nonchalant attitude toward her child to which she devoted scant or, at best, the most superficial attention.

6. *Waismann in Oxford* As regards the Oxford years, FW, in his 1943 letters to my wife, described, in stark terms, the tragedy which ended in his wife's death in 1943 and the pain he endured during the years preceding. In the meantime, we have re-read the remaining correspondence including the 1945 and 1947 letters. In a letter, which FW wrote in April, 1945, he reported, with some satisfaction, his achievements in attracting and developing a following of devoted students, in charting a new course of studies, described as "multi-layered language structures" —a field both interesting to him and recognized as very important by others— and attaining, at last, a position of respect and prestige in university circles. This favourable situation was marred, however, by FW's feeling that the University treated him rather shabbily in setting his salary at a much lower level than that paid to native Englishmen. In addition, he complained that it was very difficult for him to form deeper contacts with the people among whom he moved. He attributed the lack of more intimate personal relationships to the English scene and the character of the English people. In a letter written in June, 1945, he is, again, complaining bitterly about the difficulty of forming meaningful human relationships in England, a difficulty which was experienced with the same intensity by

his son Tommy, who had no friends among his fellow pupils. As a result of this, both he and his son were condemned to lead isolated lives; indeed FW questioned whether he and his son could ever be "happy" and have meaningful relations with other people in England. Another source of constant irritation was the climate and the "air" in Oxford, both of which, he claimed, were poor and made it difficult for his son and him—who was especially sensitive to climatic influences—to lead healthy lives in Oxford. Finally, he deplored the extremely bleak economic conditions prevailing in England at the end of the war when food rations were extremely short, with virtually no provision for certain essential foods which were necessary for children growing up, such as Tommy; when everything was run down in the country and the future looked equally bleak. In short, this was not, in his opinion, a country or an environment suitable for raising children. Based upon all of these negative factors affecting life in Oxford and in England, generally, FW seriously considered leaving England altogether and seeking a position at a University in the United States. As a basis for decision, he listed all the pros and cons of a move to the United States: The principal factor in favour of staying in Oxford was that his teaching obligation there was very low (just one two-hour seminar per week); in addition, he was rather independent and the whole arrangement in Oxford gave him plenty of free time for thinking—which is exactly what he needed to carry on his work and which he, therefore, considered to be a vital factor; by contrast, the teaching load at American Universities was considerably higher—closer to ten hours per week—and he was afraid that professors and lecturers in the United States might not be accorded the same type of independence and freedom that he was privileged to enjoy in Oxford. Perhaps the negative factors which appeared to have weighed most against a continued stay in Oxford were the bleak economic situation in England and the lack of meaningful interaction with other people. Clearly, it was an agonizing decision which he faced, but psychologically and emotionally he seemed to be inclined to leave England. After rereading his 1945 letters and his bitter experience during the war years, I can better understand why he turned "against England" and never saw or appreciated the positive side of the country and its people. It was easier for me to see and appreciate the positive side as I never intended to stay there permanently and, therefore, was never exposed to the hardships and frustrations of finding employment which refugees encountered in attempting to find employment or otherwise adjust to British society.

In view of the bitterness expressed in his 1945 letters, it is somewhat surprising that he was able to inspire and develop a following of devoted students and friends during the Oxford years. We were touched by this and we were also touched, as you were, by his magnanimity in relinquishing his post to the Chinese American logician.

7. *Waismann and America* We had no knowledge of FW's visits to the U.S. as he never contacted us at any time during his visits. The reason may well have been that we, unfortunately, did not correspond with him after 1947. In fact, the 1947 letters were the last ones we received from him. Bear in mind please, that FW

never wrote to us about the death of his son. We found out about Tommy's death through a friend of my wife's who lived in Leeds and sent us newspaper clippings which reported about the severe conflict between FW and his son and about his suicide.

Most likely FW visited the United States—if he did so—in order to explore the possibility of finding an academic position and settling there. FW had asked us to give him our opinion as to what the scene was like in American Universities and what his prospects might be if he attempted to pursue a teaching career in the United States. He also asked us to contact a married couple in Berkeley, both of whom came from Vienna and held teaching positions in the psychology department of the University of California at Berkeley. I had been discharged from the Army in 1946 and was in law school in 1947. Unfortunately, we did not have any experience or contacts which would have enabled us to give FW a meaningful opinion.

## A Waismann Memoir

J. R. Lucas

I owe a great debt to Waismann. He was a philosopher. In the sterile atmosphere of Linguistic Analysis in mid-century Oxford he exhaled an air of philosophical interest, sensitive to but not confined to the niceties of linguistic usage, and taking account of real philosophical issues. I started going to his lectures as an undergraduate, and was able to have one or two tutorials with him in his house down the Abingdon Road. As a graduate I went also to his seminars, usually given on Tuesdays (if I remember right) from 5 to 7 in Room 303 in the New Bodleian. He would read from a closely worked manuscript, obviously written for the occasion, bringing out points with great subtlety but also with literary sensitivity. He once told me that he had originally intended to read classics at the university, but the first lecture on Horace was all about textual cruces and not, as he had expected, about Horace's poetry at all; and so he switched to mathematics. For me that was a great bonus. He was able to communicate the insights from mathematical logic that underlay the Logical Positivism of the Vienna Circle. In one tutorial he said that the Axiom of Reducibility could not do as an axiom because … and started to sketch out a proof he had devised. To my shame I never tried to master it. I think it gave an interpretation that was clearly not analytic. But even to not understand was an extraordinary breath of fresh air in an Oxford philosophical world that was resolutely non-numerate. Many of Waismann's lectures expounded bits of mathematical logic of great value to me, and not otherwise accessible—a lot of set theory, Cantor's transfinite arithmetic, relations—all of which were very relevant to philosophical issues, and often provided a firm basis on which to assess ordinary linguistic usage. But Waismann did not confine himself to logic. Again and again he would convey a sense of there being more to it than what he had so far said. The would-have-been student of Latin poetry was enlarging our vision. Waismann gave the impression of being a lonely man. After a tutorial he told me how he and his wife used to drift, I think on a raft of logs, down the Danube from Vienna in the summer, with a sense of that being a life long lost. The Oxford environment did not suit him. I once had him to dinner at Merton, but High Table food and Common Room port were no joy for a diabetic. Far better to be in Vienna and go each morning to a coffee shop, read the newspapers, and discuss philosophy with friends. Not that all philosophers were friends. He never discussed relations with other members of the Vienna Circle with me, but in seminars any mention of Carnap would trigger a diatribe against him—chiefly for being like a dogmatic Lutheran preacher, laying down a rigid law with no feeling for any finer points. I ought to have tried harder to befriend him. When I read in the papers that his son had committed suicide, I hesitated to write and express sympathy, as showing undue familiarity from an undergraduate to a don. But my father said it would be all right to write, and I did. Waismann showed me a picture of his son, but I never got close to him, although I was a would-be philosopher, and he, as one contemporary remarked, had more philosophy in his little finger than the whole of the rest of Oxford. And, in spite of his reserve, he was not only a philosopher, but an infectious one. We caught it.

## Oxford Memories of Friedrich Waismann

Frank Cioffi

My acquaintance with Friedrich Waismann came about through his being my tutor for two terms and through his membership of the Voltaire society of which he became an honorary fellow (and to which he read his paper on the rival roles of vision and proof in philosophy). I also had some informal discussions with him on solipsism and on religion.

For my tutorials I presented myself to his home in Abingdon road where a boy in his early teens opened the door to me. This was his son who was later to die by his own hand thus producing another of the many tragedies with which Waismann's life was peppered.

On one occasion my assignment was to read some chapters of Infeld and Einstein's *The Evolution of Physics* and comment. Due to the manifold distractions of undergraduate life I had not gotten round to it and came with no essay and completely unprepared. What to do? Our fallen nature took this opportunity to manifest itself. I explained that having little mathematical background I could not construe the equations in the text and so had made no headway. Waismann gave me a look of disappointment and told me that he was aware of my lack of mathematics and so had assigned me a text in continuous prose without a single equation. I did not have the option of pretending that I had consulted the wrong text ('Oh was that the book you meant?') since he had lent me his own copy.

It is characteristic of the perverse asymmetry of reminiscence that though I have no recollection of which particular varieties of whoopee lured me away from my task and my obligation to him. I still wince at my recollection of his reproach and exposure of my deviousness.

In any case I was able to overcome this inauspicious beginning and found my discussions with Waismann the most profitable of my undergraduate years.

Two other occasions occur to me. In one essay I used the expression 'structure of a fact'. Waismann rose from his chair and began making stabbing movements with one hand. 'I am Brutus stabbing Cesar. What is the structure of this fact? Are the angles at which my blade entered his body part of it?' I never used the expression again.

He once challenged me to provide a definition of 'game'. He demolished all my attempts in ways, which have become familiar to us. But I am glad to have had it from the horse's mouth as it were.

Waismann's connection with the Voltaire Society came about in a roundabout way. Harold Solomon who was president of the society and a great admirer of Waismann related sadly one day that he had been to a matinée at the Scala—a Bob Hope film—and was dismayed to see Dr. Waismann filing out after the film. I understood his feelings. There was something incongruous in a mind of such distinction finding distraction in the antics of Bob Hope. We felt he must be starved of genial social contacts.

It then occurred to us that were he to attend Voltaire Society meetings this might provide the distraction he sought at the cinema. And so we created the post of honorary fellow and offered him it. The Voltaire society had been recently founded but already had a membership, which we felt drew on the brightest, and the best. Among our members were Anthony Kenny, John Searle, Nigel Lawson, Robin Farquharson, Charles Taylor, John Gross, Patrick Seale, Jim Griffin and Father Colombo.

The reasoning, which led us to feel that the opportunity for intellectual exchanges with our members would wean Waismann from Bob Hope films, can't have been very strict. Nevertheless his appointment was a success. He came regularly and seemed to enjoy himself though he did not intervene often in the discussion.

One occasion which comes to mind is that in which we were addressed by Hans Eysenck on the topic of political leadership. Eysenck argued that politicians should have academic backgrounds in the social sciences and required to demonstrate their selective proficiencies in science, economics, sociology, political science and other fields of knowledge relevant to intelligent legislation. I thought this idea a prime specimen of scientism and am sure that Waismann thought so to. But when during the interval I introduced them their exchange was disappointingly cordial. It was left to the junior members of the society to point out the drawbacks in Eysenck's proposal.

One memorable informal discussion I had with him was at a dinner given in celebration of Bertrand Russell's birthday at Bertorelli's in June 1954. (Russell was the patron of the society) The topic addressed by the speakers was religious faith. Father Colombo, who translated Wittgenstein's Tractatus into Italian and who held the position of *advocatus Dei* in the society had responded to some remarks of Ayer.

Father Colombo referred to Voltaire's reply to Pascal's question apropos of his famous wager. Given that we are mistaken in crediting our post mortem survival since we will not discover our error what do we lose by believing?

Voltaire's reply was that we lose the truth. Father Colombo said that he agreed with Voltaire. I saw Waismann nodding vigorous agreement. At some point I approached Waismann and quoted some verse on the topic which took Pascal's view rather than Voltaire's:

> 'This immortality the horse I'll put my dough on please,
> Some think his chest is weak of course
> And some don't like his knees
> But there's pots to gain
> As you're aware
> If he wins according to plan
> And there is nought to lose
> For we shan't be there
> If he proves an also ran.'

Waismann smiled his disagreement and went on to make some observations about the evolution of religious beliefs. He thought this development was characterised by what he called 'etherealisation'. Sacred events initially conceived as determinate and picturable become less determinate, less locatable in space and time. I was left unsure what his own view was. Whether he felt that this was a face-saving device or an inevitable and natural progression.

What most struck and gratified me about that occasion was how much Waismann seemed to be incongruously enjoying himself in the midst of moderately raucous undergraduates. Harold Solomon's intuition proved correct. Waismann had been lonely and his reclusiveness was not unbreachable and would respond to friendly overtures.

The most absorbing and influential exchange that I had with Waismann occurred in my last undergraduate year. He was no longer formally my tutor but I would drop him a note from time to time soliciting his view of questions which were troubling me. He would respond by inviting me to tea at the Eastgate. This was particularly welcome since my G I Bill financing had expired and tea may have been my only meal that day. (My compulsory fasting was short-lived as Alan Bullock learned of my predicament and arranged a college sinecure for me.)

My talks with Waismann incited, at an interval of several decades, my paper 'Congenital transcendentalism and the loneliness which is the truth about things' and the sections on solipsism in my 'Wittgenstein and the riddle of life'.

The issue I asked Waismann to address was what we were to make of those utterances, which claim a special place for the speaker or find the place he occupies problematic and mysterious. One of the examples I produced was that made famous by Sartre, which he found in a novel that says of its heroine: 'It suddenly flashed into her mind that she was she' on which Sartre comments, 'She feels sure that she is someone different from the others but each of the others has the same feeling of being different from everyone else'. Wittgenstein's *Tractatus* pronouncement 'What solipsism means is true but it cannot be said.' falls in the same class.

Cyril Connolly writes 'in a flash it came to me that my name and myself were something apart, something that none of the other boys were or could be, Cyril Vernon Connolly, a kind of divine 'I Am that I Am.' This could be just playfulness. It reminded Waismann of something said by Jean-Paul Richter. What Waismann had in mind may have been this: 'I can never forget … the birth of my self-consciousness; … 'I am I' flashed like lightning from the skies.' From *Truths from My Own Life* by Jean Paul Richter. Often the egological utterances fall short of an explicit claim as to the absolute singularity of the self but merely insist on its mysteriousness and problematicallity. Wittgenstein's friend 'Con' Drury writes: the more psychology we read the more we feel that this essential 'I' eludes us. We cannot put into words the mystery which we feel in the notion of the self."

What concerned me in particular was the sense of revelation, which accompanies this genre of experience, where it is nevertheless impossible to say what it is that has been revealed. E.g. 'She was she'. It is not as if Sally Beauchamp I discovered what Sally Beauchamp II had been up to. It is a matter purely of aspect change. But what is remarkable is the sense of epiphany, which accompanies it. What problem do utterances like this raise and how should they be dealt with
Were they due to misunderstanding of the role of the first person in communication?

It seemed to me that there were three views we could take of these epiphanies.

1– That they were manifestations of what Peter Hacker called 'a thoroughgoing muddle' and that only attention to the proper function of the indexicality of the first person singular, which is a degenerate referring expression, could extricate us from that muddle.

2– That their appearance of momentousness, of seeming to be saying something, was not illusory but that they set us the task of fathoming their ontological significance. Consider Powell's revelation "I was me". Why should we not treat it as a metaphysical apercu and say that Powell had become aware of himself as a transcendental ego "constituting in the sphere of his ownness … everything that was objective for him, …"

3– That though they appeared to be saying something with ontological import, we are to treat this appearance as illusory. Nevertheless however illusory their penultimacy is acknowledged to be, the sense of being on the brink of an ultimate revelation—perhaps only to be revealed post mortem—will recur throughout our lives.

Isn't this the way it is with utterances like Wittgenstein's 'Mine is the one and only world'? We acknowledge the cogency of the demonstration of the illegitimacy of a purely inner and subject-referring use for 'I ' but the intermittent sense that there is something anomalous and eventually to be revealed about personal existence, persists.

I wondered what Waismann would say of those utterances, which treat selfhood as a mystery or a revelation or a matter of ineffable singularity rather than as a logico-linguistic puzzle?

In his very first remark Waismann showed that he had recognized my problem perfectly. He said that in his own case such selfhood epiphanies took the form 'I HAVE BEEN CHOSEN'. This must be understood as empty of content. It is not like claiming to have won the lottery. It is rather like Wittgenstein's 'I am safe whatever happens' which he gave as an example of nonsense and 'a misuse of language'. How could misunderstanding the role of the distinctive role of the first person pronoun produce phenomena like these?

John Updike's response to the 'unthinkable truth' that each if us feels themselves to be the center of the universe—one among many and yet the hub around which everything revolves—was to 'scream or call on God,' As a diagnosis of this state, 'You can stop screaming Updike. The first person pronoun is a degenerate referring expression' seems inadequate.

Ryle thought he could explain the non-parallelism between the notion of 'I' and that of 'you' without 'construing the elusive residuum as any kind of ultimate mystery' (*The Concept of Mind,* p. 196). Such an account bypasses the sense of revelation, which accompanies the illusion that one has glimpsed a referent for 'I'. It leaves no scope for what Wittgenstein called the 'deep mysteriousness of the 'I''. Is this a fault?

Isn't the most philosophically astute response that one can make to utterances such as 'When anything is seen it is always I who see it'? (*Blue Book*, p. 61) Or 'I have been chosen'. (Waismann), 'I know what you mean'.

How close are the views expressed in my dealings with egological epiphanies to the views I imputed to Waismann many years ago? I am not sure. Though I am certain that he would have derided the view that expressions of absolute singularity and the like arose through semantic confusions I am not sure whether he would have gone so far as to see them as primal phenomena which it was a mistake to treat as explainable.

I have not been able find in his published work pronouncements that would permit me to settle the question so that I have nothing to go on but an impression of accord which is half a century old and may have been no more than his desire to be amenable.

### Graveside Address by Gilbert Ryle

*Gilbert Ryle was one of Waismann's chief supporters at Oxford. In the* Grabrede *here printed, which is very characteristic of its author as well as true to its subject, he speaks of Waismann's love of truth, a rarer trait than one might think. This was what Waismann missed in some of his colleagues, which made him inwardly critical of them. Perhaps it also lay behind the severity with which he would speak of other thinkers whose views diverged from what he now thought was correct: the very names of Wittgenstein and Carnap, for example, were not pronounced without obvious disapproval. The positive side of this is that all his pupils learnt that philosophy was not a game. (There is much relativism nowadays that he would have deplored.)*

We are here to say "goodbye" to Friedrich Waismann. He was our colleague and friend. But above all we learned from him. We think through him and he thinks through us. He was exiled from his own homeland; he lost his wife; he lost his son. He was buoyed up by no personal hopes; he was drawn on by no personal ambitions. But he kept his courage and he continued to search.

He cut himself loose from the comfortable half-truths in which our minds love to repose. He cut himself loose from those harsher half-truths to which our minds swing in the impatience of mere revolt. For Waismann a half-truth, whether conservative or revolutionary, was a distorting mirror. Vision begins when distortions repel and no longer attract the eye.

I quote his own words: —"A philosophy is there to be lived out. What goes into the word dies: what goes into the work lives." Friedrich Waismann is dead; his work is alive.

*The quotation from Waismann's own work is from the close of "How I See Philosophy" printed in the volume with the same title (Macmillan 1968).*

Mathieu Marion

# Waismann's Lectures on Causality: An Introduction[1]

Waismann's writings can be divided into three periods.[2] The first corresponds to his early work in Vienna under the aegis of Schlick, thus mainly to his collaboration with Wittgenstein on the first drafts of *Logik, Sprache, Philosophie*,[3] out of which came not only the book itself many years later but also transcriptions of conversations with Schlick and Wittgenstein[4] and numerous dictations reworked by Waismann, now published under the title *The Voice of Wittgenstein. The Vienna Circle*.[5] Waismann also did at that stage independent work, albeit largely influenced by Wittgenstein, on probability and identity.[6] The second period runs roughly from the moment relations with Wittgenstein were severed – towards the end of 1934 – to his arrival in Oxford, where he started lecturing in Michaelmas Term 1939. During this period, Waismann published his only book, *Einführung in das mathematische Denken*[7] but, while he completed his *Logik, Sprache und Philosophie* and even had it translated in English, plans for publication did not materialize and he chose instead to publish parts of it in *Erkenntnis* and *Synthese*.[8] The third period, extending until his death in 1959, saw the publication of a number of papers that established his reputation in England, collected since in *How I see Philosophy*[9] – a volume which contains Waismann's only published piece on causality, 'The Decline and Fall of Causality' (hereafter DFC).[10] Although usually perceived as one of logical positivists, Waismann clearly distanced himself from them in his last writings; the summary of his 1947 lecture at the Socratic Club on 'The Limits of Positivism' being evidence to this. He was also at pains to distance himself from Wittgenstein, as one can see for example from the posthumous piece

1 References are to the page numbers of this edition of the typescript 'Causality'. This is M 13 in Schulte's Catalogue (Schulte 1979).

2 (Quinton 1977, xi-xii), (Schulte 1979, 109), (McGuinness & Schulte 1994, ix).

3 The manuscript *Logik, Sprache, Philosophie* and an English translation were destroyed during the war. A reconstructed version was published as (Waismann 1976). An English version had already appeared in 1965 which is now in its second edition (Waismann 1997). For details of this story, see (Baker 1997).

4 (Wittgenstein 1979).

5 (Wittgenstein & Waismann 2003).

6 These papers are collected in (Waismann 1977).

7 (Waismann 1936).

8 Some of which are collected in (Waismann 1979).

9 (Waismann 1968).

10 (Waismann 1968, 208-256). References for 'The Decline and Fall of Causality' are, however, to this new edition.

B.F. McGuinness (ed.), *Friedrich Waismann – Causality and Logical Positivism*,
Vienna Circle Institute Yearbook 15, DOI 10.1007/978-94-007-1751-0_3,

'The Linguistic Technique',[11] written largely in reaction to *Philosophical Investigations*.

Waismann's lectures in post-war Oxford were a source of intellectual stimulation for philosophers as diverse as Michael Dummett, Stuart Hampshire, Herbert Hart, John Lucas, and Anthony Quinton. Among his posthumous papers from that period now at the Bodleian Library, his lectures notes on the philosophy of mathematics, mainly from the 1950s, were published in 1982,[12] and a long typescript from the early 1940s entitled 'Willensfreiheit' appeared in 1983,[13] with an English translation in 1994 under the title 'Will and Motive'.[14] The typescript on 'Causality', which is the last substantial piece in Waismann papers that has remained unpublished, can be seen as a companion to 'Will and Motive', although internal evidence suggests that it dates from the late 1940s, perhaps even the early 1950s. Indeed, they each approach the problem of determinism from one of its two traditional angles: 'Causality' deals with the topics of causality, induction, and determinism in physics but ends on short chapters on motives and desires as causes of our actions, while 'Will and Motive' begins by a rejection of the problem of determinism, opening the door to his theory of action. It looks almost as if the purpose of 'Causality' was to clear the field for the examination of issues covered in 'Will and Motive'. Although in earlier phases Waismann's thought was moving within a frame largely provided by Wittgenstein and Schlick, in these two pieces he seems to have set his own agenda. This is not to say that his mentors had nothing to say on these issues or that Waismann did not weave in some of his earlier ideas taken from them, but simply that he seems to have recomposed his philosophy around a theme, determinism, that was not central to their concerns.

Although Waismann's lectures on causality were steadily attended, his thoughts on this topic had in the end little influence. This is partly caused by the fact the typescript on 'Causality' remained unpublished and also by the fact that his sole paper on the topic, 'The Decline and Fall of Causality', has been perceived as presenting a form of 'eliminativism', i.e., the view that causes have been evacuated from modern science and that one should therefore do away with talk about them. Indeed, Waismann believed that "causality has definitely come to an end" and even claimed that the year 1927 saw its obsequies (DFC, 53).[15] This view had

11 (Waismann 1977, 150-165).

12 (Waismann 1982).

13 (Waismann 1983).

14 (Waismann, Schächter & Schlick 1994, 53-137).

15 Waismann is referring here to meetings at Como and Brussels in the Autumn of 1927 were physicist such as Heisenberg, Bohr, and Einstein discussed the new interpretation of quantum mechanics that had emerged in the previous months, when Heisenberg presented his uncertainty principles and, in its wake, Bohr introduced his complementarity principle. These two had discussed complementarity in Copenhagen, hence the name usually associated with that interpretation. The meetings of 1927 are indeed usually considered as a turning point as adversaries of the 'Copenhagen interpretation' never since regained the upper hand. The story is told in many places, e.g., in (Bohr

prominent supporters, from Russell, who called the law of causality a "relic of a bygone age surviving, like the monarchy, only because it is erroneously supposed to do no harm",[16] to Quine, for whom "the notion of cause has no firm place in science",[17] but it has become increasingly unpopular since the 1950s and it is now virtually without supporters. Russell himself had moved away from it by the time he wrote *Human Knowledge. It Scope and Limits*.[18] With his lecture notes, we are now able better to understand Waismann's views on causality, to see how they fit within their epoch and to which extent they may or may not contribute to modern discussions of the topic. He had distanced himself in 'The Limits of Positivism' as well as in 'How I see Philosophy'[19] from the crude anti-metaphysical agenda of the Viennese positivists. In his lectures notes, he points out that Hume did not wish to deny causation, but to analyze it (p. 94) and this point, to which I shall come back, obviously applies to his own work. For this reason, his thoughts on causality (and determinism) cannot be reduced to an inquiry into their evanescent role in modern physics and his notes contain contributions to the metaphysics of causation although, for obvious reasons, these do not display the level of sophistication which we would expect today, nor do they cover all aspects of the question. For example, J. L. Mackie's claim that a cause is an insufficient but necessary part of an unnecessary but sufficient condition or the claim that statements such as '*A* caused *B*' are entailed by counterfactuals of the sort 'If *A* had not occurred, *B* would not have occurred' have considerably renewed the discussion since the late 1960s, but he could not have foreseen them.[20]

The typescript on 'Causality' has 12 sections of unequal length and it can be divided into three parts. In sections (1)-(4), Waismann summarizes the views of Hume and Mill on causality and induction.[21] Using these as a starting point, he then examines in sections (5)-(8) the situation in modern physics in order to conclude to the disappearance of causality and that the problem of determinism is a 'pseudo-problem'. These sections are written for the non-scientific reader and their content can usefully be supplemented with that of the equally clear and non-technical 'The Decline and Fall of Causality'. So far, Waismann dealt only with the view that causal relations are established through the observation of regularities. In sections (9)-(11), he examines the thesis that one could discover causal relations through an 'understanding' of the relation between the cause and the ef-

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1958, 38f.).

16 (Russell 1917, 173).

17 (Quine 1976, 242).

18 In that book, Russell introduced the notion of 'causal lines', through which he believed to be able to show that "laws of the form 'A causes B' may preserve a certain validity" (Russell 1948, 316).

19 (Waismann 1968, 1-38).

20 See, respectively, (Mackie 1974) and the essays collected in (Sosa 1975).

21 As can be expected, Waismann's brief mentions of Kant's view that the law of causality is a condition of possibility of experience are dismissive (p. 154) (DFC, 59-60).

fect. This leads him to further criticisms of views by A. C. Ewing and of Wolfgang Köhler's claim that he can perceive the cause of his states of mind. The lecture notes conclude with a critique, in section (12) of Russell's claim that a desire is a "causal law of our action".

We speak variously of *A* causing *B*: 'Smoking causes lung cancer', 'The extra weight caused the shelve to collapse', 'The repeated blows to the head caused death', 'The flood caused the famine' or 'Mr. Baldwin's speech causes adjournment of House'.[22] What does it meant to say that *A* causes *B*? What is this 'causal' relation between *A* and *B*? Waismann assumed throughout without argument that causation is a relation between two[23] events and not, e.g., facts or tropes.[24] He also argued for a coarse-grained notion of events:[25] if events are too finely individuated, then it becomes impossible to talk of causes.[26] The position he defends throughout is a 'regularity account' according to which there is only a succession or chain of events that we may perceive but no such thing as a cause as a 'linkage' or 'bond' between events that could be either perceived or understood (pp. 157 & 163). We can only base our judgements about causation upon observation of regular succession between types of events. It should be clear from the outset that, although Waismann discusses in these lecture notes topics as varied as Heisenberg's uncertainty principle in quantum mechanics and Köhler's notion of 'insight', his purpose is clearly not to give a full discussion in each case but to provide an overall view of a 'regularity account' of his own and the *prima facie* case for its soundness.

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Belief in the causal nexus was shaken by David Hume's celebrated critique in *A Treatise on Human Nature*, Book I, part III.[27] As Waismann recalls (pp. 93-94), Hume found four characteristic points in the ordinary notion of causality, which he wishes to analyse: cause and effect are contiguous in space and time, the cause precedes the effect, the effect follows the cause with regularity, and there is a necessary connexion between the cause and the effect; this last being the sole target of his critique. This alleged necessary connexion is indeed not logical, since '*A* causes *B*' would then be of the same nature as '*p* follows from *q*', in which case it would be self-contradictory to assert *p* and deny *q*; however, affirming *A* and denying *B* does not imply a contradiction (p. 95). (The point is also made by Hume in

22 This last example, a newspaper headline, is taken from (Collingwood 1940, 290).
23 That causation is a binary relation is an assumption that has been criticized. For example, see (Hitchcock 1996).
24 For a defence of causation as a relation between facts, see, e.g, (Mellor 1995).
25 His position is thus in the same ball park as, e.g., Davidson's in 'Causal Relations' (Davidson 1980, 149-162).
26 See the remarks on p. 109 and in section 6, especially pp. 139f. on the necessary vagueness of the ordinary concept of 'cause'.
27 Alternatively, in *An Inquiry into Human Understanding*, section 4.

his examination of the maxim that 'whatever begins to exist must have a cause of existence', which is discussed at length by Waismann (pp. 96-98).) Nor can this necessary connexion be found in experience since we can only observe the succession of events, not the connexion itself.[28] As is well known, Hume assumed that the idea of a necessary connexion must therefore rest on a psychological mechanism: 'habit' is at work.

That '*A* causes *B*' *always* is thus neither logical nor verifiable save for a finite number of cases. Therefore, how could we derive a general law, covering a potentially infinite number of cases, from a finite number of cases? This is the problem of induction as uncovered by Hume, "the problem of justifying an inference from the past to the future" (p. 102).[29] Again, there is no contradiction to be inferred from supposing false generalisations such as 'All swans are white', so they are not logical and they are never completely verified either. They could always turn out in the future to be false, they are not certain. Can we prove that such empirical generalisations will hold good, i.e., that a law which has been fulfilled in all past experiences will be fulfilled in the future? In other words is there any ground for our belief in induction? Again, logic is powerless here, since to suppose that a law will not be fulfilled in the future is not self-contradictory (p. 109). An appeal to a 'principle of causality' such as 'equal antecedents always bring about equal consequents' or 'same cause, same effect' is circular since such principles are in turn supported merely by induction (p. 110). In other words, "we can never use experience to prove the inductive principle without begging the question" (p. 111).

Waismann's clear and concise statement of Hume's views on causality and induction, summarized here,[30] does not make use of secondary literature and it is likely to mislead readers into thinking that it is merely a presentation of basic, uncontroversial material. On the contrary, it is heavily oriented and rather original, as Waismann weaves in his own views. Indeed, Waismann presents empirical generalizations as 'hypotheses' and argues for their scientific usefulness by quoting a well-known passage from Hertz's *Principles of Mechanics* (p. 106), which he interprets as meaning that hypotheses are rules:

> Another way of stating the same thing is to say that the hypotheses are rules which govern our expectation of future experience, or rules for forming particular statements about unobserved future events. (p. 106)

This is reminiscent of Wittgenstein, Schlick and Ramsey.[31] Waismann never mentions Ramsey, but the parallels with his notion of 'variable hypotheticals' are more than striking:

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28 Some arguments to the contrary are discussed in sections (9)-(10).

29 Of course, reference to the future is not essential (p. 111), but useful for expository purposes.

30 For Waismann's own summary, see p. 115.

31 Wittgenstein had indeed interpreted hypotheses are 'rules for the formation of expecta-

Variable hypotheticals or causal laws form the system with which the speaker meet the future. […] Variable hypotheticals are not judgments but rules for judging 'If I meet a *f*, I shall regard it as a *y*'. (Ramsey 1990, 149)

Waismann even shares with Ramsey a form of behaviourism about beliefs, which he interprets as "patterns of behaviour" (p. 114). In everyday life, our actions are by instinct based on induction, they are not based on "discursive, argumentative thought" (p. 114).

Furthermore, Waismann rejects attempts at portraying the principle of induction as a statement about regularity or uniformity in nature or even the 'rationality of the universe', as being metaphysical statements that "say more than we can assert in good conscience" (p. 118) and variants of the principle in Keynes, Broad and Nicod, as well as Russell's attempt at justifying it on "intrinsic evidence", as being, once more, circular (pp. 115-117). Waismann's way out is to point out that the principle of induction has hitherto been understood as a 'factual statement' and this is why attempts at justifying it on the basis of experience are circular. In his account (pp. 117f.), it denotes a procedure:

The principle of induction is neither factual nor an *a priori* statement, neither synthetic nor analytic, because it is *not a proposition at all*. In actual fact it is a *rule of procedure* that codifies our activity of generalizing. (I deliberately say "codifies" and not "guides" because we act according to it even before it has been formulated.)
[…] it is never used as a *substantial premise* in scientific reasoning: it is not a *premise from which we draw conclusions*, but the *scheme in accordance with which we actually proceed when making generalizations*. (p. 118)

In short, as a 'rule' or 'scheme', the principle of induction is not a proposition – again a point reminiscent of Wittgenstein –[32] therefore it does not assert anything and it is for this very reason that it needs no justification. That justifications of induction by an appeal to experience, regularity in nature, or success in practice are all circular was already urged in the early 1950s by Peter Strawson in his influential *Introduction to Logical Theory*.[33] But Waismann did not just argue this point

tions' in the early 1930s. See (Marion 1998, chap. 4 & 5). Schlick explicitly referred to Wittgenstein when introducing that notion in 'Causality in Contemporary Physics' (Schlick 1979, 188). The idea is found in Waismann's 'Hypotheses' in (Waismann 1977, 38-59) and it is clear that he got it from Wittgenstein, at the time of their collaboration on *Logik, Sprache, Philosophie*.

32 In the *Tractatus Logico-Philosophicus*, Wittgenstein wrote that "The law of causality is not a law but the form of a law" and that "in physics there are causal laws, laws of the causal form" (Wittgenstein 1922, 6.32 & 6.321) and this may be taken to mean that the law of causality is not a proposition. (See (McGuinness 1969) for a discussion.) These passages of the *Tractatus* are also cited later on by Waismann, who concludes indeed that Wittgenstein held that "the law of causality would not assert anything" (p. 144).

33 (Strawson 1952, chap. 9, Part II).

with much clarity; his position is original, inasmuch as he brings into the bargain new ideas about the nature of the principle of induction.

Concerning Hume, Waismann makes three noticeable points of exegesis. First, he insists that "Hume has been accused of *denying* causation whereas in fact he was concerned only with analysing it" (p. 94) (DFC, 209). To my knowledge, this point was first made by William James' student, Dickinson Miller.[34] Secondly, Waismann quotes a letter from Hume to John Stewart to prove that Hume never wanted to assert that events are uncaused (p. 98).[35] Finally, Waismann is able, as an upshot of his discussion in sections (1)-(2), and in particular of the above claims, to conclude that Hume was not a sceptic (p. 113). This claim is of course not new – albeit still a matter of controversy – but, coupled with his analysis of scientific hypotheses as 'rules for the formation of expectations' and of the principle of induction as a 'rule of procedure', Waismann's defence is original.[36] Following Hume, Waismann claimed that "we must accept the inductive principle as a sort of *blind instinct* or *automatic device* acting on our mind, and forego all argumentative proof for our expectations about the future" (p. 112). That a proof is lacking should not be a problem. It would be "spurious" (p. 113) and this is a "pseudo-question" (p. 120). Precisely for this reason, Hume's arguments do not imply scepticism. It is only because the ordinary notion of causality contains the idea of a necessary connexion that we were drawn into these spurious problems, removing it does not lead to scepticism but to a better understanding of science:

> In fact, the credit of natural science is not impaired by the lack of such a proof. The only test that is required in science is the test of success in prediction. We are entitled to have faith in our procedure just so long as it does the work which it is meant to do. That is, as long as it enables us to predict future experience and so to gain control over our environment. Of course, the fact that a certain form of procedure has always been successful in practice affords no logical guarantee that it will continue to do so. But then it is a mistake to demand a guarantee where it is logically impossible to obtain one. This is not scepticism; for the fact that we are unable to offer a logical guarantee for an empirical generalization in no way entails that it is irrational for us to believe it. On the contrary, what is irrational is to *look* for a guarantee where none can be forthcoming; … (p. 113)

Hume's analysis is nevertheless incomplete in Waismann's eyes, because his account does not allow one to distinguish causal sequences from mere regular sequences. This is why Mill's methods for inductive reasoning and the canons

34 Unless, of course, these lecture notes were written before Miller published this remark: "He was not intending to mutilate our idea [of causation] or deprive it of any of its features, not to modify but to analyse it" (Miller 1945, 593). I would like to thank David Raynor for pointing it out to me.

35 This letter is quoted in Norman Kemp Smith's well-known commentary (Kemp Smith 1949, 411-413).

36 It has, e.g., nothing to do with Kemp Smith's grounds for making the same point (Kemp Smith 1949, 446-449).

they embody provide, according to Waismann, a definition: “we speak of a *causal connection*, whenever this connection can be established by means of any of the methods enumerated by Mill” (p. 60). Section (4) contains a lengthy defence of Mill against some objections. Waismann is thus far from ‘eliminating’ causation altogether. However, his conclusion is that it has a limited role in modern physics.

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Moving to physics, Waismann’s arguments in section (5) are broadly along the line of Russell’s 1912 paper ‘On the Notion of Cause’.[37] First, he points out that ‘*A* causes *B*’ implies that reality can be sliced into a temporal succession of discrete events but, since reality is in fact continuous, he concludes that the traditional notion of causality runs into allegedly insuperable difficulties (pp. 129-131). Amazingly, in speaking here of cause and effect in terms of “series of processes” (pp. 130 & 131), Waismann thus comes very close to but falls short of stating Russell’s later concept of ‘causal lines’, which is in turn the ancestor to Wesley Salmon’s ‘causal processes’.[38] At any rate, Waismann’s point is merely that it is the conception of causality as a relation between two discrete events which is not used in science (p. 131), a point hardly contested, but he does not enquire about the possibility that ‘causal processes’ may be used instead, because he insists that ‘cause’ and ‘effect’ are not to be contiguous if one is merely earlier than the other (as they are, e.g., in Russell’s ‘causal lines’)[39], and thus that the traditional notion cannot be reframed in these terms (pp. 131-132).

Secondly, he argues that the notion of causality has been replaced by the notion of ‘functional relation’ or ‘functional dependence’, a point for which Russell is famous, and which was taken for granted among logical positivists. Indeed, through an elegant discussion of planetary motion, Waismann shows that physics deals with differential equations within which nothing is recognizably a ‘cause’ or an ‘effect’:

> […] a mathematical function, generally speaking, is simply a law governing the interdependence of variable quantities. […] Physical laws are nothing but statements concerning the way in which certain quantities depend on others when some of these are permitted to vary […] The task of the physicist is to determine the exact or approximate nature of this functional dependence. (p. 133)

This last argument is also supported by a discussion of Fourier’s theory of heat (pp. 135-137). It could have been given support by other cases, e.g., the Lorentz transformations, but all this only shows that the traditional notion of ‘cause’ does not appear in the fundamental equations of modern physics. If ‘eliminativism’ is

37 (Russell 1917, chap. 9).
38 (Russell 1948, 316-317 & 453-460); (Salmon 1984) & (Salmon 1998).
39 (Russell 1948, 316).

taken to be the claim that for this very reason the notion of causality should be completely banned from our vocabulary, then it is open to the objection that the notion of 'cause' plays too central a role in the analysis of a host of other concepts, e.g., 'disposition', 'warrant', 'action', 'responsibility' to be eliminated. Furthermore, other notions such as 'law' or 'event' are here clearly in the same boat as 'cause' and philosophers of science may not wish to do away with all or even some of them. For example, Nancy Cartwright favours causes, not laws.[40]

At all events, one must delineate Waismann's position with care: as already pointed out, he merely intended to analyse our ordinary notion of causality (and enquire into its scope), not to deny it. He agreed with Hume that the element of necessary connexion has only a psychological basis, i.e., a psychological mechanism makes us expect an invariable conjunction of two events in our direct experience. It is only this last notion that he has found missing in modern physics and this is not a conclusion that has been much contested. It is not the same to declare that causality has no explanatory role to play in our theories about, say, physics or biology. Waismann does not wish to ban the idiom; he is merely an anti-realist about causes, as the above discussion of the principle of induction and law of causality should have made clear. He argues therefore in section (6) that the ordinary conception of causality that he has, following Hume, analysed, has "relevance to practical life" (p. 140) and that 'My hunger passed away because I had a good meal' is "a perfectly good description of a causal nexus" (p. 140). One feature that makes it so relevant is that 'cause', 'effect', and 'event' are *vague* terms and they should remain so, as attempts to make them precise will lead to difficulties (p. 141). Another concept causality must be introduced if vagueness is to be eliminated:

> In general, when you want to make your concepts precise, you must change the whole way of concept-formation. This you do when you pass to science. What is important to understand is that, when you study physics, you learn a new *method* of describing things. I might as well say that there are two *languages*, the language of science and the language of every-day life, and to each of them there corresponds a *particular scheme of causality*. That is, it is a mistake to suppose that there is just *one* idea of causality, which is analysed by philosophers. (p. 141)

Waismann is here not very far from Nancy Cartwright's view that there are various irreducible concepts of causality, each one with their own purpose.[41] Although he offers only the beginnings of a theory, he argues in this section in terms of his own doctrine of 'language strata':[42]

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40 (Cartwright 1983), (Cartwright 1997).

41 (Cartwright 1997).

42 This is the doctrine presented in eponymous papers that are reprinted in (Waismann 1968, chap. IV). The idea that the concept of causality is 'stratified', so to speak, is neither to be found in these papers nor in 'The Decline and Fall of Causality'; it is thus an original contribution from these lecture notes. In 'The Decline and Fall of Causal-

What we must understand is that there are two distinct *language strata*, and that the word "causality" accordingly undergoes an inflection of significance. You can only apply the term "causality" with reference to a language fit to represent things and processes; what you mean by causality will depend on the stratum of the language you use. (p. 141)

Now the point which I want to make is that the idea of causality is tied up with a certain way of describing things. And as there are different ways of describing thing – or, what comes to the same, different languages – the idea of causality adapts itself to the particular type of language. Thus scientific language has its own conception of causality, different from the idea we meet in common speech.

To put it slightly differently: *the idea of causality is a function of language, and it varies when you pass to a language of a new logical stratum.* (p. 143)

Waismann's discussion on quantum mechanics moves away from causality to determinism, which he defines along lines broadly similar to Laplace's well-known formulation (pp. 137-138, 145-146 & 146-147) (DFC, 57 & 64-65), only to point out that there is already something wrong with it, since our measurements are never infinitely precise and errors compound very rapidly (pp. 146-147).[43] This point is presented in greater detail in 'The Decline and Fall of Causality' (DFC, 65-68), where Waismann shows that very point from an example (the movement of a ball on a round board):

Even in classical mechanics the causal scheme does not always work, not under all circumstances. Whether it works or not hinges on one condition – that measurements can be made with unlimited accuracy. Causality stands and falls with this requirement. (DFC, 67)

Applications of the traditional conception of causality assume indeed that

[…] it is possible to measure precisely the state of a physical system and that there is no limit to the finiteness of our operations of measuring. Only if this condition is fulfilled, may we speak of causal laws enabling us to predict exactly the entire future of a physical system once its initial state is known to us. (p. 147)

Moving to quantum physics, section (8) contains an elegant presentation of Heisenberg's uncertainty principle (p. 148) and the related complementarity principle by Bohr (pp. 150-151). Although Waismann argues here against determinism, his

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ity', another approach is proposed instead: "while causality is thus indispensable for an interpretation of an experiment, it does not follow that it must also apply to the hidden reality which manifests itself in the experiment. The existence of causality on the macroscopic level together with acausality on the microscopic presents an inner tension which could be only be released when it was shown that ordinary mechanics is included within quantum mechanics as a limited case" (DFC, 72).

43 For a clear statement of this critique, see (Born 1958). The argument is also known through G. E. M. Anscombe (Anscombe 1981, 139).

position contains important nuances, since his claim is only that it is limited in its application:

So far as experience is concerned we can only say that *some* domains of happenings *have actually been subjected* to laws of a causal type, *other ones have not*. (p. 145)

Yet the most recent theories, adopted by physicists, almost against their will, to explain fact experimentally observed lead, not so much to a complete surrender of determinism in physics, as to the view that it is not complete nor universal, and that in fact it has limits. (p. 146)

The claim can be taken to mean that some theories are deterministic, while others are not or that theories might be partly deterministic and partly not.[44] Waismann actually argues succinctly that this is the case for quantum mechanics:

The new physics is neither a causal, deterministic theory in the old sense, nor a completely indeterministic theory […] (p. 152)

[…] what is deterministic is the mathematical law for the propagation of certain waves; what is indeterministic is that what is really fixed by the wave is not the position of the electron, but only the probability of its position. (p. 153)

Of course, Waismann refers here to the 'uncertainty' phenomena first uncovered by Heisenberg.[45] If $\Delta p$ denotes the range of values for the position of the particle and $\Delta q$ for velocity or momentum, then:

$$\Delta p \cdot \Delta q = \frac{h}{4\pi}$$

where $h$ is Planck's constant (p. 148). Roughly put, when trying to determine position with more precision, one looses information about velocity, and vice-versa. It might be useful here the supplement Waismann's comments with his lengthy discussion of the uncertainty principle in 'The Decline and Fall of Causality' (DFC, 69-82). These analyses led him to conclude that "there is no escape from the uncertainty principle" (DFC, 82) and thus that "there can be no determinism" here (p. 152). According to the lectures, the upshot is that

[…] *there is a limit to the finiteness of our powers of observation and the smallness of the accompanying disturbance – a limit which is inherent in the nature of things and can never be surpassed by an improved technique or increased skill on the part of the observer*. There is thus an essential indeterminacy in the quantum theory, of a kind that has no analogue in the classical theory. This indeterminacy can be said to have its basis in the wave properties of matter, and is therefore unavoidable. (p. 149)

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44 See (Earman 1986) for a thorough study of deterministic claims for a number of physical theories.

45 (Heisenberg 1927).

A common error about indeterminism is to refer to $\Delta p$ and $\Delta q$, above, as errors of observation while it is in fact inherent to the mathematical formalism of the theory. In fact, Waismann makes neither of these claims but argues instead that the limit is "inherent in the nature of things", i.e., that "*nature itself is indeterminate*" (p. 154):

> […] in throwing dice we cannot predict the result of any throw; what we can predict is only the *probability* of throwing a certain number. We are prevented from predicting the result, because we have insufficient information as to all the minute factors which are of relevance in the matter. But we might still imagine an experimenter who has such subtle methods of observation at his disposal and at the same time such mathematical skill that he can predict with certainty the result of a particular cast. In this case the impossibility of predicting is only a *technical* one which, at least in thought, can be overcome. Not so in the case of the electron. For here we are prevented by the very laws of nature from predicting its future behaviour. The impossibility is not due to some lack of information on the part of the observer or to some lamentable inefficiency on the part of the calculator or to some limitations of human beings, but to the very order of things. (p. 154)

This jump to an ontological conclusion is certainly striking and in need of further support, but one should note that it is in line with Waismann anti-realism about causality: not only does he hold that the principle of induction is merely a 'rule of procedure', he also infers from quantum mechanics, as our best scientific theory about the world,[46] to the ontological thesis that the 'order' or 'nature of things' is non-deterministic. In short, when it exists, causality is not to be found 'out there' but in our theories, as a satisfactory explanation of observed regularities.[47] This is the opposite from a viewpoint such as Cartwright's, who is a realist about causes but somewhat anti-realist about laws.[48] In connections with this, it is worth noting that Waismann also makes a few interesting points about laws: a brief survey of the etymology of the word 'law' and of the origins of the expression 'laws of nature' (pp. 138-139) (DFC, 62-64) leads him to the conclusion that it brings about

46 There is, however, no discussion of the logic of quantum mechanics in the lectures on causality, as opposed to some lengthy concluding remarks in 'The Decline and Fall of Causality' (DFC, 88-90). This is in line with the sort of holism advocated by Quine and furthered by Putnam in 'Is Logic Empirical?' (Putnam 1979, chap. 10) and proponents of quantum logic.

47 Again, this is not far from Ramsey: "But may there not be something which might be called real connections of universals? I cannot deny it, for I can understand nothing by such phrase; what we call causal laws I find nothing of the sort" (Ramsey 1990, 160). It is interesting to note in this context that Waismann's anti-realism in these matters goes as far as the suggestion that "quantum physics […] presents a strong case against traditional logic" (DFC, 90).

48 (Cartwright 1983, 74 & 86). Cartwright has argued since for the introduction of 'capacities', so that causal claims are not seen anymore as 'reports about regularities', as Waismann continued to do, but as 'ascriptions of capacities' (Cartwright 1989, 3) and thus "it is not laws that are fundamental, but rather the capacities" (Cartwright 1989, 181).

misleading connotations of 'coercion' and 'prescription', "as if the planets, if left for themselves, would have chosen to run off in quite different directions and only these tiresome laws of Kepler's compel them to remain in orderly orbits" (p. 139). Instead, 'laws of nature' are merely descriptive (p. 139) and, furthermore, "no law is absolutely exact" (DFC, 86).

To come back to Waismann's discussion of quantum mechanics. It is, of course, always possible to re-establish determinism through the introduction of hidden variables or parameters. Although the possibility that quantum mechanics is 'incomplete' was argued forcefully already in the 1930s with help of the Einstein-Podolsky-Rosen Paradox, hidden variable theories were only really taken seriously when David Bohm introduced was is now known as 'Bohmian mechanics', in the wake of de Broglie's 'pilot wave' model.[49] Against the very possibility of an hidden variable theory, Waismann points out a theorem by von Neumann[50] that shows that the system of quantum mechanics is (mathematically) complete in the sense that "it permits no addition that would render it deterministic", because any such addition "will necessarily lead to internal contradictions" (p. 154) (DFC, 86). For Waismann, "the crack in the wall of determinism is definitive" (p. 154). As a comment on von Neumann, this is fine but insufficient by today's standards, as some assumptions necessary to derive von Neumann's theorem have been called into doubt. Nevertheless, Waismann stands on solid ground as hidden variables theories suffered instead repeated setbacks with the Kochen-Specker Theorem[51] and with a key theorem about Bell's inequalities that shows that hidden variable theories will make predictions that are at variance with those of quantum mechanics,[52] a result which was eventually followed by experiments that confirmed quantum mechanics.[53]

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49 See (Bohm 1952), (Bohm 1957). For as recent discussion, see (Albert 1992).

50 (von Neumann 1955, chap. 4, sec.2).

51 (Kochen & Specker 1967).

52 (Bell 1966).

53 (Aspect, Dalibard & Roger 1982). Of course, convinced 'Bohmists' will argue that the issue is still not settled. But it would be dishonest to insinuate that the ball is in the camp of defenders of quantum mechanics and the 'Copenhagen interpretation'. Furthermore, Bohmians are nevertheless keen to point out that Bohmian mechanics is supposed to make the same predictions as quantum mechanics, re-establishing determinism at the price of a more complicated mathematical structure (alas, of lesser interest for physicists for that very reason) and the introduction of newer entities, e.g., the 'guiding wave'. Waismann's *Gedankenexperiment* (p. 159), quoted below, is a good reason to believe that he would have dismiss Bohmian mechanics for similar reasons (basically an application of Ockham's Razor): if there were no empirical tests to distinguish it from quantum mechanics, then statements concerning extraneous entities such as 'hidden variables' would be meaningless.

In the last sections, (9) to (12), Waismann turns away from modern physics and the issue of determinism, towards the philosophy of mind and action. So far, he had discussed only the view that causal relations are established through the observation of regularities. He now examines the thesis, which goes against his own 'regularity account', that one could discover causal relations through an 'understanding' of the relation between the cause and the effect. This view is found in A. C. Ewing's 'A Defence of Causality'.[54] Ewing's case rests around four claims: first, cause and effect do not just happen to follow each other with regularity, they are somewhat intrinsically connected; this point being linked with the idea that one may actually perceive this connexion. Secondly, the cause is (at least part of) the reason for the effect. Thirdly, the cause is said to produce the effect. Fourthly, causality involves necessity. Against this 'efficacy account', Waismann makes a number of related points.

The idea that the cause 'produces' the effect, i.e., the idea of 'efficient causation' is very sharply dismissed. Waismann even claims that it has its sources in children's and so-called primitive people's animistic conception of causality (pp. 161-162). Instead of serving us a fallacy of 'poisoning the well', he could have referred to a philosophical pedigree, starting with Maine de Biran. More seriously, Waismann points out, quoting Hume, that 'to cause' and 'to produce' are synonymous and cannot be used to define each other in a non-circular way (p. 156). But this conception of 'efficient causality', 'productive power' (p. 158) or 'active power' (p. 159) comes in for further criticism. One should note that the view here is not completely unrelated with recent views about 'causal powers' or 'capacities'.[55] Here, Waismann remains close to logical positivism, finding the view simply unintelligible. First, it appears to be modelled on our own voluntary agency: against this, Waismann quotes Mill and Hamilton on the case of paralysis to the effect that "if observation cannot even show us the manner in which the will acts upon the limbs or our mind, it can still less discover any quality in an event which makes it *produce* another one" (p. 157). Furthermore, this 'productive power' or 'quality' cannot be derived from observation by reasoning, because this would render quantum mechanics self-contradictory:

> […] if the existence of such a productive power can be inferred from our sense experiences, on purely logical grounds, then it would be a self-contradiction to say that the motion of the second billiard-ball is *caused* by the first, that is, *regularly preceded* by it, without being *actively produced* by it. Present day physics, having dispensed with the idea of efficient causality, would, if this account were right, be self-contradictory – a very strange consequence. The existence of an active power in an event which produces the effect can neither be produced in, nor logically inferred from, observation. (pp. 158-159)

Waismann also provides an interesting *Gedankenexperiment*:

54 (Ewing 1933, 98f.).
55 (Harré & Madden 1975), (Cartwright 1987).

> Suppose there was a region of the world, say A, in which everything held good that Ewing […] tells us – that is, in which the events were "intrinsically" connected with each other, so that the cause "actively produced" the effect; imagine another region of the world B in which the events merely follow each other, without being connected in this way; and imagine that the observable laws are the same in A as in B. What then, I ask, could be the difference between these two regions of the world, as far as their causal structure is concerned? Or how can we tell whether this world of ours is more like the part A or the part B? There is no way which we can tell; for there is no conceivable observation which is relevant to establishing the existence of such a relation. (p. 159)

The point of this *Gedankenexperiment* is the dismissal of claims such as 'there is an inward activity in the events' (such that the cause 'produces' the effect) as "neither true nor false, since we ourselves cannot tell what the supposed difference is to be" (p. 160). Such claims are simply "*devoid of meaning*" (p. 160).

Moving to the *Geisteswissenschaften*, Waismann quotes Georg Simmel (p. 159) on the issue of singular causation: perhaps it would explain the impossibility of finding laws in that domain (p. 160).[56] To Waismann, singular causation could not apply to the physical world unless one would postulate 'inward activity' or 'connexion' and this is unsatisfactory for the reasons just expressed. Against this, one could claim to have an 'insight' into the causal nexus. This is further countered by pointing out that this 'insight' is either a case of logical reasoning, as in 'If I cut a man's leg off, then he will have only one left', or something that rests on observation of regularities (pp. 162-164). There are only events following each other and no 'glue' or 'link', that would hold events together and which could be experienced (p. 164).

But one could further argue that there is room for singular causation in the domain of psychology: one can have an immediate awareness, an 'insight', into the connexion between cause and effect, thus one would have no need to wait until one has observed regularity. Here, Waismann quotes (pp. 165-166) and criticizes at length (the whole of section 10) Wolfgang Köhler, who adduced an number of examples in support of this point.[57] One such example is that of hearing an *alto* singing at the concert-hall and realizing that one's feeling of admiration was caused by the hearing of the *alto*'s voice. One is thus immediately aware of a causal connexion between the voice and the feeling of admiration. Waismann's point is that in all these cases, Köhler "confuses the *object* of a wish, of a feeling of alarm, etc., with the *cause* of the wish, the feeling of alarm, etc." (p. 166). It may be that consciousness exhibits directionality or intentionality (Waismann quotes p. 166 a famous passage by Brentano)[58] but the object of which one is conscious can hardly be said to be a 'cause'. In admiring the *alto*'s voice, one is immediately

56 (Simmel 1977, 106f.).
57 (Köhler 1930, chap. 10).
58 (Brentano 1973, 88f.).

aware of what it is that one is admiring, but this does not mean that one is aware of the cause of the admiration. The two are simply not logically related:

> The first statement – that [Köhler] knows that his admiration is directed towards the *alto* voice – in no way entails the second statement, that the admiration *depends* upon that voice. There is no logical connection between the two: the one may be true, and the other false. For example, if Köhler had happened to take a dose of mescal just before he went to the concert, he may have been in the disposition to admire anything he came across in the concert-hall that night. In such a case we should judge that the cause of his admiration was the mescal, and not the singing; though, even in this case, the singing was the *object* of his admiration. (p. 169)

Köhler always slips in his discussion between 'object' and 'cause', and Waismann concludes that his "whole philosophy of causation rests on a somewhat slipshod manner of expression" (p. 170), a very interesting critique indeed.[59]

Discussion of a further example by Köhler brings out key distinctions between 'cause', 'reasons' and 'motive'. Here, Waismann is moving into territory covered by the typescript 'Will and Motive' and his discussion is limited to making a few important points against confusing these notions; points related to the central thread of the lectures, i.e., his defence of the 'regularity account'. Köhler relates an evening at the restaurant:

> After sitting for half an hour in a restaurant, full of smoke and of talk all around d me, I feel 'nervous' and ready to go. My 'nervousness' *refers to* those properties of my environment. I know this, not only because in past experiences I may have discovered the rule that under such conditions I shall feel uneasy after a time. I experience myself directly as disturbed and confused *by* these surroundings.[60]

Köhler claims that in this case he has an insight into the cause of his action (leaving the restaurant), namely his uneasiness in this situation and the dislike of smoke and talk. As Waismann points out, however, this is not the cause but the *motive* for his going: "the *real* cause may be some excitement growing on his nerves, but he need not be aware of this cause" (p. 174). The uneasiness and dislike of smoke and talk are to be invoked instead as the motive for his action, supposing that he had been asked, and not to be confused with the cause. Waismann provides here reasons of a general nature to avoid such a confusion. He argues in two steps. In section (10), he argues for the distinction between 'cause' and 'ground' or 'reason' and in section (11), he distinguishes between 'reasons' and 'motives'. The distinc-

59 It is a pity that Waismann did not take also into account here Michotte's experiments that supposedly show that his subjects perceived causal connexions (even in cases where there isn't one). The "slipshod manner of expression" is all over the place in the subjects' own description of their perceptions. See (Michotte 1963).

60 (Köhler 1930, 273).

tion between 'cause' and 'motive' should be transparent from the discussion of Köhler's example:

[…] all his argument amounts to is that the motive is not discovered by induction: we are immediately aware of it. But this is precisely on of the differences between motive and cause. (p. 174)

To distinguish 'cause' from 'ground', Waismann makes three points. Here, Waismann is heavily indebted to Wittgenstein's discussion in the *Blue Book*, to which he keeps very close.[61] First, a causal explanation will appeal to processes situated in time, while a logical explanation will refer to timeless entities such as rules (p. 171). Secondly, contrary to causes, reasons cannot be discovered by observation (pp. 171-172). Here, Waismann appeals without reference to a version of what is known from Wittgenstein as the 'rule-following argument':[62]

Let us imagine that someone writes on a board the numbers 0, 1, 4, 9, 16 in this order. We, watching him, may suppose that, in doing this, he is following a definite rule, e.g., that he is writings down the squares of the integers in order. Have we now found out this rule by observation? Not at all: our supposed rule is merely a hypothesis, which would account for the numbers he has actually written down. But the figures written down are always subsumable under an infinite number of mathematical laws. How are we now to tell which rule he in fact followed? By making him continue the figures? But even if he wrote a thousand figures, he still might have been obeying any one of an infinite number of rules. (p. 172)

However, should the man tells that he has been following the rule $y = x^2$ for the values 0, 1, 2, 3, 4, we would then have the 'ground' for his writing down these figures and this is distinct from the 'cause':

So we must distinguish between *ground* and *cause*, for we learn of both in different ways. The *cause* for his writing down certain figures may lie in the fact that he was taught so in school and that this teaching has created a disposition, e.g. left definite traces in his nervous system and his brain; the *ground* for his procedure is the *rule* which he states when asked for the ground. (p. 172)

Thirdly, contrary to grounds, causes cannot be appealed to in order to justify an action (p. 173). In the above example, the appeal to the rule $y = x^2$ justifies the man's actions, but should he had made a mistake, giving the cause of his mistake would hardly count as a justification of it.

Waismann further distinguishes between 'reason' and 'motive' on similar grounds in the short section (11): a reason justifies an action, but a motive does not; motives "have no justifying power" (p. 178). In the above case of the man

61 Waismann even lifts silently at p. 174 a sentence from the *Blue Book* (Wittgenstein 1958, 15).

62 (Wittgenstein 1953, §§ 198-242).

writing the series '0, 1, 4, 9, 16' on the board, the rule $y = x^2$ provides the ground but this has nothing to do with his motive to do so (p. 178).

Waismann also provides a central point in his discussion of the ambiguities of the words 'why' and 'because' (pp. 174f.). In the sentence 'I believe this mathematical proposition because I have just seen its demonstration', one is not referring to the cause of one's belief but to its ground. Similarly, in 'I made an effort because I decided to do so and so', one is referring to one's motive and not to the cause of one's action.[63] Waismann would say that in both cases the relation is 'intrinsic' as opposed to 'extrinsic' when 'because-' refers to a causal connexion. Wittgenstein would have spoken here of, respectively, 'internal' and 'external' relation.[64] What is meant here is that the motive is fully determined by one's expression of one's motive, not by some relation to be found by observation. The point is of importance since it is the source of our view that, although they might be wrong, motives are not to be doubted in the sense that, as in the above case,

> [ …] there is no sense in questioning the motive given. It would be preposterous to appeal to some inductive evidence in the past to confirm that I made the effort because I had decided to do so. (p. 175)

In making thus essential distinctions between, on the one hand, 'motive' and, on the other hand, 'cause' and 'ground', Waismann has set the stage to his 'Will and Motive' but he also undermined attempts at refuting the 'regularity account' by showing how the rival 'efficacy account' cannot be supported by an appeal to singular causation coupled with a confusion between these notions.

In the final section (12), Waismann looks at Russell's account of desire as a "causal law of our actions"[65], an account that he finds "unnatural and perverted" (pp. 159 & 163). Waismann's main critique of Russell's account, which is taken from Wittgenstein,[66] is that Russell sees the connexion between a desire and its object as

> […] established by *experience* and, note, *afterwards*, after we have observed what it is that will bring a certain restlessness or discomfort to an end. (p. 181)

This view leads to absurd consequences. As Wittgenstein would put it:

> If I wanted to eat an apple, and someone punched me in the stomach, taking away my appetite, then it was this punch that I originally wanted. (Wittgenstein 1975, § 22)

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63 Here, Waismann stands apart from the sort of theory set forth by Davidson in 'Action, Reasons and Causes' (Davidson 1980, 3-19). and closer to Collingwood (Collingwood 1940, 285-337). On Wittgenstein's conception, see (Schröder 2001).

64 (Wittgenstein 1975, § 21).

65 (Russell 1921, lecture 3).

66 (Wittgenstein 1975, §§ 21-22).

Russell's mistake is to see relation between the desire and its object as 'experiential', while it should be 'semantic' (p. 183). (Again, Wittgenstein would have said 'internal'.) Waismann's discussion might appear at first sight unrelated to the rest of the lectures but this is not quite the case. Not that Russell should be seen as also providing support for the rival 'efficacy account', but because his account brings about further confusions about a central point Waismann had been trying to make in the previous sections concerning the distinction between 'cause' and 'motive'. Here too, the object of one's desire wish, expectation, etc. is "determined, fully determined by the expression" of the desire (p. 182); "*desire is tied up with language*" (p. 183). This is a key point, which is further defended by Waismann against the obvious counter-example of children and animals in the very last pages (pp. 183-184). Again, Waismann is moving here into territory covered by 'Will and Motive' and does not provide a full discussion.

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Friedrich Waismann

# The Decline and Fall of Causality

1. The year 1927 is a landmark in the evolution of physics—the year which saw the obsequies of the notion of causality. To avoid misconceptions, it should not be thought that the concept fell a victim to the unbridled antipathy of certain physicists or their indulgence in fancies. The truth is that men of science came, very reluctantly and almost against their will, to recognize the impossibility of giving a coherent causal description of the happenings on the atomic scale, though some of them—curiously enough, amongst them Planck, Einstein, de Broglie, Schrödinger—could never bring themselves to accept wholeheartedly so drastic a renunciation of classical ideals.

'But how can one find out such a thing? You mean physicists have not yet discovered the true causes?' No; I mean causality has definitely come to an end: atomic science has penetrated to a depth where an entirely new orientation is called for. As Niels Bohr put it: 'In the general problem of quantum theory, one is faced not with a modification of the theories describable in terms of usual physical concepts, but with an essential failure of the pictures in space and time on which the description of natural phenomena has hitherto been based.'[1]

The vast shift of perspective and, what goes with it, the change in our whole intellectual climate—that is the subject we must contemplate if we wish to reach a deeper understanding of the new outlook. In dealing with a matter of such dimensions, it is always good to choose a historical approach. This I shall do. I shall first touch upon the classical conception of causality as it gradually took shape in the work of Newton and his predecessors in the seventeenth century, and then discuss some of the reasons which led to its abandonment.

Attempts made by philosophers to clarify the notions of cause and effect, to formulate more precisely the content of the law of causality and, if possible, to throw light on the sort of validity which one may claim for it were not too successful. A much clearer picture of causality emerges from science.

2. Man has learnt mechanics from the stars. Indeed, the first great feat of the dawning age of science was the construction of celestial mechanics. Given Newton's laws of motion and the inverse square law, the problem of planetary motion can be condensed into a set of differential equations. Their solution has the following property: given the configuration of the solar system (the positions and velocities of the planets) at some initial time, the configuration at any other time can be calculated with any degree of accuracy without further recourse to observation. In other words, the solar system is such that its initial state determines its entire

1 *Atomic Theory and the Description of Nature*, Cambridge, 1934.

B.F. McGuinness (ed.), *Friedrich Waismann – Causality and Logical Positivism*,
Vienna Circle Institute Yearbook 15, DOI 10.1007/978-94-007-1751-0_4,

future; and 'determines' here means: makes computable on the basis of laws stated in precise mathematical form.

This, in essence, is the scheme as it emerged from the *Principia*. In Einstein's words, 'Newton's theory of gravitation is probably the greatest stride ever made in the effort towards the causal nexus of natural phenomena.'[2] It should be noticed that this scheme departs from the pre-scientific idea of causation. The popular view is openly animistic: the cause brings forth the effect, produces it. On a more enlightened level—say, on that of Hume or J. S. Mill—we meet with the idea that two events, *C* and *E*, are related as cause and effect if they fulfil certain conditions, namely if (1) they are contiguous in space and time (2) *C* precedes *E*, and (3) C is unfailingly followed by *E*. A fourth trait that seems to cling to the idea, necessity or compulsion, was submitted to a destructive analysis by Hume: he did not 'deny' causation as one so often hears it said, he tried to purge the concept from extraneous elements. Now what our example shows is that this notion, whether the naive or the purged one, does not apply here: it would be most arbitrary to pick out one configuration of the solar system and call it the 'cause', and another such configuration and call it the 'effect'. Indeed; in the symbolic language evolved in science these terms drop out altogether. What takes their place? The concepts of functional dependence. We find phenomena. so correlated in nature that, when one measurable quantity, characterizing a certain physical state (say, the Earth's distance from the sun), varies, this corresponds to a change of another (say, the Earth's acceleration towards the sun), according to a simple mathematical rule. The one quantity (acceleration) is then said to depend upon, or to be a 'function' of, the other (distance). A well-known example is the way the pitch of a note (expressed by the frequency of a vibrating string) varies with the length of the string, is a function of it.'[3] It should be noticed that, if one variable quantity is a function of another, this does not imply the existence of any causal relation between the two, but merely a correspondence between the values they may take. Thus the pressure of a gas contained in a closed vessel depends on its temperature and its volume. This dependence is expressed in the law

$$pv = RT,$$

where $p$ denotes the pressure, $v$ the volume, $T$ the temperature, while $R$ is a constant. This equation may be solved for any of the three variables as a function of the two other ones,

$$p = RT/v \quad \text{or} \quad v = RT/p \quad \text{or} \quad T = pv/R,$$

without suggesting that a change in volume or temperature is the 'cause' of a change in pressure, or *vice versa*.

The knowledge of such laws enables us to derive, and thus predict, the future

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2 *Sidelights on Relativity*: I, *Ether and Relativity*. II, *Geometry and Experience* (trans. G. B. Jeffrey and W. Perrett), London, 1922.

3 A discovery ascribed to Pythagoras.

from the present, generally the unknown from the known.[4] For, to return to the solar system, information about its present state can similarly be used to calculate the condition in which it was at any past moment of its history, the reason being that the equations of dynamics are insensitive to a change in the direction of time —i.e. remain invariant under a change from $t$ to $-t$.

It would lead us too far to relate in detail how a system of dynamics was built up capable of accounting for the phenomena in the heavens and on earth. The decisive step was the introduction of the quantitative method through the work of Galileo. On its basis, a rational formulating of the laws of nature became possible, culminating in Newton's *Principia*. With the rise of the quantitative method, causality began to take on a new aspect. Briefly, the question 'why' came to be superseded by the question 'how'. Indeed, when Galileo turned to inquire *how* bodies fall and not *why* they fall,[5] this must have looked to his contemporaries rather trivial, a side-stepping of the deeper issues involved. Yet it was this 'side-stepping' more than anything else that started science on its career.

One may, of course, still think of gravity as the true cause underlying free fall and all the rest: but then gravity, or force in general, is not an *event*, while only events are supposed to stand in causal relationship. What the scientist has in mind when he expresses himself in some such way is that Newton's law of attraction is part of the formulae which govern planetary motion. However, and this is the point, in these formulae there does not occur anything that answers to the popular idea of cause and effect. They describe what happens under given conditions, they do not state the cause (read: the reason) *why* things happen in the way they do. I do not mean to say that there is *no* causal nexus: there is, only not in the sense envisioned by philosophers, namely as a relation between two events: this scheme, lacking the right sort of flexibility, must be dropped. The point had not escaped Newton when he declined to go in search for a cause of gravity ('*hypotheses non fingo*', General Scholium of the *Principia*).

Extending the result to other domains, we may say: the laws of classical physics are such that, if the state of an isolated system defined at a certain point of time is known, it can be computed for any other instant so that the whole course of events can be plotted in advance. That is the essence of determinism. Physicists were successful in establishing laws of such a type also outside celestial mechanics, though for certain limited fields only—e.g. for the phenomena of heat conduc-

4 'The most direct, and in a sense the most important, problem which our conscious knowledge of nature should enable us to solve is the anticipation of future events, so that we may arrange our present affairs in accordance with such anticipations.' H. Hertz, Introduction to tM Principles of Mechanics (translated by D. E. Jones), London, 1899.

5 'It does not seem expedient to me now to investigate what may be the cause of acceleration' (*Discorsi*, translated as *Dialogue concerning the two chief World Systems*, Chicago, 1953, third day), his main concern being to find a law according to which it takes place.

tion or those of electromagnetism in free space. For cases of the latter sort—where processes are supposed to take place in a continuum and propagate by nearby action in contrast with Newton's gravitation presumed to act instantaneously across empty space—the principle of causality must be reformulated, e.g. as follows. What takes place at a certain point at a given time depends entirely upon what has been happening in the immediate neighbourhood 'just before'. (That is a crude way of putting it: technically speaking, ordinary differential equations have to be replaced by partial ones and initial by boundary conditions—a further step towards the shaping of the concept of causality.) However, it would be wrong to close our eyes to the fact that the situation is different in other fields: there are, for example, no similarly precise laws in biology—no one could have foreseen the exact ways of evolution; and even in the science of inanimate nature, for so long considered the stronghold of determinism, there are kinds of phenomena—e.g. Brownian movement, turbulent flow—which resist causal analysis.

What, then, are we to think of the dictum 'Every event has a cause', i.e. is determined by laws—often referred to as the 'Law of Causation'? For some ranges of facts it seems true, for others doubtful. Actual evidence is far too meagre to substantiate such a sweeping statement. Even in those cases which may seem most favourable to such an interpretation, the validity of a law can never be established with mathematical precision. Nonetheless belief in a universe governed by laws which allow of no breaks was steadily growing until it became almost an article of faith. Thus according to Helmholtz, 'The causal law bears the character of a purely logical law even in that the consequences derived from it do not really concern experience itself but the understanding thereof, and that therefore it could never be refuted by any possible experience.'[6] On this view, it is nothing but the demand to understand. In another passage, however, he is more outspoken, declaring that 'the final aim of all natural science is to resolve itself into mechanics', at a time when the very idea of mechanics was still closely linked to that of an iron chain of relentless necessity. 'The great abstract law of mechanical causality', wrote the biologist E. Haeckel,[7] 'now rules the entire universe as it does the mind of man', and so on. This was written one year before the discovery of quanta which was to knock over all our ideas on the subject.

3. Before turning to the new ideas, a word on the rise of the notion of causality. Strange as it sounds, belief in a firm Causal Order of Things has not always held sway in the dominion of science. It rather is the result of a long historical development which began with Kepler and Galileo and reached its height with Laplace. Growing out of the new science, that conviction may, in its career, have absorbed into its meaning traces of an earlier outlook, which still deeply colour its meaning.[8] Be that as it may, in Newton's time, at any rate, determinism had yet to prove

6 *Physiologische Optik*, III, translated as *Helmholtz's Treatise on Physiological Optics*, New York, 1924.

7 *The Riddle of the Universe*, London, 1929.

8 See section 5.

its worth. Nothing is more characteristic of this than the attitude taken up towards a problem which then began to arouse the interest of mathematicians and astronomers alike—is our solar system *stable*? Will Saturn wander off into space as a consequence of its slowing down and receding from the sun, or will it remain a member of our system? Will the acceleration of Jupiter cause it to come nearer and nearer to the sun in ever narrowing spirals until it crashes into the sun? And is a similar fate in store for our moon—to come smashing down to earth? Or will these heavenly bodies reverse their present trends before that happens? In other words, will the perturbations increase with time, piling up to disastrous dimensions, or will their effects merely oscillate between certain definite limits? Such questions are part of the problem of the stability of the solar system. Newton himself was of the opinion that divine intervention may be necessary from time to time to put the solar system back to order and save it from destruction, either through collisions of its members or by the planets scattering into space[9]—an echo, perhaps, of the medieval view that God participates in the day-to-day running of the universe? Even Euler had his doubts: there were too many forces involved, with interactions too complicated to subject them to mathematical analysis. Newton and Euler represent, between them, the temper of an age when the idea of causal determination had not yet attained the rank of an undisputed principle, and when scientists did not wriggle away in horror at the suggestion of God's possible intervention in the universe. It was only later when Laplace came to attack the 'grand problem', that belief in an unbroken causal chain was vindicated. He was able to show that notwithstanding perturbations and fluctuations in the planets' motions caused by their interaction the solar system will retain its inner structure. In particular, the length of the Earth's year is invariable—a fact of obvious significance for the continuance of organic life on this planet.[10]

With Laplace the principle of causality began to take on its present form, in which it was to reign supreme for more than 150 years. In the solar system everything operates with clockwork precision. There are no hidden conditions, and hence no possibilities of surprise. As with the sun and its family, so with any self-contained physical system. Determinism, with its belief in a gap-less chain of causes, came to be regarded not so much as a heuristic principle, a lodestar in the light of which certain domains of fact can be ordered, as an inherent feature of nature herself. Any account that failed to conform to these standards—e.g. in biology—was deemed unsatisfactory and dismissed as 'unscientific' (see Haeckel).

It was this success that led Laplace to proclaim determinism in a most rigor-

9 Newton's reason for holding such a view is the existence of 'some inconsiderable irregularities' in the otherwise concentric orbits of the planets, 'which may have arisen from the mutual actions of comets and planets upon one another, and which will be apt to increase, till this system wants a reformation.' (*Opticks*, Query 31.)

10 Proved by him in 1773. It may be remarked, in passing, that his solution holds only of a strictly mechanical system (the 'ideal' case), leaving out of account such things as tidal friction (partly dissipating into heat), light pressure, and so on.

ous and uncompromising form. In his *Philosophical Essay on Probabilities*[11] he says, in words justly celebrated for their verve and vision: 'We may regard the present state of the universe as the effect of its past and the cause of its future. An intelligence which at a given moment knew all the forces that animate nature, and the respective positions of the beings that compose it, and further possessing the scope to analyse these data, could condense into a single formula the movement of the greatest bodies of the universe and that of the least atom: for such an intelligence nothing could be uncertain, and past and future alike would be before its eyes.' You see, the problem of the solar system all over again, only magnified to comprise the whole universe! The world an immense machine moving with absolute precision. The essence of this view is that all events in nature are predetermined by law, and that this determinism permits of no flaws in the chain of causes.

It was, however, for Lamettrie to carry this view to its utmost logical consequence, declaring (in his book *L'homme machine*)[12] that man is a machine. Every atom in the world obeys laws, whether it is part of a lifeless substance or of a living organism. Every atom of the human body must therefore move according to the same mechanical laws which govern the motion of the planets: its behaviour is uniquely determined by the configuration and state of motion of all the other atoms in the universe. But if so, there cannot be such a thing as freedom of choice: what a man will do, the words he will utter, even how he will feel and think, all this is the necessary outcome of the events preceding it. Man, then, acts like a machine, a clock, following the same blind mechanical necessity as everything else. A formidable argument. The idea of complete and unbroken determinism has always bewitched thinkers since the time of Democritus. Everyone, of course, feels that there is something wrong with it: human behaviour is not as exactly foreseeable as a lunar or solar eclipse. Yet that does not answer the argument. Indeed, if one holds on to the Laplacian program, according to which the fate of every single atom is predetermined, there seems no way out from the conclusion that every limb of our body must move with necessity in the way it does, and that our persuasion of being able to move our limbs freely as we please must be due to some illusion. Given the premisses, the conclusion seems unavoidable. No one can act against the laws of nature: and to act in accordance with them wouldn't *be* acting—I would not be the doer of my deeds. Nature would rather act *through* me, and I, a mere onlooker, would passively follow the movements of my own mouth when I speak, of my own hand when I write, … That is the dilemma. Theorists were haunted by it for centuries. It took scientists a long time to find out what was wrong with it. In fact, it was not before the rise of a non-causal mode of thinking, expressed in the uncertainty principle, that the ground was cut from beneath this outlook: at least one weak link of the underlying view of nature was laid bare and disposed of. (Some disagree, e.g. Schrödinger.)

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11 Translated by Truscott and Emsey, New York, 1902.

12 Critical edition by A. Vartarian, Princeton, 1960.

4. Now for a more philosophical point. What is regarded as a rational explanation, as 'satisfactory', 'natural', 'acceptable' and what not is, in part, tied to the whole climate of opinion of a time and apt to change with it, and in part, and this seems to me the more important aspect, tied to something unhistorical: the rationality of a certain way of looking at the world. To illustrate what I mean, predilection for mechanical explanation, quite foreign to Aristotle but typical of the period from Newton[13] to Einstein, or belief in the continuity of nature as expressed in the adage *natura non facit saltus*[14]—these represent not so much objective truths as attitudes of mind which, for a time, hold thought in a firm position. Another such attitude, only much more deep-rooted and hence much more difficult to dislodge, is belief in an immutable causal order. Above all, however, the scientist is guided in his research by a deep faith in the comprehensibility of nature, by the conviction that notwithstanding the kaleidoscopic character of phenomena, there lies some simple and clear pattern that can be fully comprehended—a faith not arrived at on the strength of evidence but prior to it and, in this sense, not rational.[15] As Planck says: 'Faith points the way and sharpens the senses.'

When a 'new attitude became firmly established, men sought to make it seem objective and necessary by giving it a philosophical setting … The people who are under the influence of a 'Weltanschauung' want to *fix* it, to make it seem not so much a particular *attitude* as a *necessary fact*. They then endeavour, by expressing it in a metaphysics, to give it a universal validity.'[16] This is one of the great rôles played by metaphysics in history, though not the only one. To put it differently, an attitude has a propensity to crystallize into a metaphysics, just as, conversely, a metaphysics is, in part anyhow, an intellectualization of certain attitudes.

A conspicuous instance of this process is Kant. He was writing at a time when the ideas of Newton had gained currency and the principle of causality began to dominate the scientific scene. Curiously enough, the same laws which, not so long ago, because they failed to conform to *their* standards of satisfactoriness, were spurned by the schoolmen as absurd, at the most as superficial, evading the real issue, were now hailed as rational, transparent to the mind like geometry, and, like geometry, they were raised to the Olympian heights of the *a priori*. The idea of causality (which after all was not yet so firmly implanted in Newton's mind) was now given the status of a category or of a rigid scheme without which an understanding of reality would be impossible. The Law of Causation came thus to be turned into a presupposition for the *possibility* of experience so that it can-

13 'I wish we could derive the rest of the phenomena of nature by the same kind of reasoning from mechanical principles.' (Preface to the 1st ed. of *Principia*.)

14 According to Buffon, all classifications in biology are an 'error in metaphysics', in so far as we see discontinuous species, genera, etc. in nature where, in actual fact, there is only a great chain of beings.

15 'The eternal mystery of the world is its comprehensibility.' (Einstein, 'Physics and Reality' , *J. Franklin Institute*, vol. 221, pp. 349-382).

16 T. E. Hulme, *Speculations*, London, 1924.

not be criticized on the basis of experience.[17] In the *Critique of Pure Reason* Kant said that whenever something happens it 'presupposes something upon which it follows according to a rule'. Between cause and effect, moreover, there exists a 'necessary connection', and as this has no empirically verifiable meaning (Hume), Kant concluded that its origin must be sought in the nature of our intellect which impresses its own mark on the things.[18] In the *Critique* the principle of inertia, of action and reaction, and of conservation of matter is claimed to have *a priori* validity, while in the *Metaphysical Foundations* even the law of gravitation, and in the *Opus Postumum* the whole body of principles of Newtonian mechanics is presented as a priori truths independent of experience.

It should be noticed that Kant published these ideas in 1781 (first ed. of the *Critique*), that is 8 years after Laplace had shown the solar system to be governed by laws which leave no loop-hole for supernatural interference. Kant, who took a deep interest in astronomy (and even made a contribution to it), must have been aware of Laplace's work and the stir it made at the time. This may illustrate what I have been saying—that new ways of thinking evolved in science, when they have become firmly established, tend to harden into metaphysics so that they are regarded as necessary, as a sort of inescapable grooves within which thought is bound to run. The historical tracing of the rise of such ideas is then a means of loosening up such a cramped attitude and regaining intellectual freedom.

It was only C. Maxwell who raised a warning voice against the taken-for-granted manner in which determinism was accepted by his contemporaries. 'The promotion of natural knowledge', he wrote, 'may tend to remove that prejudice in favour of determinism which seems to arise from assuming that the physical science of the future is a mere magnified image of that of the past'. Prophetic words, but of course unheeded at the time.

5. As shown in the foregoing, the idea of causation, at first pretty vague and shot through with animistic elements, became narrowed down and refined into a differential law, expressing the idea that the course of natural phenomena is uniquely determined by certain conditions. This by no means exhausts the account of the matter. To mention only one question, if laws of any degree of complexity are permitted it is always possible (as shown by Fourier[19]) to subsume a given sequence of events, however irregular, under a mathematical law.[20] The principle of causality would, in this case, become devoid of factual content and turn into a triviality. But physics is not trivial. Two ways offer themselves to escape that con-

17 Cf. Helmholtz.

18 'The understanding does not draw its laws from nature but prescribes them to nature.' (*Prolegomena*, § 36.)

19 *Théorie Analytique de la Chaleur*, translated as *The Analytical Theory of Heat*, New York, 1955.

20 Mathematically speaking, any curve, however haphazard and bizarre—provided it is continuous within a certain interval or has only a finite number of discontinuities—can always be covered by a formula (Fourier series).

sequence. One, suggested already by Maxwell, is to demand that the run of events should be completely independent of place and time, that is, such that a causal connection is universally valid. Against this it may be said that the 'constants of nature' (e.g. the constant of gravitation) need perhaps not be absolutely constant but may slowly change with the age of the universe, and with them the laws of nature, in which case a historical element would enter into their formulation. In point of fact, several recent cosmological theories envisage just such a state of things. It seems therefore wiser not to commit ourselves to a definition which rules out such possibilities from the start. The other possible way is to demand that a law should be (a) simple and (b) such that it can serve as a basis for making predictions. The queer and rather surprising thing is that the traits (a) and (b) seem to go together—a fact often adduced as a sign of a 'rationality' of nature. Without going into this point, perhaps only this much may be said. As there are no precise rules for determining what is and what is not 'simple' (the concept of simplicity being half aesthetic, half pragmatic), the notion of causality, so far as it is based on it, seems also to lack precision so that it is not possible to state exactly what is and what is not subject to causal laws—a discouraging result as 'we shall certainly not be prepared to formulate the difference between chaos and order by saying that the former is accessible to an eminent mathematician only, the latter to a mediocre.'[21]

Now for another aspect. It is true that the terms 'cause' and 'effect', and the observation of the regularity with which they follow each other, are very useful in practical life and in the infancy of science. Thus it is often cited as an instance of a causal relation that a body, when deprived of support, falls to the ground. Here we have apparently all the characteristic traits of causality as enumerated by Hume or Mill: the causing event is 'depriving the body of its support' , the effect 'falling to the ground', and the two are connected by regularity. As J. S. Mill says, 'The Law of Causation, the recognition of which is the main pillar of inductive science, is but the familiar truth, that invariability of succession is found by observation to obtain between every fact in nature and some other fact which has preceded it.[22]

But to say such a thing does not penetrate to the heart of the matter. What is overlooked here is that science, instead of establishing connections between facts *as such*, rather analyses them. This analysis consists in decomposing (in thought) the unique course of actual events into elements which are simpler, recurrent and capable of measurement. The 'uniform' relations of which philosophers are so fond are to be found not between the events themselves, but rather between the elements into which they have been dissected. To give an example, the astronomer decomposes the acceleration of our moon into a part due to the attraction of the earth and into a part due to that of the sun; and he does so despite the fact that none of these partial accelerations has ever been the subject of observation: they are

21 M. Schlick, 'Die Kausalität in der gegenwärtigen Physik', *Ges. Aufsätze*, Vienna, 1938.

22 *A System of Logic*, Bk. III, chap. v.

theoretical constructions. What can be observed is the phenomenon as a whole. Yet it is only when it is so dissected into components that clear and simple laws emerge. The event as such is without interest to the scientist. There is no science which deals with an event such as ‘the sun rising above a sea of purple’. Far from aiming at producing a faithful and detailed copy of reality, science is an endeavour to comprehend it. And this it does, firstly by analysing it into component parts, then seeking for laws to connect them and finally reconstructing reality out of these elements—or, in short, by analysis and synthesis. One tries therefore in vain to set up laws which, as Mill puts it, connect ‘every fact in nature’ with ‘some other fact which has preceded it’. This is not and has never been the program of the natural sciences. At best, it is a beginning towards describing the methods of research.

It will by now, I hope, have become clear what is wrong with the accounts of causality offered by most philosophical writers. Science is concerned with the relations holding between the simplified elements into which the facts of observation are dissected. It is when these elements are represented by quantities that functional dependencies between them may be recognized leading to the discovery of physical laws. The *law of nature* thus takes the place of the relation between cause and effect and makes it redundant to analyse this relation more precisely. At the same time the difficulties encountered prevent the concept of determinism from assuming sharp outlines.

The expression ‘law of nature’ is, in its use, intimately linked to that of ‘explanation’. In the history of it we shall find stored up—like layers of rock deposited one on top of the other—different senses, reflecting differing modes of thought some of which have survived to the present day and still live on as overtones of the expression or as half-conscious associations. The idea of a law of nature is essentially *modern* in origin and belongs to a whole cluster of ideas which came to be expressed at about the same time—indicating an important change in human thought when the need was felt to have names or words to stand fo; various activities and ways of looking at things. Such words as ‘arrange’, ‘classify’, ‘organize’, ‘category’, ‘method’, ‘system’, ‘systematic’, ‘regular’ appeared with their modern meaning in language. Only two of them are earlier than the seventeenth and most of them are not found till the eighteenth century. Thus ‘arrange’ was a military term like ‘array’, and ‘regular’ was used only of monastic orders until the close of the sixteenth century.

The Latin *lex* was first applied to material phenomena by Bacon. Later in the seventeenth century ‘law’ came to be used in the same sense, though it did then not quite mean what it does today. The laws of nature were conceived of at that time as commands of God. It is not without significance that the expression is found first in the hands of theologians—of those, for instance, who followed the teachings of John Calvin. This protestant leader was working towards a conception of God as the Absolute Ruler of the universe, governing it by laws decided upon at the beginning of creation. This theological use has left traces upon our language—we still speak today of nature 'obeying' laws, although we no longer think of them as being imposed upon nature by the will of the Divine Legislator.

It was only by a slow and not always smooth process of emancipation that the idea threw off its more obvious theological associations and acquired a new significance. Galileo, for instance, called his quantitative rules 'principles', 'ratios' or 'proportions'. His 'principle of inertia', restricted to terrestrial objects, is, apart from this difference, the same as Newton's 'first law of motion', Newton using the term 'law of nature' freely as it had become current at his time, though not unopposed by some, such as Robert Boyle, who thought the term 'an improper and figurative expresssion'. When an arrow is shot from a bow, he wrote, 'none will say that it moves by a law, but by an external impulse'. Thus, 'law of nature' was at first construed as a command of God and then as something inherent in nature. 'Nature is constrained by the rational order of her law which lives infused in her'—this dictum of Leonardo da Vinci marks, perhaps, the point where the one conception turns into the other.

The change of meaning is itself indicative of the more rational view of life which was beginning to dominate men's minds. It was the time when the miraculous was falling into disrepute and Hobbes protested against the element of the supernatural in the medieval 'romances'—the same time when the word 'romantic' made its appearance as a derogatory term to brand what is false, hollow and unnatural.[23] Hobbes' writing is characteristic of the Age of Reason which rose in the later part of the seventeenth century (the time of Newton's manhood). Intellectually, men's minds seem to have been influenced above all by that conception of impersonal law and order governing the universe—a conception scarcely entertained in the preceding century. Poets and philosophers alike were delighted by the perfect *order* in which, in their view, the cosmos was arranged. The appreciation of nature's regularity, rather trite to us, was for those men a source of poetic inspiration—anyhow filled them with enthusiasm. ('Enthusiastic' like 'fanatic', which originally meant 'possessed by a god or demon', underwent a similar inflection of sense about the middle of the seventeenth century.) For us, breathing in a different air it is almost impossible to recapture that mood, yet it forms the background against which the rise of the term 'law of nature' in its modern sense must be seen. It is but one term of a whole group whose more or less parallel changes of meaning curiously reflect that movement of thought.

The great watchword of the time, however, was *Reason*. It must have exerted a deep fascination as it was turning up time and again in the writings of the period—occupying, e.g., the central place in the systems of Spinoza, and generally in the philosophy of the rationalists. In Spinoza particularly, we seem to catch a note of that majestic harmony of celestial mechanics which was just growing up towards the end of his life. Indeed, the program of this school of thought was to apply the mathematical mode of reasoning to *all* problems, whether metaphysi-

23 See, for instance, L. P. Smith, *Four Words* (S.P.E. Tract 17).

cal, moral or scientific—to exalt the powers of reason and rational method at the expense of blind faith, revelation and Fancy, just as this power was glorified by Milton—

> whence the soul
> *Reason* receives, and *Reason* is her being (*Paradise Lost*, V).

The cult of Reason suggested belief in the rationality of being and thus created the atmosphere in which science could thrive. Withhout such belief there would be no science, though it need not be expressed in words. Those who did give expression to it, the rationalist thinkers, paid only lip-service to the spirit—today their systems are petrified and belong to the curios of history, while the faith that prompted them lives on in science, opening ever new horizons.

I shall not endeavour to trace the origins of the idea of a law of nature as that would mean to go back to the Greeks and even to their predecessors in the East—a job for which I don't feel qualified. What I have in mind was no more than to cast a sidelight on its chequered career. For in the light of that history we begin to understand why words like 'law' or 'cause' tend to change their meaning with every context and let us see each time other facets. The idea of fate or destiny standing above men and gods, as we find with the Greek tragedians, the aesthetic conception of mathematical harmony as expressed in the Pythagorean 'cosmos', the biblical idea of God as the law-giver of the universe associated with that of rationality—these are some of the sources which have contributed to deepen the term with their significations. Growing out of its long history, the word has absorbed into its meaning ideas and modes of thinking inherited from many different ages, races and civilizations. Part of its meaning has been shaped by the Pythagorean brotherhoods who led a life of mathematical and religious contemplation. It has travelled from Babylon to the Holy Land where it was in the hands of the priests, and from there to Rome, to the Stoics, taking up some of the teachings of Democritus. Then, at the time of the Renaissance, it came to the northern countries until, with Newton's *Principia*, it became the cornerstone of science. In the course of this Odyssey, the term underwent a series of strange transformations until, with the birth of modern science, it definitely took shape in the form of a mathematical equation. Yet, in the interpretation given to the modern formulae by physicists and philosophers, much of the older signification is still alive or at least dormant—like the pagan gods who, masked as demons were living on into the Christian era.

6. To return to Laplace: if there *were* a being that *had* the power—. If—! It all seemed a question of efficiency. In *principle*, it was thought, all the information as regards the particles in the universe *could* be gathered; in principle, the equations for them could be written down; in principle, they could be solved; in principle the entire future could be foreseen. That to accomplish such a task far surpasses our faculties seemed no objection as this was entirely due to our limitations. If, faced with a system of enormous mechanical complexity—such as a roulette wheel—we resort to probability laws, we avail ourselves of a makeshift device that only

reflects our ignorance but could, in principle, be dispensed with. We are in the position of a millionaire who, instead of insisting on a detailed account of the state of his finances acquiesces in being given a rough estimate.

Laplace's calculator represents the high-water mark of the tide of determinism as it was rising steadily since the beginnings of modern science. However, the words in which he proclaims the triumph of determinism at the same time reveals its weakness (though this was not noticed for a long time). For what is requisite for his program is detailed, i.e. *complete* and *precise* information as to the state of all the particles in the universe at a given instant of time. Hence the question arises: is this possible? Disregarding completeness for the moment and considering precision, it was of course always recognized that absolute precision is out of reach, only this was minimized: the accuracy of any measurement, It was supposed—rather light-heartedly as it would seem now—could be increased to any degree by improved technique. Irrespective of whether absolute precision will ever be attainable with our blunt instruments, we can at any rate go on refining our measuring methods, it was assumed, and proportionally our predictions concerning the future will become more and more reliable. That there is *no limit* to this approach, this was, ultimately, the tacit assumption underlying classical physics, and one so brilliantly vindicated by the successes in astronomy.

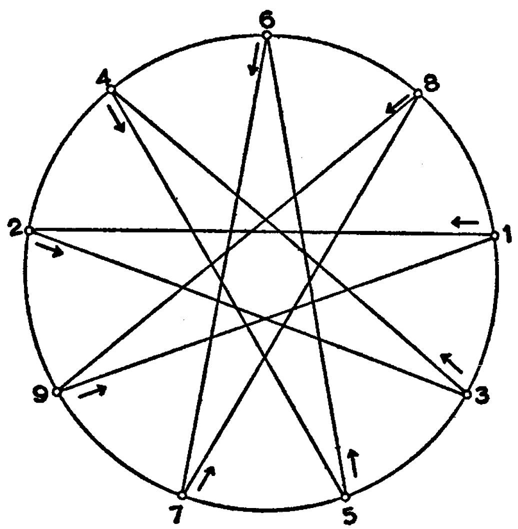


Fig. 1

To come from the heavens down to earth, is it really quite certain that mechanics permits accurate prediction under all conditions?[24] Let me consider a simple

24 Doubts as to that were first raised by R. V. Mises (*Probability, Statistics, and Truth*) 2nd ed., London, 1957, and even before in an article in *Die Naturwisssenschaften*, 1922.

case to throw light on the question.[25] Suppose an elastic ball moves freely on a surface, say, a round board at the edge of which it is elastically reflected. Both ball and surface are supposed to be perfectly smooth and elastic so that no friction occurs. Once set in motion, the ball will, under the idealized assumption, move on for ever, rebounding from the wall, thus providing us with a sort of perpetual billiards. Given the initial conditions (position and velocity), what will be the track described by the ball? There are two widely different possibilities: the ball may describe a closed track of finite length so that the motion will be a periodic one, or it may start on a zigzag path never to return to a position occupied once before. (Figures 1 and 2). What sort of track will be followed depends on the initial state. What is peculiar about this case is that small causes will have big effects. Indeed, the slightest change in the initial state, say, in position or direction of motion may

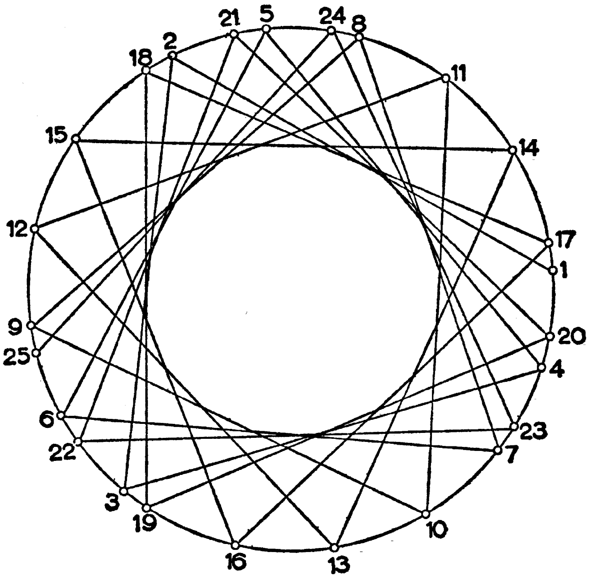


Fig. 2

be enough to turn a closed path into an open one. Can we in this case really predict the path with certainty? Only under one condition: that position and direction at the start are known with *complete accuracy*. The least error in these data would foil any attempt on our part at predicting the future. The simplest case of a periodic motion is that the ball just rolls across the board from end to end, traversing a diameter. What will happen if the ball starts not exactly in the line of the diameter but turns aside from it by a very small angle $\phi$? In this case, the ball will arrive at the point 2' instead of at 2. (Fig. 3.) Suppose the line 1-2' subtends an angle $\alpha$ at the centre of the circle, then the sort of track described by the ball will depend

25 For a similar example cf. M. Born, *Physics in My Generation*, London, 1956.

upon the ratio $\alpha/2\pi$: if this ratio is a *rational* number, say, $p/q$, the ball will, having been reflected at the wall exactly $q$ times, return to its original place, while in the opposite case (that the ratio is *irrational*), it will never return. Suppose now that we do not know precisely the initial angle of divergence, allowing for a latitude $\Delta\phi$; then we cannot decide to what category the track will belong, whether it will be an open or a closed one. Conditions may even be such that the uncertainty $\Delta\phi$, however small at the beginning, may increase with time (Fig. 4) so that, if we only wait long enough, the ball may at a given moment be found *anywhere* on the board. In such a case, we should know absolutely nothing about its position, and determinism has turned into complete indeterminism.

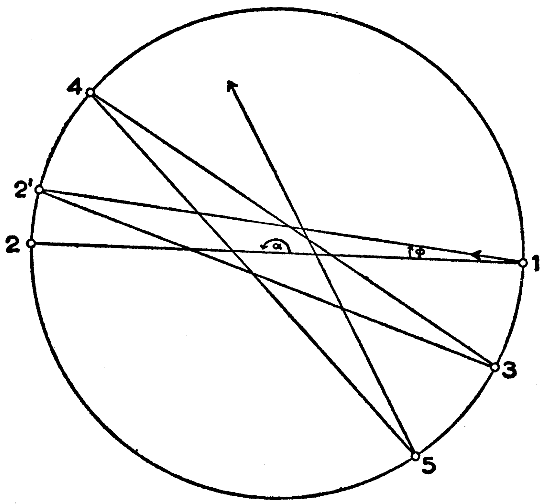


Fig. 3

To avoid such a consequence one has to demand mathematical precision regarding the initial state. That, however, raises a serious question—namely, is there any sense in speaking of such a thing? Suppose the distance of the ball's surface to the wall is measured at a given instant, does it make sense to say that it amounts to two centimeters? Owing to the coarse-grainedness of matter, the ball's surface is not a mathematical surface, any more than that of the wall. Seen from close by, such a surface dissolves into a cloud of molecules, blurred and ever-changing. The same is true for a measuring rod—the particles at its ends perform a wild, irregular dance, blurring its length. In view of this, one comes to recognize how utterly Utopian the idea of absolute precision is.

The point of our example will now be seen. Even in classical mechanics the causal scheme does not always work, not under all circumstances. Whether it works or not hinges on one condition pthat measurements can be made with unlimited accuracy. Causality stands and falls with this requirement. Should for some reason or other its fulfilment be impossible, we should be prevented from making predictions, and the principle of causality, so far as its criterion is predictability, would break down: though not ‘false’ in the current sense, it would become inapplicable since its presupposition is unrealized.

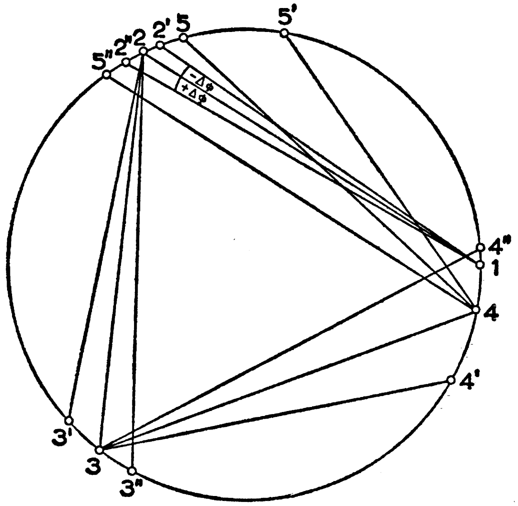


Fig. 4

A last point: it should not be concluded that the example discussed provides a case of *acausal* motion. One would rather be inclined to say that there exist circumstances (such as a slight unevenness on the ground or the surface of the ball) which are too small to be detected in ordinary observation but which none the less determine the path in the normal causal way. Taking into account such ‘hidden parameters’, there may still be a gleam of hope of maintaining the idea of determinism. In the *initial* phase, anyhow, the course can be predicted, and it is only for the more distant future that it becomes uncertain. Thus the case presents a curious half-way house, showing not so much the fall as the decline of causality—the point, that is, where the principle begins to lose its applicability. The uncertainties we shall meet on the atomic level are of a much more radical sort, with the consequence that we shall be forced to give up entirely the idea of deterministic laws in favour of statistical ones.

To sum up: determinism is an *idealization* rather than a statement of fact, valid only under the assumption that unlimited accuracy is within our reach, an assumption which in view of the atomic structure of our measuring instruments is anything but realistic. It is chiefly in astronomy, the foremost domain of precision, that we can at least *approach* the classical ideal.

7. The story of the mechanical conception of nature, with its stress on mechanical models, stands in marked contrast with that of the causal conception.

Both have come to an end, though in different ways: for while the former was never really refuted in a strict sense but, suffering decline, was allowed to die quietly of sheer inanition, the end of causality came dramatically, with a bang, not a whimper. I am speaking of Heisenberg's uncertainty principle.

First a remark. As we descend to the atomic level, we are entering a strange world, stranger than anything out of the Arabian nights. Indeed, we should not expect, in a field which lies so far outside the reach of our senses, to find the same sort of relations and laws as those which hold in our large-scale world. Of course, all new experience makes its appearance within the frame of our perceptions, and is describable with the words of plain language: otherwise communication would break down. But it would be an illusion to think that the behaviour of the things in the world around us and their properties can be extrapolated into the atomic domain. On the contrary, as we cross the boundary we must be prepared to find that our common notions and ideas desert us: and that's exactly what has happened in quantum theory.

To return to a point mentioned previously, Newton's scheme of mechanics is an idealization characterised by the fact that it draws a picture of nature, if only a quite schematic one without filling in the details,[26] that is independent of the means of observing. Indeed, classical physics may be described as that idealization in which we can study natural phenomena without referring to ourselves. This assumption, however deeply rooted in our whole way of thinking, indeed in the forms of our language, is no longer true of the two great achievements of this century, relativity and quantum mechanics. In both cases, the whole aspect was changed with the recognition of the part played by the observer. In particular in the case of quantum mechanics, the widening of the horizon and the emergence of new ranges of fact have shaken the presuppositions on which classical physics was built, leading to a revision not only of our customary concepts ('particle', 'path', 'wave', etc.) but even of the demands deemed indispensable for rational explanation. That it was an idealization became clear only as soon as one came across phenomena which defy any account in terms of classical theory. Thus arose the need to analyse the conditions underlying the shaping of our concepts—a circumstance of significance far beyond the special theory under consideration.

The difficulty for the non-scientist, or philosopher, is to free himself of the bondage of the customary outlook. A much deeper insight into the phenomena of nature has in fact been gained as a result of such emancipation, but at the cost of a far-reaching renunciation with regard to describing and comprehending the behaviour of atomic particles within the compass of our ordinary notions. Thus it is not possible to trace the motion of such a tiny object in space and time; it is not possible to order the events on this scale in such a way that they form causal chains; it is not even possible to arrange them in a coherent manner within the frame of

26 These are the laws of force, stated by Newton only for the case of gravitation. It is these laws which put content into the otherwise empty frame of Newtonian mechanics.

space and time; after all, the only forms in which we can visualize them. The usual demands for visualization and causal order are unfulfillable.

Owing to lack of time to go into all that, I shall confine myself to one example, just to illustrate the sort of situation we are here confronted with. The ideal of a complete scientific description, it was thought, consists in this: to describe precisely what happens at any point in space at any instant of time. It may be called the Laplacian ideal. It reigned supreme from the beginnings of modern science in the seventeenth century till 1927. At this time, however, it became clear that the idea of such a continuous description is incompatible with experimental facts. Though it is very tempting to think that, if we could only have kept a particle under steady observation we could have traced its path, we shall see before long why this is not possible. We are forced to give up the whole space-time picture of events on the atomic scale—and with it the demand for visualization—and resign ourselves to an unpicturable state of affairs. This situation has important consequences. One of them—as stressed before—concerns causality. If the idea of an unbroken description has to be abandoned the principle of causality cannot be maintained either: for this principle is in science chained to the possibility of such a description. If a continuous description is no longer possible the very foundation of the causal principle melts away; indeed, the principle assumes continuous description as a necessary condition: the breakdown of the latter, therefore, entails the breakdown of the former. So much for the general background against which the fall of causality must be judged.

Let us now see in a bit more detail why such a description is not possible. When we want to 'see' a particle (an electron, proton, nucleon, …) we must illuminate it. Ordinary light will not do: its wave-length being about a hundred million times larger[27] than an electron, it will simply pass around it. We shall therefore have to use radiation of extremely short wave-length (hard X-rays, $\gamma$-rays). The shorter the wave-length, the more energetic the radiation, according to the Einstein law $E = h\nu$. To observe a particle means, then, to shoot at it with high-energy photons which, after they have hit the target, rebound from it with diminished energy (Compton effect) and are reflected into the eye of the observer, or onto a photographic plate. So far so good. But now for the crucial point: the disturbance suffered by our particle in its collision with a photon, it may be thought, is calculable and so predictable. Strangely enough, this is not so: the collision process is unsurveyable in its finer details. Try as we may, each time we attempt to 'see' the electron it gets a kick—and is somewhere else. While ordinary objects such as we handle in daily life can be observed without disturbing them, the situation on this scale is entirely different. Observing an electron is, unavoidably, interfering with it. In the act of observation it is pushed by a photon, and this must alter its velocity. The situation is sometimes described by saying that such a minute object, if

27 The dimensions of an electron are of the order of $10^{-18}$ cm, the wave-length of visible light lies between 4,000 and 8,000 Å, 1 Å $= 10^{-8}$ cm (1 Ångstrom unit).

observed, i.e. interfered with, is taking a zigzag course, being tossed about under the impact of the photons like a boat in a heaving sea. In reality, it is far worse: for we cannot even speak of 'the same' particle. Suppose we observe an atomic object and an instant later a similar one near-by, then we can't even be sure that it is 'the same'. Owing to the interaction between the object and the process of observing, which cannot be controlled, it is not possible to follow its course continuously. Two observations, even if following one another very shortly, should rather be regarded as disconnected events, and it is not possible to combine them unambiguously into a single comprehensive picture. Nor is there any way of telling what 'happens' between one observation and the next. In other words, any picture of what is 'really' going on contains gaps which cannot be filled in. That is why any attempt at tracing the path of an atomic particle is doomed to fail. As a consequence, the question as to whether a particle, really and truly, is the same is not only undecidable but devoid of meaning. (The reader is referred to the mystifying experiment in which electrons pass an opaque screen with two holes in it and where one can't tell afterwards through which of them an electron has gone: not only is the question undecidable, it is meaningless on account of the fact that the electrons, in passing, suffer diffraction, i.e. behave like waves and thus are lost in the wave pattern.) All the evidence points to the need to revise drastically the notion of particles in the classical sense—itself taken over from common experience. such as motes in a sunbeam, specks of dust—to give up the idea of a precise path along with identifiability, permanence (i.e. continuous existence in space)—in short, the attributes of thinghood. An electron, in contrast (say) to the pen before me, is not objectifiable in a manner independent of the way it is observed: any observation is made at the expense of breaking the connection between the past and the future, owing to the uncontrollable disturbance it creates. This circumstance, the unavoidable interference with the run of events together with its unsurveyability, introduces an element of uncertainty into any possible observation and thus blocks the way to a causal analysis. Moreover, the interaction between the object and the device used in observing it frustrates any clear line being drawn between a property of the object—e.g. the location of an electron—and the agency through which it is observed. Owing to the arbitrariness of this distinction, atomic particles can no longer be described in the same way as ordinary objects of sense perception.[28]

Summarizing, it may be said: a new class of facts connected with the existence of the quantum of action imposes upon us not only, as in relativity, a certain limitation of the concepts hitherto employed, but a radical renunciation in regard of any attempt to describe, or pictorially represent, what is going on in the atomic world within the framework of space, time and causality. To use Heisenberg's words, 'Atoms … possess geometrical qualities in no higher degree than colour, taste, …

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28 It is not even feasible to speak of a definite number of particles within a given volume of space.

The atom of modern physics can only be symbolized by a partial differential equation in an abstract multi-dimensional space … Every type of visual conception we might wish to design is, *eo ipso*, faulty.'[29]

Strange consequences, you may think, but after all only to be expected when one attempts to look at the unlookable.

8. Before going into the reasons which have forced physicists to forgo the ideal of determinism, one point should be made perfectly clear. All our knowledge concerning the inner structure of atoms is ultimately derived from experiments in which atoms emit energy or collide with one another. In any such experiment causality is already presupposed so far as the large-scale apparatus is concerned. If this were not so, physicists could not even understand their experiments nor draw any conclusions from them. Thus the blackening of a photographic plate, or a flash on a scintillation screen is the effect of a particle impinging on it. In other words, we assume the existence of causal chains which lead from an event on the atomic scale to the effect observed in experiment. What Kant did not and could not foresee was that, while causality is thus indispensable for an interpretation of an experiment, it does not follow that it must also apply to the hidden reality which manifests itself in the experiment. The existence of causality on the macroscopic level together with acausality on the microscopic presents an inner tension which could only be released when it was shown that ordinary mechanics is included within quantum mechanics as a limiting case.

After this preliminary remark, let us turn to the main question. In which way does the manner in which phenomena are observed enter the picture of the physical world? Suppose we let a particle fly through a small hole in a diaphragm behind which a photographic plate or a scintillation screen registers its arrival, then we can make sure of its location with as much accuracy as we please. Such an experiment, however, tells us nothing about the velocity of the particle, or its momentum. If we want to find out the latter we have to make use of some movable part of the apparatus—say, a screen that can freely swing round a hinge. A particle shot at it will, owing to the law of conservation of momentum, transmit its impulse to this part of the apparatus, thus permitting us to infer from the observed motion the particle's momentum. In this case, we know next to nothing about its location: the particle may have hit the screen anywhere, and this introduces an unavoidable latitude into the result of the experiment. Let us call such experiments *complementary* as what we learn from the one cannot be learnt from the other. Such experiments stand in a relation of mutual exclusion to one another: no experiment can be devised that would inform us accurately both of location and momentum. The study of this relationship has led to the formulation of what must be regarded as one of the dominating principles of atomic physics—the uncertainty relation.

To say that all measurement is imprecise is a platitude. The fact brought to light by Heisenberg, however, goes much further than that: not only is there no

29 *Philosophic Problems in Nuclear Science*, London, 1952.

absolute precision but a definite limit to it, depending on the experimental situation. More precisely, if one devises an experiment to trace the motion of an atomic particle by measuring its initial position and velocity it turns out that these two measurements stand in a relation of mutual exclusion. Any experiment that aims at measuring both will—owing to the unavoidable interaction of the object with the measuring instrument—only lead to a blurring of the results. The essentially new aspect of the matter is that the latitudes left in determining the two quantities are coupled according to the rule

(uncertainty as to position) × (uncertainty as to momentum) = const.

In symbols

$$\Delta x \,.\, \Delta p \approx h$$

where $h$ is Planck's constant ($h = 6.625 \times 10^{-27}$ erg sec). $h$, though exceedingly small, is not zero, with the consequence that the two uncertainties cannot dwindle to nil: the ideal of absolute precision is strictly unattainable. Indeed, the smaller $\Delta x$, i.e. the better we succeed in measuring the particle's position, the larger $\Delta p$, i.e. the less accurate will be the *momentum*, and *vice versa*. *Precise* information about the one implies *total ignorance* of the other. (If $\Delta x \rightarrow 0$, $\Delta p \rightarrow \infty$.) It is as if we had purchased knowledge of the one at the price of uncertainty as to the other, *complete* knowledge at the price, of *complete* ignorance. Hence the name 'uncertainty relation', 'relation of indeterminacy' (*Unschärferelation*).

The departure from classical theory jumps to the eye: while in the latter it has always been taken for granted—if tacitly—that the inaccuracies involved in measurement are (1) negligible and (2) independent of one another, the new principle asserts that they are related in the way described. Its significance lies in this: that *Planck's quantum of action sets a limit to the possible accuracy of any measurement*, depending on the experimental arrangement, a limit which, in favourable circumstances, can be reached but never surpassed (or 'underpassed', as I should rather have said). It thus introduces a new and fundamental feature into the picture of the physical world, connected with the existence of an indivisible quantum $h$ which, from the classical standpoint, really is an irrational element. There seems to be some sort of connection between two kinds of discontinuity in nature—the quantum of action and the atomic structure of matter, though one cannot yet clearly see what the relation is.

As the breakdown of causality is an immediate consequence of the principle, it is of importance to understand clearly what it amounts to. But first an objection. Is the principle not in conflict with the notorious fact that position and velocity of an ordinary object can be measured without ado? That this is possible is due to the extreme smallness of Planck's constant. Writing $mv$ instead of $p$, we may re-state the principle in the form

$$\Delta x \,.\, \Delta v \approx h/m.$$

This shows that, as the mass $m$ increases, the right-hand side of the relation tends to zero. For heavy bodies, *h/m* is too small to have an appreciable effect: hence the impression that there is no limit to the possible accuracy of measurement. Given a body weighing one gramme, we can in principle fix its position within $2 \times 10^{-13}$ cm (a distance of the order of the dimensions of an electron) and its velocity within $2 \times 10^{-13}$ cm/sec—or 6 microns per century. But replace the body by an electron, and you have an altogether different situation. Suppose the electron's position is to be determined within $10^{-5}$ cm (wave-length of ultraviolet rays), then the uncertainty in speed will be 500 km/sec. Considering that such an accuracy is not much to boast of—it would be like locating a grain of shot within a margin of 100 km—let us try to do better and fix the position within $10^{-10}$ cm, then the indeterminacy in speed will be 50,000 km/sec. Conversely, if the velocity is to be measured accurately, say, within a micron per second, the electron may be found anywhere within a distance of 50 km—its location will be quite 'unsharp'. So much to illustrate the Heisenberg relation.

To guard against misconceptions, it should perhaps be stressed that it is not because our instruments are too blunt that these quantities, position and momentum, cannot be measured simultaneously: this would still be a contingent feature. The root lies deeper: even if the particle is in no way physically interfered with by our observing it so that its physical state is not altered we are still prevented from acquiring information about it which goes beyond the limits laid down by the uncertainty relation. As the same relation can also be derived on the basis of very general ideas—e.g. when one thinks of a particle as a wave-group—it certainly can have nothing to do with the bluntness of our instruments.

But this only raises another and more radical question. Is there any *sense* in ascribing such properties to a particle? If one says, 'It is not possible to measure exactly both position and momentum', this is a misleading way of putting the principle: for it now looks as if the particle, before it had been observed, did in fact possess some definite properties which, however, are disturbed by the act of observation. God, if I may say so, looks down and sees where the particle is and how it moves: He *knows*, while we—owing to a conspiracy of nature?—are kept in the dark: a situation reminiscent of views held before relativity. The earth, it was thought at the time, like a ship, sails through an ether-sea, carrying with it the physicists and their instruments. However, the instant an interferometer is placed in position to ascertain the earth's motion something strange happens: the arms of the instrument experience a shortening in the line of motion so that the attempt is baffled. There *is* motion, there *is* contraction, only the two things are so precisely adjusted that they cancel out—that despite motion the effect is nil. So in our case: the object *has* position and momentum, unfortunately, however—. Is nature really so spiteful? But here we are falling just into the sort of error we have to avoid, namely, conceiving of such objects in analogy with the things around us. In imputing current physical properties to them, we are treating them as if they were just grains of sand, only smaller: which is a mistake. At least we cannot do so without sinning against the first commandment in science: never make statements that cannot be checked.[30] Great care should therefore be taken over the manner of formulating the Heisenberg relation. A more correct expression would be to say: there is no physical law in which reference is made to the exact position and momentum of a particle. Each of these quantities can be determined with any degree of accuracy, only not under the same experimental conditions—it is just the point that their study requires mutually exclusive arrangements. We can thus obtain 'contrasting pictures, each referring to an essential aspect of empirical evidence'.[31] The mistake, then, is to ascribe to an atomic object properties such as position and momentum *independent of observation*—as if such properties were inherent in the atom itself, regardless whether it is observed or not. In taking such an attitude, one fails to see that, in contrast to ordinary physics, such terms have meaning only insofar as the attributes they refer to can be observed in experiment. In speaking of location or velocity of an atom one must, therefore, not lose sight of the whole experimental situation of which they form part: divorced from it they no longer have a physical meaning.

This mistake, however, is only the consequence of another and still deeper one—that we think in the categories of ordinary language and use, along with it, the forms of pictorial visualization. Following this habit of thought, we are naturally tempted to apply common notions, such as space and time, motion and path, thing and property, cause and effect in an uncritical way to a domain, far beyond the range of ordinary experience, to which they are not adapted. Or, looked upon the other side, a description of atomic events cannot be squeezed into the ready-made moulds of ordinary language, fitted as they are for describing our familiar world. What is needed is the shaping of a set of new concepts, permitting us to deal with what is entirely outside the compass of ordinary language. The latter, having been moulded under the influence of certain features which the outside world constantly presents to its users, bears still the stamp of them, in the vocabulary and the logical forms which have gone into its shaping. Classical physics is essentially an extension—and refinement—of the ways of thinking, observing and describing which we all employ in every-day life. Therefore it has not given rise to the sort of philosophic questions which are apt to confound the student of quantum theory. The novel thing about the latter is this: the phenomena studied in this part of physics present features which cannot be accounted for within the framework of classical concepts. To give a rational account of them, it was first of all necessary to become aware of certain presuppositions underlying the application of these concepts so that they may be modified and, if possible, adjusted to the new conditions. But the moulding of a system of concepts means nothing less than the creation of a new language, a new mode of thinking. (Uncommon sense, not common sense is required for such a feat.) It is the recognition of the inadequacy of ordinary

30 Thus the orbits of the electrons within the atom in Bohr's first theory have about as much claim to reality as the hell-circles of Dante.

31 N. Bohr, *Atomic Physics and Human Knowledge*, New York, 1958.

concepts and ordinary language which may be of some interest to the philosopher.

The state of affairs sharply formulated in the Heisenberg principle has two consequences. The first concerns the mode of being of atomic particles. Here it must be said that they are never completely objectifiable, i.e. as completely describable in classical terms as objects on the ordinary scale are: their description always contains an uncertainty which is in part objective, due to the uncertainty relation, in part subjective, due to our incomplete knowledge. (The latter can in some circumstances be reduced to nought.) As Heisenberg says: 'they form a world of potentialities or possibilities rather than one of things and facts.'[32]

The other consequence closely connected with the first one is that it is *impossible to predict* the result of an observation with certainty; what can be predicted is only the probability of any such result. And that means that causality ceases to operate on this level. Not that this amounts to a 'refutation' of the causal principle in a strictly logical sense of the world. However, it cuts the ground from beneath the Laplacian program, removing the presupposition on which it rests, namely, that the state of the particles in the universe can, even in principle, ever be known with sufficient accuracy. Indeed, what has turned out is that the *present is unknowable* (not completely knowable) so that from the data available no conclusive inferences can be drawn with regard to the future. Whether one should speak here of a breakdown of causality or rather of the category of substance is a matter of taste.

9· I do not share the view that mathematics is such a terrific obstacle that it must be avoided at any price. In fact, it is the only means of making the situation really clear. In what follows, I shall confine myself to quite simple elementary mathematics such as any schoolboy will be able to follow.

Wave mechanics, the work of de Broglie and Schrödinger, has been modelled after Einstein's theory of light quanta ('needle radiation'). The uncertainty principle may be illustrated first for photons so that whatever we can learn about *their* behaviour will, within the frame of this article, equally well apply to that of material particles. Einstein's theory (light possessing an atomic structure) is not exactly a revival of Newton's corpuscular hypothesis. For photons are not corpuscles which move according to Newton's law of motion. How, then, do they move? The answer to this question holds a surprise: 'the path of a photon' is an expression which has no physical meaning. We say, for instance, that the sun emits photons and that, if a screen is placed in their way, they will not hit the ground. Moreover, we can, by using the laws of wave optics (superposition of waves), calculate the amplitudes in a given experimental setting; as the amplitudes are related to the average number of photons we can find out, for any region in space, the average number of photons which are present there. All this we can do, except one thing: describe the path of a single photon on its way from the sun to the Earth.

Let us see why this is not possible. We are accustomed to speak loosely of a light ray in empty space and picture it as a straight line—a precise line without

32 *Physics and Philosophy: The Revolution in Modern Science*, New York, 1958.

thickness. However, if we want to give substance to this idea we run into difficulties. How should such a ray be produced? A straight line, in geometry, is determined, say, by a point and its direction. If we wish to produce a light ray which travels along a straight line we have first of all to make sure that it passes through some definite point in space. This we can do by letting light pass through a small hole in a screen. The smaller the hole, the more precisely marked will be the point through which the ray has to pass. However, if we make the hole very small, e.g. so small that its width is but the wavelength of the light used, diffraction occurs: the light having passed the hole bends round, producing on the far side of the screen a characteristic pattern of light and dark rings. This shows that light, considered as a shower of photons, does not continue in its original direction (if it had one), but suffers deflection. Result: we have fixed a point, but at the price of making the direction *indeterminate*; i.e. we have succeeded in making photons pass fairly precisely through a point in space but failed to let them have a definite direction.

Let's try again, this time by making the *direction* definite. To this end, we may use two screens, each with a hole in it, and let light pass through the holes. Then the direction of the beam between the two holes will be pretty precise. If, however, the holes become very small diffraction will take place with its undesired consequences and thus foil our plan. To avoid this, we have to make sure that the holes are large in comparison with the wavelength. In this case, however, the beam of light will be of appreciable thickness. Result: we have fixed the direction of the ray, but at the price of leaving the point poorly defined; we have failed again.

We are thus faced with a dilemma. If we succeed in making light pass through a well-defined point it will be scattered; and if we succeed in giving it a precise direction it will no longer pass through a point. We can't have it both ways. What is at the bottom of this dilemma? The answer is: the physical existence of such a precise rectilinear ray is at variance with wave optics. If the laws of wave optics hold—as, in fact, they do—there cannot be such a thing as a 'precise path' of a light ray or a photon; or more correctly, one may speak of such a path, in a loose sense though, meaning a ray of some thickness, small for us, yet large compared with a wavelength.

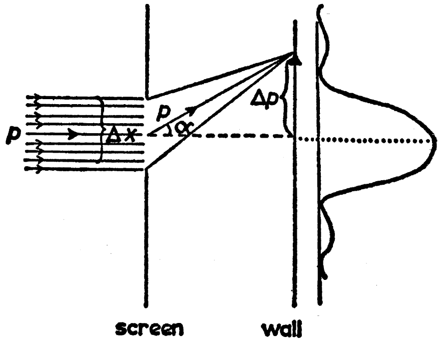


Fig. 5

Let us now consider a bit more in detail what happens in an experiment of the sort just described. Imagine a screen with a hole in it. Let $\Delta x$ denote its width. Suppose (monochromatic) light passes the hole in a direction perpendicular to the screen (Figure 5). According to a simple calculation—which you find in any text-book—only *part* of the incoming light moves on in the original direction; another part is deflected. The bulk of the latter is bent aside by a certain angle $\alpha$ ('the first diffraction maximum'). The fact that concentric light and dark fringes appear on the wall opposite or on a photographic film placed there allows us to assert that photons have passed through; precisely at what spot they passed remains so far unknown. The width $\Delta x$, therefore, represents the latitude allowed to the location of a photon on its flight through the screen, or, as one usually says, the 'uncertainty' of its position. As a swarm of photons passes through, some of them will undergo deflection—in the diagram upwards or downwards as the case may be—while others continue in their line of motion. To be deflected means to acquire an additional momentum in the direction parallel to the screen, the total momentum remaining constant. If $p$ denotes the original momentum, and $\Delta p$ the newly acquired one, a glance at Figure 5 shows that $p$, $\Delta p$ and $\alpha$ are related, roughly, by the equation

$$\sin \alpha \approx \Delta p/p$$

or

$$\Delta p \approx p \sin \alpha. \tag{1}$$

On the other hand, wave theory asserts that the three quantities $\Delta x$, $\alpha$ and $\lambda$ (the wave-length) are connected by

$$\Delta x \cdot \sin \alpha \approx \lambda. \tag{2}$$

Before going on, let us see what the last equation means. A is constant for a given sort of light. The meaning of (2) is that the product of the two factors on the left-hand side is constant: the smaller $\Delta x$, the larger will be $\sin \alpha$, and therefore $\alpha$. That is to say, the better you succeed in reducing the hole to a point, the stronger will be the scattering. If $\Delta x = \lambda$, $\sin \alpha = 1$, and $\alpha = 90°$: when the width shrinks to just one wavelength, the direction of the light moving behind the hole becomes *completely uncertain*: a photon may now fly on to any place, and light will be scattered in all directions. To avoid this, to hold a photon (so far as possible) in its track, you have to keep down the second factor of the product: but if so, $\Delta x$ will go up.

We can now see why any attempt at producing such a ray is doomed to failure. Indeed, what is it we are trying to do? To send light through a point-like hole ($\Delta x = 0$) and, at the same time, preserve its direction ($\alpha = 0$). The fulfilment of these conditions, however, is incompatible with equation (2) according to which the product of the deviations must be finite. The impossibility of producing such a light ray is therefore due to the wave nature of light and not to any shortcomings of the experimenter. The best we can do, by way of approach, is to reduce both $\Delta x$ and $\alpha$, for example, by putting $\Delta x = \sin\alpha = \sqrt{\lambda}$. Suppose the experiment is performed with ultraviolet light ($\lambda = 10^{-5}$ cm), then equation (2) leads, roughly, to $\Delta x = 3 \times 10^{-3}$ cm and $\alpha = \frac{1}{5}$ degree: that is, to a beam 30 microns thick and of so little scattering that, if you 'aim' with it at a spot 1 metre away you may be 3 millimetres beside the mark. The shorter the wave-length, the better the approximation to the 'ideal' case of a ray without thickness. Whatever the technique used, the actual path will always be unsharp.[33]

To turn to a more general question, is it possible to observe a photon that has (a) a definite position (b) a definite momentum? Let us look once more at our experiment. If there were no such thing as diffraction, if photons were small hard pellets obeying Newton's laws of motion, all would be well. A photon would simply fly through the hole with unchanged velocity till it hit the wall behind. By making the hole smaller and smaller, we should succeed in forcing the photon to traverse an almost precise straight line. As it is, photons represent only one aspect of light, its wave nature being the other. Wave nature means diffraction, diffraction means change of momentum, and so departure from classical ideas is unavoidable.

To see how the two things, position and momentum, are related let us go back to our two equations. From (2) follows

$$\sin\alpha \approx \lambda/\Delta x;$$

if this is substituted in (1) we obtain

$$\Delta p \approx p\,\lambda/\Delta x$$

or

$$\Delta x \cdot \Delta p \approx p\lambda \qquad (3)$$

$p$, the momentum of a photon, is by definition

$$p = hv/c. \qquad (4)$$

As frequency $v$ and wave-length $\lambda$ stand in the simple relation

33 That's why it is not possible to give a physical meaning to the phrase 'the precise path of a photon'.

$$\nu\lambda = c \; (c = \text{velocity of light})$$

we have

$$\nu = c/\lambda$$

If this is written in the place of $\nu$ in (4), we obtain

$$p = hc/\lambda c = h/\lambda;$$

substitution of this expression in (3) yields

$$\Delta x \cdot \Delta p \approx h \qquad (5)$$

That is the celebrated Heisenberg relation. What does it mean? It means that it is impossible to get hold of a photon which possesses (a) a sharply defined position (b) a sharply defined momentum. For in this ‘ideal’ case we should have $\Delta x = 0$ and $\Delta p = 0$, and therefore $\Delta x \cdot \Delta p = 0$, in contradiction to (5).

As momentum is related to space as energy is to time (energy being the fourth component of the energy-momentum vector), the uncertainty relation can also be transferred to energy and time:

$$\Delta t \cdot \Delta E \approx h \qquad (6)$$

Generally speaking, two variables, a kinematic and a dynamic one, are always linked together by a law such that the accuracy in the one restricts the possible accuracy in the other.

One more point: a photon, for all we know, may describe a precise path; it’s only when we try to observe it that the difficulty arises. As Heisenberg says, ‘Natural science does not simply describe and explain nature; it is a part in the interplay between nature and ourselves; it describes nature as exposed to our method of questioning,’[34]

10. What has been said of photons, applies word for word to electrons, or for that matter to any material particles. For exactly the same sort of experiment can be carried out with particles. Suppose a shower of electrons falls on a thin crystal; the atoms in the crystal act as a lattice of diffracting centres. If electrons behave like waves, similar effects are to be expected. In fact, when the experiment was performed by Davisson and Germer (in 1927, only three years after de Broglie had introduced the idea of matter waves), light and dark rings appeared on a photographic plate where it had been hit by the electrons after their passage through the crystal. Just as in the case of the photons, we can reason: the blackening of the plate shows that electrons have impinged on it, the arrangement of the dark spots in the form of concentric rings, i.e. of a diffraction pattern, that the impinging electrons do not continue in their original direction before the crystal was placed in their way—as they ought to do if they were to obey Newton’s first law of motion. Only a certain part of them do so; another part is deflected towards the first ring, a smaller percentage towards the second ring, and so on. In other words, some parti-

34 *Loc. cit.*

cles are not deflected at all, some are to a certain degree, some to a higher degree, and so on. Can this curious behaviour, perhaps, be explained by assuming that the electrons, as they fly past the atoms in the crystal, interact with them? To take the parallel case of the photons, can *their* behaviour be explained by supposing that they interact with the rim of the hole? What such interaction could, at most, explain is that those near the rim are deflected: but then we should expect a gradual falling off of the intensity of light on the far side of the screen, not periodic zones of light and darkness. The inevitable conclusion is that diffraction is due to the wave nature of light, and that photons do not obey Newton's laws. As with photons, so with material particles: they do not move according to classical laws—the phenomenon of diffraction rather points to a sort of wave pattern underlying their behaviour.

Applying the same sort of mathematical analysis to atomic particles, we are led to the relation

$$\Delta x \cdot \Delta p \approx h,$$

which expresses the uncertainty principle for such particles. Although this result is obtained from an analysis of the processes involved in measuring, it is neither limited to the special circumstances of the experiment nor is its content a purely negative one. Indeed, the very fact that it sets a limit to the accuracy of any particle description and renders it incomplete points to what is complementary to it, a wave description. Both modes of description would be contradictory within the framework of classical concepts, while the conflict is evaded by what Bohr calls the complementarity principle. As this touches on an important point a few words may be added.

There is a trend to explain physical reality in terms of particles: this was the ideal of classical physics since the times of Descartes or even Democritus. Opposed to it is a trend to account for the phenomena in terms of waves only: that was Schrödinger's original conception to which he still seems attracted. However, the curious thing is that none of these descriptions exhausts the possibilities presented in experiment: each of them leaves out something that can only be supplied by the other. A complete description, or as this is impossible, a maximum description must take into account both aspects. 'By playing with both pictures (particle and wave), by going from one picture to the other and back again, we finally get the right impression of the strange kind of reality behind our atomic experiments.'[35] That this reality should be such that two different and mutually exclusive sets of concepts are needed to describe it is certainly a surprising and at the same time a very remarkable result. There is nothing absurd in the notion of a wave-particle duality: it appears so only when one attempts to combine both aspects into a single comprehensive picture. From the new vantage point classical physics must appear one-sided—treating matter as composed of particles and light as composed

35 W. Heisenberg, *loc. cit.*

of waves, whereas each of them presents both aspects: only there is no conflict between these aspects since they never manifest themselves in the same experimental setting.

11. The uncertainty principle marks the final break with the past. It does not merely state that, though there may be causal laws governing atomic events, they are at present unknown: it flatly denies the existence of such laws. This is the Copenhagen interpretation (Bohr and Heisenberg), accepted today by the great majority of physicists. In view of such far-reaching consequences for our whole world picture, it is understandable that a wave of doubt should have been created by the principle when it was first formulated. Many were the attempts to get round it. To mention only one, Einstein, always a non-believer, suggested an ingenious thought experiment to outwit the uncertainty relations. In a discussion (at the Solvay Congress in Brussels, 1930) at which Bohr was present, he pointed out a possible way to determine time and change in energy of an atomic event without any uncertainty. Consider, he said, a box capable of holding radiant energy (e.g. lined with perfect mirrors). Weigh the box. Now release one photon from it (by means of a clockwork inside the box that works a shutter), at a moment fixed with as much precision as you want. Weigh the box again. The change in mass tells you the energy lost, according to the formula $E=mc^2$. In this way, concluded Einstein, one could, in principle at least, measure the energy of the photon emitted and the time of its escape with any desired degree of accuracy, in contradiction to the Heisenberg relation. Bohr spent a sleepless night over the argument. The next morning, however, submitting the conditions of the experiment to a searching analysis, he was able to clear up the discrepancy. It would lead us too far to go into details here. Only so much may perhaps be said: Einstein had failed to apply his own theory of general relativity to the case in point. The box, hung e.g. on a spring-balance, moves in recoil from the escaping photon, upwards or downwards as the case may be; its position is changed in the gravitational field of the earth, and this, according to general relativity, changes the rate of the clock rigidly connected with the box: in the case of a downward movement, the rate will be accelerated, in the opposite case decelerated. This gives rise to an uncertainty in regard to the exact time of the photon's flight. On the other hand, there will also be an uncertainty in measuring the weight of the box, and therefore the change in energy. Calculation shows that the two uncertainties, referring to time and energy, stand in exactly the relation as is demanded by the uncertainty principle. Einstein was defeated, not convinced. And if a man of his imagination did not succeed, who will? There seems to be no escape from the uncertainty principle.[36]

12. Let us pause for a moment and see where we have got to. We had to give up many things thought indispensable for a rational explanation—the postulate of continuity of description and consequently a tracing of the motion of atomic particles, thus foregoing to connect their past and future behaviour; next, a sharp

36 The reader will find many other examples discussed in Bohr's Gifford Lectures.

distinction being made between the phenomena and the means chosen for their observation, based on the impossibility of getting to know their detailed interaction; further, permanence, individual existence and recognizibility of such particles along with ascribing well-defined states to them, i.e. renouncing the demands for visualization by the use of space-time pictures; and finally, a causal understanding of the phenomena. We had to give up so much: what do we stand to gain?

With regard to the experiment with photons which pass through a hole, it was pointed out that any attempt at tracing their course in detail must end in failure, with the consequence that the behaviour of a single photon is unpredictable. The same goes for electrons, protons, … Are we, then, to give up physical science altogether as a bad job? Far from it. Though it is perfectly true that we cannot follow the behaviour of a single particle on its journey in space and time, we may try something else. Suppose, for example, we shoot an electron from a sort of gun, aiming always in the same direction, say, at a spot on a photographic plate where its impact is recorded. Let us repeat the experiment a large number of times, it will then be found on inspection that the points of impact lie scattered about—a fact which defies the time-honoured rule 'same cause, same effect'. Indeed, if exactly the same experiment is repeated (supposing this is possible), the result will each time be different, showing fluctuations. My present point, however, is this: although each electron hits the target in a different place, these places are arranged in an orderly way, within concentric circles forming a perfectly regular pattern. *Order is thus born out of randomness*. For the result can be explained by saying: light rings appear where many electrons impinge on the plate, dark ones where none or only few fall. What cannot be predicted of one individual electron, namely where it will arrive, *can* be predicted of a large number of them, and very accurately. If we, then, cease to care for the fate of a single particle and turn instead to that of a crowd we regain the apparently lost faculty to formulate laws to predict the future. Suppose that a million electrons are sent in the same direction, quantum mechanics allows us to foretell how many of them are likely to hit the centre, what fraction to fall on the first ring, the second ring, and so on; predictions the more accurately confirmed, the larger the crowd.

What emerges is a *statistical* theory unconcerned with individuals and taking account only of large assemblages. Its structure is such that statements derived from it, when translated into terms of observation, describe the average behaviour, or the probability with which an electron will be found at a certain place: where it will in fact be, thereof the laws keep silent.[37]

In Laplace's view, nature is predetermined, a gigantic clockwork, each atom describing a path according to unswerving mechanical necessity. In the modern view, the universe may still be a machine—a misleading metaphor—a machine, however, more in the nature of a roulette wheel than of a clock. The behaviour of a

37 This view—that underlying the regularity we observe there is complete randomness on the atomic scale—was first expounded by F. Exner, an experimental physicist in Vienna, in 1919.

wheel, though unpredictable in detail, on the average exhibits a marked regularity. That is the aspect stressed in the study of quantum phenomena. While with Laplace chance was nothing but the name of ignorance, it now forms a fundamental and irreducible element in any description of nature.

Let me illustrate this with an example. If we have before us, say, a milligram of radium comprising a vast number of atoms, within 1,600 years half of it will have disintegrated. Supposing now it were possible to pick out one single atom, no one can tell whether it will erupt within the next 10 seconds or go on existing in its present state for the next 10,000 years. There is absolutely no clue to foretell what will happen. And that is not due to our human ignorance: it is *objectively uncertain* when the atom will disintegrate. Natural law sets down merely statistical decrees. Like in the case of men and women in London over the age of 60 who are going to die within the next year, only laws of a statistical kind can be formulated. (Are we, then, to look upon nature from the viewpoint of an Insurance Company? Not exactly: for while the question as to why a single atom behaves in the way it does is unanswerable, the parallel question with regard to a human being permits of a wide range of answers—one of the reasons why physics cannot and should not· serve as a model for the social sciences.)

After reading this account you may be inclined to say: surely there must be some difference in the state of two such atoms; since the one is just on the point of undergoing disintegration while the other is not, they cannot be alike. So you turn the Law of Causation into a definition: you *choose* to call two states 'the same' if they are followed by the same consequents, while in the contrary case they are the same only in appearance, not in reality. Very well; but this distinction is of use only under one condition—that it can be substantiated by really finding out what the difference in question is. Otherwise it is an empty verbal distinction. Now one of the queer things brought to light by atomic physics is that two atoms may be wholly and exactly alike—I don't mean approximately alike as two eggs or two raindrops but alike as two whole numbers, i.e. absolutely indistinguishable. And this bars the way to turning the Law of Causation into a mere definition. The fact remains that of two atoms in all respects perfectly alike the one is doomed, the other is not. All one can say is that an atom erupts whenever it likes to: and if it does, this is, quite literally, an uncaused event.

But may it not be that there are still some undiscovered laws which, if taken into account, would permit us to foretell the precise instant of eruption? Surely we are not omniscient? Such an assumption can be disproved by a number of arguments. To mention only one: it is known that the nucleus is surrounded by a 'mountain of potential' sloping down at the inside, and an α-particle is imprisoned within this 'crater'. To get out of it, it must have sufficient energy to climb over the mountain wall. However, it is found that its energy is scarcely half that value. According to classical laws, therefore, it could never escape. Quantum theory, on the other hand, provides us with means to calculate with what probability such a particle, considered as a wave, may escape, even if its energy is too small to sur-mount the wall—a result well-confirmed by experiment.[38] Now if there were deterministic laws permitting us to make precise predictions, an α-particle could not also be regarded as a wave ('representing probability') and these laws could not agree so closely with the results obtained from probability considerations. In addition, there are other reasons equally strong to dispose of such a possibility. When all is said and done, radium disintegration goes on spontaneously undetermined by causes—a reminder of how remote we here are from the world of ordinary experience.

To sum up the case of causality: in the world as we perceive it there is not only no sufficient evidence in support of it, but, what is worse, there cannot be any. For what appears on this level as the causal order of the world can always be regarded as the result of a huge number of elementary processes which come into play here, and their levelling influence—irrespective of the question whether the 'true laws', those governing the behaviour of photons, electrons, … are, or are not, of a causal type. Each particle may behave in a haphazard way, straying hither and thither as chance would have it, and yet, owing to the enormous number involved, the irregularities will in the end be smoothed out, presenting a perfectly regular picture. The common experience 'same cause, same effect', so often taken for the principle of causality, has in fact little to do with the deeper problem. Considering that two states which go under the name of 'the same cause' are, from a strict point of view, alike only in appearance, while the true states, determined by the particles involved, their arrangement in space and state of motion, hardly ever really agree, one comes to see how little weight such an observation carries. Indeed, any large-scale observation informs us only of the mean value of the quantities involved while leaving the details undetermined.

To add one point as regards the inaccuracy to be expected in any such law—if we check a statement, say, about Brownian movement and observe under the microscope just n granules we shall find it inaccurate within a margin of $\sqrt{n}$. That is to say, if we meet with 100 particles we must be prepared to find fluctuations about the value predicted by theory of about 10—which is 10% of the whole. If the number is much larger, say, $10^8$ the departure to be expected will be of the order $10^4$, that is one hundredth of 1%. This illustrates how the degree of accuracy tends to increase with the total number of grains of pollen, molecules or atoms by whose interplay the effect is brought about. To give an idea of the order of number involved in a large-scale observation, 32 grams of oxygen contain $6 \times 10^{23}$ $O_2$ molecules representing a mechanical system with $2 \times 3 \times 6 \times 10^{23} = 36 \times 10^{23}$ free parameters (neglecting the inner structure of the O-atoms). A causal analysis of the behaviour of such a system requires twice as many data, in contrast to gas theory that makes use only of two, pressure and temperature. On account of this, the latter is only capable of making probability assertions to be tested by means of statistical

38 This theory was developed by the Russian physicist G. Gamow, who lives in the U.S., and independently by Condon and Gurney.

methods. That these assertions are *almost* causal, i.e. have probabilities very close to 0 or 1, does in no way change the situation. As in any large-scale observation $10^{24}$ or more molecules are involved there is extremely little departure from, and hence extremely high accuracy of, any law empirically found and tested. But no law is absolutely exact. For this reason, recourse to ordinary experience will never be able to prove the existence of strictly causal laws.

It is only when we descend to the atomic level that the question of causality can be put to the test; and here all the facts speak decidedly against it. The only theory known at present capable of connecting and unifying an enormously wide range of phenomena, quantum theory, is in sharp logical contradiction with it. It goes without saying that this theory, on account of the openness of experience, may have to be revised in the future: will it perhaps be so revised as to lead back to determinism? If anyone cherishes such hopes I am afraid I will disappoint him. For according to a theorem proved by von Neumann[39] such wishes are unfulfillable. What von Neumann has proved is this: given quantum mechanics in its present shape, it is not possible to modify, complete or extend it—say, by introducing hidden parameters—such as to transform it into a deterministic theory; for any such extension would render the theory so modified self-contradictory. Hence only two courses are open to us—either to abolish quantum theory in its entirety and start afresh from scratch, or retain the present foundation while radically renouncing a return to determinism. But there is no room for tinkering with the theory. In view of its undeniable great successes, it is difficult to believe that it should be entirely false. And if it is on the right track causality does not stand a chance.

As for the renunciation of deterministic ideals, let me remind you of some precedents in the history of science. While Kepler laboured all his life to find the key to the riddle of the universe—why the planets are arranged at just the distances from the sun as they are—Newton's celestial mechanics not only left the question unanswered but eliminated it from science. Again, it should be remembered that classical mechanics was achieved at the price of foregoing the search for a cause of uniform motion. Until the time of Galileo it was believed that a body could move with constant speed only if it was pushed by force; the turning point came when it was realized that uniform motion, like rest, is a state that demands no explanation. Similarly, the physicists succeeding Maxwell, after many unsuccessful attempts to find a mechanical substructure for his theory, had to resign themselves to the fact that the field concepts can not be further reduced to mechanical terms.[40] Today, it seems, we are passing through a similar period of readjustment of thought as that

39 *Mathematical Foundation of Quantum Mechanics*.

40 First expressed by H. Hertz. In contrast with this, Lord Kelvin avowed in 1884: ‘I am never content until I have constructed a mechanical model of the object I am studying. If I succeed in making one, I understand; otherwise I do not. Hence I cannot grasp the electromagnetic theory of light. I wish to understand light as fully as possible, without introducing things that I understand still less.’ The mood of renunciation is clearly perceptible in such utterances.

in which Galileo was laying the groundwork of modern science. In particular, just as people at that time had to accustom themselves to the idea that uniform motion *has no cause*—which, at least at the time, entailed a certain renunciation as regards the demands for a causal understanding—so today we have to shift to a position from which things emerge in a new perspective. Modern physics has led to the insight that causal analysis is, after all, only *one* way of understanding nature alongside of which e.g. invariance, equivalence, symmetry, but also complementarity, take their place as categories of rational explanation. I mean, just as Kepler's search for a fourth law, in the light of our present knowledge, was illusory and had to be re-interpreted as a historical question concerning the evolution of the planetary system, so the demand for a 'direct representation of physical reality in space and time' may well prove just as ill-starred—with all due respect for Einstein.

13. It is hardly to be expected that the revolutionary change in our concepts thus inaugurated will remain confined to the study of quantum phenomena. For one thing, the notions of space and time will probably have to go into the melting pot. For the uncertainty relations reveal an unsuspected connection between geometrical and dynamic properties of elementary particles, i.e. between momentum and energy on the one hand and the possibility of locating them in the frame of space and time on the other. Our intuitive ideas, appropriate as they are for ordinary experience, cannot, it seems, be transferred to the atomic realm. The continuity of space and time stands indeed in strange contrast to the discontinuity of matter, energy and action which has become so prominent a feature of our picture of the world.

Even logic, to all appearance *a priori par excellence*, is not unaffected by the general drift, and new ideas based on Bohr's principle of complementarity begin to take shape.[41] To conclude with a few words on this situation. Suppose that the momentum of an electron has been determined with a high degree of accuracy, then its location will be indeterminate. Suppose, however, that someone none the less asserts that the particle is, at that instant, in a certain place—is his assertion true or false? If it were true this would mean that the electron can certainly be found in the place specified, if false that it cannot possibly be found there. To insist that it must be either true or false, even if it may be imposssible for us to decide the issue only brings us into conflict with the Heisenberg relation (complete knowledge of momentum entails total ignorance as to location). On the other hand, to say that the assertion is neither true nor false is incompatible with the law of excluded middle. So what?

What we have before us is a conflict between logic and physics. If logic is right quantum theory must be wrong, and if quantum theory is right (in particular the uncertainty relation), then classical logic must be wrong (in particular the law

41 G. Birkhoff and J. v. Neumann, 'The Logic of Quantum Mechanics' (*Annals of Mathematics* 37 (1936); C. F. v. Weizsäcker, 'Komplementarität und Logik' (*Die Naturwissenschaften*, 1955). Even Heisenberg has given his blessing to this enterprise.

of excluded middle). According to the Copenhagen interpretation, the assertion is undecidable, neither true nor false. Let us consider once more what the law of excluded middle really boils down to. Guided by it, we should have to say that only one alternative holds—that our particle is here, in this place, or is not here, namely somewhere else; and that this is true on purely logical grounds. If so, we are constrained to ascribe a definite position to the particle, whereas quantum theory says that this whole idea must be discarded. That is, roughly, the clash between logic and physics.

It wouldn't be surprising if someone, turning over the situation in his mind, were to say, 'Even if there is no possible experiment to decide the issue the assertion taken in itself must be true or false. It certainly is understandable and hence meaningful: if it is neither true nor false what *can* it be?' In such an argument, however, due regard is not paid to the fact that an assertion concerning the electron's location has physical meaning only insofar as it is connected with a certain experimental arrangement. But the point is that, once the momentum is measured in experiment, this *destroys* the possibility of making, at the same time, another arrangement for determining the location and thus does away with the very precondition under which the assertion makes sense. Indeed, one gets into hopeless difficulties when one ascribes properties to particles independent of the means of observation. To put it in another way, the trouble with insisting that the assertion 'must be true or false, only we don't know which' is that it creates the impression that the electron *did* occupy a quite definite place, unfortunately one not determinable in experiment—whereas what one really ought to have said is that the whole idea of describing an electron in particle terms breaks down here. In saying, 'But a particle must be *somewhere*' one is overstepping the bounds laid down by the complementarity principle, i.e. one is trying to stretch one aspect beyond the limits of its application to the exclusion of the other. To uphold the law of excluded middle is, in this case, tantamount to disregarding the wave aspect, while the real point is the inadequacy of anyone mode of description which at best tells only half the story.

14. To mention just one feature of the sort of logic—some call it 'complementarity logic'[42]—that is emerging from the discussion, in atomic theory we meet with statements which *exclude* one another, such as a description of momentum and of position—'complementary' statements as we may call them. What is characteristic of them is that there is an embargo against using them in the same context. This feature is not an entirely new one, for it has a parallel in the case of classical logic: giving the content of a proposition $p$, we can do two things with it, assert it or deny it; though the result of each operation makes perfectly good sense, this no longer holds good of their conjunction. Any two such propositions behave like complementary statements in quantum theory. It is this feature of ordinary logic which is generalized in complementarity logic. Given a statement $s$, there

42 So von Weizsäcker, *loc. cit.*

will be other statements (in general infinitely many)

$$s_1^*, s_2^*, s_3^*, \ldots$$

which are complementary to the given one. (For example, *s* being the statement of the momentum of a particle, complementary statements will be: that the particle is at a distance of 1 cm, 2 cm, 3 cm, …, say, from the zero point of a certain scale.) To a given statement *s* there is, then, a set of statements complementary to it such that, when *s* is decided in experiment (is true or false), any member *s** of that set is *undecided* (neither true nor false). And just as in ordinary logic the joining of *p* and not *p* is taboo, so is in the new logic the combining of *s* and *s** into a conjunction. But while in ordinary logic it is only in exceptional cases that the conjunction of two meaningful statements is meaningless, in the new logic that holds to a much wider extent. In this due recognition is paid to the peculiar logical situation encountered before in connection with the law of excluded middle. Indeed, classical logic is tied to the presupposition that alternatives, at least in principle, are decidable and that statements—so far as they assert or deny something—can be divided into the two classes 'true' and 'false'. Once the assumption underlying this dichotomy is shaken it will be difficult to keep up that law. But that is exactly the situation that confronts us in quantum theory. For here it is a *law of nature* which, given the precise momentum, prevents us from deciding issues as to the location: undecidability is a direct consequence of the uncertainty relation. It is of course perfectly true that in describing experimental evidence we have to make use of plain language and common logic. Our present concern, however, is in the logical relations within that theory, and here ordinary language can no longer serve as guide.

To mention another difference, while in ordinary logic the statements *s* and '*s* is true' (or not *s* and '*s* is false') are equivalent, i.e. have always the same truth-value, in the new logic they are not. Indeed the truth or falsity of *s* entails the truth or falsity of '*s* is true' but not conversely. For if the assertion '*s* is true' is false it does not follow that *s* is false—it may be undecided. Supposing, on the other hand, that the foregoing assertion is true, then *s* is true. For this reason the classical equivalences hold good only for the case of truth, not of falsity, thus revealing a curious asymmetry between these concepts. It is because *s* and '*s* is true' are on a different logical level and therefore not translations of one another that Frege's method of defining connectives like 'and', 'or', 'if' in terms of truth-tables is no longer usable in the new field. It would lead us too far to explain how these terms can be defined. Only so much can be said: in the system of Birkhoff and von Neumann the formula

$$a \vee \bar{a}$$

is always valid. Considering, however, that the symbols which occur in it are defined in a different way and take on a different sense, it will be seen that it is no longer the expression of the law of excluded middle. The difference comes out when it is noticed that, while $a \vee \bar{a}$ is always true, '$a$ is true or $\bar{a}$ is true' is not: in this sense the classical law breaks down. What emerges is a nonfunctional (and hence non-tautologous) logic in which all the classical rules are formally preserved except the two distributive laws. However, the interpretation given to the formalism has to be changed so that, in fact, we have a new logic embodying new ways of thought.

The philosophical lesson to be drawn from this development is that even logic is not exempt from the pressure of facts. Needless to say that logic does not describe the real world and can therefore not be refuted: but it may be well- or not well-adapted to prevailing conditions. What underlies ordinary logic, the dichotomy of statements, no doubt reflects the character of certain deep-seated features of our world, in particular the fact that issues, on the whole, are decidable.[43] However, on closer scrutiny of these principles, including their application in the realm of science, their limitation became manifest. Brought face to face with the sort of facts revealed in the study of quantum phenomena, these principles prove ill-adapted and have to be modified. The account given is in contrast with views commonly held on logic, according to which it has nothing to fear from discoveries regarding the actual world. The underlying idea seems to be that physics, being based on experiment, may any day be overthrown by new facts coming to light, while logic, being of infinite hardness, cannot—so that, in the case of a collision, physics will get all the bumps. To use Brouwer's words, 'Admittedly, the application of the logical principles to natural phenomena sometimes gave the wrong results, but in that case the blame was always laid at the door of the axioms of the conceptual system, never at the door of logic.'[44] Quantum physics, I submit, presents a strong case against traditional logic, and belief in it as the only possible one has become a form of provincialism.

Lastly, what about the assertion—put forward, for instance, by the distinguished physicist P. Jordan[45]—that the gaps in the causal chains open the way to moral freedom? I think it is a mistake; but time does not permit me to go into the question.

43 Not always: counter-factuals ('If Hitler had been liquidated at the time before Munich, then etc.') are as a rule undecidable, neither true nor false, at best plausible.

44 'Mathematik, Wissenschaft und Sprache' (*Monatshefte f. Math. u. Phys.* 1929).

45 *Science and the Course of History*, New Haven, 1955.

# Causality

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## Contents

## (1) Hume's Analysis of Causal Connection.

The problem of causality is one of the central topics of Hume's philosophy. There are several reasons for its importance: (1) Of all the relations it is the only one in virtue of which we can pass beyond the immediate impression of the senses or an idea of the memory and thus step outside the realm of the given. The only relation "that can be trac'd beyond our senses, and informs us of existences and objects, which we do not see or feel, is *causation*."[1] (2) It is a relation which underlies our belief in an external world. To quote another passage from the *Treatise*: "We readily suppose an object may continue individually the same, tho' several times absent and present to the senses; and ascribe to it an identity, notwithstanding the interruption of the perception, whenever we conclude, that if we had kept our eye or hand constantly upon it, it would have convey'd an invariable and uninterrupted perception. But this conclusion beyond the impressions of our senses can be founded only on the connexion of *cause and effect*; nor can we otherwise have any security, that the object is not chang'd upon us, however much the new object may resemble that which was formerly present to the senses."[2] The problem of a permanent and continuous world can, therefore, not be dealt with until the nature of the causal relation has been cleared up. (3) Causation was invoked by Leibniz, Locke and Berkeley to pass from the world or sense to a transcendent reality, to infer the existence of God, and so on. Locke argued: There is at least one existence of which I am intuitively certain, namely *my own.* "(…) man knows by an intuitive certainty, that bare nothing can no more produce any real being, than it can be equal to two right angles".[3] In other words, the existence of the self requires a *cause*. This cause must be eternal, since otherwise it would have had a beginning and thus require yet another, and so on *ad infinitum*. Therefore the cause can only be an eternal being, God. Berkeley followed this pattern of reasoning with the only difference that he asked for a cause of our *sense-impressions*. As there is no matter from which they can come he concluded that they must come from God. The possibility of metaphysics as a science is thus closely tied up with an analysis of causation.

Now when we attempt to analyse the ordinary idea of causality, we find in it the following four characteristic points:

(1) Cause and effect are *contiguous* in space and time.

(2) The cause *precedes* the effect.

(3) There is a constant *regularity* such that, whenever the cause occurs, the effect occurs too.

1 David Hume: *A Treatise of Human Nature,* Book I, Part III, Section 2. –All quotations taken from L. A. Selby-Bigge's edition, Oxford 1888.

2 *Ibid.*

3 John Locke: *An Essay in Human Understanding,* Book IV, Chapter 10, § 3.—Quotation taken from *The Works of John Locke in Nine Volumes.* 12th ed. London 1824, vol. II.

Up till now nothing has been mentioned that would not be admitted by anyone as involved in the idea of causation. Indeed, in looking for the possible causes of a given event E, we may safely exclude all events happening *long* before E, and all events happening *at a great distance* from E; and events occurring *after E* would never be *called* causes of E—evidence that the principle "The cause precedes the effect" must be regarded as part of the *definition* of the terms "cause" and "effect". Assuming these conditions in their most rigorous form one arrives at the idea that no event can be regarded as the cause of E in a proper sense, unless it occurred in the immediate spatial and temporal neighbourhood of E. So the causal relation between two events C and E seems to imply their contiguity in space and time. Action-at-a-distance (temporal as well as spatial distance) is thereby ruled out. In cases in which it does seem that there is a time-gap between cause and effect—e.g., between an infection and the outbreak of the disease—a closer investigation reveals the existence of a chain of events which link C with E, such as the entry of germs into the blood, their spread through the organism, the production of chemical poisons, or toxins, the effect of these on the cells of the body resulting in the specified symptoms of the disease, and so on. And even "when in any particular instance we cannot discover this connection, we still presume it to exist."[4] We feel absolutely convinced that nothing can happen that has, so to speak, a delayed action effect—that may happen to-day, without producing any effect whatsoever, and then suddenly will "burst into effect", say, in a year's time. Such a relation, though conceivable from a purely logical point of view, would not fit into the scheme of causality as we actually conceive it, and thus we may safely conclude that it lies in the nature of causality, or rather of our conception of it, that C and E should be contiguous in space and time, and that C should precede E. Moreover, everyone would agree that causation implies a certain regularity, expressed by the rule: whenever C, then E.

(4) In addition to this, popular opinion assumes that the antecedent, the cause, brings on the consequent, the effect, *compulsorily*, i.e. that the cause is *necessarily* followed by the effect. There seems then to exist a sort of "tie" that connects, or unites, cause and effect.

It is this fourth point, the assumption of a *necessary* connection between the events, against which Hume's criticism is directed. Hume has been accused of *denying* causation, whereas in fact he was concerned only with *analysing* it. He confined himself to discussing the analytic question, What is it that we are asserting when we assert that one event is causally connected with another? Just as Berkeley did not *deny* the existence of chairs and tables in the every-day sense, but only tried to make clear what we *mean* when we speak of their existence, in like manner Hume set himself the task of *clarifying* the idea of causation and *cleansing* it from certain ingredients which faulty reasoning had added to it.

4 Hume, *Treatise*, I, III, 2.

I shall now give a brief outline of Hume's analysis. According to a popular view there is an element of necessitation present in causal relation. Now this alleged necessity can be *neither of a logical nor of an experiential character.*

Suppose first it was of a logical character; this would mean that the relationship between cause and effect was of the same nature as that which holds between the premise and the conclusion in an inference. Now where one statement $p$ entails another one $q$, it must be self-contradictory to assert $p$ and deny $q$; for instance, if I say "That is red", pointing to some red object, this entails "that is coloured"; if I were to assert the former and deny the latter, i.e. if I were to say "That is red and not coloured", I should be saying something that is self-contradictory. In other words, if $p$ entails $q$, the conjunction $p$ and not $q$ (in symbols, $p \cdot {\sim}q$) must be *meaningless* (self-contradictory). Now in order to see whether cause and effect are *logically* related in the way in which ground and consequence are, we need only apply our criterion and ask whether it is self-contradictory or not to assert that the event $C$ has occurred *without* having been followed by the event $E$. Now is it self-contradictory (meaningless) to consider a situation in which the impact of one billiard-ball is not followed by a motion of the second, but, say, by a change in colour of both or by a rise in temperature? Clearly such a situation has never been observed; but at the same time we notice that there is not the slightest difficulty in portraying such a happening down to the last detail; so we *can* imagine such a case; it is, then, not self-contradictory to consider such a possibility. From the first appearance of an event we can never infer logically what effect will result from it. Therefore the relation of cause and effect is utterly different from the relation of ground and consequence in a logical inference. I cannot by any strain of the imagination conceive what it would be like to see a thing that is red and not coloured; in fact, I do not even know *what* I should imagine, for there is nothing to be imagined; whereas the rule that $C$ is always followed by $E$ can be denied without self-contradiction.

To express the same thing in Hume's own words:

> We are apt to imagine that we could discover these effects by the mere operation of our reason, without experience. We fancy, that were we brought on a sudden into this world, we could at first have inferred that one Billiard-ball would communicate motion to another upon impulse; and that we needed not to have waited for the event, in order to pronounce with certainty concerning it. (…) But to convince us that all the laws of nature, and all the operations of bodies without exception, are known only by experience, the following reflections may, perhaps, suffice. Were any object presented to us, and were we required to pronounce concerning the effect which will result from it, without consulting past observation; after what manner, I beseech you, must the mind proceed in this operation? It must invent or imagine some event which it ascribes to the object as its effect; and it is plain that this invention must be entirely arbitrary. The mind can never possibly find the effect in the supposed cause, by the most accurate scrutiny and examination. For the effect is totally different from the cause, and consequently can never be discovered in it. Motion in the second Billiard-ball is a quite distinct event from motion in the first; nor is there anything

in the one to suggest the smallest hint of the other. A stone or piece of metal raised into the air, and left without any support, immediately falls: but to consider the matter *a priori*, is there anything we discover in this situation which can beget the idea of a downward, rather than an upward, or any other motion, in the stone or metal? (…) When I see, for instance, a Billiard-ball moving in a straight line towards another; even suppose motion in the second ball should by accident be suggested to me as the result of their contact or impulse; may I not conceive, that a hundred different events might as well follow from that cause? May not both these balls remain at absolute rest? May not the first ball return in a straight line, or leap off from the second in any line or direction? All these suppositions are consistent and conceivable. Why then should we give the preference to one, which is no more consistent or conceivable than the rest? All our reasonings *a priori* will never be able to show us any foundation for this preference. In a word, then, every effect is a distinct event from its cause. It could not, therefore, be discovered in the cause; and the first invention, or conception of it, *a priori*, must be entirely arbitrary.[5]

In other words: *the causal relation is not a logical relation*. One event *follows* another, but it never follows *from* another—if this latter expression is taken in its logical sense.

Hume actually puts the argument still more forcibly by examining the proposition "that whatever begins to exist must have a cause of existence".[6] (Remember this was one of the sources of metaphysical speculation for Locke and Berkeley.) It is commonly taken for granted in all reasonings. But if we examine the maxim more closely, we shall discover in it no mark of intuitive certainty; nor is it demonstrably certain. It has not that intuitive certainty which we ascribe to a proposition of arithmetic, or geometry, or to a proposition such as "What is red cannot be green at the same time." "All certainty," says Hume, "arises from the comparison of ideas, and from the discovery of such relations as are unalterable, so long as the ideas continue the same. These relations are *resemblance, propositions in quantity and number, degrees of any quality, and contrariety*; none of which are imply'd in this proposition, *Whatever has a beginning has also a cause of existence*. That proposition therefore is not intuitively certain."[7] Nor is it capable of demonstration. The question at issue is whether anything that begins to exist must owe its existence to some cause. In order to prove this, it would be necessary to show that it is impossible for anything to come into existence without some productive principle. But this impossibility cannot be proved. Any attempt to prove that there must be a cause begs the question. It is

easy for us to conceive any object to be non-existent this moment, and existent the next, without conjoining to it the distinct idea of a cause or productive principle. The separation,

5 David Hume: *An Enquiry Concerning Human Understanding,* Sect. IV, Part I, § 24f.—Text and Chapter numbering follow L. A. Selby-Bigge's edition of *Hume's Enquiries Concerning the Human Understanding and an Enquiry Concerning the Principles of Morals*, Oxford 1894.

6 *Treatise*, I, III, 3.

7 *Ibid.*

therefore, of the idea of a cause from that of a beginning of existence, is plainly possible for the imagination, and consequently the actual separation of these objects is so far possible, that it implies no contradiction nor absurdity; and is therefore incapable of being refuted by any reasoning from mere ideas; without which 'tis impossible to demonstrate the necessity of a cause.[8]

It is on this basis that Hume rejects various arguments for the necessity of a cause. The first which he assigns to Hobbes is that, since time and space are homogeneous, the occurrence of an event—say of a flash of lightning—at some definite place at some definite time must be due to a cause peculiar to that place and time, which determines and fixes the existence; for otherwise the event would have to remain in eternal suspense, and the object could never begin to be, for want of something to fix its beginning. But there is no more difficulty in supposing the *time* and *place* to be *fixed* without a cause, than to suppose the *existence* to be determined without a cause. There are two different questions involved here: the first is *whether* the object shall exist or not; the second *where* and *when* it shall begin to exist; if the absence of a cause is intuitively absurd in the first case, it must be so in the other; both suppositions are on the same footing, and must stand or fall by the same reasoning.

The second argument, attributed to Dr Clarke, labours under another difficulty. "Every thing, 'tis said, must have a cause; for if any thing wanted a cause, *it* wou'd produce *itself*; that is, exist before it existed; which is impossible. But this reasoning is plainly inconclusive; because it supposes, that in our denial of a cause we still grant what we expressly deny, *viz*. that there must be a cause, which therefore is taken to be the object itself; and *that*, no doubt, is an evident contradiction."[9] Whereas to say that anything "comes into existence, without a cause, is not to affirm, that 'tis itself its own cause; but on the contrary in excluding all external causes, excludes *a fortiori* the thing itself which is created. An object, that exists absolutely without any cause, certainly is not its own cause; and when you assert, that the one follows from the other, you suppose the very point in question, and take it for granted, that 'tis utterly impossible any thing can ever begin to exist without a cause, but that upon the conclusion of one productive principle, we must still have recourse to another."[10]

A third argument, ascribed to Locke, is that "whatever is produc'd without any cause, is produc'd by *nothing*; or in other words, has nothing for its cause. But nothing can never be a cause, no more than it can be something, or equal to two right angles."[11] The weakness of this argument is obvious. If "we exclude all

8 *Ibid*.
9 *Ibid*.
10 *Ibid*.
11 *Ibid*.

causes we really do exclude them, and neither suppose nothing nor the object itself to be the causes of its existence."[12]

A fourth argument is that every effect must have a cause, because it is implied in the very idea of effect. In other words, "every effect necessarily pre-supposes a cause; effect being a relative term, of which cause is the correlative. But this does not prove that every being must be preceded by a cause; no more than it follows, because every husband must have a wife, that therefore every man must be marry'd."[13]

The result, then, is that the general maxim "Whatever begins to exist must have a cause of existence", so often taken as an expression of an "absolute and metaphysical necessity", is *not self-evident*. Like all other propositions concerning matters of fact, it has an entertainable opposite, and however little we may incline to accept this opposite as credible, it is not to be ruled out as being in itself inconceivable, The maxim is also not demonstrable from truths more ultimate than itself. Hume's commentators have, as a rule, assumed that Hume questions the validity of the maxim. This is a complete misunderstanding. Neither in the *Treatise* nor elsewhere does Hume raise the question as to the *truth* of the maxim. His discussion concerns solely the *grounds* upon which our *belief* in it really rests. That is, he tries to get clear about the epistemological *status* of the maxim—whether it is a part of a body of *a priori* knowledge, or whether, *if* it is true, its truth is due to *experience*. And *all* he tries to establish is that the maxim cannot be regarded as a self-evident, or logical, or necessary, or rational truth, i.e. as a statement the opposite of which is self-contradictory. I have to add, however, that this does not mean that Hume entertained doubts as to the validity of the maxim. On the contrary, he expressly disavowed any such interpretation in a letter to John Stewart (1754): "(…) I never asserted so absurd a Proposition as *that anything might arise without a Cause*: I only maintain'd, that our Certainty of the Falsehood of that Proposition proceeded neither from Intuition nor Demonstration; but from another Source."[14] This, then, should settle the dispute.

There is, then, no logical necessity inherent in the statement that a thing that begins to exist must owe its existence to some cause. "Every demonstration, which has been produced for the necessity or a cause, is fallacious and sophisticated."[15] Nor is there such a necessity inherent in the statement that a particular event E is caused by another particular event C. Thus we may summarise the result so far found by saying that *the relation of cause and effect is not a logical relation*. This was the first part of Hume's analysis of the idea of causation.

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12 *Ibid.*

13 *Ibid.*

14 J. Y. T. Greig (ed.): *The Letters of David Hume.* Oxford 1932, vol.I, p. 187.

15 *Treatise*, I, III, 3.

Having thus ruled out the proposal that the necessity, claimed for the causal relation, is of a *logical* nature, we must now turn to the next question as to *whether this necessity can be found in experience.*

When we look about us towards external objects, and consider the operation of causes, we are never able, in a single instance, to discover any power or necessary connexion; any quality, which binds the effect to the cause, and renders the one an infallible consequence of the other. We only find, that the one does actually, in fact, follow the other. The impulse of one billiard-ball is attended with motion in the second. This is the whole that appears to the *outward* senses. (…) In reality, there is no part of matter, that does ever, by its sensible qualities, discover any power or energy, or gives us ground to imagine, that it could produce any thing, or be followed by any other object, which we could denominate its effect. Solidity, extension, motion: these qualities are all complete in themselves, and never point out any other event which may result from them. The scenes of the universe are continually shifting, and one object follows another in an uninterrupted succession; but the power or force, which actuates the whole machine, is entirely concealed from us, and never discovers itself in any of the sensible qualities of body. We know, that, in fact, heat is a constant attendant of flame; but what is the connexion between them, we have no room so much as to conjecture or imagine.[16]

"All events seem entirely loose and separate. One event follows another; but we never can observe any tye between them. They seem *conjoined*, but never *connected*."[17] "The first time a man saw the communication of motion by impulse, as by the shock of two billiard-balls, he could not pronounce that the one event was *connected*: but only that it was *conjoined* with the other."[18] "But were the power or energy of any cause discoverable by the mind, we could foresee the effect, even without experience; and might, at first, pronounce with certainty concerning it, by mere dint of thought and reasoning."[19] In reality, we are not able to deduce, however subtle our reasoning, what effect will result from a given cause.

Let an object be presented to a man of ever so strong natural reason and abilities; if that object be entirely new to him, he will not be able, by the most accurate examination of its sensible qualities, to discover any of its causes or effects. Adam, though his rational faculties be supposed, at the very first, entirely perfect, could not have inferred from the fluidity and transparency of water, that it would suffocate him, or from the light and warmth of fire, that it would consume him. No object ever discovers, by the qualities which appear to the senses, either the causes which produced it, or the effects which will arise from it; nor can our reason, unassisted by experience, ever draw any inference concerning real existence and matter of fact.[20]

16 *Enquiry*, VII, I, § 50.
17 *Ibid.*, VII, II, § 58.
18 *Ibid.*, § 59.
19 *Ibid.*, VII, I, § 50.
20 *Ibid.*, IV, I, § 23.

Consequently, there is not, in any single, particular instance of cause and effect, any thing which can suggest the idea of power or necessary connexion.[21]

Thus the result of the second part of Hume's analysis can be expressed in these words: What experience shows is that one event, the cause, is regularly followed by another event, the effect; that is, experience shows us only a regular temporal *succession* of events, but never the *manner* in which the effect arises from the cause. We never observe any tie that connects the events, or, what comes to the same, any relation of necessitation which holds together particular events, since it is impossible to conceive of any observation which would have the slightest tendency to establish the existence of such a relation.

Thus the element of necessitation, with which causation is credited by popular belief, is neither to be found in logic nor in experience: the effect cannot be deduced logically from the cause; that is, it is no self-contradiction to affirm C and to deny E; and experience never shows us anything more than that, in actual fact, one event *follows* the other; it never reveals any such thing as a bond between the events. How, then, do we come to believe in a *necessary* relation of cause and effect? How is it that so strong a conviction is implanted in our breasts? This brings us to the third and last part of Hume's analysis.

Hume's answer to the last question is that our belief in a *necessary* connection rests on a psychological mechanism. And it is characteristic of Hume's method that he does not rest contented with destroying the arguments for the necessity of causation, but penetrates the psychological roots from which this belief springs. I now proceed to give an account of Hume's psychological theory of causation, or rather of our belief that there is an element of necessity in this connection.

When, in my experience, the event A was every time followed by the event B—for instance, the impulse of one billiard-ball on another, and the setting in motion of the latter—the ideas of A and B get so closely connected in my mind, the bond established by association becomes so intimate, that the idea of A, quite by itself, whether I wish it or not, evokes the idea of B. To change the example, make the following experiment. Imagine you strike a match to light a cigarette. Immediately the idea of smoke rising from the cigarette will come into your mind—and come quite by itself, without your doing anything about it; and what is more, you will feel that you have to make a definite *effort* to *suppress* (or keep out) the idea of the smoke rising. The idea will *force* itself on your mind, even against your will. We are, in this sense, under a certain *compulsion*, whenever we imagine A—the lighting of a cigarette—also to think of B, the smoke rising from it. It is here, then, that we experience in our own mind a feeling of being constrained or compelled. And now we project this element of constraint or compulsion, which, after all, we have only experienced in our own consciousness, on to the world of external objects and events, and so come to believe that the event A *necessarily* brings on the

21 *Ibid.*, VII, I, § 50.

event B. We thus succumb to an illusion of a peculiar kind in that we are tempted to give a new interpretation to the experience of constraint, which ultimately is a personal or subjective experience, namely, to regard it, as if it were an objective necessity, whatever this may mean; in other words, to look upon the influence of habit as if it were a power that is inherent in the things themselves.

I shall now give this account of the matter in Hume's own words:

> To be fully acquainted with the idea of power or necessary connexion, let us examine its impression (…) It appears, then, that this idea of a necessary connexion among events arises from a number of similar instances, which occur, of the constant conjunction of these events; nor can that idea ever be suggested by any one of these instances, surveyed in all possible lights and positions. But there is nothing in a number of instances, different from every single instance, which is supposed to be exactly similar; except only, that after a repetition of similar instances, the mind is carried by habit, upon the appearance of one event, to expect its usual attendant, and to believe that it will exist. This connexion, therefore, which we *feel* in the mind, this customary transition of the imagination from one object to its usual attendant, is the sentiment or impression from which we form the idea of power or necessary connexion. Nothing farther is the case. Contemplate the subject on all sides; you will never find any other origin of that idea. (…) When we say, therefore, that one object is connected with another, we mean only that they have acquired a connexion in our thought (…)."[22] "This therefore is the essence of necessity. Upon the whole, necessity is something, that exists in the mind, not in objects; nor is it possible for us ever to form the most distant idea of it, consider'd as a quality in bodies. Either we have no idea of necessity, or necessity is nothing but that determination of the thought to pass from causes to effects and from effects to causes, according to their experienc'd union."[23]

The ultimate result of Hume's analysis, then, is this: there is no such thing as causal *inference* in the sense that stating the occurrence of one event C does not *entail* stating the occurrence of another event E. When the mind passes from an idea or impression of one object to that of another, it is the *imagination* that is operating, not the *understanding*. It is *habit*, not *reason*, *instinct*, not *insight*, that is at work. "Custom", so Hume says, i.e. habit, here operates in and through the laws of association; and it is upon this associative cohesion of ideas that the transition from causes to effects and from effects to causes rests.

## (2) The Problem of Induction.

Here a new question presents itself. If the essential part of causation is the *regularity* with which the effect follows the cause, how is it that we can ever maintain such a regularity to subsist, since what we observe is always a single instance or such a connection? How are we to account for the regularity? Think of how we

22 *Ibid.*, VII, II, § 59.

23 *Treatise*, I, II, 14.

proceed in every day life. We said that E was called the effect of a cause C, if in many cases it was observed to follow C each time without exception. Should we not ask: *how many times?* A physician who has tried a medicine in six cases and has seen the patients get better six times may feel confident that his remedy was the cause of the recovery of his patients (provided, of course, that in his former experience they did not get well without the medicine), but undoubtedly it is possible that in all future cases the remedy will fail to have the desired result; and then we shall say that those first six times were nothing but a *chance*, the word "chance" simply meaning the absence of causality. If instead of six times the experiment were repeated successfully a hundred times, surely everybody would believe in the beneficial effect of the medicine; nevertheless it must be admitted that the future may bring exceptions and destroy the regularity. A hundred times will be considered better than six, but clearly *no* definite number will be considered absolutely satisfactory; for if in even one single case C were *not* followed by E, one would no longer feel justified in calling C the cause of E, and for all we know such a possibility cannot be excluded with certainty.

The situation, then, is this: the statement "C is the cause of E" seemed at first sight to mean nothing but "C is *always* followed by E"; but this latter statement can never be verified, because of the unfortunate "always" it contains. Verification would be possible only if a finite number was substituted for "always", for instance the number 500 in the example of the medicine, this number being arbitrarily chosen. But it goes without saying that no definite finite number will do, because what we want to establish is that the medicine will *always* cure the patient, no matter, how often the attempt is made. Thus it seems that what we have to do in order to satisfy ourselves of the existence of such a causal connection is to observe *all* its single instances; but this can never be done on account of the possibly infinite number involved. So we come up against a curious dilemma which was first envisaged by Hume: a general law cannot be derived from a finite number of instances, and an infinite number of instances cannot be observed. Hume was thus led to contemplate a new philosophical problem, totally unknown to the history of thought before his time—the so-called *Problem of Induction.* It should be noticed that this problem did not arise in the systems of the rationalists because, on their view, a causal relation was a *logical* relation. They were of the opinion that the effect could be deduced logically from the cause, just as, e.g., in geometry a theorem can be derived from certain given axioms. It was the destruction of this rationalist account which brought to light the existence of a new and unsuspected problem. The problem of induction is, roughly speaking, the problem of justifying an inference from the past to the future. More precisely, the problem of induction includes the following three questions:

(a) How do we come to transfer a connection between events observed in the past to cases in the future which have not yet been observed?
(b) What sort of validity can be claimed for such generalisations?
(c) Have we any *right* to generalise in this way? Can induction be *justified*?

I shall now discuss these three questions one after the other.

(a) Is there any *logical* operation that carries the mind from the past to the future? The answer to this question has already been given: it is not through logical inference that I extend my knowledge; for I can never infer one event from another; nor is it through observation, since I cannot observe the future; what really makes me pass from a connection observed in the past to a connection in the future is nothing but habit. This, then, is the agent at work; it operates, without allowing for reflection, in a wholly automatic manner. When we have found, in a certain number of instances, that a certain event C (say, the taking of a drug) was followed by another event E (say, the recovery of the patient), the occurrence of a new instance of the kind C immediately arouses in our mind an expectation of an event of the kind E. The mere fact that something has happened a number of times causes us to expect that it will happen again.

Though this, in Hume's view, is the pattern according to which the mind works and arrives at a generalisation, he makes no claim that it is the path by which the mind *ordinarily* advances to newly acquired beliefs. No one, we are told by Hume, who has reached the age of maturity, will any longer be influenced by "custom" *alone*. In the course of our earliest experiences we quickly learn that the connection between causes and effects is invariable, and we have come to rely so perfectly on this experience, that from a single new experiment we are ready to argue to the future. It is only because we are from time to time faced by contrary experiences that we find ourselves compelled to hold this experience in check and not allow the single instance to determine our expectation. But ultimately it is the *psychological mechanism of association* which accounts for the transition from connections observed in the past to those in the future. For without this mechanism we should not be able to generalise, or to learn from the past, or to make any step in acquiring new knowledge. Thus the more refined method of procedure in which we have recourse to the experiential fact that the connection between cause and effect is invariable, or in which we carefully *weigh* the factors in an experiment, arises from habit, not *directly*, but through a number of intermediate stages.

Suppose a chemist describes the qualities of a chemical compound which he has produced for the first time; he will not doubt that a compound, produced in the same manner by some other person in some other place, will exhibit exactly the same qualities. How can he know that, since it is the very first time that he has observed this substance? It is perfectly true that in a case like this the expectation does not rest on association; for there *was* no previous instance of this sort. Nevertheless in the end the expectation *does* go back to habit. It is based on the fact that this particular experience is preceded by a vast number of *other* experiences which show how chemical compounds in general behave, which factors are relevant and which are not, etc. If that were not so, if we had no previous experiences to learn from, we should not know whether the qualities of that particular substance did not depend, e.g. on the shape of the vessel in which it is stored, or on the day of the week on which the experiment was carried out, or on the age of the experimenter, or the position of the stars, and so on. In short, the induction made in this case does *not rest only* on a single observation, but is supported by a huge number of other rules, laws, and regularities, which, in the final analysis, are the result of an accumulation of many analogous experiences in the past, that is, of habit, or association. They form, as it were, a background of rules into which any new instance is inserted; so a new observation need not be established by freshly acquired association every time it occurs.

Let me add just one more thing. A world in which there was no uniformity, i.e. in which analogous experiences did not recur time and again, such a world, I say, would afford us no occasion of acquiring habits and establishing associations; in such a world—even if we did not perish in it at once—we should certainly not arrive at empirical generalisations. The fact that we do frame general statements concerning the external world, can only be accounted for by the mechanisation of association.

(b) What sort of *validity* can be claimed for such general statements? Suppose I have seen swans 40 times; and every time I saw one I noticed that it was white. When I now pass to the generalisation "All swans are white", I am plainly saying more than that the 40 swans I have inspected, or all the swans that have ever been inspected, or all swans which I shall inspect, say, in the next 10 years, are white. I am saying that *all* swans are white, and "all" must include all *possible* swans, at whatever time and whatever place. An appropriate way of rendering this would be to say "At any place and at any time, if there is something that is a swan, then that something is white". Thus the statement "All swans are white" transcends all possible experience which I, or any person, or any number of persons, can ever obtain. To express the same thing differently, let me introduce a technical term. A statement is said to be *verifiable*, in a strong sense, if, and only if, its truth can be established in experience. Using this terminology, we may say that the statement in question is *not conclusively verifiable*. The same applies to such general statements as "Phosphorus is a poison", "All men are mortal", "Bodies tend to expand when heated." It is of the very nature of these statements that their truth cannot be established with certainty by any finite series of observations. Once it is recognised that such general statements are meant to cover an indefinite or even an infinite number of cases, it must be admitted, that they can not, even in principle, be verified conclusively. But if they can never be verified—how can we ever know that they are true? What sort of validity can we claim for them? That is the problem with which we are concerned.

Now the answer advanced by Hume is that the validity of such propositions cannot be established *a priori*, that is, by mere reasoning. "(…) It implies no contradiction," he says, "that the course of nature may change, and that an object, seemingly like those which we have experienced, may be attended with different or contrary effects. May I not clearly and distinctly conceive, that a body, falling from the clouds, and which, in all other respects resembles snow, has yet the taste of salt or feeling of fire? Is there any more intelligible proposition than to affirm, that all the trees will flourish in December and January, and decay in May and June? Now whatever is intelligible, and can be distinctly conceived, implies no contradiction, and can never be proved false by any demonstrative argument or abstract reasoning *a priori*."[24] Thus thought *alone*, unaided by experience, is unable to establish the truth of such propositions. Thought alone, i.e. logic, can only discover that two propositions contradict each other, or are compatible or incompatible, or that the one follows from the other, etc. That is to say, by logical reflection we can only discover certain *relationships* holding between any given statements, but we can never establish the truth of the statements themselves. According to Hume, it is only by *experience* that the validity of such generalisations can be determined. On the other hand, we have just seen that experience can never verify conclusively any such statement. We may conclude from this that a factual statement, if it is general, has two characteristic features:

(1) It can never be proved (verified) completely by experience.
(2) It can never be proved by logical reasoning alone.

In other words, no general statement whose validity is subject to the test of experience can ever be absolutely certain. Every inductive inference may turn out to be untrue. No matter how often it is verified in practice, there still remains a possibility, however slender, that it will be confuted on some future occasion.

> A German emperor (…) when, being cautioned to keep out of the way of a cannonading, (…) replied, 'Tut! man. Did you ever hear of a cannon-ball that killed an emperor?' (…) This same argument has been employed at least once too often. Some centuries back a dauphin of France, when admonished of his risk from small-pox, made the same demand as the emperor—'Had any gentleman heard of a dauphin killed by small-pox?' No; not any gentleman *had* heard of such a case. And yet, for all that, this dauphin died of that same small-pox.[25]

Even when all the 40 swans that I have inspected are white, this does not rule out the possibility that when I come across a swan for the 41st time, it will be black. The fact that a law has been substantiated in $n$ cases, affords no logical guarantee that it will be substantiated in the $(n + 1)$th case also, no matter how large the number $n$ is taken to be. And this means, that no general statement referring to a matter of fact can ever be shown to be *necessarily* and *universally* true. It can at best be plausible, trustworthy, credible, or reliable. This applies to all general statements if they have a factual content. There is always something hypothetical about such statements: they may be corroborated or weakened by actual sense-experience, but however firmly we believe them, it is always conceivable that future experience will bring evidence against them. This, then, is the conclusion which must be accepted by every consistent empiricist.

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24 *Enquiry*, IV, II, § 30.

25 Thomas de Quincey: *On Murder Considered as One of the Fine Arts*. London 1924 (=Holerth Library 22/23), p. 24.

But if every hypothesis is fallible, what, then, is the point of framing such hypotheses? Why do we construct them? The answer is that they are designed to enable us to anticipate the course or our experience. To quote the opening sentences of Hertz's famous *Principles of Mechanics*:

> The most direct, and in a sense the most important, problem which our conscious knowledge of nature should enable us to solve is the anticipation of future events, so that we may arrange our present affairs in accordance with such anticipation. As a basis for the solution of this problem we always make use of our knowledge of events which have already occurred, obtained by chance observation or by prearranged experiment. (…) When from our accumulated previous experience we have once succeeded in deducing images of the desired nature, we can then in a short time develop by means of them, as by means of models, the consequences which in the external world arise only in a comparatively long time, or as the result of our own interposition. We are thus enabled to be in advance of the facts, and to decide as to present affairs in accordance with the insight so obtained.[26]

Here we are presented with a picture of the essential task of science. This task is the attainment of knowledge; the practical aim of knowledge in its turn is *prediction*, and we have good reasons to regard as the defining characteristics of knowledge those properties of it which make prediction possible. (As the French say, "Savoir pour prévoir".) Prediction requires mental anticipation of future events. This cannot be done by taking the real objects and waiting for what will happen—that would be trying out not predicting: we wish to predict events *before* they actually have happened. It is necessary, therefore, to replace the real objects by something else that can represent them, that is, by *symbols* which can easily be handled. These symbols may be our mental pictures which we can arrange and rearrange in our imagination, or written signs, figures and mathematical symbols, or perhaps even little models. Again to quote Hertz: "We form for ourselves images or symbols of external objects; and the form which we give them is such that the necessary consequents of the images in thought are always the images of the necessary consequents in nature of the things pictured."[27]

Another way of stating the same thing is to say that the hypotheses are *rules* which govern our expectation of future experience, or rules for forming particular statements about unobserved future events. There is no need to say *why* we require such rules. It is plain that on our ability to make successful predictions depends the satisfaction of even our simplest desires, including the desire to survive. I have already observed that the essential feature of our procedure with regard to the formulation of these rules is the use of past experiences as a guide to the future. This fact explains why science, which is essentially predictive, is also to some extent a description of our past experience. But it is noticeable that we tend to disregard

26 Heinrich Hertz: *The Principles of Mechanics in a New Form.* With an introduction by H. von Helmholtz. Authorised Engl. Translation by D. E. Jones and J. T. Walley. London 1899, p. 1.

27 *Ibid.*

those features of our experience which do not lend themselves to forming a basis for successful prediction.

Now this activity of framing laws to predict the future, biologically so important, has gradually become a pleasure in itself. The process of acquiring knowledge, at first nothing but an indispensable means of mastering things and situations for the purposes of practical life, has had the same fate as other useful activities: just as speaking developed into singing, so the pursuit of knowledge developed into science. The human mind takes a delight in constructing a system of hypotheses, it enjoys this activity, no matter whether or not we can derive any practical advantage from it.

The strength of this position is that it can claim to describe the way in which we do actually think in ordinary life, in the empirical sciences, and in historical inquiries. We nowhere find anything that is secure against any possible mistake. No scientific theory is sacrosanct and infallible; and, we may add, there is no reason why the philosopher should regret this. Modern physics, for instance, is perfectly reconciled with the idea that the formulation of natural laws must be considered as hypothetical and may have to be revised some day. The progress of scientific knowledge is none the worse for this attitude, and the impossibility of a logical proof of the universal validity of the natural laws need not and does not shake in the least our practical confidence. And exactly the same applies to psychology and history. We must put up with the situation as it is. To anyone who demands more than this dubious assurance we can only reply: "Friend, you ask for better bread than is made from wheat."

With this account of the actual procedure that we find in every day life and in the sciences, we complete our discussion of the validity of empirical propositions so far as they are universal.

(c) We now come to the last part of our discussion, to the question whether there is *any ground for believing in induction*. Is there any way of proving that an empirical generalisation which is derived from past experience will hold good also in the future? Is this method of forming empirical generalisations capable of justification? That is by far the most important part of the problem of induction.

Hume himself puts the question like this: "What is the foundation of all conclusions from experience?" "When it is asked, *what is the nature of all our reasonings concerning matter of fact*? the proper answer seems to be, that they are founded on the relation of cause and effect. When again it is asked, *What is the foundation of all our reasoning and conclusions concerning that relation*? it may be replied in one word, Experience. But if we still carry on our sifting humour, and ask, *what is the foundation of all conclusions from experience*? this implies a new question, which may be of more difficult solution and explication."[28] Let us take an example:

Our senses inform us of the colour, weight, and consistence of bread; but neither sense nor reason can ever inform us of those qualities which fit it for the nourishment and support of

28 *Enquiry*, IV, II, § 28.

a human body. (…) But notwithstanding this ignorance of natural powers and principles, we always presume, when we see like sensible qualities, that they have like secret powers, and expect that effects, similar to those which we have experienced, will follow from them. If a body of like colour and consistence with that bread which we have formerly eat, be presented to us, we make no scruple of repeating the experiment, and foresee, with certainty, like nourishment and support. Now this is a process of the mind or thought, of which I would willingly know the foundation. It is allowed on all hands that there is no known connexion between the sensible qualities and the secret powers; and consequently, that the mind is not led to form such a conclusion concerning their constant and regular conjunction, by anything which it knows of their nature. As to past *Experience*, it can be allowed to give *direct* and *certain* information of those precise objects only, and that precise period of time, which fell under its cognizance: but why this experience should be extended to future times, and to other objects (…), this is the main question on which I would insist. The bread, which I formerly eat, nourished me; that is, a body of such sensible qualities was, at that time, endued with such secret powers: but does it follow, that other bread must also nourish me at another time, and that like sensible qualities must always be attended with the like secret powers? The consequence seems nowise necessary. At least, it must be acknowledged that there is here a consequence drawn by the mind, which wants to be explained. These two propositions are far from being the same, *I have found that such an object has always been attended with such an effect*, and *I foresee, that other objects, which are, in appearance, similar, will be attended with similar effects*. I shall allow, if you please, that the one proposition may justly be inferred from the other: I know, in fact, that it always is inferred. But if you insist that the inference is made by a chain of reasoning, I desire you to produce that reasoning. The connexion between these propositions is not intuitive. There is required a medium, which may enable the mind to draw such an inference, if indeed it be drawn by reasoning and argument. What that medium is, I must confess, passes my comprehension; and it is incumbent on these to produce it, who assert that it really exists, and is the origin of all our conclusions concerning matter of fact."[29]

"Should it be said that, from a number of uniform experiments, we *infer* a connexion between the sensible qualities and the secret powers; this, I must confess, seems the same difficulty, couched in different terms. The question still recurs, on what process of argument this *inference* is founded? Where is the medium, the interposing ideas, which join propositions so very wide of each other? (…) When a man says, *I have found, in all past instances, such sensible qualities, conjoined with such secret powers*: And when he says, *Similar sensible qualities will always be conjoined with similar secret powers*, he is not guilty of a tautology, nor are these propositions in any respect the same. You say that the one proposition is an inference from the other. But you must confess that the inference is not intuitive; neither is it demonstrative: Of what nature is it, then?[30]

In these words, Hume has shown, to my mind irrefutably, that the transference of the past to the future is not founded on any *logical* reasoning. There is no argument, carried out by purely logical means, which can show that a law, which has been fulfilled in all known instances, in the past, will be fulfilled in the future. For to assume that it will not, is not self-contradictory. Thus logic is powerless to

29 *Ibid.*, § 29.
30 *Ibid.*, § 32.

conduct any such proof. But if so, then it becomes plain, that we have no logical ground whatsoever for expecting the bread we shall eat at our next meal not to poison us, or for expecting the sun to rise to-morrow, or for any of the other expectations that control our daily lives. Such a state of affairs seems most disconcerting.

It has been argued that we *have* reason to know that the future will resemble the past: for to know this, it has been said, only one thing is required, namely, the *principle of causality*. Given this principle, every inference by induction can be reduced to a syllogism in the following way: observation teaches that A was the antecedent of B; as, according to the causal principle, equal antecedents always bring on equal consequents, it follows that B will be the consequent of A also in the future; this means that the connection of A and B is a universal one, that is, a connection which holds at any place and at any time.

Two things must here be said, to clear up the situation. The first is, that an event never repeats itself *exactly*. At the best, two events may *resemble* each other so much that it would be difficult to tell the difference: but a closer scrutiny will always reveal some difference between them. If so, however, we should have to state the causal principle in the form that *similar* causes are followed by *similar* events—a statement which is by no means true without exception. "Nothing so like as eggs; yet no one, on account of this appearing similarity, expects the same taste and relish in all of them."[31] Moreover it is well known that very small differences among the causes may give rise to quite considerable differences among the effects. Pascal in his *Pensées*: "Cleopatra's nose: if it had been shorter, the whole face of the earth would have changed." An ever so small increase in the temperature of a powder magazine may give rise to an explosion. It is just a fact that there are circumstances which are relevant and others which are not. We do not know in advance which circumstances and which differences will have a bearing upon the effect; to find out on what circumstances the effect depends, is the very task we are engaged on. So it is no good arguing that when A is the antecedent to B, similar antecedents will always be followed by similar consequents; for whether that is so or not, experience alone can teach; so that the recourse to the causal principle is of no avail.

In other words: granted that we are given a principle according to which equal antecedents are always followed by equal consequents, then what we can conclude from this, in a strict sense, is only that if A has once been followed by B, an event *exactly like* A will be followed by an event *exactly like* B. But we cannot prove, by reference to the causal principle alone, that an event *similar* to A will be followed by an event *similar* to B; and yet that is the statement which we require to have proved.

Now the second point which I wish to make is that the principle of causality must itself be regarded as the result of a vast amount of experiences made in the past, i.e. as an assertion the truth of which, in its turn, is supported by induction.

31 *Ibid*., § 31.

To refer to the principle of causality in order to warrant induction is, therefore, to go round in a circle. It would be different, indeed, if it could be shown that the principle of causality was a *rational* principle, i.e., a principle the truth of which was independent of, and prior to, experience.[32] I shall, for the moment, not embark upon such an examination, but shall come back to the point later. For our present needs it is quite enough to have shown that, even *if* the principle of causality were a rational principle, it would be futile to appeal to it in order to justify the method of induction, because of the difficulties encountered in point (1).

After the attempt to give a rational account of induction has failed, some may be inclined to say, "Very well then, induction is neither self-evident, nor can it be proved by reasoning be it ever so subtle; but that it *is* valid and reliable, can, in fact, be seen from experience." But here again Hume has shown by a very remarkable argument that any reference to experience is circular. I now turn to this last part of Hume's argument.

The problem we have to discuss is whether there is an experiential reason for believing in what is called the "uniformity of nature". This principle states, briefly speaking, that the *future resembles the past*. What is excluded by this principle is the possibility that when in the present instant an event C brings about an event E, it may, at another time, be accompanied by a quite different event, say F. For instance, when iron has been found to dissolve in hydrochloric acid, this will hold good not only on the earth, but on some other planet, not only now, but in 1000 years time. This brings us back to the question, Assuming that a law has always held in the past, have we any reason to suppose that it will hold in the future?

It has been argued that we *have* reason to make that assumption, because what *was* future has constantly become past, and has always been found to resemble the past. This shows, it has been said, that the principle in question has been established by a vast amount of our experience in the past; for it has been shown that any doubt as to the validity of the inference from the past to the future was needless. And that proves, so the argument runs, that my expectation is justified, and my belief in the uniformity of nature warranted. But it is easily seen that such an argument really begs the question. If observation confirms an inductive law, this certainly proves that my expectation *was* justified, that the inference from former cases to later ones *was* correct, but the fact remains that this observation proves the principle in question *only* for the cases *so far observed*. As soon as I take the instances in which my expectation has been fulfilled in the past as a guarantee that it will be fulfilled in the future too, I take the very principle for granted which I was setting out to prove. True, observation shows that it is admissible to transfer an inductive law from cases which have been known previously to others which in the meantime have likewise become known, but that does not tell us anything in the least about those cases which have not yet been observed, it does nothing to

32 See Immanuel Kant: *Critique of Pure Reason*, Transl. by Norman Kemp Smith. London 1929, A 190-203, B 235-249.

bridge the gulf between past and future, and that is precisely the point on which the whole problem hinges. The reference to experience, instead of solving the problem, has only pushed it back one stage. And it is plain that any other empirical principle which was put forward as a justification of induction would beg the question in like manner. For the only grounds which one could have for believing such a principle would be *inductive* grounds, and so we should come to face exactly the same question.

Incidentally the reference to the *future* in this question is not essential. The same question arises when we apply the laws that govern our experience to *past* things of which we have no experience—as, for example, in geology when we infer the existence of glacial times, or interglacial events, or in astronomical theories, when we attempt to find out the origin of the earth and the solar system, or speculate abut the state of our universe a billion years ago. So it is not the contrast of future and past that matters. The question we really have to ask is this, When two events C and E have been found to be often associated, and no instance is known of the one occurring without the other, does the occurrence of the one of the two, in a fresh instance, give any good ground for expecting the other? So it is really the problem of *extending knowledge* from cases that have been already observed to unobserved ones which constitutes the core of the matter. Inference from the past to the future is only one particular aspect of this general problem.

So logic, as well as experience, has failed to give us any proof for the validity of induction. *Logic* cannot supply us with such a guarantee, since it is *not* stultifying or self-contradictory to assume that the course of nature may change in the future (in fact, there is some speculation going on at present in this direction.) It cannot, because, more generally, logic does not tell us anything about the world of facts. *Experience* cannot confirm the inductive principle, for all inferences from experience suppose, as their foundation, that the future will resemble the past; but as regards unobserved cases, it is the inductive principle alone that can justify any inference from what has been observed to what has not been observed. All arguments which, on the basis of experience, argue as to the future or the unexperienced parts of the past or present, presuppose the inductive principle; therefore we can never use experience to prove the inductive principle without begging the question.

Thus all knowledge which, on the basis of experience tells us something about what has not been observed, is based upon a belief which can neither be confirmed nor confuted by experience, yet which appears to be as firmly rooted in us as many of the facts of experience. How, then, do we come to cherish such a belief? The answer to this has already been given in the first part of our discussion: there we have seen that the belief is rooted in a psychological mechanism of habit or association. This mechanism works not only in men, but also in infants, and even in brute beasts.

When a child has felt the sensation of pain from touching the flame of a candle, he will be careful not to put his hand near any candle; but will expect a similar effect from a cause which is similar in its sensible qualities and appearance. If you assert, therefore, that the understanding of the child is led into this conclusion by any process of argument or ratiocination, I may justly require you to produce that argument; (…) You cannot say that the argument is abstruse, and may possibly escape your enquiry; since you confess that it is obvious to the capacity of a mere infant. If you hesitate, therefore, a moment, or if, after reflection, you produce an intricate or profound argument, you, in a manner, give up the question and confess, that it is not reasoning which engages us to suppose the past resembling the future, and to expect similar effects from causes, which are, to appearance, similar.[33]

Thus Hume concludes, "that the supposition, *that the future resembles the past,* is not founded on arguments of any kind, but is deriv'd entirely from habit, by which we are determin'd to expect for the future the same train of objects, to which we have been accustom'd."[34]

After having surveyed the course of Hume's argument, we shall now lay bare the principles on which the whole reasoning is founded. These are two:

(1) There is nothing in any object, considered in itself, which can afford us a reason for drawing a conclusion beyond it.

(2) Even after the observation of the frequent or constant conjunction of objects, we have no reason to draw any inference concerning any object beyond those of which we have had experience.[35]

The first principle rules out any possibility that the effect may be discovered in, and deduced from, the cause; the second, that we may infer the future from the past. Thus we must ultimately accept the inductive principle as a sort of *blind instinct* or *automatic device* acting on our mind, and forego all argumentative proof for our expectations about the future. As B. Russell puts it, "If the principle is unsound, we have no reason to expect the sun to rise to-morrow, to expect bread to be more nourishing than a stone, or to expect that if we throw ourselves off the roof we shall fall."[36] Hume himself seemed to have felt some scruples as to the final result of his reasoning. "Their secret nature", he says of bodies, "and consequently all their effects and inference, may change, without any change in their sensible qualities. This happens sometimes, and with regard to some objects: why may it not happen always, and with regard to all objects? What logic, what process of argument, secures you against this supposition? My practice, you say, refutes my doubts. But you mistake the purport of my question. As an agent I am quite satisfied in the point; but as a philosopher, who has some share of curiosity, I will not say scepticism, I want to learn the foundation of this inference. No reading, no enquiry, has yet been able to remove my difficulty, or given me satisfaction in a matter of such importance. Can I do better than to propose the difficulty to the

33 *Enquiry*, IV, II, § 33.
34 *Treatise*, III, 12.
35 Cf. *Treatise*, *ibid.*
36 Bertrand Russell: *The Problem of Philosophy*. London 1912, p. 68f.

public, even though, perhaps, I have small hopes of obtaining a solution?"[37] From this it would appear that Hume did not feel quite happy about his result. But really we should not feel too gloomy about it. We may agree with him that there can be no other justification for inductive reasoning than its success in practice, while insisting more strongly than he did that no better justification is required. For it is his failure to make this second point clear that has given his views that air of paradox which has so much contributed to their being misunderstood. It seems that Hume has seen a problem without, however, being able to solve it; whereas we should realise that the problem is a *spurious* one, that it is a gross mistake to regard natural science as logically open to reproach until we have found a guarantee for induction. In fact, the credit of natural science is not impaired by the lack of such a proof. The only test that is required in science is the test of success in prediction. We are entitled to have faith in our procedure just so long as it does the work which it is meant to do. That is, as long as it enables us to predict future experience and so to gain control over our environment. Of course, the fact that a certain form of procedure has always been successful in practice affords no logical guarantee that it will continue to do so. But then it is a mistake to demand a guarantee where it is logically impossible to obtain one. This is not scepticism; for the fact that we are unable to offer a logical guarantee for an empirical generalisation in no way entails that it is irrational for us to believe it. On the contrary, what is irrational is to *look* for a guarantee where none can be forthcoming; to demand certainty where probability is all that is obtainable.

## (3) What is the Principle of Induction?

It has often been said that we have to rely on induction in order to perform the simplest things in life. When I feel hungry and eat bread, I trust that it will nourish me, when I walk, I trust that the ground will support me and not, for instance, give way beneath my feet. Is this why the performance of all our vital functions seems to be based on induction? Such a view, though containing some truth, goes too far. In order to see in what direction it goes too far, let us pause for a moment and consider the nature of belief, such as the belief that bread will nourish me, the ground support me, etc.

An idea of what belief is may be gained by considering the behaviour of a man forcibly dragged into a fire. He will resist with all his might, strike out with hands and feet like a madman. Is this a sort of reasoning? Does he, e.g., remember bad experiences in the past, does he tell himself in a low voice, surely the probability is extremely high that the fire will burn him? No; he strikes out like a madman—well, this *is* belief. When people in everyday life do a thousand things, relying, as we should say, on induction, if they believe that fire burns and water quenches

37 *Enquiry*, IV, II, § 32.

thirst, belief almost never appears as articulated thought, but consists in what people *do*. What, then, is it that is wrong with the view in question? It is the idea that our actions in everyday life are founded on *discursive, argumentative thought*, which, in its turn, is based on induction.

I think that the strangeness of the idea that, in performing actions of everyday life, we rely on inductive reasoning would be greatly diminished if it were expressed in the language of a behaviourist theory of belief, i.e. a theory which interprets a belief—such as in the examples adduced—as a *pattern of behaviour*.

Now if the account I have given is right, if belief is often not the rational sort of thing we take it to be, we see at once why it is so futile to seek rational grounds for it. On the contrary, we can now see more clearly than before that the sort of belief we have in mind when we say that fire burns and water quenches thirst, shows itself in the whole of our behaviour, our relations etc. From this it appears that what we call "belief" is more like the operation of an *instinct* than of reason. It is only when this instinctive belief is translated into words, that the problem arises; "Are there any grounds for holding this belief? Can it be proved?" Once we realise, however, that what we call "belief" is more adequately defined as a certain pattern of behaviour, we begin to see that this sort of inquiry is pointless. There are a great many beliefs which are not the product of any reasoning, and of which we only become aware as a result of translating a pattern of behaviour into words. Once it is clear that certain beliefs belong rather to the instincts with which we are endowed by nature than to rational thought, it is no longer so surprising that no rational vindication can be found for them. And if this is admitted for a certain *class* of beliefs, it does not take much effort to persuade ourselves that the same will apply to all sorts of belief as far as they are empirical generalisations.

Let us, however, not be rash. The argument just produced contains a flaw which I shall now attempt to bring to light. We should clearly distinguish between two different sorts of question: the question as to the *origin* of our beliefs and the question as to their *justifiability*. The dispute is not as to whether my belief that bread will nourish me is *caused* by some blind instinct or is a pattern of behaviour put into words, but only whether this belief *is true* and, if so, whether it can be demonstrated, i.e. derived from some truths which are more universal or more fundamental than that in question. Even if it is conceded that most of our beliefs held in everyday life *do* spring from habit or indeed consist of a pattern of behaviour, the question still remains whether we have any *good grounds* for entertaining such beliefs. What we are really discussing when we examine the problem of induction is not a *psychological* question concerning the way in which certain beliefs have come to be entertained by the mind, nor an *historical* question concerning the way in which such beliefs have been acquired or have developed or have gained hold over us, but an *epistemological* question concerning the *justifiability* or *rationality* of such beliefs.

Having made the issue clear, let me resume the discussion of the problem. What Hume has shown is that logic as well as experience are unable to prove the truth of a universal statement concerning matters of fact. On the other hand, the principle by which all empirical generalisation is governed, the principle of induction, is *not* self-evident, nor capable of a demonstration; and any attempt at proving the principle by appealing to experience is bound to be circular. Experience goes as far as it goes. In other words, every empirical generalisation is a sort of *jump* from particular cases to a universal law, and there seems to be no guarantee that we are right to jump. What the empiricist says comes to this: If you *want* to jump, jump; you must take the risk; and that is all that can be said about it.

One last word before leaving this subject: Ought we not to formulate in a precise and unequivocal manner what the principle of induction states? Hume himself has expressed this none too clearly, merely giving the vague statement that "the future resembles the past." Now the unsatisfactory state of the present discussion on the problem of induction seems to me to be due to preoccupation with the problem of the *validity* of induction; the more fundamental question concerning the *meaning* of the principle of induction has rather been neglected, and it is this latter which needs careful analysis.

How, then, are we to state the principle of induction? First of all it should be noticed that this principle is not a consequence of the purely mathematical axioms of the calculus of probability. On the contrary, the application of this calculus *presupposes* some regularity in the world. Now the principle of induction seems to express a characteristic feature of the actual world, which may be called "*the uniformity of nature.*" The belief in the uniformity of nature is the belief that everything that has happened or will happen is an instance of some general law to which there is *no* exception. Thus the principle of induction seems to state an *assumption concerning the general constitution of nature*. Jevons and Peirce interpreted the principle of induction as assuming that our observations represent "fair samples" of a thoroughly statistical world.[38] Broad, Keynes and Nicod introduced more refined formulations of the principle of induction, such as the "principle of limited depth and variety."[39] Now whatever these formulations amount to, they labour under one fundamental difficulty: since they are supposed to be assertions about the actual world, they must themselves be *factual statements of a universal character*. But *if* they are universal statements concerning matters of fact, they themselves stand in need of being supported by inductive evidence: how, then, can they express the principle of induction? You see what the situation is like: if the principle of induction states something about the actual world, it is itself dependent on induction and can therefore, not *be* the principle of induction. If, on the other hand, it does not assert anything about reality, if it has no content it is

38 Cf. William S. Jevons: *Pure Logic and Other Minor Works*. Ed. by R. Adamson and H. A. Jevons. London 1890; Charles S. Peirce: *Collected Papers*, vols. II, VI. Ed. by C. Hartshorhe and P. Weiss. Cambridge, Mass. 1932-1965.

39 Cf. Charlie D. Broad: *Scientific Thought*. London 1923; John M. Keynes: *A Treatise on Probability*. London 1921; Jean Nicod: *Foundation of Geometry and Induction*. Transl. by P. P. Wiener. London 1930.

difficult to see how it can be used as a premise in making generalisations. If you say the principle of induction is one of the *necessary conditions for the possibility of knowledge*, as Kant did,—well, we shall examine this point later on.

Other writers have tried to formulate the principle differently. Thus B. Russell says in his *Problems of Philosophy*:

> The principle we are examining may be called the *principle of induction*, and its two parts may be stated as follows:
> (*a*) When a thing of a certain sort A has been found to be associated with a thing of a certain other sort B, and has never been found dissociated from a thing of the sort B, the greater the number of cases in which A and B have been associated, the greater is the probability that they will be associated in a fresh case in which one of them is known to be present;
> (*b*) Under the same circumstances, a sufficient number of cases of association will make the probability of a fresh association nearly a certainty, and will make it approach certainty without limit."[40]

The principle enunciated in this way applies only to the expectation of a *single* fresh instance. It is, however, easy to modify the statement so as to make it apply to a general law. We are thus led to restate our principle as follows:

> (a) The greater the number of cases in which a thing of the sort A has been found associated with a thing of the sort B, the more probable it is (if no cases of failure of association are known) that A is always associated with B.
> (b) Under the same circumstances, a sufficient number of cases of the association of A with B will make it nearly *certain* that A is *always* associated with B, and will make this general law approach certainty without limit.[41]

According to Russell's view we have to seek, not for a proof that an observed regularity *must* be fulfilled in a fresh case, but only for some reason in favour of the view that it is *likely* to be fulfilled. Of this principle Russell observes that it can neither be *proved* nor *disproved* by an appeal to experience. He concludes from this that

> we must either accept the inductive principle on the ground of its intrinsic evidence, or forgo all justification of our expectations about the future. If the principle is unsound, we have no reason to expect the sun to rise to-morrow, or to expect bread to be more nourishing than a stone, or to expect that if we show ourselves off the roof we shall fall. When we see what looks like our best friend approaching us, we shall have no reason to suppose that his body is not inhabited by the mind of our worst enemy or of some total stranger.[42]

40 Bertrand Russell: *The Problem of Philosophy*. London 1912, p. 66.
41 *Ibid.*, p. 67.
42 *Ibid.*, p. 68f.

Unfortunately, Russell's explanation does nothing to remove the difficulty. For again we come face to face with the question, "How do we know that the principle of induction—even if stated as an assertion about a certain *probability*—is true?" If it is taken to be a statement about the general make-up of the universe, we are faced with the old difficulty, that it is an empirical generalisation which must itself be based on induction, and, therefore, cannot be *the* principle of induction. It is futile to argue that it must be accepted "on the grounds of its intrinsic evidence"; for, first of all, the principle referred to is certainly not as self-evident as, e.g., the statements "Any two things together with any two other things make four things"; "What is white cannot be black at the same time", "A door which is open cannot be shut at the same time", "It is impossible for a square to be round" etc. That is, we cannot *imagine* what it would be like to see a square that is round or to see a white patch that is black, or to see a door which is open and shut at the same time, nor can we imagine a group of five things consisting of 2 + 2. On the other hand, we *can* imagine a world in which there is no order at all or less order than in this world of ours, a universe in which an event A is sometimes followed by B, sometimes by C and sometimes by D. It is certainly not nonsensical to contemplate such a possibility. As there is no intrinsic difficulty in imagining such situations, we cannot argue that the principle of induction is self-evident in the sense in which the other statements may be said to be self-evident; that is, it cannot claim to be a necessary or logical or *a priori* truth. And moreover, there is a fatal objection to accepting the inductive principle "on the grounds of its intrinsic evidence", namely that a similar principle, that of causality, has turned out to be untrue.

I shall now try to give my own account of the matter. Consider the case of a man who is planning to build a bridge. He will first choose a suitable site and examine the ground; then he will make a sketch and draft a blue print; then he will proceed to make certain calculations, compute the pillars' strength, and so on, rather than leave it to chance. When you now ask him, "Why do you do all that? Are you sure that your bridge will not collapse?", he will make some reply as, "Well, you see there are certain theorems in mechanics concerning solidity, and what I have done is to apply these general laws to the conditions of this particular case." When again you ask him, "And why do you suppose that your general laws are true?", he may give you some detailed account of the way in which the laws have been established, describing experiments, explaining the principles of mechanics and setting out the mathematical method of applying these laws of nature. But if, after all has been explained, you still carry on your inquiry and ask him, "What makes you believe that your laws established by experiments in the past will hold good in the future?", that is, if you insist that his explanations will not do and inquire whether he has any *ground* for believing that the laws will not change in the future, then he will be at a loss. It looks as if he *ought* to give you some information in order to bridge the gulf from the past to the future; and that is why he will finally refer you to the principle of induction. But that does not get us any further. For if it is taken to be a *factual* statement, we are confronted with the old difficulty—how can we ever know that it is true? If it is said that it is true on *experiential* grounds, the answer is circular, since generalising from evidences in the past already *requires* the principle in question. And if the principle is claimed to be true on non-experiential grounds—by saying some such thing as, "We *know* the universe to be rational,"—we say more than we can assert with good conscience; in fact, we come into conflict with physics. So it does not work either way.

Now this failure is due to the whole way of presenting the question. How does he know? Given all the facts—the experiments, the tests and so on,—he *proceeds* in this way. That is all. The "principle of induction" is just a word to *denote* this procedure. It is a sort of short-hand for this whole pattern of reasoning. It is a case, not of *knowing that*, but of *knowing how*. No information about facts, however detailed, however profound, can help us. For facts are *what they are*: they never point beyond themselves. In order to arrive at an empirical generalisation we should have to make use of the very principle we are in search of.

So all the learned talk by Broad, Keynes, Nicod and others was to no effect, because they did not see the salient point. The principle of induction is neither a factual nor an *a priori* statement, neither synthetic nor analytic, because it is *not a proposition at all*. In actual fact it is a *rule of procedure* that codifies our activity of generalising. (I deliberately say "codifies" and not "guides" because we act according to it even before it has been formulated.)

Let those who still think that the principle *asserts* something compare the principle of induction with a well-established law, such as Newton's general law of gravitation. This law can be stated exactly with the help of the mathematical symbolism. Further in astronomy Newton's law is used as a premise from which a number of consequences can be derived (Kepler's laws). What strikes us in the case of the principle of induction is two things: (1) that it cannot be stated with that sort of precision that is characteristic of Newton's law, or, indeed, of any law of nature; (2) that it is never used as a *substantial premise* in scientific reasoning: it is not a *premise from which we draw conclusions*, but the *scheme in accordance with which we actually proceed when making generalisations*. That is what is meant by saying that it is a rule of procedure.

In philosophy we often ask questions which look like, but are not, requests for factual information. "How do you know that you have got a toothache?" "Why, I feel it". Nonsense! One is in such a case tempted to ask, "Well, is there anything else you can do with your toothache but feel it?" And this way of asking points to the fact that the words "I feel it" are a *bogus* explanation. What I really want to achieve with these words is to *shake off* the question as unjustifiable. I simply have a toothache, and that is the end of the matter; no appeal to any other evidence is relevant or even admissible.

Now if I ask someone, "Why do you suppose that the laws of mechanics will operate in the future?", and if he, in reply to this question, refers to the principle of induction, this again is a bogus explanation. Notice the stereotyped form of his answer—"because of induction": it is just like the other case in which one says, "I know I have got a toothache *because* I feel it." What a queer use of the word "because"! It looks as if "because" introduces an explanation; *but it does not*: for what comes after it is not a new piece of evidence, or indeed any factual information, but only serves the purpose of bringing out the *logical status* of what has been said before. "Because I feel it" denotes the character of *immediacy* of the datum, "I have a toothache"; "because of induction" denotes the character of the *procedure* which we follow in making generalisations. But there is no ground, not a whit of factual information behind these words; that is why I called them *bogus explanations.*

I spoke of the principle of induction as a *rule of procedure* or a *pattern of reasoning*. But here a word of caution must be added to prevent muddled thinking.

People have sometimes curious ideas about induction. They seem to think that induction is a *method* which guides us in deriving general laws from single facts of experience. Now let me say as clearly as I can that this is a myth. Is it not fantastic to think of a machine for making discoveries? And yet that is precisely the point to which the view "induction is a method" boils down. For if induction were just to consist of a set of mechanical rules which lead you from single statements of observation to a universal law, you could teach people how, by applying these rules, to make discoveries; and then there would be no intrinsic difficulty in designing a machine which does the same thing. Just as you have a *calculating* machine, you could build an *inducting* machine: if you put in your observational statements through a slot on one side, on the other side out comes the law. Too good to be true. No, there is no such machine, and there is no recipe for making discoveries. What actually happens is rather like this: you have a lot of facts, all obtained by observation; but there seems to be no intelligible connection between them; so you are worried, you consider the matter from time to time; you rack your brains—and nothing happens. If you approach your friend, the philosopher, for advice, you will be shocked to find that he cannot teach you anything: no expert on induction, no authority on Concomitant Variation can help you in your distress. There you are, left with no method, no rule, no recipe, absolutely forlorn and with a feeling that everything you have heard on induction was rubbish. You sit down, or walk up and down, and wait, till one day, if you are a genius, you will have a brain wave: in a flash of inspiration you will see things arrange themselves in a beautifully new way, a delightful pattern bringing order and harmony to what before seemed unconnected and unintelligible. So you put forward your law very tentatively; and then you begin to check it: from your general assumption you gradually get down to predictions as to what will happen in certain specifiable conditions and wait till your prediction can be tested. In other words, you *jump* from the cluster of facts to the theory, and then systematically and methodically, you try your best to confute your own theory by trying out every possibility of its being wrong, that is, by putting it to the test of experience whenever you can. And mind that you leave no loophole through which any bit of your theory can escape. That is the actual situation; and there are people who called that whole procedure induction.

Granted, there *are* cases to which the scheme of induction *does* apply. Suppose that a doctor in a ward has frequently noticed that dark-haired people have a disposition for stomach trouble, fair-haired people for lung trouble, then he will naturally proceed to form the maxim that people with dark complexion, on the average, or in so-and-so many cases out of hundred, are exposed to such and such diseases, etc. This *is* a case of simple generalisation—just in the same sense in which, from a few single swans I have seen, I infer the general statement "All swans are white". But it must be stressed that induction, in this sense, has a very limited scope of application: most cases, above all the important discoveries in science, are of a different nature; they are not reducible to that simple scheme. The idea of the unconscious, or the idea of atoms, or the theory of matter as a packet of waves, and so on, were not arrived at in this way. They have not been *derived* from single facts of observation *at all* and they belong to a quite different order: I mean, a law is of a totally different order from the evidence by which it may be supported. There is no rational method which leads you from the facts to the theory. There is no more any method for making discoveries than for writing poems: the discoveries which *can* be made according to such a recipe, I am afraid, will be no better than poems fabricated by some similar device.

If this account is right, it throws a flood of light on the philosophical discussions which centre on the principle of induction. If that principle is a rule of procedure, then the problem of induction, as ordinarily conceived, is a *spurious* problem: the question how it can be justified is a *pseudo-question*, and this explains the odd fruitlessness observed in so many philosophical attempts at its solution. The principle lays down the rule in accordance with which we proceed; we may justify our procedure in any given case by referring to the general principle of induction; if, however, we seek for a *guarantee*, or a *justification* of this principle, we are in a hopeless muddle.

## (4) J. S. Mill's Account

There is an old rule, formulated long ago in scholastic philosophy, that warns us against confusing *post hoc* and *propter hoc*. This means that from the fact that an event E happened *after* another event C we must not infer that E happened "because of" C. In other words, the rule asserts, that the meaning of the statement "E follows C" is entirely different from the meaning of the statement "E is the effect of C." But what is the difference between the two meanings? This question is the philosophical problem of causality.

Now there is no doubt that we do continually apply the rule and that it is a perfectly good and sound rule which people ought to follow even more often than they do. If we take a certain medicine and get well after it, it would be rash to suppose that the medicine was the *cause* of our getting well. Or if we try to discover the cause of a war, we know we are looking for much more than merely for events which *preceded* the war. It is plain, therefore, that we actually are in possession of some kind of criterion, which enables us to distinguish between events that merely *follow* each other and events that *cause* each other; for we do make this distinction every day, and we make it with sufficient accuracy to have a good part of our behaviour guided by it.

What, then, is this criterion? In order to find it, we have to see how the truth or falsehood of any causal statement is checked, that is to say, the criterion we are in search of, must already be contained in the way in which a causal law is verified. This leads to the question, How do we verify the statement that the taking of some medicine was not only the *antecedent* but also the *cause* of the recovery of the patient? At a first glance there seem to be in this case two different ways of verification: observation and insight.

(1) We try the medicine many times and on many different patients. If we find that in every single case a person suffering from a particular disease is cured, we shall say: the recovery after the use of the medicine was not pure *chance*, but was *caused* by it. In other words, if the event E *always* occurs after the event C, and if C never occurs without being followed by E, that is, if E *invariably* follows C, then we do not hesitate to call C the "cause" and E the "effect". It should be noticed that we do this whether we are able to "explain" the cure or not; there are cases in which all we know is that a medicine is good, without in the least knowing how it works.

This brings us to the criterion which we have already met in Hume's discussion—the criterion of *regularity*. Now the conception of causality has been severely criticised since Hume; therefore it will be well to consider the matter once more.

Perhaps the best known objection against identifying causality with regularity is the observation that nothing is more regular than the succession of day and night, and yet we do not call the one the cause of the other. The answer to this is that we have explained the relation of cause and effect in terms of "events"; but "day" and "night" are not events (that is, instantaneous changes, or successions of instantaneous changes), but *intervals of time*, distinguished by brightness and darkness and possessing more or less of permanency; therefore our explanation does not apply. As J. S. Mill puts it, "between the phenomena which exist at any instant, and the phenomena which exist at the succeeding instant, there is an invariable order of succession; and, as we said in speaking of the general uniformity of the course of nature, this web is composed of separate fibres, obtaining invariably among the separate parts. To certain facts certain facts always do, and, as we believe, will continue to, succeed."[43] Now the succession of day and night cannot be regarded as one of those single fibres.

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43 John Stuart Mill: *A System of Logic Ratiocinative and Inductive*. Book III, chapter V, § 2.—All quotations from Mill according to the *Collected Works of John Stuart Mill*, Vol. VII. Toronto-London 1973.

A second remark is more pertinent. When we define the cause of anything to be "the antecedent which it invariably follows", we do not mean by this "the antecedent which it invariably *has* followed in our past experience." What we mean is, not only that the antecedent *has* been followed by the consequent, but that it always *will* be so. And this would not be true of day and night. We have no good reason for believing that night will be followed by day under *all imaginable circumstances*. If the earth ceased to rotate, e.g. as the result of a sudden increase in the tidal friction, night might be eternal on one part of the earth, and day eternal on the other. Or, if an opaque body or medium were interposed in a straight line between the earth and the sun and were to remain permanently in this position, the light of the sun would be extinct, and it would be always night. Again, the same might happen, if the sun were surrounded by a dark cosmic cloud; etc. That is why we do not call night the cause, nor even a condition of day. On the contrary, we see that the regular succession of day and night is itself conditioned by a lot of other circumstances. We might express this by saying that this succession is a *derivative sequence*, depending on something else.

The lesson we may learn from this is that there is no need to interpret a regular succession in every case as an example of a causal connection. Though a fact may, in experience, have always been followed by another fact, yet if the remainder of our experiences teaches us that it might not *always* be so followed, we have no right to speak of a causal nexus.

Let us, however, consider some more experiences in order to see whether our account was right. "The No. 4 bus arrives at Carfax each morning at 8.40; each time, it is followed a minute later by the No. 6 bus; experience shows that this is a regular sequence; according to the regularity view, the occurrence of the No. 4 bus is therefore the cause of the arrival of the No. 6 bus". "Every morning the cock crows before the sun rises; therefore the cock's crowing produces the sunrise." "Nothing is more regular than the sequence of notes in a song. Shall we say, on that account, that one note *causes* the next?" "Suppose a little boy on his way to school passes a slot-machine every morning. When he passes it and looks into the opening, he regularly finds some chocolate there. Hence the boy's approaching and looking into the opening was the cause of the chocolate's being there." "We may well imagine a philosopher exclaiming "How absurd all this is! Is it not a conclusive refutation of the regularity view?"

It is not; for it is easy to see that the regular sequences are mere *derivative* sequences, their regularity being due to some external circumstance. For instance, in the case of the buses, we *know* already that their "regular succession" is simply accounted for by the fact that the buses run according to a timetable; the timetable is a rule which is followed by the bus driver, and this is the full explanation of the fact. So there is no need to seek for a particular causal connection to explain the sequence. Moreover, it is plain that the occurrence of the No. 4 bus is not an *invariable* antecedent of that of the No. 6 bus: we can think of many factors which would prevent this particular succession from materialising (one bus may be late, etc.), and this is enough to dismiss any idea of a causal connection in this case. In the case of the boy who finds some chocolate in the machine, we would say that the cause of this repeated event was unknown. It may just be due to chance, or we may discover the cause in the form of a benevolent old gentleman who, just before the boy passes, drops a penny into the slot, presses the button, and leaves the chocolate in the opening. Our refusal to take the regular succession as evidence of a causal connection is justified by the fact that we have ample evidence for assuming that the machine is not acted upon by the mere approach of a human body. There is a vast number of experiences which show what sorts of things have an influence on the machine and which have not. That is why we reject the idea of a causal connection.

This raises the general question, How are we to discriminate some sequences as non-causal even though they are uniform over a very wide range; and some other sequences as causal though we may have observed only one or a few instances of them? In other words, how do we know that one event is an *invariable* antecedent of another? The answer to this was first given by John Stuart Mill in his canons of induction. Mill's goal was to give rules that would enable us to single out from among the circumstances which precede or follow a phenomenon those and only those with which it is connected by a causal law. There are two ways of doing that. One method is to compare different instances in which the phenomenon occurs. Another is to compare instances in which the phenomenon *does* occur, with instances (in other respects similar) in which it does *not*. These two methods are called by Mill the *Method of Agreement*, and the *Method of Difference*. The *Method of Agreement* may be formulated as follows:

If two or more instances of a phenomenon A have only one circumstances B in common, the circumstance in which alone all the instances agree (that is B), is the cause (or effect) of A.[44]

The *Method of Difference* may be expressed as follows:

If an instance in which the phenomenon A occurs, and an instance in which it does *not* occur, have every circumstance in common save one, that one occurring only in the former; the circumstance in which alone the two instances differ, is the effect, or cause, or an indispensable part of the cause of the phenomenon.[45]

These two methods have something in common, and that is the successive exclusion of the various circumstances, which are found to accompany a phenomenon in a given instance, in order to ascertain which are those among them that can be absent consistently with the existence of the phenomenon. The Method of Agreement stands on the ground that whatever can be eliminated is not connected with the phenomenon by any law. The Method of Difference has for its foundation, that whatever *cannot* be eliminated, is connected with a phenomenon by a law. Of these methods, that of Difference is more particularly a method of artificial experi-

44 Cf. *System of Logic*, III, VIII, § 1.
45 Cf. *Ibid*., § 2.

ment. For it is in the very nature of an experiment, to introduce into the preexisting state of circumstances a perfectly definite change, while that of Agreement is more especially the resource employed where experimentation is impossible.

These two methods are frequently combined in what may be called the *Joint Method of Agreement and Difference*; to these are added two further canons which complete the enumeration of the means which we possess for exploring the laws of nature.[46] One is the *Method of Residues*. Its principle is this:

Subducting from a given phenomenon all the portions which, by virtue of preceding inductions, can be assigned to known cases, the residue of the phenomenon will be the effect of the remaining antecedents.[47]

Of all the methods of investigating laws of nature, this is "the most fertile in unexpected results: often informing us of sequences in which neither the cause nor the effect were sufficiently conspicuous to attract of themselves the attention of observers."

The methods so far framed, however, cannot be applied to those

> Permanent Causes, or indestructible natural agents, which it is impossible either to exclude or to isolate; which we can neither hinder from being present, nor contrive that they shall be present alone. (…) The pendulum, for example, has its oscillations disturbed by the vicinity of a mountain: we remove the pendulum to a sufficient distance from the mountain, and the disturbance ceases: from these data we can determine by the Method of Difference the amount of effect due to the mountain.

But the pendulum cannot be removed from the influence of the *earth*: "we cannot take away the earth from the pendulum, nor the pendulum from the earth, to ascertain whether it would continue to vibrate if the action which the earth exerts upon it were withdrawn. On what evidence, then, do we ascribe its vibration to the earth's influence? Not on any sanctioned by the Method of Difference; for one of the two instances, the negative instance, is wanting"; nor by the Method of Agreement, "for though all pendulums agree in this, that during their oscillations the earth is always present, why may we not as well ascribe the phenomenon to the sun, which is equally a co-existent fact in all the experiments?"[48]

If, therefore, there were no other methods available, we should be unable to determine the cause of the vibrations of a pendulum. But we have still a resource. "Though we cannot exclude an antecedent altogether we may be able to produce, or nature may produce for us, some modification in it. By a modification is here meant, a change in it, not amounting to its total removal." In the case of our pendulum, for instance, though we cannot exclude the influence of the earth, we can modify it in quantity: we can increase or diminish the distance of a pendulum from the centre of the earth, by placing the pendulum on the top of a mountain or in the

46 Cf. *Ibid.*, § 4.
47 Cf. *Ibid.*, § 5.
48 *Ibid.*

bottom of a pit. If a modification in the distance is *always* followed by a change in rate of the oscillation, the other circumstances being the same, we may safely conclude that the oscillations are traceable to the influence of the earth. The method by which this result was obtained, may be termed the *Method of Concomitant Variations*; it is regulated by the following canon:

"Whatever phenomenon varies in any manner whenever another phenomenon varies in some particular manner, is either a cause or an effect of that phenomenon, or is connected with it through some fact of causation."[49] So when we find that the conjunctions or oppositions of different stars bear no influence on the fortunes of mankind, this shows that the stars have no such power.

We may summarise this discussion by saying, The idea of causation was reduced by Hume to a number of ideas the most important of which is regularity; this account labours, however, under the difficulty that not *every regular* sequence is regarded as a *causal* one. The empirical concept of causality was better formulated by J. S. Mill in his five canons of induction. These canons offer criteria by which one can determine whether there is, or is not, a causal connection between certain phenomena. So his five canons may be regarded as expressing the *definition of causality*; that is to say, we speak of a *causal connection*, whenever this connection can be established by means of any of the methods enumerated by Mill.

Before leaving this subject let us consider some other objections against the empirical concept of causality, as viewed by Hume and Mill. It may be said that, on this view, it would be absurd to inquire into the causes of a war. For history does not repeat itself, there are no regular sequences, and therefore causality, interpreted in the sense of Hume and Mill, cannot be applied. The answer to this is, I think, that the expression "causes of a war" may mean three different things: either the events, conditions, processes, inherited racial dispositions and other factors which, in actual fact, influenced the impulses of the masses; or the motives which led the leading statesmen of the nations to their actions, or alliances, pledges given, statements made by responsible cabinet ministers on the policy of their government, in short things which are *grounds* or *reasons* in much the same sense in which these terms are used in law. In the first sense, what the causes have been, *may* be found by observation, that is, by resolving the whole web of causality into separate fibres—though it must be admitted that we are still very far from a real understanding of historical processes. And yet there seems to be a grain of truth in common sense according to which the actions and reactions of the people can, in many cases, fairly well be predicted. "A man who at noon leaves his purse full of gold on the pavement at Charing-Cross, may as well expect that it will fly away like a feather, as that he will find it untouched an hour after. Above one half of human reasonings contain inferences of a similar nature, attended with more or less degrees of certainty proportioned to our experience of the usual conduct of

49 *Ibid.*, § 6.

mankind in such particular situations."[50] There is, then, a sense in which the causes of a war *may* be determined by reference to the usual behaviour of people in certain situations. Needless to say, our knowledge of such behaviour is derived from experiences in the past through generalisation; and here the concept of causality, as defined by Hume and Mill, *does* apply. But if by the "causes" we understand the *motives* by which the heads of a government were guided in their decisions and actions, then these motives *cannot* be determined by observation, or by experiences in the past alone, and we shall correspondingly have to distinguish between *cause* and *motive*. And the same applies to the *reasons* that I have mentioned (e.g. formal pledges).

Take another question. If somebody were to tell us that more people die on Thursdays than on any other day, we should at first be sceptical, and even if he were able to produce ample statistical evidence in favour of his statement, we should still hesitate to attach any weight to it: we should prefer to say that the facts were due to *chance*. Now take another case and contrast it with this. When a chemist announces the existence and properties of a newly discovered substance, if we are confident of his accuracy, we feel assured that the conclusions he has arrived at will hold universally, though the induction is founded on but a single instance. We do not withhold our assent, waiting for frequent repetitions of the experiment, provided that there is nothing out of the way in the experiment. Here, then, is a general law of nature, inferred without hesitation from a single instance. Now the question which I want to discuss is this, Why do we proceed so differently in the two cases? Or, to put the question in the words of Mill: "Why is a single instance, in some cases, sufficient for a complete induction, while in others, myriads of concurring instances, without a single exception known or presumed, go such a very little way towards establishing a universal proposition? Whoever can answer this question", adds Mill, "knows more of the philosophy of logic than the wisest of the ancients, and has solved the problem of induction."[51]

One might argue that, in the example considered, we have an *insight* into the fact that the name of the day cannot possibly have an influence on the rate of mortality. Before accepting such an appeal to insight let us see whether we can not find some other explanation. When we ask the chemist why he is prepared to acknowledge the newly discovered law on the strength of a single experiment, he would make some such reply as, "Well, you see, that is exactly the sort of thing that has happened innumerable times before in similar circumstances; so the experiment is supported by the whole mass of analogous experiences in the past." On the other hand, the statement about the death-rate is *not* supported by anything remotely similar in our experience; so the new statement, even if corroborated by statistical evidence, would still be outweighed by the *whole remainder of our experience*; that is why we cannot accept it, but would rather say that a relation

50 Hume, *Enquiry*, VIII, I, § 70.

51 *System of Logic*, III, III, § 3.

such as that observed is just due to *chance*; which is only another way of saying that there is *no* causal correlation. It would be different, indeed, if no chemical experiment had ever been made before and if the experiment in question were the first one of its kind; in such a case, we should not be prepared to accept the law on the strength of a single case: we should have to spend a lot of time in experimenting before we could persuade ourselves that what we have observed is due not to chance but to a law.

To express the same thing in a slightly different way, we might say that, in making generalisations, we are already *influenced* by the types of laws we know. It is a mistake to think that we can proceed, in every individual case, unbiased by any former experience. On the contrary, our activity of generalising, of framing hypotheses and so on, is already guided along definite channels in virtue of the whole pattern of our past experiences, as embodied in laws, which form, as it were, a *background* into which any newly observed regularities are inserted. That is, we try to construct our hypotheses in a style which makes them conform to the whole background. Now to generalise the observation that more people die on Thursdays means to introduce an element which is not compatible with the remainder of our experience, that is, which cannot be fitted into the edifice of laws already accepted, without reconstructing the whole. Thus in answer to the question why it is that we proceed in so different a fashion, we may say that in accepting a general law we do not consider just the *evidence* for it, but also the *type* of law it is, and whether it corresponds to the type of laws which are already generally acknowledged.

Thus it is not observation by itself, but observation *seen against the background of other laws* that influences our judgement.

The type of objection considered so far rests on the fact that we *know* there to be *regular sequences* which still lack the character of being *causal* sequences, We are now in a position to answer the objection. There is something right and something wrong with it. What is right is the fact that the mere observation of a regularity in a *single series* of events is not yet sufficient to establish its *causal nature*. So far the objection is perfectly right and draws our attention to an important point. What is *wrong* with the objection, however, is the failure to see that, in the last analysis, it is the *conformity of an observed regularity* with the pattern of a great number of other laws, *all based on regularity*, which decides in favour or against the presence of a causal connection.

To make things still clearer, consider the present state of psychical research. There is a vast mass of evidence accumulated, and nonetheless many scientists hesitate to accept it. Prejudice? Yes, but a justifiable one: the facts do not seem to fit into the picture of the world as we know it. The difficulty is in part to establish evidence, and in part to find out *what would constitute evidence*. The latter difficulty is due to the entire absence of any type of connection to which supporting cases, if available, could be assimilated.

To return once more to the example of more people dying on Thursdays than on any other day: what makes us so reluctant to accept statistical evidence in such a case is the idea that the seven-day week is purely conventional, that it could be changed—as indeed it was changed at the time of the French Revolution—without affecting in the least the natural course of events. That is why we fail to see what the name of the day can have to do with the death-rate. Suppose, however, that new facts have come to light which suggest that there is something like a cycle in nature, a cosmic rhythm of seven days, say, that a new sort of radiation undergoes a periodic change during this time. That would at once alter our whole attitude and make us look at a week as something that is based on a periodicity of nature. To say, in these circumstances, that the rate of mortality has something to do with the day of the week would be a plausible hypothesis and not an extravagant idea.

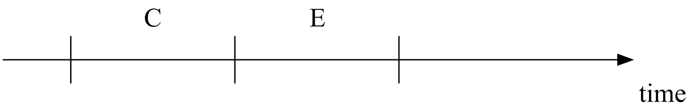
Let us now turn to another problem. Imagine for a moment the world of experience to consist of one kind of experience only, say of a sequence of musical notes which repeat themselves in a given order without interruption. The question which I should like to put to you is whether you think that there is any sense in asking whether this series of notes is a *causal* sequence in which one note is produced by the anteceding and produces the next, or a mere regular succession? I should think there is none. In actual fact, such a case illustrates a situation where we have no system of laws to refer to, and nothing but a single series of experiences which are such that none of Mill's canons can be applied. For, on the present supposition, we can neither hinder a note from occurring, nor are we able to produce some modification in it. But if the methods by which we ordinarily establish a causal connection, can no longer be applied, the idea of such a connection becomes pointless.

## (5) The Scientific Scheme of Causality

In the course of time two different pictures of what causality is have been evolved in science: the one is the classical picture, the other that picture which has emerged in quantum mechanics. I wish, first of all, to give an account of the *classical idea* of causality as it has been worked out by the natural sciences from the 17th century on.

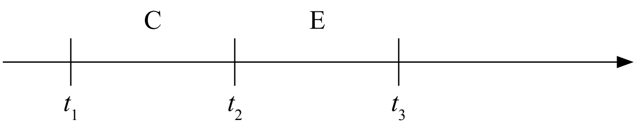
I shall begin by contrasting the way in which philosophers speak of causality with the way in which causality enters science. Up till now we have primarily been concerned with stating and analysing the idea of causality in terms which are familiar to philosophers. Thus we said that two events C and E are connected as cause and effect if they are contiguous in space and time, if C precedes E and if C is always followed by E. A similar formulation of what philosophers commonly understand by "cause" can be gathered when you look up any philosophical dictionary. Thus Baldwin gives the following definition of cause and effect: "Cause and effect (…) are correlative terms denoting any two distinguishable things, phases, or aspects of reality, which are so related to each other that whenever the first ceases to exist, the second comes into existence immediately after, and whenever the second comes into existence, the first has ceased to exist immediately before."[52]

This definition may seem lucid and straightforward enough. It labours, however, under a difficulty which makes its acceptance impossible to the scientist and which I shall now try to bring out. The blemish in this definition, and in fact in a whole case of similar definitions, is the *temporal contiguity* of cause and effect which the definition asserts. The idea underlying the definition is roughly this: the cause is one event which endures for a certain finite time; and the effect is another event likewise enduring for a certain finite time; and it is supposed that when the moment C ceases to exist, E comes into existence immediately, i.e. without any separating time-interval;
like this:




This idea, however, gives at once rise to very serious difficulties. First of all, we must understand quite clearly that if time is regarded as a series of instants, it is a series in which no two instants are consecutive, since between any two there are always other instants. Since there are other terms between these others, and so on *ad infinitum*, it is obvious that there are an infinite number of instants between any two, however nearly simultaneous these two may be. A series having the property that there are always other terms between any two, so that no two are consecutive, is called "dense". Thus the instants of time in order of their succession form a "dense" series. Now if an "event" had no duration and took up only one instant, it follows that there could be no consecutive events, so that the above definition would not apply.

Consider, however, the case that the two events, cause and effect, each take up a certain finite time, as is indeed suggested by the wording of the definition; suppose C to last from $t_1$ to $t_2$, and E from $t_2$ to $t_3$:




Now what can we say about the relation of the two events? Suppose C is the lighting of a cigarette, i.e. the contact of a cigarette with a burning match, E is the rising of smoke from the cigarette; in this case it will take a few seconds till the effect, the smoke, comes into existence. But how, exactly, are we to conceive of the whole process? Are we to imagine that the two events are purely static, that is, that the cause, the contact of the burning match with the cigarette, is one state, go-

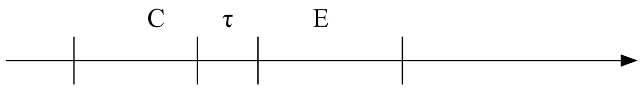
52 James Mark Baldwin (ed.) *Dictionary of Philosophy and Psychology*. New York-London 1901, vol. I, p. 164.

ing on without change, and that the rising of the smoke is another state which lasts until the cigarette is finished? But you all know that this is not so: the contact of the burning match with the cigarette is nothing purely static, in which no change takes place, but is itself a series of processes: the burning flame gradually heats the cigarette until the temperature is so high that the tobacco gets ignited, that is, a chain of physical and chemical processes is set in motion which begins with bringing the match under the cigarette and ends with the burning of the tobacco. So the cause is nothing static; no more so is the effect, for the burning of the cigarette is plainly a process going on in time; so instead of two *states*, we have two *series of processes* which are causally connected. And besides—would it not be very strange to consider the possibility "that the cause, after existing placidly for some time, should suddenly explode into the effect, when it might just as well have done so at any earlier time, or have gone on unchanged without producing its effect?" Thus we may rule out the possibility that the cause is one state, involving no change within itself, and the effect another such state.

But to escape from the one horn of the dilemma is to run into the other. For suppose the cause is a process involving change within itself, or, as we might equally say, a number of processes following one another, then it is clear that it is not the *whole* of the processes which fall into the time interval ($t_1$, $t_2$) which are the cause of E, but only part of them, namely those parts which are in direct temporal contact with the effect E; for all the earlier parts of C, since they are not contiguous to the effect, cannot be relevant to it, and therefore (by our definition) cannot influence it, so they may be disregarded and we see that it can only be a *part* of C which is the cause of E; but as soon as we embark upon such a reflection, there is no stopping: for if I pick out any *smaller part of C*, which falls into a subinterval of ($t_1$, $t_2$), we may repeat exactly the same reasoning: the part so picked out will contain earlier and later portions, and the earlier portions might again be altered without altering the effect, provided only that the later parts are the same. In this way we shall be led to diminish the duration of the cause without limit, and however much we may diminish it, there will still remain an earlier part which can be disregarded since it is irrelevant to the effect.

Now it might seem that the right way out of this difficulty would be to say that the true cause is only reached when we pick out *the state at a momentary instant*. But if so, we have to remember that to speak of contiguity in time loses all sense, as there are no two consecutive instants, and therefore no two consecutive point-events. Moreover, we should be faced with an infinity of momentary states, whereas all our concept-formation, and indeed our whole language, is designed to take into account only two or three or several events, in all cases a *finite* number, that can be said to be causally related. But now we come up against an unforeseen complication: for strictly speaking, we can no longer single out any finite number of events since any event—such as the lighting of the cigarette—turns out, on closer inspection, to be a process involving change within itself, and, therefore, involving causal relations between its earlier and later parts. And the concepts of cause and effect, and indeed our use of language, gives us no means of dealing with the ever changing Flux of Becoming, or of mastering a problem of such an enormous complexity. Our language seems fitted to deal with what is static rather than with continuous change.

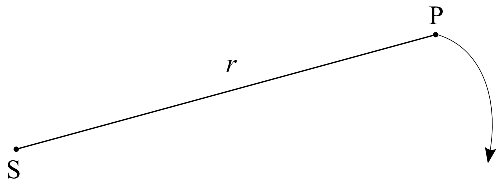
Since it has turned out to be impossible to attach any clear meaning to cause and effect regarded as events "immediately following" each other, let us see whether any other possibility is left open to us. Shall we say that there is an *infinitesimal* time interval between cause and effect? But there are no infinitesimal intervals, and, the idea of the infinitely small having been discarded on purely mathematical grounds, every interval is finite. Now if we consider that some finite lapse of time $\tau$ may be between cause and effect, i. e.




we come up against insuperable difficulties: "However short we make the interval t, something may happen during this interval which prevents the expected result. I put my penny in the slot, but before I can draw out my ticket there is an earthquake which upsets the machine and my calculations." So we can never know that this cause will have just this effect; there always remains the possibility of some unexpected interference. In other words, the supposed cause is not by itself, sufficient to ensure the occurrence of the effect.

To sum up: the customary presentation of causality as a relation between two events contiguous in space and time, and satisfying certain other conditions, is unacceptable to the scientist; no clear meaning can be given to such terms as "event" or "contiguous in space and time". If cause and effect be considered as two *unchanging states*, it is a mystery why the one should suddenly explode into the other; furthermore experience shows that there are no such purely *static* conditions but only changing *processes* which, more or less gradually, pass into one another. If, however, cause and effect be considered as two *series of processes*, then the cause is not the cause, for its earlier part cannot be relevant to the effect; so it can only be the last part of the series C which is *the* cause. So we are driven to say that it is not the whole series of processes but only the state at the last momentary instant, in the interval $(t_1, t_2)$, i.e. a point event, which is the cause. But if cause and effect be considered as two *point-events*, they cannot be contiguous, since the time-series contains no consecutive members. Therefore this formulation of causality is worthless.

For these reasons science has ceased to employ the terms "cause" and "effect"; or more correctly, the scientist may still go on talking of "cause" and "effect", when he is talking loosely, e.g. in a popular lecture; but in a strict scientific theory these terms have no place. What, then, is it that supersedes the terms "cause" and "effect"? The answer to this will be found if you look at the way in which science actually proceeds. Let me illustrate the situation with an example taken from astronomy. Suppose we want to study the motion of a planet. Let us assume that there is a big gravitational mass S (the sun) and a smaller mass P (a planet):




The two masses attract each other with a force which, according to Newton, is proportional to the product of their masses, and inversely proportional to the square of the distance between them. So we shall have a gravitational pull acting on the planet, i.e. a force expressed by the formula

$$f = c\,\frac{m_1 m_2}{r^2}$$

or rather a whole field of force governed by this formula. On the other hand, we have certain dynamical equations; one of these asserts, for instance, that the force which acts on a mass is proportional to the mass and to its acceleration. It is now a *mathematical* matter to state the problem in the form of certain differential equations, and to solve them. The solution has the following property: given the position of the planet at any given instant, the "initial time" $t = 0$, and given its motion (its quantity and its direction) at this instant, the position and the velocity of the planet *at any other time* are calculable. In other words, the system consisting of Sun and Planet is such that its momentary condition at any given instant determines (i.e. fixes by a rule) the condition at any other instant. That is all. There is no longer a need to say that one configuration of the system is the "cause" and another the "effect", or to rack one's brains as to whether two events which are cause and effect are contiguous in time. There is nothing in such a system that could be properly called "cause" and nothing that could be properly called "effect". And there is no point in saying that one position of a planet "causes" or "produces" or "brings about" or "compels" another position. And there is no need to speak of an event, for how would you define an event? As a momentary state? Or as a change of the system within a small slice of time? The fact is that we have no need to speak of events, but only of quantities which can be observed. Once we get a symbolism for describing precisely what happens, the words "cause" and "effect" and "event" drop out altogether. It is true, an astronomer may still say, when he is talking vaguely, or wants merely to indicate the subject, that gravitation is the *cause* of the planets' motion; but what he really means is that the law of gravitation enters the system of formulae which describe this motion fully and adequately; and in these formulae there occurs nothing that can be called a cause, nothing that can be called an effect, and nothing that can be called an event; instead we have a rule according to which any particular position and velocity of the planet *depends* on the position and the velocity it had at any given instant. What such a system of formulae exhibits, is the way any momentary state of the system sun-planet depends on the state at a single instant. It has therefore been suggested that the notion of causality should be replaced by that of a *functional relation* connecting the states of a physical system at different instants.

Now what is a function? An expression such as

$$x^2 - 3x + 2$$

has no definite numerical value until the value of $x$ is assigned. For example, if $x = 0$, the value of the expression is 2; if $x = 1$, its value = 0; if $x = 2$, its value = 0; again if $x = 3$, its value = 2. In the same way we may find by direct substitution the value of the expression for any integral, fractional or irregular number of $x$.

$x$, which has no definite number value, but which can take on any value we please, is called a *variable*. The value of the expression $x^2 - 3x + 2$ will then depend on the value of the variable $x$ and vary when $x$ varies. We say that the value of this expression is a *function* of $x$ and write:

$$f(x) = x^2 - 3x + 2$$

so when

| | |
|---|---|
| $x = 0$ | $f(x) = 2$ |
| $= 1$ | $= 0$ |
| $= 2$ | $= 0$ |
| $= 3$ | $= 2$ |

Now a mathematical function, generally speaking, is simply a law governing the interdependence of variable quantities. Thus the area of a circle depends on its radius, and changes when the radius changes: the area is therefore a function of the radius: $y = \pi x^2$. The circumference of a circle depends likewise on its radius, and to any change of the radius corresponds a change of the circumference: thus the circumference is another function of the radius $z = 2\pi x$. In like manner the surface and the volume of a sphere are functions of the radius of that sphere.

Physical laws are nothing but statements concerning the way in which certain quantities depend on others when some of these are permitted to vary. Thus the length of a rigid rod depends on its temperature and varies when the latter varies: it is therefore a function of the temperature. The volume of a gas enclosed in a cylinder is a function of the pressure on the piston, provided the temperature is kept constant. Atmospheric pressure is a function of altitude above sea level. The pitch of the note emitted by a string on a violin is a function of the length of the string. The task of the physicist is to determine the exact or approximate nature of this functional dependence.

The ideas of function permits an exact description of motion. If a particle moves in space, we can follow up the process completely when we know *where* the particle is at any given time, i.e. when we know how the position of the particle varies with time. The position of a particle is completely known when its three rectangular coordinates $x, y, z$ with respect to a fixed system of reference are given. The mathematical task of describing its motion is then to find out how $x, y, z$ vary with the time $t$, i.e. to give the coordinates as functions of $t$:

$$x = f(t),\ y = g(t),\ z = h(t)$$

For instance, if we put

$$x = \cos \mathrm{w}t,\ y = \sin \mathrm{w}t,\ z = \mathrm{c}t,$$

we get a spiral motion where w is the angular velocity.

So far we have been considering the case of a function of *one* variable. But it is easy to see that there are cases in which a quantity depends on two, three, or more variables. Thus the area of a rectangle depends on the length of its two sides $x$ and $y$, i.e. it is a function of $x, y$. The area of an ellipse is a function of its major and of its minor axis, and so is its circumference. The pressure of a gas enclosed in a cylinder is a function of its volume and its temperature; the gravitation force acting on two masses is a function of these two masses and their distance, i.e. a function of three variables.

It is important to notice that the idea of a mathematical function does not imply the existence of a relationship of "cause and effect" between the variable quantities. Thus Boyle's law for a gas contained in an enclosure at constant temperature states that the product of the pressure $p$ was the volume $v$ in a constant $c$:

$$p.v = c$$

This relation may be solved for either $p$ or $v$ as a function of the other variable,

$$p = \frac{c}{v} \quad v = \frac{c}{p}$$

without implying in the least that a change in volume is the "cause" of a change in pressure anymore than a change in pressure is the "cause" of the change in volume. There is perfect symmetry with regard to the two variables. It is only the form of the *connection* between them which is relevant to the physicist.

You will now understand better what is meant by saying that the mathematical solution was the problem involved in the path of a planet consists in giving a formula which shows how the position and velocity of the planet at an instant of time $t$ depend on $t$ and on the position and the velocity of the planet at a given instant $t = 0$.

$$\begin{aligned} x &= f(x_0, y_0, z_0, u_0, v_0, w_0, t) \\ y &= \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \\ z &= \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \\ u &= \ldots\ldots\ldots\ldots,\ldots\ldots\ldots\ldots \\ v &= \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \\ w &= \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \end{aligned}$$

In other words, the result of such an enquiry is that the momentary state of a planet is a function of time (i.e. varies with time) provided that the "initial condition" of the planet is given.

This is a relatively simple problem. Let us now proceed to consider some other cases which are more complicated. Take next a system in which not *one* planet but a *number* of planets revolve round the sun. If the planets did not exert gravitational forces on each other, we should be faced with the same problem as before: we should have to write down the equations for each planet separately, solve these equations and thus find that the state of the solar system at any instant (i.e. the configuration and velocities) is a function of that instant and the state of the system at a given moment. In actual fact, however, the situation is more complicated, for any two planets act on each other with a gravitational force which is relatively small owing to the littleness of the planetary masses. If we take them into account, we are led to a new and more complex system of differential equations which we have to solve. The mathematical difficulties involved in this problem are so great that it has not yet been possible to find a rigorous solution, despite the efforts of the greatest mathematicians. (This is the so-called three-body problem.) Methods have been worked out, however, which enable us to approximate the solution with a degree of accuracy. The gravitational attraction of the other planets on the earth, say, results in a slight modification, or disturbance, of the elliptical orbit which the earth would otherwise describe if it were controlled by the sun alone. These small deviations can be calculated by a step-by-step procedure according to the principles of "celestial mechanics". But the outcome is essentially the same: the states through which such a system passes are completely determined as soon as its state (= configuration + velocities) at a given instant is known; and the word "determined" here means that the state in which the system is at a particular instant is calculable from the data mentioned; or in mathematical parlance, the state at the time $t$ is a function of the state at the instant $t = 0$.

Consider now a somewhat different case—heat conduction. Take the case of a flat disc of metal, with parallel faces, one of which is kept at a fixed temperature $q_1$, while the other is kept at $q_2$. Suppose, for instance, one face is kept in contact with icewater, the other with boiling water ($Q_1 = 0$°C, $Q_2 = 100$ °C). Here there will be established a permanent state and a uniform flow of heat from the hotter to the colder face, and the temperature may be taken to fall uniformly from $q_1$ at one face to $q_2$ at the other, if the disk is throughout of the same material and the same conductivity. Hence, if we consider any plane drawn in the disk parallel to the faces, it is clear that the same quantity of heat will pass across by such plane per second when the permanent state is established.

Consider next the case of such a disk under different circumstances. Suppose one half of its circumference is kept at a fixed temperature, say 0°C, while the other is maintained at 100 °C. For some time after the first application of heat at the edge the temperature at each point of the plate will gradually change, but ultimately each point will acquire a stationary temperature and a continuous flow will take place in the disk. Now it is a problem of heat-conduction to find the final stationary distribution of temperature across the disk. This is found by solving a certain differential equation.

It was the pursuance of such problems that brought Fourier to the discovery discussed in the foregoing, that namely of a method of discovering by a mathematical function any distinction of temperature along the circumference, however irregular, arbitrary, or odd. Fourier set himself the following problem: suppose you have a cylindrical disk: the temperature at the initial time $t = 0$ is distributed in some quite irregular, say, in a *completely arbitrary* manner. The problem is to find out what distribution the temperature will assume at any other time $t$. Now what is important in Fourier's treatise is that a quite new idea comes in—that of an *arbitrary* distribution of the temperature in the given body. How can *any* distribution, however odd and irregular it may be imagined, be reported by a mathematical function, i.e. by a mathematical law? This seems at first sight to be at variance with the very idea of a law: for how can the arbitrary, random, chaotic be governed by a law? Here I have to speak of the fundamental change brought about by Fourier's *La théorie analytique de la chaleur* (1822).[53] The mathematicians of his time were surprised and thrilled by his discovery according to which *any* distribution of temperature (or of a physical quantity whatever) over any range in space can always be covered by a mathematical law. Fourier devised an ingenious means to cover any curve, however bizarre, by a strict mathematical law, provided that formulae of a sufficient complexity are permitted. (Fourier's series of cosines and sines.) Thus, if the change in temperature during a day is represented by a graph, this curve can always be subsumed under a mathematical law, no matter how irregular and odd it may appear. Consequently, we can no longer distinguish between regular and irregular distributions, sequences of events, or processes. The idea of function was made so flexible by Fourier as to embrace any curve, even if it be discontinuous. Fourier succeeded in finding a general formula, or in constructing a formula, which covers any such problem.

Fourier's solution can be modified to take account of changes of heat along the surface of the plate, e.g. by heat conduction or radiation into, or from, the surrounding medium. Finally the temperature at any point of the boundary may be prescribed as a given continuous function $F(x. y. t)$; the problem is to find out what the temperature at any point will be and how it will change with time. In other words if the initial condition of the plate is known, and if it is further known what amount of heat flows through any part of its boundary during a certain interval $t$, the state of the plate at any instant of that interval is fully determined. This typical problem is a specimen of a "boundary-value" problem, in which it is required to find that solution of the different equations of heat that fits prescribed initial conditions.

53 Cf. Jean Baptiste Joseph Fourier: *La théorie analytique de la chaleur*. Paris 1822.

It is now possible to sketch the picture of causality which underlies the *deterministic* view of nature. It will be best to illustrate this conception with the example of the disk. Suppose we have a very thin metal disk on which the temperature is distributed in some arbitrary way. Consider the disk, for the sake of simplicity, as twodimensional. If the disk is left to itself, i.e. if it is a practically isolated system, then a short while afterwards we shall have a slightly different distribution in which the differences are a bit less pronounced. We may picture this by drawing a second disk parallel to the first at a distance which represents the lapse of time. If you repeat this a number of times, you will get a series of disks all parallel to the first which will show a gradual levelling of the temperature differences there were in the beginning. If we now, owing to the continuity of the process, insert more and more disks between any two, we shall, in the end, instead of a series of disks separated from each other, get a continuous series, i.e. a cylinder, and we may now state our result by saying:

Supposing the disk is an isolated system, the states it passes through are fully determined by the state of the system at the instant $t = 0$. Or, in geometrical terminology: *the whole three-dimensional cylinder (its content of events) is fully determined by the base alone*; and "determined" means: any slice of the cylinder (i.e. the temperature at any point and the rate of change of the temperature there) is calculable from the data.

If the condition is not fulfilled, if, for instance, heat is permitted to pass from the disk into the surrounding medium or vice versa, the result is essentially the same; there is only one difference: what was to be known is the "initial state" of the disk plus the flow of heat through the boundary within the interval $(0, t)$. Or, in geometrical parlance: *the whole three-dimensional cylinder (its content of events) is determined by its base and its convex surface.*

Let me remind you that the example considered is an idealized case introduced for the sake of simplicity to give you a model which you can easily picture in imagination. In actual fact, there are no such things as flat twodimensional disks, but only three-dimensional bodies or regions in space. In order to understand the scheme of causality as envisaged in classical physics, we have to conceive of space and time as a four-dimensional manifold—which is of course no longer open to intuitive grasp—and we have to say: Consider any closed region in space R, i.e. any portion of three-dimensional space bounded by a continuous surface: enclosing some material or physical mechanism, for instance a number of mass particles, or a body with different temperatures, or a gas, or an electric field, etc. the successive phases through which such a physical system passes in course of time may be regarded as the cross-sections of a cylinder in a four-dimensional space of which the initial state of the system within R is the base. Now the idea of causal determinacy may be expressed by saying: *Given the base R of a four-dimensional cylinder of altitude t and further its convex surface, the interior of the cylinder (its content of events) is thereby determined.* Or in non-geometrical parlance: If the initial state of a system within a region R is known, and if further everything is known that happens along the boundary of R during an interval of time, every state of the system within that interval is determined. Any law of the kind which enables us to determine the interior of the cylinder from data given, is called a *causal law*, or, better, a *law of the causal type*.

## (6) Comments on a New Conception.

Let us now contrast the scientific conception of causality, as it has gradually emerged since the time of Newton, with the conception that we find in every-day life. A number of differences will at once be noticed. In science we are not interested in finding out the cause of one or another particular event; we are interested in plotting the whole course of the phases through which a physical system will pass or has passed, i.e. in plotting an infinite series of continuous changes; and, moreover, in plotting it in a particular way, by referring only to initial and boundary conditions. In every-day life we speak of distinct events and ask what the "cause" or the "effect" of such and such an event is; when we pass to the advanced sciences, the ideas of an "event", of a "cause" and an "effect" evaporate, and we are left instead with a framework of the sort indicated. As we have no events, no causes and no effects, the question as to the contiguity of cause and effect does not arise. Nor does the question as to whether there is any *necessary connection* between cause and effect. A law just describes what happens, but it does not describe the *necessity* of the happenings. And here can be found another source of our predilection for thinking in this way. The word "law" may mean two entirely different things: the law of the state, imposed by the Parliament or the Government or the King etc. This law is a rule which prescribes and prevents respectively a number of things that in some cases conflict with what people desire: If, in such a case, they none the less obey the law, they feel it as a *compulsion*, as something to which they are *subjugated*.

(…) compulsion is a very complex notion, involving thwarted desire. So long as a person does what he wishes to do, there is no compulsion, however much his wishes may be calculable by the help of earlier events. And indeed, the state compels the citizens to obey the law through *sanctions*, i.e. through certain coercive measures annexed to a law as a means of enforcing it, as a penalty, for instance. In this sense the law *does* compel us to do certain things or to abstain from others.[54]

In the natural sciences the word "law" has a quite different importance: a natural law is not a prescription ordering things to behave in a certain way, or a prohibition forbidding certain other things, but simply a formula which describes how things actually do behave. There is no ghost of an "ought" or a "must" involved in such a formula: it is *des*criptive, not *pres*criptive. What these different things

54 Russell, "On the notion of cause", p. 10.

have in common is that they are both called “laws”, or “rules”, or “formulae”. It is really a matter of regret that two so distinct ideas are expressed by the same words. Since laws of nature only describe in a condensed form what happens, an idea of compulsion is out of the question. The laws of celestial mechanics do not lay down rules for the planets saying how they ought to move, as if the planets, if left to themselves, would have chosen to run off in quite different directions and only these tiresome laws of Kepler’s compel them to remain in orderly orbits; no, these laws in no way “coerce” the planets, they merely assert how they actually do move. But if we allow ourselves to be taken in by the ambiguity of the term “law”, we easily come to think that nature is compelled by laws to act in this and in no other way; this, then, is anther source of our belief in an element of necessitation in causality.

The two conceptions of causality are very different. But what is it that lies behind this difference? Why are the ideas of cause and effect, used in every-day life, so different from the conception of the sciences? This brings me to a very important point. Let me put it like this: “cause”, “effect”, “event”—all these are very inaccurate ideas. Suppose a man takes a dose of arsenic and dies. Then the drinking of the dose of arsenic will be called the cause of his death. But the drinking of the arsenic is not a sharply defined process—will you say that it begins when his lips come in contact with the arsenic, or when he begins to swallow it, or when it goes down into the stomach? And as to dying, every doctor will tell you that this is a very complex process including many different phases. You do not die at once, you die in bits. So there is not one clear-cut event, the cause, which brings on another clear-cut event, the effect, but rather a vaguely bounded bundle of processes which are followed by a similarly loose bundle of other processes. But this is precisely the reason why the words “event”, “cause” and “effect” are so useful in practical life. Suppose you intend to set out everything that happens to you with the most minute detail and scientific accuracy—how on earth could you then talk of breakfast, a conversation, a walk, a doze? Such words are deliberately chosen to fulfil a function which they can only fulfil when they are sufficiently elastic. No two breakfasts are alike; what use would the word be if it were to include all the fine details? The word is useful only when it *leaves out*, when it is inaccurate. Nearly all our notions in every-day life are of this nature: they are vague, schematic, flexible, elastic. From the Flux of Becoming we single out one rolling wave in which we take an interest and call it the cause; we single out another such wave and call it the effect (e.g. drinking arsenic and dying). Thus the ideas of event, cause and effect are tied up with the schematic way our language represents things. As soon as we approach reality with scientific exactitude, we see that what we called an event (the drinking of arsenic) resolves itself into a continuum of changing processes or momentary states. I shall try to help you to understand the situation by giving a somewhat related example. Suppose you have to describe a part of the surface of the earth. You can do this by speaking of hills, mountains, peaks, valleys, basins, etc. This is for many purposes a very good description.

But suppose you want to describe the exact geometrical shape of some part of the earth's surface, then you see at once that these notions won't do. A hill, a peak, a slope—these words have no precise geometrical meaning; it is clear that you will have to employ a quite different sort of description—for instance a geometrical surface given by its equation in analytic geometry, or a map with contour lines. To this second way of describing corresponds the scientific conception of laws of a causal type. If your description is to reflect all the finer details of a causal nexus—such as drinking arsenic and dying, lighting a cigarette and smoke, impact of one billiard ball on another—you have to resolve the vaguely bounded wave of the Stream of Becoming which you call the cause or the effect into a field of processes passing continuously into each other, and then you will have reached a stage of scientific description on which you can no longer single out and delimit anything that could be called "cause" nor anything that could be called "effect". So the ideas of cause and effect have a relevance to *practical* life, they are, like breakfast and a walk, a dance etc., ideas which are important, if crude, means to find our bearing in life. Causes, effects, events are things which stand out only when you look at them from a certain distance, as it were, taking care not to look too closely. Of such ideas one might say that they are *approximate* or *perspective* notions—notions applicable only in a certain perspective setting. Now what happens when you pass to the advanced sciences is that the whole perspective *changes*, and therefore all these notions lose their appropriateness and their importance.

If you now ask, "Is the popular, or the common sense notion of cause and effect wrong? And is the scientific conception of functional dependence the true one?", I can only say: What on earth makes you think so? Both are "right"; but each on its own level. Do not think that the ideas of cause and effect have lost their value altogether and must now be superseded by the functional scheme. Do you really think that there is a great advance in describing a walk in terms of a solution of a system of differential equations? No, the old ideas are by no means ousted by the upstart notions of physics; by no means. Only they have different jobs. It would be folly to describe breakfast in terms of field-physics or quantum mechanics. No, *it is just breakfast*, and that is a perfectly good term—good in the sense that it conveys exactly what we wish it to convey. And if you say, "My hunger passed away because I had a good meal", this is a perfectly good description of a causal nexus. There is nothing wrong with your description, here you have cause and effect; that is to say, *that is the way* in which the words "cause" and "effect" are used. *What is wrong comes out when you construct a physical theory of cause and effect by attempting to give a too precise account*—e.g. by saying that "they are distinguishable things, phases or aspects of reality, which are so related to each other that whenever the first ceases to exist the second comes into existence immediately after (…)."[55] Paradoxical though it sounds, it is just this striving after accuracy which spoils the approach. It is *essential* that "cause", "ef-

55 Baldwin (ed.), *Dictionary of Philosophy and Psychology*, vol. I, p. 164.

fect" and "event" should be left *vague*. In this rests their whole usefulness—and it is therefore important to *abstain from any attempt at making them precise*, or at laying down exact conditions for their relationship, in other words, if you try to force scientific accuracy on the words as used in every-day life, you will get involved in all sorts of difficulties, of which Baldwin's definition has given us an example. I could also have said: Do not be exact in the wrong place, what does the word "exactness" mean? Is it real exactness if you are supposed to come to tea at 4.30 and come when a good clock strikes 4.30? Or would it only be exactness if you begin to open the door the moment the clock begins to strike? But how is this moment to be defined and how is "beginning to open the door" to be defined? Would it be correct to say "It is difficult to say what real exactness is, for all we know is only rough approximation?" And should we infer from this that no one can really be punctual? In such a case we plainly adopt a wrong ideal of accuracy, which is all right in science or in mathematics, but out of place in circumstances like these. Philosophers constantly have before their eyes the method of science, and are irresistibly tempted to ask and answer questions in the way science does. This tendency is one of the sources of metaphysics, and leads philosophers into complete darkness. For instance, one attempts to make the concepts of ordinary language as precise as possible—with the result that puzzles inevitably spring from just this endeavour.

In general, when you want to make your concepts precise, you must change the whole way of concept-formation. This you do when you pass to science. What is important to understand is that, when you study physics, you learn a new *method* of describing things. I might as well say that there are two *languages*, the language of science and the language of every-day life, and to each of them there corresponds a *particular scheme of causality*. That is, it is a mistake to suppose that there is just *one* idea of causality, which is analysed by philosophers; no, statements of the causal type belong to *two essentially different* categories, one in which you speak of cause and effect and say, for instance, that the same cause always brings on the same effect, and another one in which you speak of a functional dependence, or of determinism in the sense I have outlined. What we must understand is that there are two distinct *language strata*, and that the word "causality" accordingly undergoes an inflection of significance. You can only apply the term "causality" with reference to a *language* fit to represent things and processes; what you mean by causality will then depend on the stratum of the language you use.

Thus it is a rule characteristic of the every-day notion of causality that we say "whenever C, then E". Such a rule is adapted to the practical ends in which we are interested; and therefore we see philosophers laying great stress on the existence of invariable uniformities of sequence of this kind and trying to base a definition of causality on such regularity. And this is quite all right so long as they take care that their statements remain of a vague qualitative sort. I am persuaded, for instance, that if you threw a stone at this window, the glass will break. Yes, I have seen such things happen a number of times before, and I have no good reason to doubt the regularity. But notice, the words "stone", "throw", "window" are of a comfortable vagueness. Try and make them precise, and your rule will no longer hold universally. What, for instance, if the stone was very small? Or if it was thrown with very low velocity? Or if the window-pane was made of very strong or very thick glass, or of a special sort of glass which is unbreakable? Well, as soon as you state your conditions with sufficient accuracy, the rule is no longer true; and you see at the same time another thing: as soon as the antecedents are stated sufficiently fully, they have become so complicated, that it is very unlikely that they will ever recur. Consider the case of a falling body: if *all* you wish to say is that it falls—all right, I will not quarrel. But this is an inexact qualitative statement. But suppose you are interested to find out *how fast* it falls. This depends upon the shape of the body and the density of the air. Even if there was no air-resistance, the latitude would make a difference, and the altitude above sea level. "Theoretically, the position of the sun and moon must make a difference. In short, every advance in a science takes us farther away from the crude uniformities which are at first observed. (…) The principle 'same cause, same effect', which philosophers imagine to be vital to science, is therefore utterly otiose."[56] Or better, it is characteristic, not of science, but of our scientific idea of causality where we expect that like causes will have like effects—"like" in the sense of practical life.

One thing more: you notice that in a problem of physics, say in Fourier's study of heat conduction, no such principle is assumed. The mathematical treatment of such a problem is such that you can always find the solution, however, the initial and the boundary conditions may vary; that is, the solution is so flexible, elastic, as to adapt itself to all possible antecedents, That exactly the same antecedents will be followed by exactly the same consequents, is now a very special case, unlikely ever to occur, which, however, can be subsumed under the general law of Fourier. Fourier's law is such that it enables you to determine what will happen no matter what the special antecedents may be. Thus the scheme is in no way tied up with the principle "like causes, like effects" and provides, in fact, much more freedom to consider an infinite number of varying antecedents.

In all these points the scientific scheme of causality and the unsophisticated common idea part ways. But there is one thing which both have in common: both connect states or phases at one time with states or phases at another time, and both express this in the form of an implication: If A, then B. Only that the man in the street speaks of crude things, "event", "cause", whereas the scientist talks of initial or boundary conditions. A law of nature is calculated to adapt itself to an indefinite number of initial and boundary conditions; it is therefore of a higher generality than a statement like "Whenever A, then B". "Whenever metal is heated it expands". "Whenever a man is born, he will die." These latter ones are vague qualitative statements, not laws of theoretical physics. A law of theoretical physics covers an *infinite range* of different series of processes—for instance, the class of

56 Russell, "On the notion of cause", p. 8.

*all possible* motions in a gravitational field, or the class of *all possible* flows of heat in a conductive body—and is therefore much more general and much more precise than a statement of the form "Whenever A, then B". How a law of physics is capable of meeting these two demands cannot be explained accurately without going into mathematics; suffice it to say that a law of nature, as conceived in physics, has the form of a differential equation, and a differential equation is equivalent, not to a particular curve or graph or function, but to an *infinite class* of such curves or graphs or functions which have something in common: just that which is brought out by the differential equation; so it comes that a differential equation is on the one hand precise and is on the other hand so general as to adjust itself to an infinite variety of particular conditions. It should be noticed that such a law cannot be expressed in ordinary language, for no sentence formed of words could do the same job. The combination of an extremely high degree of generality with precision can only be achieved in a mathematical symbolism; and that is, I suppose, the reason why mathematics is so important in natural science.

Now the point which I want to make is that the idea of causality is tied up with a certain way of describing things. And as there are different ways of describing things—or, what comes to the same, different languages—the idea of causality adapts itself to the particular type of language. Thus scientific language has its own conception of causality, different from the idea we meet in common speech. To put it slightly differently: *the idea of causality is a function of language, and varies when you pass to a language of a new logical stratum*.

## (7) The Principle of Causality

It is now time to pass to a new question, What exactly does the principle of causality state? Does it state that *every* event that has a beginning, is caused by some other event? Or that the course of every happening in nature is governed by laws which allow of no exception? Or should we state it by saying, "It is a law that everything that happens in nature is subjected to some law or other of the deterministic type"? You see, if you express it like this, the "law of causality" would be a sort of second-order law saying that laws *exist* for everything. If a law of nature is expressed in a language of physics, the law of causality (or principle of causality) would state something *about* the language of physics, or *about* the system of physical laws: it would be, to use a modern expression, a statement which does not belong to the language of the sciences, but to their meta-language; not to physics, but to "meta-physics". A somewhat different view is expressed by Wittgenstein: "The law of causality is not a law but the form of a law." "'Law of causality'—that is a general name. And just as in mechanics, for example, there are 'minimum principles', such as that of least action, so too in physics there are causal laws,

laws of the causal form."[57] So the law of causality would not assert anything, but we should rather think of it as the "form of law". It is therefore a bit surprising to read in the *Tractatus* a little further on, "If there were a law of causality, it might be put in the following way: There are laws of nature. But of course that cannot be said: it makes itself manifest."[58] What Wittgenstein meant was that if you look up a book on theoretical physics, you will find there laws of a certain type; but according to the philosophy he then held, this cannot be expressed *by* language, but rather "shows itself" *in* language. The mysticism of "showing itself" has meanwhile been cleared up: Wittgenstein was quite right in saying that the law of causality cannot be expressed by the language of physics; he concluded from this, that it cannot be expressed at all, failing to see that it can be expressed in the meta-language, e.g. in the form: "In the language of physics there occur laws of such and such a type". Now is *this* the principle of causality?

It is clearly possible that there exist laws for *some* happenings; but what we want to say is more: that there exists a law for *every* happening in nature; or as L. J. Russell put it, "every change has a cause", "everything which happens is completely determined by its causes."[59] That is a far more sweeping statement than the modest one that there *are* certain laws of a causal type. What are we to think of such universal statements about causes, or the so-called principle of causality? Have we any reason to suppose it to be true?

Let us first turn to experience.

Physicists have succeeded in establishing laws of such a type for *special* and *limited* fields of phenomena. "Newton's theory of gravitation", says Einstein, "certainly marks the greatest step ever taken in linking up natural phenomena causally."[60] Another domain in which all happenings can be subjected to laws of such a deterministic character is that of the phenomena of *heat conduction;* a third that of *electromagnetic forces* in free space. Astronomy, thermodynamics and electrodynamics are thus chapters of physics in which theories of this type have been established. But it would be untrue to say that deterministic laws have been found for *every* happening or kind of happening in nature or in the man-made world. We have no precise deterministic laws in biology which can compare with those of classical physics; and even in physics itself we come up against certain phenomena on a small scale, for instance Brownian movement (dance of dust particles) in which no physical theory has succeeded in determining (predicting) the movements of all the tiny particles. What, then, are we to think of the dictum "Whatever happens is completely determined by some law"? So far as experience

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57 Ludwig Wittgenstein, *Tractatus Logico-Philosophicus*. Transl. by D. F. Pears and B. F. McGuiness. London 1961; 6.32, 6.321.

58 *Tractatus Logico-Philosophicus*, 6.36.

59 Leonard J. Russell: "The principle of causality": *Proceedings of the Aristotelian Society* 46 (1945/46), p. 105.

60 Albert Einstein: "Relativity and the ether" In: *The World as I See It*. London 1935, p. 193.

is concerned we can only say that *some* domains of happenings *have actually been subjected* to laws of a causal type, *other ones have not.* That is *all* experience can say. Empirical evidence is far from sufficient to prove a general statement of this sort. Hence a new problem arises: what is the logical status of the principle of causality?

If it is not experiential, what can it be? Is it a *prophecy* saying that we shall succeed in every new field of research—biology, psychology, history—, in finding such deterministic laws? Or is it an *expectation* that if we only try hard we shall in the end discover some such laws? Or is it a *postulate* which bids us to stick so long to a subject until we have laid bare its causal mechanism? Or is it a *methodological maxim* (rule) which guides the procedure of science advising the scientist to go in this direction? In other words, is it a *heuristic* or *regulative principle*, which asserts nothing but rather regulates the scientist's procedure? Is it merely a pious hope that may be frustrated any day? Or is it an *a priori truth* laying down a *necessary condition* for gaining any possible knowledge of reality? You cannot complain you have too little variety to choose from. Now, these are conflicting claims, and different schools of thought have indeed tried to advance any one of these different interpretations.

To give an example from modern literature: C. D. Broad, in his *Examination of McTaggart's Philosophy*, sets out four propositions involving causation which seem to him "prima facie self-evident". The first of these four propositions is: "Every *change* has a cause". This proposition he believes most people will regard as self-evident, "if they will be honest with themselves."[61]

## (8) Difficulties of Determinism

Since Galileo scientists have begun to discover in the physical world a growing number of laws found to be invariably fulfilled, and their belief in the existence of physical laws has steadily increased. Thus in the minds of scientists the belief has grown up that the physical world is an immense machine turning in a manner exactly determined and in such a way that a complete knowledge of its state at a given moment would enable all its future states to be predicted. This theory of a rigorous and universal *determinism* was laid down particularly by Laplace in his *Essai philosophique sur les probabilités* (1814), in which that great mathematician wrote the words justly famous for the exactness of the idea, and the elegance with which it is conveyed:

> Given for one instant an intelligence which could comprehend all the forces by which nature is animated and the respective situation of the beings who compose it—an intelligence sufficiently vast to submit these data to analysis—it would embrace in the same formula

61 Charlie D. Broad: *Examination of McTaggart's Philosophy*. Cambridge 1933, vol. I, p. 232.

the movements of the greatest bodies of the universe and those of the lightest atom; for it, nothing would be uncertain and the future, as the past, would be present to its eyes.[62]

Physics, the science of dead matter had almost up to the present day seemed to be the very stronghold of determinism, and even the opponents of that principle had seemed to be ready to leave this field to it. Yet the most recent theories, adopted by physicists, almost against their will, to explain facts experimentally observed lead, not so much to a complete *surrender* of determinism in physics, as to the view that it is *not complete nor universal,* and that in fact it has limits. What I wish to explain here is why and how this unexpected change has been brought about in scientific thought.

For a long time the tendency of scientists has been to regard the universe as being formed of elements having at each instant a certain arrangement in space—a distribution changing through time. This was the idea of the atomists in antiquity, and again it was this idea which Descartes had in mind when he said that we should attempt to explain physical facts "by figures and motion." In these words Descartes made an attempt to chart the future course of modern science. This idea is completely in accord with the principle of determinism, which states that a knowledge of the position and velocity of the elements of the physical world at a given instant is sufficient to determine completely their later motion.

The most perfect type of explanation in accordance with the Cartesian ideal is supplied by the corpuscular theories. In these theories the assumption is that matter consists of particles or corpuscles, or material points, i.e. of minute, simple and indivisible elements whose extension is so slight that they can be treated as geometrical points. The spatial distribution of these particles, and their motion in time, are supposed to account for the properties of matter.

At the beginning of this century it looked as if two classes of particles would suffice to account for the properties of matter and to reduce the entire material universe to a vast collection of protons and electrons. If, further, it were to prove possible to find exact laws governing the motion of these particles, then the Cartesian ideal of a description of the physical world in terms of "figures and motion" would have been fulfilled, and simultaneously the demands of the doctrine of universal determinism would have been met. It looked as if physicists were on the point of reaching an ideal pursued for many hundreds of years. Certainly, if classical mechanics were really applicable to material particles and if at a given instant we could know exactly the positions and the velocities of the vast number of particles which between them form the material universe, then the entire future course of this material universe would be rigorously determined, and the ideal defined by Laplace in the words quoted would be attained, at least in principle.

Now it is here that we come to a point which is of the utmost importance in understanding the present situation: it has always been assumed in classical me-

62 Pierre Simon de Laplace: *A Philosophical Essay on Probabilities*, transl. By F. W. Truscott and F. L. Emory. New York 1902, p. 4.

chanics—and assumed tacitly—that it is possible to know exactly—i.e. to measure precisely—the magnitudes which define an instantaneous state of a physical system. True, it has been admitted then, our actual measurements are never absolutely precise, that there is, rather, always a margin of uncertainty. Thus, for instance, we cannot give the position of the moon at a given instant with mathematical accuracy, because our instruments are not ideal and observations carried out with them do not determine a real number such as $\sqrt{2}$ in a strictly mathematical sense. All measurement, it has been said, is more or less approximate and has a hazy edge. Though this maxim has been generally acknowledged, there has been a tendency to minimise it. It has been thought that there was no limit to the fineness of our powers of observation; or rather, every limit, it was thought, was only a technical one that could be surpassed by an improved technique or increased skill on the part of the observer. Whether or not we can attain absolute precision in our measurements, it has been thought that we can go on forever refining our measurements, and in the same degree our predictions concerning any future state of the system will approach the truth more and more. That there is no limit to this approach, this was, in the last analysis, the presupposition made tacitly by classical mechanics. Now we must understand quite clearly that the whole application of the scheme of causality is based essentially on this assumption—that it is possible to measure precisely the state of a physical system and that there is no limit to the fineness of our operations of measuring. Only if this condition is fulfilled, may we speak of causal laws enabling us to predict exactly the entire future of a physical system once its initial state is known to us. The essential assumption is contained in the latter phrase, "once its initial state is known". If the nature of things were such that we should be prevented from ever getting a *complete* knowledge of the initial state, we should no longer be in a position to predict the entire future, and causality, in the sense of predictability, would break down. It is precisely this latter assumption which has turned out to be false. So we are faced with an entirely new situation, which has emerged since 1926 and which has proved a turning-point in our conception of causality. This concept, as outlined in the preceding discussion, was found to be incompatible with actual conditions as revealed by experiment.

The reason for this was the discovery of a new class of phenomena—quantum phenomena—which it is impossible to interpret in terms of the older theories. In 1905 Einstein made a notable suggestion which has proved fruitful in predicting experimental results. This was the hypothesis of the existence of "light quanta", as it were atoms of light. According to this hypothesis, the energy of radiation is to be treated as though it were done up in bundles.

The most fundamental feature of the new theory, is that a particle does not have a sharply defined position and a sharply defined velocity. To understand this, it is useful to think of a particle as a sort of wave-packet, blurred in space. The indefiniteness as to position can be minimised, to be sure, by making the wave packet very small; but in that case it can be shown that the packet will spread rapidly. Consequently, if we were to observe the position of the particle a little later and then calculate its velocity by dividing the distance covered by the time taken, any one of a wide variety of results might be obtained. Thus, a small packet means a large indefiniteness in velocity. On the other hand, if we attempt to fix the velocity of the particle within narrow limits, this will result in a large indefiniteness in the position. In general, it can be shown (and deduced from the principles of the new wave-mechanics) that, if $\Delta p$ denotes the effective range in the possible values that might be found by observation for the position of the particle, and if $\Delta q$ indicates the similar range for velocity, then, very roughly,

$$\Delta p \cdot \Delta q = \frac{h}{4\pi}$$

where $h$ is Planck's constant. ($h = 6.55 . 10^{-}$) This principle was first enunciated by Heisenberg in 1927 who called it, in German, the principle of "Unbestimmtheit", "Unschärferelation". This term has been variously translated as "indeterminacy", "indefiniteness", "uncertainty".

The principle asserts that there is a fundamental uncertainty about our knowledge of a particle. More accurately, there is always some uncertainty as to the position of a particle, and also its state of motion. What Heisenberg's formula shows is that these two uncertainties are not independent of each other: As their product is constant, the smaller the one is, the greater will be the other. Thus, the more we know about the position of a particle, the less can we know about its velocity; the better we know its velocity, the less can we say about its position. What has turned out to be impossible, is to know simultaneously and exactly both the position and the motion of a particle. This is an essentially new discovery which represents a complete departure from the old Newtonian system of mechanics.

What has been said seemed to be in conflict with the fact that both position and velocity are capable of precise measurement. In ordinary physics, we have no difficulty in determining both of these qualities simultaneously; e.g. from two snapshots of a rifle bullet, its position and velocity at a given instant can both be calculated. Heisenberg pointed out, however, that this could be done only because on the scale of observation used in ordinary physical measurement, the indeterminacy is so minute as to be lost in the experimental error. It is quite otherwise for an electron, or an atom, or a molecule.

Consider, for instance, how an electron might be located with *atomic* precision. We might use a microscope; but then we should have to use light of extremely short wave lengths in order to secure sufficient resolving power. Under these circumstances the effect of the light on the electron can no longer be neglected. If we are to "see" the electron, at least one photon ("atom of light") must bounce off it and enter the microscope. In rebounding from the electron, however, this photon will get a strong kick. Thus at the moment at which we locate the electron, its velocity undergoes discontinuous charge. Moreover, there is an indefiniteness about the magnitude of this change, for it will vary according to the direction in which the scattered photon leaves the scene of action. We cannot limit closely the range of all possible directions for the scattering photons that enter the microscope, by stopping down the aperture, without a serious loss of resolving power, and in similar manner all other attempts fail. So we come back to the uncertainty principle.

It appears, then, that we cannot *at the same time* assign to an electron or other small particle a definite position and a definite velocity (or energy). At least we cannot do this and attach any meaning to the statement in terms of physical observation; and physicists have become increasingly clear about the principle that only those magnitudes which can be observed (directly or indirectly) have physical significance. Thus our classical notion of a particle as something that can move along a sharply defined path, having at each instant definite position and velocity, is not applicable to electrons or protons or atoms or molecules. These small bits of matter may be said to have some *particle* properties, but they also possess certain *wave* properties, so that, in the classical sense of the word, they are neither true particles nor true waves. Darwin the grandson of Charles Darwin proposed to call them "Wavicles".

The upshot of the matter is *that there is a limit to the fineness of our powers of observation and the smallness of the accompanying disturbance—a limit which is inherent in the nature of things and can never be surpassed by an improved technique or increased skill on the part of the observer.* There is thus an essential indeterminacy in the quantum theory, of a kind that has no analogue in the classical theory. This indeterminacy can be said to have its basis in the *wave properties* of matter, and is therefore unavoidable.

The uncertainty I have spoken of is not a thing to be regretted. It seems only that we are prevented by a "conspiracy of nature" from ever getting a full knowledge of the state of an electron, as long as we have not freed ourselves from the old idea that an electron is a particle with a definite position and a definite speed. In actual fact the trouble lies deeper, it lies in the fact that the very picture we make ourselves of an electron as a tiny ball does not represent reality adequately, that electrons and atoms are not material objects in the ordinary sense at all, that they behave in such a curious way that the common ideas of "particle", or "wave", can no longer be applied to them. It is only as long as we stick to the picture of a tiny ball, that we are disappointed to find that it should be impossible to measure both the position and the speed of an electron. But once we realise that our ordinary language and our ordinary ideas of material "objects" are not fit to express the nature of electrons, we begin to see that the very principle of uncertainty is one of the *defining characteristics* of these new structures. It characterises an electron that all measurements are uncertain. It now dawns on us why it is hopeless to conquer that uncertainty by some technical device—as if it were due to some inefficiency on the part of the experimenter and not to the nature of things.

It will conduce to clarity to elucidate once more what is really the central point of the new quantum theory. Let us therefore imagine that we have a sceptical experimenter, who refuses to believe in the quantum theory, and sets to work to show that he can fix the position and speed of an electron at the same time with as high accuracy as he pleases. To make his experiment easier he will take the electron to be at rest. Our experimenter claims to have got an electron precisely fixed and at rest. We will crossexamine him about his work and see what he has found.

Q. How did you know the electron was there?
A. I saw it.
Q. An electron is a pretty small thing, and not easy to see. How did you manage?
A. I had a microscope.
Q. Even a microscope can only see things of the size of a wave-length of light. You can't be much of a precisian if you say you knew exactly where it was from that. I thought you said you would guarantee to know *exactly* where it was.
A. Yes, but you see I had taken a course in optics at the university, and so I was not caught out so easily as that. I invented a special X-ray microscope. It uses a wave length of a thousand-millionth of an inch. With that I ought to be able to fix it close enough for you. Anyhow, I think I have done fairly well.
Q. Well, I haven't yet heard of an X-ray microscope on the market, but I suppose there will be one soon. Perhaps it would be pedantic to want you to do better. What did you see?
A. It was rather tiresome to get it going, but when I had done so an annoying thing happened. I know the electron was there or thereabouts, because I had put it there; it was at rest because otherwise it would have gone off while I was getting the microscope ready. Well, I was adjusting the microscope, and the electron was coming into focus beautifully, when it seemed to give a jump and ran away. So that experiment was spoilt and I had to start again.
Q. Did you have better luck next time?
A. No. It was most curious; exactly the same thing happened every time. I think there must be something wrong with the microscope stage. I am going to have a shot at improving it. But as the microscope was certainly right in principle for seeing things of a thousand-millionth of an inch, and as the electron stayed there all the time I was focussing, it seems to me that I must be right. It is only a matter of overcoming the troublesome details that turn up in all experiments.
Q. It is not a matter of troublesome detail, and there is nothing wrong with your microscope stage. Your trouble is not with the electron being there and staying there, it is with the *seeing* of it. You can't see the electron without light to see it by, and the light disturbs the electron and drives it away. It does not matter how many different experiments you design, you will always get caught out in one way or another. There is no escape from the Uncertainty Principle.

The old particle theory breaks down not because it is impossible to imagine a particle at rest at a definite place, but because any method that can be contrived to observe that it is there always introduces a disturbing element. Ordinary experiments with gross matter are made with instruments so designed that they do not perceptibly disturb the object measured. It would be a curious way of measuring the length of this table to apply a gigantic instrument, say by putting a liner on it, since the table would be crushed and no measurement obtained. The measuring instrument is always chosen lighter or weaker than the object measured; but this cannot be done when the object is the lightest thing that there is, an electron. Every experiment which we may design to observe an electron, introduces some disturbance and an unpredictable one. That is really the salient point. If we are to see an object, that object must be illuminated, that is, hit by light waves which will then be reflected from the object into our eye. Now it has been found to be a universal rule that waves of every kind exert a pressure on an obstacle reflecting. This must therefore be true of light, and we may state the condition for seeing an object equally well in this way: if we are to see an object, that object must be hit by light-waves, and must itself recoil in consequence. So the mere fact that we see the electron guarantees that it is set in motion; even if it was at rest before we saw it, it cannot be so afterwards. The mere carrying out of the experiment spoils the result aimed at. Notice that if we are content with knowing the position rather inaccurately, we need not use light of a very short wave-length, and shall not then get much recoil; but if we want the position accurately, we must have a short-wave length, and then the recoil will be large. So we see the uncertainty principle is maintained: high precision in position or velocity can only be attained by the sacrifice of precision in the other. One might think that there are methods of defrauding the uncertainty principle by resorting to some cunning device or other. In order to see that none can succeed I should have to go through a lot of examples, to show every time in detail how the attempt fails. But there is no time to do so, and so I can only sum up the argument by saying: it is not by any means always easy to detect the flaw in an experiment designed to conquer the uncertainty principle, but there is always something wrong. Each time we find the defect in our process, we must install some extra piece of apparatus to put it right, and the addition, in the course of overcoming the old difficulty, always introduces a new one. There is no escape from the uncertainty principle.

Consider now the assumption that an electron is a particle. This implies that it is "really" placed at a precise spot, and "really" has a precise speed. Now if it is to mean anything at all to say that the particle is placed at a precise point, it must mean that we think we could devise methods of *accurately* finding where that point is. Now from considering various experiments we have seen that there is a definite limit to the accuracy with which position and velocity can be simultaneously determined. Hence there is no warrant in experiment for our assumption that the electron behaves like a pure particle.

This must revolutionise our ideas concerning the principle of causality. We are accustomed to take it for granted that a *full* knowledge of the present would enable us confidently to predict the future. When we are defeated in our attempts at prophecy, we attribute it to ignorance, with the tacit assumption that with *more* knowledge of the present we could have done better. In the past it never occurred to anyone that the present is definitely unknowable; but we have just seen that the mere effort to know it cannot help introducing new errors in the determination. We used to be faintly ashamed of the fact that we were not omniscient, but now ignorance has become respectable.

In the new mechanics we cannot assume that both the position and the velocity of a particle are known simultaneously. The consequence is that there can be no determinism. Actually, the predictions of the new theory are very different from the exact predictions of classical mechanics. Classical mechanics is such that it permits the following predictions; Let the state of a mechanical system be characterised by having six *n*-quantities specified, say the three *n*-coordinates of the positions and the three *n*-components of the velocities of *n* particles moving about in space. A knowledge of these quantities at a definite time $t = t_0$ combined with a knowledge of the laws of mechanics allows us to determine the six *n*-quantities at any other time. Quantum Mechanics, on the other hand, states the following: Let all the quantities of the system that are simultaneously measurable be determined at the time $t = t_0$. A knowledge of physical laws then enables us to state the *probability* with which the system will assume a given state at a given time. So what we can say with certainty is only that the *probability* for the presence of a particle at a given place at a given time is such and such. But we can never state with certainty where the particle will actually be at that time. In other words, while the older mechanics claimed to apply exact and inexorable laws to every phenomenon, the new physics only give us laws of probability, and though these can be expressed in exact formulae, they still remain laws of probability. One thing more must be said here. The new physics is neither a causal, deterministic theory in the old sense, nor a completely indeterministic theory, which would open the doors to chaos. The new physics has both deterministic and indeterministic features. What is deterministic in the new theory is the *law for the propagation of certain waves*, the de Broglie waves. That is, the propagation of these waves is *causally determined* by the initial conditions, just as the propagation of electric waves is in the classical theories. What is new, however, is that this law (Schrödinger's wave equation) can no longer be interpreted as describing any actual process such as the propagation of some real agent in nature; the wave has merely a symbolic significance, it signifies the *probability* of the occurrence of a particle at a given place. It is a sort of probability cloud. If we want to verify a statement of this new physics, we have to verify that the probability calculated is actually such and such. This can be done, not by making a single experiment, say by observing a single electron through a microscope, but by repeating the experiment a large number of times, observing, say, the position of the electron every time and forming the mean value of all these positions. So what we can derive from the theory is a statement describing the *average position* of the electron. So statements about *probability* are really statements about *frequency* which admit of an experimental verification.

An important feature of the new physics which should be clearly understood is that it is impossible to predict the fate of a single, individual electron, whilst we can make predictions—and quite definite ones—as to the behaviour of a very large number of electrons, that means the laws of the new physics are *statistical laws*. The larger the number of particles is, the better we can predict their behaviour; incidentally, that is why we can predict the behaviour of material objects in ordinary physics. To take a similar example: you cannot tell whether a particular person will live or die in the next year, except in very rare cases; but you can tell—at least in normal circumstances—, how many persons in London will die in the next year, because the death rate of the population is nearly constant. In the same sense, though we cannot say anything definite about the fate of an individual particle, we can make precise predictions as to the behaviour of a large number of particles, say those that constitute a body. This is what is meant when we speak of "statistical laws." These statistical laws can be rendered mathematically in such a way that they seem to refer to the propagation of waves. But these waves must be taken in a symbolic sense only, as waves of probability of observing the electron at any point. This probability is proportional to the *intensity* of the electron-wave there. This is the general rule governing the relation of wave and particle.

To summarise then: what is deterministic, is the mathematical law for the propagation of certain waves; what is indeterministic is that what is really fixed by the wave is not the position of the electron, but only the probability of its position. Though the new theory can make predictions, these predictions do not state what will actually happen, but merely what will probably happen. Now probability statistics were already applied in the older stages of physics (e.g. in the kinetic theory of gases), but they were considered rather as a sort of *makeshift*, which could be dispensed with if we had only enough time to work out a problem mathematically. It was always felt that it was only in order to save time and trouble that we resorted to probability. What has turned out in quantum mechanics, however, is that the element of probability is an *ultimate element* of our description of nature, which cannot be eliminated. To make things still clearer consider an example: in throwing dice we cannot predict the result of any throw; what we can predict is only the *probability* of throwing a certain number. We are prevented from predicting the result, because we have in sufficient information as to all the minute factors which are of relevance in the matter. But we might still imagine an experimenter who has such subtle methods of observation at his disposal and at the same time such mathematical skill that he can predict with certainty the result of a particular cast. In this case the impossibility of predicting is only a *technical* one which, at least in thought, can be overcome. Not so in the case of an electron. For here we are prevented by the very laws of nature from predicting its future behaviour. The impossibility is not due to some lack of information on the part of the observer or to some lamentable inefficiency on the part of the calculator or to some limitations of human beings, but to the very order of things. Even an intelligence such as that imagined by Laplace, "which would comprehend all the forces by which nature is animated and the respective situation of the beings who compose it—an intelligence sufficiently vast to submit these data to analysis", even such an intelligence would not be able to predict the future. *Nature itself is indeterminate.*

Thus the classical ideal, as expressed by Descartes and Laplace, have suffered bankruptcy. After all, Heisenberg's uncertainty principle, with its prohibition of an exact and simultaneous knowledge of position and velocity, is the very expression of the fact that it is impossible to know, at the same time and exactly, both "figures and motion".

Now some people might object to this by saying, "All this is true only for the present stage of physics; but all that amounts to is that this stage is not satisfactory; one ought instead to try to improve physics in such a way as to make predictions of absolute accuracy possible." If there is anyone who cherishes such hopes, he will be disappointed, for the fulfilment of his wish is *mathematically impossible*. It has been shown by an analysis of J. von Neumann that quantum mechanics is a theory that is in some sense *closed* or *complete*: it permits of no addition that would render it deterministic; more precisely speaking, the addition of any new assumptions to quantum mechanics which would reinstate determinism, will necessarily lead to internal contradictions, Therefore the theory is not capable of being extended in such a way as to admit deterministic predictions. The crack in the wall of determinism is definitive, and there is no hope of getting out of the situation.

Some adherent of Kant might try to defend his position by saying: "Well, how do you know that quantum mechanics is not, after all, a *false* theory? It may be that at some time in the future physicists will work out a better theory, capable of giving a complete account of the motions of an electron. On that day Kant's philosophy will be triumphant, and physicists will regret to have ever departed from his lines." At first sight it might appear that this argument is sound, but this is not borne out on closer inspection. The question at issue is not whether quantum mechanics is *true* or *false*—deciding that must be left to the experts—but rather whether it is *legitimate*. If Kant were right, it could not even be possible to *entertain* such views as modern physicists do; to give up causality, even in part, would mean robbing ourselves of the condition of knowledge, which could lead to one result only, to utter confusion. But that is not so. Though we have given up causality in the sense defined, science has *not* collapsed and its claim to knowledge has *not* been frustrated. For we are now offered *statistical* laws in the place of *causal* ones, and these statistical laws, though not inexorable, *do* give us information about the behaviour of reality. We *can* make predictions and rely on them for all practical purposes. The conclusion to be drawn from that seems to me this: even if one day quantum mechanics should be found wanting and be replaced by another theory, it gives a *possible picture* of the material world. This picture is neither inconsistent, nor absurd; whether it is true is a different question which can be settled only by experience. But the very fact that we have to *turn to experience* is decisive: for it is thereby admitted that it is possible for experience to confirm or refute quantum mechanics, and hence to confirm or refute the validity of the principle of causality. To repeat, what cannot be disputed is that quantum mechanics is an admissible, legitimate theory, which may turn out to be true or false, but which cannot be ruled out from the outset as inadmissible. This alone is of importance. No matter whether it is true or false, the mere fact that quantum mechanics is a theory which can be checked by experiment, shows that Kant was on the wrong track and that his attempt at a rational justification of the principle of causality has failed.

## (9) Causality as Understood Connection

I have said that there seem to be two different ways of establishing a causal nexus. The one is the discovery of a *regularity* in a sequence of events; and that is the only one I have so far discussed. I proceed now to consider the second way in which, it would seem, the existence of a causal relation can be established: the *understanding* of the relation between cause and effect.

The advocates of this view point out that the regularity view fails to account for the fact that we not only *observe* but, at least sometimes, *understand* the connection between cause and effect. On the regularity view, they say, if A shoots B, his shooting him has no more intrinsic connection with B's death than has, e.g., my drinking tea. The only connection consists in the circumstance that his death in fact follows it and would always do so under given conditions. But, they say, that is not enough. The idea of causation, on their view, implies the following four things:

> (1) The effect is held to be continuous with, dependent on something in the cause so that the two do not merely happen in regular succession, but are intrinsically connected with each other. (2) The cause is held to explain the effect, to answer not only the question—how?—but the question—why? so that the demand for causes is primarily a demand for reasons, which implies that there is a logical or quasi-logical connexion between the two such that the cause is at least part of the reason for the effect and helps to make the occurrence of the latter intelligible. (…) (3) The cause is held actively to produce or determine the effect in a sense in which the effect cannot be said to produce or determine the cause. (4) Causality involves necessity. If there is a causal law connecting A and B, it is not only the case that B does follow, but that it *must* follow.[63]

I shall now discuss these points more in detail, and begin with (3), i.e. with the view that the cause *produces* the effect. Here we come across the idea of the

63 A. C. Ewing: "A defence of causality", *Proceedings of the Aristotelian Society* 33 (1933), p. 98f.

efficacy of causes, or that quality which makes them produce, or generate, their effects. The terms, says Hume, "of *efficacy, agency, power, force, energy, necessity, connexion,* and *productive quality*, are all nearly synonimous; and therefore 'tis an absurdity to employ any of them in defining the rest."[64] "Shou'd any one leave this instance, and pretend to define a cause, by saying it is something productive of another, 'tis evident he wou'd say nothing. For what does he mean by *production?* Can he give any definition of it, that will not be the same with that of causation? If he can: I desire it may be produc'd. If he cannot: he here runs in a circle, and gives a synonimous term instead of a definition."[65]

"Some have asserted that we feel an energy or power in our own mind; and that, having in this manner acquired the idea of power, we transfer that quality to matter, where we are not able immediately to discover it. The motions of our body, and the thoughts and sentiments of our mind (say they) obey the will. But to convince us how fallacious this reasoning is, we need only consider, that the will being here considered as a cause has no more a discoverable connection with its effects than any material cause has with its proper effect. So far from perceiving the connection between an act of volition and a motion of the body, it is allowed that no effect is more inexplicable from the powers and essence of thought and matter. A person that has lost a leg or an arm by an amputation, endeavours for a long time afterwards to serve himself with them." How could he, if the relation between an act of volition and a motion of the body were something that is grasped immediately? "Nor is the empire of the will over our mind intelligible. (…) We have command over our mind to a certain degree, but beyond *that* lose all empire over it: and it is evidently impossible to fix any precise bounds to our authority, where we consult no experience. In short, the actions of the mind are, in this respect, the same with those of matter. We perceive only their constant conjunction; nor can we ever reason beyond it. No internal impression has an apparent energy, more than external objects have."[66]

Let me once more state the theory of causation that we are examining. According to it, the type or model of causation, as well as the exclusive source from which we derive the idea, is our own voluntary agency. Here, and here only (it is said) have we direct evidence of causation: we know that we can move our bodies and we are conscious of our power before we have experience of results. An act of volition is accompanied by a consciousness of effort, of force exerted, of power in action which is necessarily causative. This feeling of energy or force, we are told, is an assurance, prior to experience, that we have the power of causing effects. Volition, therefore, it is asserted, is *something more* than an invariable antecedent; it is a cause, in a different sense from that in which physical phenomena are said to

64 *Treatise*, I, III, 14.

65 *Ibid.*, I, III, 2.

66 *Treatise*, I, III, 14 (Appendix).

cause one another: it is an *efficient cause*. From this it is an easy step to the further doctrine that volition is the *sole* efficient cause of *all* phenomena.

We have heard already the arguments which Hume brought forward against efficient causality. It will clarify the situation, if I quote a few more passages from Mill and William Hamilton. “I cannot admit”, says Mill,

> that our consciousness of the volition contains in itself any *a priori* knowledge that the muscular motion will follow. If our nerves of motion were paralysed, or our muscles stiff and inflexible, I do not see the slightest ground for supposing that we should ever (…) have known anything of volition as a physical power, or been conscious of any tendency in feelings of our mind to produce motions of our body, or of other bodies.[67]

And quite in accordance with this is the following statement of Hamilton:

> A person struck with paralysis is conscious of no inability in his limb to fulfil the determinations of his will; and it is only after having willed, and finding that his limbs do not obey his volition, that he learns by this experience, that the external movement does not follow the internal act. But as the paralytic learns after the volition that his limbs do not obey his mind; so it is only after volition that the man in health learns, that his limbs do obey the mandates of his will.[68]

The passages quoted should settle the dispute. Now, if observation cannot even show us the manner in which the will acts upon our limbs or our mind, it can still less discover any quality in an event which makes it *produce* another one. Take again the example of the collision between two billiard-balls. All we observe is that the state of motion of the one ball is succeeded by the state of motion of the second ball. With close observation one would observe that, as the two balls come in contact, there is a slight deformation of the surfaces of contact, the balls get a bit flattened, and this phase is followed by a recovery from the deformation, during which processes the first ball is (gradually) brought to rest and the second ball moves away with the original state of motion of the first ball. At no time while the balls are in contact can one see why the process must occur as it does and not in some other way, or how the collision produces the effect. Prior to the observation of a collision of two balls one could not predict the consequence of the collision. But, you might object to this, if the surface of the ball is flattened on the place where the two balls come in contact, this will arouse an elastic force in the ball which you could feel if you pressed your fingers against it, and this elastic force pushes on the second ball and sets it in motion. Well and good; but how do you know that the elastic force, when acting upon the second ball, will set this ball in motion, and not e.g. deform its surface permanently? To say because the second ball is *elastic* is of no use, for this is merely another way of stating that it behaves as it does, which you will know only *after* you have observed the collision or some

67 *System of Logic*, III, V, § 11.
68 William Hamilton: *Lectures on Metaphysics and Logic*. vol. II, Lect. xxxix, pp. 391f.

similar phenomenon. So the fact remains that *all* we can observe is that the motion of one ball is arrested and a similar motion is acquired by the second ball. But we do not see how the one phenomenon is *produced* by the other.

I say that we do not see how one phenomenon is *produced* by another. But this raises a much graver question, What can possibly be *meant* by saying that one event "produces" another? Is there any sense in the term "producing" which goes beyond that of "causing" as defined by Hamilton and Mill? It seems that the idea of producing involves that of an *activity*. We seem to think, in holding this view, that there is something in the cause at work which actively produces the effect. "The cause," says Ewing in the paper quoted, "is held actively to produce or determine the effect in a sense in which the effect cannot be said to produce or determine the cause."[69] A similar view is expressed by Professor Löwenberg. "I am forced," writes this author, "to impute to events an inward activity which, if we know it, would account for the observed intimacy of their connection." So we must further ask what is meant here by "activity" or "actively producing"? You notice that these expressions cannot be taken in their everyday sense, as when we speak of the activity of a man or an animal; for we do not want to ascribe volition and intelligence to a billiard-ball. In what sense then, are they to be understood? If the terms "activity", "produce" mean anything, then a useful way of discovering this meaning is to ask "how do we know that one event—such as the collision of two billiard-balls—produces another, such as the setting in motion of the second ball?" For the answer to the latter question will show us what exactly is meant by the term "produce".

It is plain that no amount of empirical inspection can ever reveal to us the presence of an activity *in* the events. For what we learn through our senses is nothing but the succession of events or a regularity in their succession. Observation can never establish the existence of anything, which, by its very nature, defies all examination. Now it might be hoped that, if we have any knowledge of such a productive quality, it must be derived by reason, by some philosophical argument which infers its existence from what is given in sense experience. But this will not do either. For, if the existence of such a productive power can be inferred from our sense experiences, on purely logical grounds, then it would be a self-contradiction to say that the motion of the second billiard-ball is *caused* by the first, that is, *regularly preceded* by it, without being *actively produced* by it. Present day physics, having dispensed with the idea of efficient causality, would, if this account were right, be self-contradictory—a very strange consequence. The existence of an active power in an event which produces the effect can neither be produced in, nor logically inferred from, observation.

To make the matter still clearer, try the following experiment in thought. Suppose there was a region of the world, say A, in which everything held good that Ewing and other philosophers of the same school tell us—that is, in which the

69 *Op. cit.*, p. 99.

events were “intrinsically” connected with each other, so that the cause “actively produced” the effect; imagine another region of the world B in which the events merely *follow* each other, without being connected in this way; and imagine that the observable laws are the same in A as in B. What then, I ask, could be the difference between these two regions of the world, as far as their causal structure is concerned? Or how can *we* tell whether this world of ours is more like the part A or the part B? There is no way in which we can tell; for there is no conceivable observation which is relevant to establishing the existence of such a relation. The two worlds which we have described would appear exactly alike in all respects to any observer: what, then, is the dispute about? Consider another example. G. Simmel, in his *Probleme der Geschichtsphilosophie*, makes a remark which he thinks very important for understanding history. He is inclined to suppose that there may be something in the world which he calls “individual causality”. What he says is this:

(…) the idea of a general law is identified with the idea of causal efficacy. However the generally acknowledged equivalence of these two concepts is not immune to logical objections. We are not prepared to acknowledge a causal relationship in the absence of a causal law. In other words, the fact that *B* follows *A* is recognized as the causation of *B* by *A* only if a *law* obtains to the effect that in every case—in other words, invariably—*B* follows whenever *A* occurs. However it seems to me that there is no logically necessary connection between the idea of causation and the idea of a law (…) At one point in space and time, an event *A* causes an event *B*. At another point in space and time, it causes another event *C*. There is no doubt that we can conceive of a possible world in which *A* invariably—according to timelessly valid laws—produces *C*, just as it produces *B* in the world that in fact exists. For this same reason, there is no logical objection to the possibility of a third world in which the effects of *A* are variable. The essential point is the following. This hypothesis does not replace causation with a relationship of purely arbitrary temporal succession. On the contrary, all the definitive conditions which distinguish causation from the latter—the immanence, efficacy, and necessity of the connection—are retained. There is only one difference. Instead of invariably being fulfilled by the same propositions, they are fulfilled by a varying set of propositions. The conceptual possibility entertained here should not be confused with a thesis once maintained by a logician: that if the law of causality ceased to hold on some distant fixed star, the result would be universal chaos. The consequence of the hypothesis proposed here would be just the contrary: causality would continue to hold in all of its objectivity and strictness. Actually, the domain within which causality obtains would be even more extensive. Instead of being valid for all cases, however, the content of any law would be valid only for one case. The valid content of a causal law would change for each successive case.[70]

This possibility, claims Simmel, opens up a new vista in psychology. The difficulty of discovering laws in mental life may be traceable to the fact that in psychology that type of inductive causality prevails.

70 Georg Simmel: *The Problems of the Philosophy of History*. An Epistemological Essay. Transl. and ed. By Guy Oakes. New York 1977, p. 106f.

This immediately raises the question, In what would a world in which an inductive causality holds differ from a world which is acausal and chaotic? In the immanence, efficacy, and necessity of the connection, you say? Well, how would you define these terms?

This brings us to the core of the matter. So far we have been discussing the question whether it is *true* to say that one event actively produces another, or whether there is an inward activity in the events which accounts for their outward regularity. But now it is time to shift our attention to a new direction. We must first get clear what sort of question it is that is being asked,—namely whether there is an inward connection between events in the world. Is it a factual question which can be answered by performing certain empirical tests? If Hamilton and Mill are so anxious to show that it is impossible to discover any sort of glue that binds the events together, this sounds at first as if they were making a factual statement just as if they were saying in an experiment, "The glass is not glued to the vessel, it is held against it by the air pressure." But this would be a mistake. For whereas the latter is a statement that can be checked by observation the former regarding the character of causality is not. And this is the very point of the whole discussion. An experiential statement may be either true or false; even if it is false, it has a perfectly good sense, for we can imagine what it would be like if it were true; whereas the statement "there is an inward activity in the events" is neither true nor false, since we ourselves cannot tell what the supposed difference is to be. If all the evidence we can possibly appeal to is insufficient to prove the existence of such an activity, and equally insufficient to disprove it, then we must face the far graver issue whether there is any difference at all between affirming and negating its existence. What Hamilton and Mill really wanted to say—though they did not formulate it clearly enough—is that a statement such as "There is an activity in the events" is *devoid of meaning*. If I were asked what Hamilton really denied, I should answer, not, "that there is a productive quality in the events which makes the one produce the other", but rather that there is a *sense* in such a tenet.

I summarise then: a statement has meaning for us only if it makes *some* kind of difference to us whether it is true or false. If I cannot explain in any way what would be different in the world if the statement were false instead of true, or vice versa, then the statement does not express any factual content; it is not a genuine proposition at all, but a mere series of words which does not convey any thought, however strongly its appearance may suggest that it does.

There remains only one point to be cleared up, and that is how it comes that this theory has got such a hold over the mind of many philosophers, i.e. to inquire into the psychological background out of which the theory of efficient causation springs. The insistence with which so many philosophers would like to force this view upon us, in conjunction with the fact that it turns up in nearly every period of the history of philosophy, makes it natural to seek for some hidden motive behind the scene, i.e. a motive which does not come out into the open in the *arguments* advanced by the advocates of this view, but which is derived from some general disposition in human nature. Now, it is the natural tendency of the mind to facilitate its conception of unfamiliar things by assimilating them to others which are familiar to us. I quote Hume: “There is an universal tendency among mankind to conceive all beings like themselves and to transfer to every object, those qualities, with which they are familiarly acquainted and of which they are intimately conscious.”[71] Accordingly, our voluntary acts, being the most familiar to us of all cases of causation, are taken as the prototype of causation in general, and all phenomena are supposed to be directly produced by the will of some sentient being. This is proved by two pieces of evidence. First, by the child’s conception of physical causality. He says, for example, that the sun pushes the clouds, because it does not like them; or that the sun moves past along hills and trees to follow the child as he walks because the sun *does* like him. The three-year-old beats the saw that has cut him—as his parents and grandparents berate the bad weather, as if saws and clouds were operating in devilish and more or less effectual conspiracies against them. The second piece of evidence is that primitive people do really believe that the sun and the moon, the earth and the sea and the air have understanding and are active powers. To pay homage to them and implore their favour is quite natural to savages. All languages carry in their structure the marks of being formed when this belief prevailed. The distinction of verbs and participles into active and passive, which is found in nearly all languages, must have been originally intended to distinguish what is really active from what is merely passive; and in many (if not in all) languages, we find active verbs applied to those objects in which savages suppose a soul. Thus we say the sun rises and sets, the moon changes, the sea ebbs and flows, the winds blow. Languages were formed by men who believed these objects to have life and active power in themselves. It was therefore proper and natural to express their motions and changes by active words. There is no surer way of tracing the sentiments of nations before they have records, than by the structure of their language, which will always retain some signature of the thoughts of those by whom it was developed. When we find the same sentiments indicated in the structure of all (or nearly all) languages, those sentiments must have been common to the human species when languages came into being.

We come thus to see that the manner of thinking represented by “animism” is instilled into language right up to the present day. Indeed, why have our languages developed so many terms like “work”, “produce,” “generate”, “control”, etc. with a connotation of efficacy if what was experienced and meant was only: “precede and follow with regularity”? The connotations, the overtones which accompany these terms, indicate clearly that some of the ideas which gave them birth are still alive. When a force is described as a state of activity of a body, it is easy to trace the animistic origin of this idea.

71 David Hume: *The Natural History of Religion*. London 1757.—Quoted from *David Hume. The Philosophical Works.* Ed. By T. H. Green and T. H. Grose. London 1882, vol. IV, p. 309f.

We are now in a position to *understand* why so many philosophers try to think in terms of production, creation, efficiency. In these attempts an *archaic type* of thinking comes to the surface. The theory of efficient causality, we may venture to say, is a relic, a survival of animism. There is much in the speculations of philosophers that is a faded mythology; and the theory which we are examining is, at bottom, a mythological idea in disguise, which has died out among scientists but is still alive among philosophers. That is to say, the original animistic philosophy maintains its ground in the mind, underneath the growths promoted by cultivation, and keeps up a constant resistance to their thrusting their roots deep into the soil. In the subconscious e.g. in our dreams, we are all animals. The theory which I am criticising derives its support from this substratum. Its strength does not lie in argument but in its affinity to an obstinate tendency of the human mind which springs from a primitive and archaic layer of thought. It is this philosophy of animism which Löwenberg unwittingly proclaims in the words I have quoted: "I am forced to impute to events an inward activity which, if we knew it, would account for the observed intimacy of their connection." That is precisely the way children and savages think.

I come now to point (2), i.e. the view that a causal explanation should give us the *reason* why the cause is followed by this particular effect. In other words, our knowledge of causal laws should be obtained by such an *insight into the real nature of the cause* as would enable us to anticipate the effect *a priori*; only then should we attain a real *understanding* of the connection between cause and effect.

Supposing there were a case in which we believed we really and completely "understood" the working of a certain medicine in the human body: in such a case we should not have to wait for any repetition of the sequence treatment-recovery in order to assert a causal connection between these two events; rather could we assert it even before it occurred a single time. If a surgeon amputates a man's leg, he will know beforehand that the man will be one-legged afterwards. Nobody thinks that we must wait for a long series of experiences in order to know that amputation results in the loss of a limb. We feel we "understand" the whole process and therefore know its result without having experienced it. Those, who believe in this second way of establishing a causal law—through *insight* into the nature of the cause—will immediately add that it is the only real way, the only legitimate method, and that our first criterion—regularity of occurrence—was nothing but a sort of *clue* which might be good enough for a scientist, but can never satisfy the philosopher.

But let us examine what exactly is meant by "understanding" as the word is used here. It is usually supposed to be a matter of logical reasoning. And there is indeed a logical element in the case we have just been examining. That amputation of a leg makes a man one-legged, is a *logical inference*; that is to say, "A man has two legs, one leg is cut off" entails, "The man retains one leg only"; so in fact I have not to wait for experience in order to know that the man will be one-legged afterwards; I can predict his condition with absolute certainty, just as I know that

$2-1=1$ without appeal to any experience. But, unfortunately, it is easy to see that this has nothing to do with causation. I have not inferred the effect from the cause, but I have simply expressed the information I am given in a different way, I have passed from one description to another according to the rules of logic or mathematics. I have transformed a description, but I have not made a prediction. We see, then, that there is nothing mysterious about the apodeictic certainty of this inference. But we realise at the same time that logical reasoning is powerless to predict the future. We cannot, by logical reasoning alone, foretell whether the man will survive the amputation, and if he survives, how much time it will take him to recover etc.; the only thing we can say with apodeictic certainty is that he will be left with one leg—which does not tell us any more than we knew already; and this is the characteristic mark of all logical reasoning.

So we see that, at least in our experience, we were led to think we grasped the causal connection without reference to previous experiences because we mistook a logical relation for a causal one. Let us examine another experience: what is the difference between a case in which we understand that a certain medicine must have a certain effect, and another case in which we just know by experience that it does have that effect? It is evidently this: in the second case we observe only two events, the application of a drug and, after a certain lapse of time, the recovery of the patient; in the first case we know how the gap between cause and effect is filled by an unbroken chain of events which are contiguous in space and time. The drug, e.g., is injected into the veins, we know it comes into immediate contact with the blood corpuscles, we know that these will then undergo a certain chemical change, they will travel through the body, they will come into contact with a certain organ, this organ will be affected in a particular way, and so on. In this way we infer that in the end the patient *must* be cured, provided that all the other events follow each other in the way we have assumed. And how do we know that they do follow each other in this way? All we know is that in former experiences in the laboratory this has always been the regular course of things; and we apply this knowledge to the new case. So ultimately our "insight" into the causal nexus rests on the observation of regular sequences in the past.

From all this we must draw the conclusion that causation in the cases considered is defined by regularity of sequence. The two things which have emerged from the discussion seemed to me to be these:

I : the "understanding" of a causal relation is *not* a process of logical reasoning.

II: the causal relation between two separate events is actually explained or understood when we can conceive the two as being connected by a chain of intermediate events. If some of these are still separated, we have to look for new events between them, and so on, until all the gaps are filled up, and the chain has become perfectly continuous in space and time. But evidently *we can go no further*, and it would be nonsense to expect more of us. If we look for the causal link that links two events together, we cannot find anything but another event (or several ones). Anything discovered in the causal chain, will be a link, but it would be foolish to look for the linkage—a sort of cement that binds together the links.

This shows that there is a perfectly good sense in the demand to look into the whole causal chain which connects cause and effect, but that we are totally mistaken, when we think that this chain could consist of anything but events, that it could be a kind of mysterious tie, called "causal relation." The conception of such a tie is due to a mistake that is very common in the history of philosophy: the continuation of a thought beyond its logical bounds. After we have filled all the gaps in the causal chain by inserting further and further events, some philosophers want to go on after all the gaps are filled. So they invent a sort of glue and assure us that in reality it is only their glue that holds the events together. Unfortunately we can never find the glue; there is no room for it, as the world is already completely filled by events which leave no chinks or crannies between them. Even to-day there are some philosophers who say that we directly *experience* causation, e.g., in the act of volition, or even in the feeling of muscular effort. But whatever such feelings of willing or of effort may be, they are certainly events in the world; they can be *glued* to other events, but they cannot *be* the glue.

So we find two different senses for that "insight" that is demanded by rational philosophers: the one is simply that of logical inference ("If a man loses a leg by amputation, he will be one-legged afterwards".) But that has nothing to do with causation. The other sense in which we may speak of an "insight" is that we understand a particular causal nexus only when we can follow up the full unbroken causal chain which leads from the cause to the effect. We may complain of the lack of understanding when we know only that one event C is regularly followed by another event E, without being able to trace the whole chain.

It looks, however, as if these two senses were not meant by those who reject the regularity view. What, then, is it they have in mind when they speak of an "insight" into the nature of a given causal connection? The best way of answering this question is to look at some experiences which have been adduced by those philosophers and to see what weight attaches to them. Now the most forcible examples of experience that I have been able to trace, I found in W. Köhler's book, *Gestalt Psychology*. There is a whole chapter in it dedicated to the discussion of "insight"; so we should at least be able to find out what these people mean. Now the experiences which Köhler produces give further circumstantial evidence in favour of the objection, quoted above, that, if A shoots B, his shooting, on the regular view, has no more intrinsic connection with B's death than has my drinking tea or an earthquake at the other end of the world. I shall now quote a few of Köhler's examples:

(1)

One attitude in which I sometimes find myself is admiration. But I am never simply "admiring". My admiration is always "of" something; it does not occur as something by itself and indifferently. Nor is there the slightest doubt about what its object is at a given moment. In the concert-hall, yesterday at 9 o'clock, it was that *alto* voice, singing calmly, confidently

> and seriously, to which my admiration was directed—not the nose of my neighbour, not the back of the conductor, none of the thousands of other objects and events before me. How is that? Admiration is a directed attitude; the voice is heard as singing at a definite place. Do I state that the direction of the first goes to the place in question and stops there, as a long stick might be fastened between me and that place, and end there? Do I notice something like that and say, then: "Oh, I guess this curious attitude of mine somehow has something to do with that singing"? I certainly do not. As my attitude arises, it is experienced as being the natural outcome of what characterizes the singing voice. So long as the attitude persists, it is felt as being founded upon the properties of that performance. No indirect criteria, no coefficients of correlation are needed, then, to teach me about some probable connection here, because this actual attitude is *experienced as depending directly upon something definite*.[72]

In other words, my admiration is caused by the singing voice; and I grasp the causal connection intuitively, without reference to any regularity in the past, or the like.

> (2)
> Some weeks ago I saw my little child smiling for the first time, and I was charmed. How did I know that my attitude was concerned with that smile? (…) One side of my child's face is a little darker because of a shadow. Before a sufficient number of experiences have occurred, I might as well refer my being charmed to that shadow![73]

Again, I know that my being charmed is *caused* by that smile, and I know this immediately.

> (3)
> After a long walk, on a hot summer day, I drink a glass of fresh beer. There is a cool touch and a characteristic taste in my mouth; there is also great pleasure. Did I have to learn gradually that the second refers to the first? That it has nothing to do with the spider I see on the wall, or the size of a chair? I did *not* learn it. I am no more sure of my enjoyment as such, and of touch and taste by themselves, than I am of enjoying just this touch and taste. Enjoyment is felt as the *adequate attitude belonging to* those actual experiences, or as their natural result.[74]

> (4)
> One beautiful night in Tenerife, when I was working calmly at my desk, I was suddenly frightened as I have never before been frightened. The house was rattling and shaking violently—my first experience of an earthquake! There was no doubt whatever about my being frightened *by* that sudden rattling and shaking. Once more the attitude—if fright may be called an attitude—*was felt as obviously and naturally produced by that new experience.* We do not gradually learn that unexpected events of a strongly dynamical type will be accompanied by fright, as though *a priori* any other experiences, a friendly face or a smell of

72 Wolfgang Köhler: *Gestalt Psychology*. London 1930, p. 270f.

73 *Ibid.*, p. 271.

74 *Ibid.*, p. 272.

a rose, might be accompanied by fright just as well. Fright is *experienced* as jumping at us right out of the very nature of certain definite events.[75]

Now all the experiences given tend to show that we have not to wait for a number of experiences in order to recognise a certain causal connection, but that we can intuitively and immediately become aware of such a connection, and gain an insight into its nature. But do the experiences really show that, I wonder?

## (10) Insight

Now the mistake which Köhler makes here may be expressed by saying that he confuses the *object* of a wish, of a feeling of alarm, etc., with the *cause* of the wish, the feeling of alarm, etc. Let us see what this difference consists in.

Most people would say that there is one thing by which mind may be characterised, that is "consciousness". We say that we are "conscious" of what we see and hear, or what we remember, and of our own thoughts and feelings. So "perception", "memory", "thoughts" and "beliefs" are different ways of being conscious. There is one element which seems obviously common to the different ways of being conscious, and that is that they are all *directed to objects*. Whenever we are conscious, we are conscious *of* something. The consciousness, it seems, is one thing, and that of which we are conscious is another thing. This direction towards an object is commonly regarded as typical of every form of consciousness, and sometimes of mental life altogether. We may take as the representative of this school Brentano. He tried to distinguish between physical and psychical phenomena by ascribing to the latter a sort of *vectorial property* which he called "intentionality of consciousness". To quote his own words:

> Every mental phenomenon is characterized by what the Scholastics of the Middle Ages called the intentional (or the mental) inexistence of an object, and what we might call, though with not wholly unambiguously, reference to a content, direction toward an object, or immanent objectivity. Each contains something in itself as an object (which is not to be understood here as meaning a thing), though not each in the same way. Every mental phenomenon includes something as object within itself, although they do not all do so in the same way. In presentation something is presented, in judgement something is affirmed or denied, in love loved, in hate hated, in desire desired and so on. This intentional in-existence is exclusively of mental phenomena. No physical phenomenon exhibits anything like it. We can, therefore, define mental phenomena by saying that they are those phenomena which contain an object intentionally within themselves.[76]

This is Brentano's famous definition of mental phenomena or consciousness.

75 *Ibid.*, p. 272f.

76 Franz Brentano: *Psychology from an Empirical Standpoint*. Ed. by Oskar Kraus. Engl. ed. by Linda McAlister. London 1973, p. 88f.

To return to Köhler, we may say that he is quite right in saying that when he is frightened (as in his experience of the earthquake in Tenerife) he is *immediately aware* of what he is frightened of, when he is in a state of admiring, he is immediately aware of what it is he is admiring (the voice of the singer), when he has pleasure whilst drinking a glass of beer, he is directly aware of his enjoying the beer, and so on. We can quite agree with him that we have not to wait for further experiences to know *what* he enjoys, *what* he is frightened of, *what* he is admiring, etc. But what I totally fail to understand is why he should adduce these facts as evidences to prove that he is directly aware of a *causal connection*. I may very well be aware—and there are many cases in which I am actually aware—of an object towards which an emotion of mine is directed, without knowing in the least what the *cause* of my emotion is. As sometimes we suddenly feel that we have caught a cold without knowing where and when, thus some morning we may feel a sort of silent anger which does not seem to have a basis in any particular happening. There is just an ominous cloud of anger brooding in us. Such a cloud of anger may easily find some object upon which to discharge itself, and then that particular thing will appear as the adequate object of the anger. We should say then "I am angry about such and such", and there can be no doubt that we *are* angry about such and such. We are immediately aware of what we are angry about. But does this, in itself, tell us anything about the *cause* of our anger? By no means! We can do no more than *guess* as to the first cause of our angry mood. It may be some climatic condition working upon our organism. In Italy there is a lower measure of punishment provided when the crime was committed in time of sirocco, or it may be disturbed digestion, or the after-effect of a forgotten dream, or some other unknown circumstance. In no case do we directly perceive any causal connection. Such an example illustrates that the mere fact that I am aware of the object of my anger (or my fright), is no guarantee that I am aware of its cause.

This is borne out when we turn to a closer examination of Köhler's experiences. It is seldom, if ever, between a consequent and a *single* antecedent that a causal connection subsists. It is usually between a consequent and the sum of *several* antecedents; the occurrence of *all* of them being requisite to produce the consequent. In such cases it is very common to single out only one of the antecedents under the denomination of cause, calling the others merely conditions. Thus, if a person eats of a particular dish, and dies in consequence, people would be apt to say that eating of that dish was the cause of his death. There need not, however, be any invariable connection between eating the dish and dying; but there certainly is, among the circumstances which took place, some combination or other, on which death is invariably consequent: as for instance, the act of eating the dish, combined with a particular bodily constitution, a particular state of present health, or some other circumstance; the totality of which circumstances perhaps constituted in this particular case the *conditions* of the phenomenon, or, in other words, the set of antecedents which determined it, and but for which it would not have happened. It is the whole of these antecedents, the assemblage of all these conditions which is the real cause. What, in the case we have supposed, disguises the incorrectness of the expression, is this: that the various conditions, except the single one of eating the food, were not *events*, but *states*, possessing more or less permanency; and might therefore have preceded the effect by an indefinite length of duration, for want of the event which was requisite to complete the required concurrence of conditions: while, as soon as that event (eating the food), occurs, no other cause is waited for, but the effect begins immediately to take place: and hence the appearance is presented of a more immediate and close connection between the effect and that one antecedent, than between the effect and the remaining conditions. Even though we may think it proper to give the name of cause to that one condition the fulfilment of which completes the tale and brings about the effect without further delay, that condition has really no closer relation to the effect than any of the other conditions. *All* the conditions were equally indispensable for the production of the consequent; and the statement of the cause is incomplete, unless in some shape or other we introduce them all.

If this is so, it is very puzzling that, in the examples given by Köhler, we should be intuitively aware of the causal connection between a consequent and a *particular one* of the antecedents and not the others, the totality of which determines that consequent. Take the example of the little child smiling: Köhler says he was charmed by it. But certainly the cause of his being charmed was not *only* the smile, but a lot of further circumstances (which he does not enumerate because some of them will be understood without being expressed): that he was not absent-minded or preoccupied in that particular instant, that he was a good-humoured fellow, that he was not angry at something or depressed, and so on. Young girls often laugh without any particular cause at all, just for exuberance of enjoyment of life. This readiness to laugh may, at any instance, become associated with any external occasion, and this occasion is then called the *cause* of the laughter. But to acquiesce in this opinion would be to take a rather superficial view. We cannot be certain, whether in associating the laughter with a particular circumstance we have really singled out one of the more important causes. There may be all sorts of other circumstances which have even a closer connection with that laughter, e.g. a state of gaiety produced by something that happened before, etc. Take the case of post-hypnotic suggestions, i.e. suggestions given to patients during trance which take effect after wakening. By such means one can make a patient feel a pain, or be hungry or thirsty, or perform some strange or fanciful action after emerging from his trance. The effect in question may be ordered to take place not immediately, but after an interval of time has elapsed, and the interval may be marked by a certain signal. The moment the signal is given, the subject will carry out what he was ordered to do. Suppose now someone was given the order to laugh at a particular instant of time. Now when this instant comes, he really laughs, but, as he has forgotten everything that is connected with the hypnosis, he will, when asked why he laughs, mention some causal circumstance which appeared amusing to him in just that instant; he will honestly believe that it was this that made him laugh. In this case it is quite clear that what he *calls* the cause is not the real cause. This is a warning against putting too much confidence in that "insight" to which Köhler appeals. It is noticeable that Köhler himself admits "that in hundreds of cases we are very far from experiencing how one state of affairs is brought about by others." But if so, the question arises, "Where are we to draw the line between the cases in which we *see*—or *seem* to see—how the cause produces the effect, and the others in which we fail to attain such an insight?" I think what Köhler's examples prove is that we are immediately aware of the *object* of our admiration, our desire, our anger, etc. Instead of saying this, he states something quite different, namely that we are directly aware of the *cause* of our admiration, etc. It is interesting to see how he slips from the one statement into the other. He writes: "My admiration always is 'of' something; (…) Nor is there the slightest doubt about what its object is at a given moment. In the concert-hall, yesterday at 9 o'clock, it was that *alto* voice. (…)"[77] Quite so; but then he goes on to say: "the attitude (…) is felt as being founded upon the properties of that performance. (…) this actual attitude *is experienced as depending directly upon* something definite"[78]. But the latter is certainly a quite different statement. The first statement—that he knows that his admiration is directed towards the *alto* voice—in no way entails the second statement, that the admiration *depends* upon that voice. There is no logical connection between the two: the one may be true, and the other false. For example, if Köhler had happened to take a dose of mescal just before he went to the concert, he may have been in the disposition to admire anything he came across in the concert-hall that night. In such a case we should judge that the cause of his admiration was the mescal, and not the singing; though, even in this case, the singing was the *object* of his admiration. In saying this I am not suggesting that he *may have been* under the influence of a drug, I am merely pointing out that the two statements are different, and that the one does not entail the other. Köhler, however, seems somehow to believe that he has proved the latter when he brings evidences to establish the former.

Similar observations apply to the other examples. "When, on a hot summer day, I enjoy a cool drink, my enjoyment is felt to refer to, *or* to be based upon, the properties of the drink, but not to the spider on the wall." Notice the "or"! Nobody can deny that his enjoyment *referred* to the glass of beer; but this is *not* tantamount to saying that it was *based upon* it, in the sense of "caused by it". Suppose someone had all his life detested the taste of beer; one day, however, he enjoys a glass of beer immensely, but he has met with a sudden piece of good luck: he has inherited £500.000. Would you still say that the glass of beer was the *cause* of his enjoyment? The *object* of his enjoyment, yes; but not the proper cause. And yet Köhler, by the use of the innocuous little word "or", makes us (and himself) believe that the two things are one. People really ought to be a bit more careful with words I

77 Köhler, *Gestalt Psychology*, p. 270.
78 *Ibid.*, p. 271.

am afraid, Köhler's whole philosophy of causation rests on a somewhat slipshod manner of expression.

But, you might object to this, even if we can think of other explanations, it is not plausible that they should *apply*. Think of Köhler's experience of the little child smiling for the first time; he felt charmed. Can anyone reasonably doubt that the smiling was *the* cause, and the *sole* cause of his being charmed? Is this not, after all, a case in which we are immediately aware of a causal connection? To see more clearly in the matter, take another of his examples: "After sitting for half an hour in a restaurant, full of smoke and of talk all around me, I feel 'nervous' and ready to go. My 'nervousness' *refers to* those properties of my environment. I know this, not only because in past experiences I may have discovered the rule that under such conditions I shall feel uneasy after a time. I experience myself directly as disturbed and confused *by* these surroundings."[79] "As a layman I would say that I wanted to go *because of* my uneasiness in this particular situation, and as a psychologist I shall admit that the layman's expression is absolutely correct, because it corresponds once more to the fact that there is insight into the direct determination of that tendency toward a definite test of activity."[80] But if we accept this account, we are apt to commit a second mistake—to confuse the *cause* with the *motive*. Suppose Köhler had been asked why he wanted to go, no doubt he would have made some such reply as "because of my uneasiness in this situation, because I don't like the smoke and talk in this restaurant." Now this answer is mistakenly regarded as stating the *cause*, whereas in fact it states his *motive*. It is of tremendous importance to keep the two apart.

In order to clear up the difference between cause and motive, let us begin with another important difference which is a bit easier to explain—the difference between *cause* and *ground* (or *reason*). How do we distinguish between a cause and a ground? Let's try to find the answer with the help of an example. Suppose somebody, looking at a red light, says "red". If it is now asked *why* he made this utterance, this question can be understood in two entirely different senses, and, correspondingly, the answers may take two totally different forms. There is one sense in which he may say "I said 'red' because this colour *is* red, or more precisely, because this colour is *called* 'red'." In this reply he refers to the *use* of the word "red", or to its *ostensive definition*. Then he has given us the *ground* (or the *reason*) of his utterance. Or we may be told that in his early childhood, when he picked up language, he had to go through a process of learning by which certain associations between the noise "red" and the colour red were created, connections in the nerve paths established, etc; and through all that he finally acquired the language habit, on seeing a red object or a red light, to respond by saying "red". In this case the answer states the *cause* of his utterance.

79 *Ibid.*, p. 273.
80 *Ibid.*, p. 291f.

In the first case I have given a *logical* explanation of his use of the word, in the second case a *causal* explanation. If I refer to the *cause* of his saying "red", this cause will lie in a certain process, or processes, *going on in time*, such as seeing a red light and having previously acquired the "conditioned reflex" of saying "red" in such a situation; on the other hand, when I refer to the *definition* of the word, i.e. to the rule according to which I use it, the rule is something *timeless*, though the process of *learning* the rule, or expressing the rule, or referring to it, is a process which goes on in time. Thus the first difference we find is that the *cause* refers to time, the *ground* does not.

There is a further difference between cause and ground, namely that giving the *definition* of the word "red" *justifies* his utterance, giving the *cause* does not. In making this sort of distinction, we look, so to speak, at different aspects of the matter which correspond to what may vaguely be called the "*logical*" and the "*psychological*" aspect. When we do logic, we are concerned with certain timeless entities such as "definition", "rule", "ground", "meaning", "truth", "falsehood" etc; in psychology we concern ourselves with processes or statements going on or subsisting *in time*.

Notice that the question of *motive* does not arise at all in the example I have considered. The *motive* comes in when the question is what a person's *aim* or *purpose* was when he said "red". Suppose he explains "I said 'red' because I wanted to warn the driver", then he would have stated the *purpose* of his utterance, and this purpose is commonly called the motive. Incidentally, we should further distinguish between *motive* and *intent*. We speak of intent when there was a time-interval between the thinking and the acting, and in law it is important to investigate this further. However, for our present purpose there is no need to go into that.

Consider one more example which will help us to bring out another important difference between cause and ground. Imagine someone writing down various figures while he does a sum. When asked why he wrote just these particular figures, he may reply in two different ways. He may say "You see, I was adding these numbers and, in doing this, I followed such and such a rule." He then states the *reason* for his behaviour. Or he might have said, "In my brain processes of such and such a kind were going on which innervated the muscles of my fingers in such a way that they made movements so as to write down these figures," Then he states the *cause* of his action.

It now strikes us that, on the whole, we do not know the *cause* of our actions; it is at any rate extremely easy to be mistaken about it. Yet, strange to say, we assume that a person cannot be mistaken about the *reason* for any of his own actions. In fact, he is rather the only one who knows the reason for them. That is to say, we *call* the reason for a man's action what he gives as its reason. The cause of an action can only be discovered by observations, and is *hypothetical* in the sense that further experience can confirm or confute the causal nexus. Someone watching me, say a doctor, or a physiologist, may tell what caused my action as well, if not better, than I myself can tell that; but I am the only person who *knows* the reason why I acted so.

I should not be surprised if someone were to object to this by saying, Surely we can also ascertain the reason for an action by observation from the outside. This objection rests on a misunderstanding which must be cleared up before we can proceed. Let us imagine that someone writes on a board the numbers 0, 1, 4, 9, 16 in this order. We, watching him, may suppose that, in doing this, he is following a definite rule, e.g., that he is writing down the squares of the integers in order. Have we now found out this rule by observation? Not at all; our supposed rule is merely a hypothesis, which would account for the numbers he has actually written down. But the figures written down are always subsumable under an infinite number of mathematical laws. How are we now to tell which rule he in fact followed? By making him continue the figures? But even if he wrote a thousand figures, he still might have been obeying any one of an infinite number of rules. It is quite different if he tells which rule he has been following. Suppose he says "I have been using the formula $y = x^2$, and I have substituted for x the first 5 integers 0, 1, 2, 3, 4. The expression "the rule he is following" has now altered its meaning. In this latter sense the rule is determined by what the calculator *says*, not by observation of the figures which he is writing down; though these may help us to *guess* the rule.

It is plain that the rule which he states when we ask him why he wrote down these particular numbers may be entirely different from the one which we supposed him to be following. Thus he might have said, "I did not follow any rule, I just wrote down whatever numbers came into my head." "The rule I was using was different; it was only by chance that these first numbers coincided with the beginning of the series of the squares. For instance, my rule was $y = \frac{x}{50}(24 + 35x^2 - 10x^3 + x^4)$." "I chose the first and second number at random, then I added 3 to the last one, and then I went on with squares." "I was following such and such a rule, but I made a slip in writing, or a mistake in calculation". These examples will give us some idea how infinitely many possibilities there are, and how unfounded it would be to suppose that we can discover the reason for a man's action by observation.

So we must distinguish between *ground* and *cause*, for we learn of both in different ways. The *cause* for his writing down certain figures may lie in the fact that he was taught so in school and that this teaching has created a disposition, e.g. left definite traces in his nervous system and his brain; the *ground* for his procedure is the *rule* which he states when asked for the ground.

An objector may say at this point, Is there really a difference between cause and ground? Suppose I ask a person for the rule he is following and he makes some reply: his reply, after all, is itself nothing but a response to a stimulus—to my questioning him. That is, my words produce in him a definite sound-reaction—that's all. Now, when a person makes the objection we are considering, he already confuses ground and cause, namely the question *qua* the legitimate *ground* for the reply and the question *qua* a sort of *releasing stimulus* for making a reply. Suppose someone, asked for the rule, makes a nonsensical reply; this reply, too, may have been released by the word-stimulus of the question. The answer, when taken in this sense, does not interest us. The reply is of interest to us only in so far as it *corresponds* to the sense of the question.

But—our interlocutor will go on, if he is an obstinate person—may it not be that what a person gives as his *reason* is at the same time the *cause* of his action—his *motive*? His knowledge of the rules of arithmetic, for instance, may be the cause of his following them in doing a sum. Is it, then, right to draw such a sharp line between the two notions? Should we not rather say that what, from a logical point of view, is the reason, appears from a psychological point of view as a cause?

Let us, however, distinguish between a *reason*, say a rule, and the *thinking* of the reason (or the rule). To think of a rule in arithmetic may, indeed, be the cause of its being followed. Notice, however, that the cause of the fact that a rule is being followed may also lie in something different—for instance, in the habit of doing a sum in this way; this habit, in its turn, may be the result of an antecedent process of training. At any rate, to say that whenever I do something *in accordance* with a rule, I must have been aware of the rule, or must have rehearsed it to myself, is unrealistic. A chess player, when he is not a beginner, makes a move without thinking of the rule; his acting in accordance with the rules is just due to habit; and so in other cases.

Besides, it can be seen that ground and cause are totally different from the mere fact that the cause can never be appealed to in order to *justify* an action. I may, for instance, justify a move in chess or a step in calculation by referring to the rules of chess or the laws of arithmetic, but *not* by referring to my *awareness* of these rules or laws. If a person calculating makes a mistake in writing down some figures, his mistake may be caused by some process in his mind, e.g. by a fit of absent-mindedness, or the like; but the cause, whatever it may be, does *not* justify his mistake. On the other hand, reference to the rules of arithmetic *does* justify his action of writing down such and such figures.

So much for our general preliminary conception of the difference between cause and reason, or *causal* and *logical* explanation. I shall later on find occasion to go into the nature of *motives*. For the present it will be enough to say that a motive has one thing in common with a reason—that is, that it cannot be established by inductive evidence; rather, a motive is what a person gives as his motive; and that is the only way we can find out conclusively what the motive is. There are also some important *differences* between a motive and a reason. However, so as not to interrupt the discussion, I shall for the moment postpone consideration of these differences. With the results obtained let us return to Köhler.

Now Köhler's interpretation of the examples I have cited is due to a confusion of *logical* and *causal* determinants of a state of mind, an action, etc. To see this remember the last examples quoted. "After sitting for half an hour in a restaurant, full of smoke and of talk all around me, I feel 'nervous' and ready to go. My 'nervousness' *refers to* those properties of my environment. I know this, not only because in past experiences I may have discovered the rule that under such conditions I shall feel uneasy after a time." "As a layman I would say that I wanted to go *because of* my uneasiness in this particular situation, and as a psychologist I shall admit that the layman's expression is absolutely correct, because it corresponds once more to the fact that there is insight into the direct determination of that tendency toward a definite sort of activity." Notice the salient characteristic of this argument: Suppose Köhler were asked why he wanted to go, he might indeed reply "because of my uneasiness in this situation, because I don't like the smoke and talk in this restaurant." But he erroneously takes this answer as a statement of the *cause*, whereas in fact it states his *motive*. The *real* cause may be some excitement, growing in his nerves, but he need not be aware of this cause; whereas he knows why he wishes to go, and knows it absolutely, without any possibility of error; and just this shows that his answer states the motive for his going. Köhler, in discussing his examples, makes a point of saying that he could not guess the causal connection between his state of mind and the outward situation, except by the indirect procedure of scientific induction. Empirical rules of concomitant variation, and so forth, would be all he could find out about this connection. But all his argument amounts to is that the motive is not discovered by induction: we are immediately aware of it. But that is precisely one of the differences between motive and cause.

What is misleading here, and has in fact misled Köhler, is the vocabulary: in the first place the word "why", which may ask for reason, or motive, or cause; further the words "because" and "determine", which refer both to logical and to causal determinants; and even the word "explanation" and all related words; the confusion is inherent in the very mode of expression.

I sum up then: the ambiguous use of the word "why", asking for the cause and asking for the motive, together with the idea that we can *know*, and not merely *guess*, our motives, gives rise to the confusion that a motive is a cause of which we are immediately aware, a cause "seen from inside", or a cause directly experienced. If you now look back on the examples given by Köhler, it will not need much effort to realise that he is constantly taken in by the ambiguities of speech, which make him confuse, on the one hand, the *object* with the *cause*, and, on the other hand, the *motive* with the *cause*.

The same applies to another writer on this subject, S. Kerby-Miller, in an article on causality, published together with others in the volume *Philosophical Essays for Alfred North Whitehead* (1936). In this essay he attempts "to show that there is a class of judgments asserting causal connection which cannot be properly interpreted on the regularity view and to indicate how such judgments are presupposed by the regularity view itself".[81] The author, in opposition to the "regularity theory" thinks that there is an "intrinsic causal connection". The example he gives

81 S. Kerby-Miller: "Causality". In: *Philosophical Essays for Alfred North Whitehead*. London-New York-Toronto 1936, p. 176.

in defence of the “intrinsic connection” theory is this: “the causal relation asserted in such a proposition as ‘I believe this mathematical proposition *because* I have just seen its demonstration,’ does not derive its meaning or its confirmation from inductive evidence.”[82] Certainly not; but the confusion of cause and ground is so glaring that it is hardly necessary to go into it: in saying “I believe this mathematical proposition *because* I have just seen its demonstration”, I am referring to the *ground* of my belief, not to its cause. Other examples adduced by the author are: (1) “I dislike him because of an unpleasant remark I heard him make.” (2) “I made an effort because I had decided to do so and so”. (3) “Suppose for instance that I am hearing music which I have heard before, and though the music itself is not particularly depressing I feel depressed while hearing it. I recall that I first heard it in circumstances in which I was very depressed. I now make the judgment that ‘this music makes me feel depressed *because* I heard it first under such and such circumstances’”.[83] In example (2) I state the *motive* of my action. Notice that in this case there is no sense in questioning the motive given. It would be preposterous to appeal to some inductive evidence in the past to confirm that I made the effort because I had decided to do so. After all, I am the only person in the world who is in a position to *know* why I made the effort. Case (3) is different: in saying why the music makes me feel depressed I am stating the *cause* of my depression. Now there are many causal factors involved in this whole experience, but the judgment asserts only one, a causal connection between what I felt in the past and how I feel now. This, of course, rests on induction. Though we may reach a very high degree of subjective certainty, the causal relation can only be confirmed on the basis of induction; no analysis of the experience alone will give conclusive grounds for asserting the causal relation. There can be no instances in which we can say on the basis of inspection alone that the later state could not have happened except for the earlier. And if we do so, we are, of course, subject to error. All this is admitted by the author. “None the less”, he goes on to say, “under favourable circumstances we may proceed differently and by a process of developing what seems implicit in our feeling of depression, we recall the state of affairs which, we believe, is uniquely relevant to our feeling. We seem to recognize that this unique feeling (roughly characterised by depression) contains mnemic elements derived from the earlier experience.”[84] But if in the feeling of being depressed we recognize elements of an earlier experience, then we know what is the *object of our depression*. We have not, in some mysterious way, obtained an insight into a causal connection that is not based on induction, but as the author puts it, on the “perception that one event contained an intrinsic ‘reference or connection’ to an earlier one”[85]. In suggesting this sort of view he slips back into the confusion of the *cause* of the depression with its *object*. It may well be that I know what it is

82 *Ibid.*

83 *Ibid.* p. 189.

84 *Ibid.*, p. 190.

85 *Ibid.*

that depresses me in this music; but if so, the "intrinsic reference" of which the author speaks, is the reference of a cognitive act to its object. Notice how the author unconsciously strives to obscure this point and makes the reference to the object appear as a causal connection by using the expression "an intrinsic reference *or* connection". An intrinsic reference is one thing, and a causal connection is quite a different thing, and to connect both by an "or" is just to slur over the decisive point: how utterly unwarranted it is, this "or"!

But the issue is more involved, in that the words "because I heard it first under such and such circumstances" may partly refer to the *object* and partly to the *cause* of my experience of being depressed. In so far as they refer to the object, I am, when I recall as clearly as possible the circumstances and recognize in them some depressive elements of an earlier experience, *directly conscious* of what it is I feel as depressing and it would be in vain to dispute me out of that: the reference of my present depression to that earlier experience is indeed an "intrinsic" one, if that word is taken to mean a non-causal one, as is the reference of a word to what it means. But in so far as the words quoted indicate a causal relation, I cannot with certainty exclude the possibility of an error. That my present frame of mind is due to that past experience, can only be established by a careful account of all the circumstances. Thus I have to make sure that I am susceptible to past impressions, that I have not lost my memory, that experiences of that kind tend to have an effect on the subconscious layers of my mind, and so on. Now the consideration of all such possibilities involves a vast amount of scrutiny, and it goes without saying that, in the last analysis, we have to make use of some connections or other established by observation in the past, that is, of inductive evidences.

I sum up, then: so far as the words "because …" refer to the *object* of my frame of mind, the relation is an intrinsic one. So far as they refer to a *causal* connection, the relation is *not* an intrinsic one. Now notice how all that is veiled by the mode of expression our author is using when he speaks of a "perception that one event contained an intrinsic reference or connection to an earlier one." This makes it appear as if the causal connection *were* an intrinsic reference which we can directly perceive; whereas the author ought to have said that the connection is *in part* an intrinsic and *in part* a causal one, and that only one of them can be perceived directly.

A similar remark applies to example (1): "I dislike him because of an unpleasant remark I heard him make." In this case I do not state my motive; it would be odd to use the word "motive" in such a context: for it makes sense to speak of the motive of an action, but not of the *motive of a dislike*. We may say, however, that the unpleasant remark he heard him make is the *object* of his dislike; in so far the relation between the dislike and the remark is an *intrinsic* one; but the words "because of" may in part *also* refer to the cause of his dislike; then it can only be established by inductive evidences (think, for instance, of the fact that we are in the habit of transferring the dislike from the remark to the man; which is not necessary.)

## (11) Motive

I propose now to go more thoroughly into the question of what a motive is. It has often been said that everyone must himself know the motive which actuates him to do a certain thing. Suppose I am asked, "Why are you turning off the light in your room?" and I reply, "Because I want to go to bed"; if I were further asked, "Are you sure?" I should reply, "Surely I must know why I am doing it". This points to the fact that here the giving of the motive is the criterion of the motive. Whoever gives the motive, knows the motive.

What the motive is, can be discovered by asking, "What is it that you remember when you remember the motive for which you have done something?" This includes very different cases. One large group of cases, at any rate, are those where we recall thoughts which we entertained in or before carrying out that particular action. Suppose, for instance, you went into the river to take a bath *because* you felt hot and you remember this motive; then you may remember having said, "Now it's really too hot, let's go into the water", or, "How pleasant would it be in the water"; and so on, and so on. You may have said this aloud or in a low voice, to yourself or to other people. But we should suppose that this was your motive even in the case that no such idea was ever expressed, supposing merely certain feelings preceded your going into the water or others accompanied it. But now we are tempted to suppose that there must be something in common to all these things which justifies us in saying that you had taken a bath *because* it was hot. It is a peculiar situation that, on the one hand, we are inclined to assume that there is something that all these cases have in common, and that, on the other hand, we cannot help admitting that we do not know of any such thing that occurs regularly in all cases. The reason for this is an old and primitive conception of language, according to which to everything that is expressed in the same way there is supposed to correspond some definite constituent part of reality. This conception is primitive in the same way as it is primitive to look for an object designated by every noun, or to suppose that every property is a constituent of the objects that it qualifies, so that it would make sense to say of an object that it is "congeries" or a cluster of such properties. Such primitive conceptions are at the roots of many of our philosophical perplexities, and they are much more deep-seated than one would believe.

We say that the motive of my action was such and such if, out of many things all more or less akin to one another, one at least has occurred. And the ways they are related may vary. Think, to change examples, of the many different things which are called "movement" (that of a man, of a hand of a watch, of sound, of heat, of light). I can do nothing better to illustrate my point of view than by contrasting it with the view held by Socrates in Plato's dialogues. If I were asked what knowledge is, I should enumerate some cases of knowledge and then add the words "and anything like these". No common constituent part is to be found in all of them, for the simple reason that there is no such thing. The customary view of universals is connected with the idea that the meaning of a word is something present, or contained, or referred to in the use of the word. But in fact we often use words in a loose manner—in situations which are in *some respects similar*. And there is no need to define precisely what the phrase "In some respects similar" means. Thus we shall have to put up with the fact that the notion of a "motive" is not sharply bounded.

Yet one may offer some observations so as to make the meaning of the term a bit clearer and, above all, to distinguish "motive" from "reason".

We sometimes use the two expressions in the same sense: the question for the *motive* of an action may actually be put in the form, "What was your *reason* for doing this?" Remember that giving the motive and giving the reason are both answers to the question, "Why did you do that?"; a fact which, of course, does not favour the drawing of clear distinctions.

But without being pedantic we may call attention to cases where the difference is fairly clear. Stating a rule of arithmetic, for instance, will be regarded by everyone as the *ground*, not as the *motive*, of someone's writing down such and such figures. The motive may come in, when the question arises why it is that he has taken up mathematics. The use of the word "motive" is reserved for cases which have some significance in our life. Ethics and law are therefore the provinces where this word is used most frequently. A judge will be interested to discover the motives for the defendant's actions; and we all take an interest in the motive of others when we are concerned with human relationships.

Giving the reason for an action *justifies* the action; giving the motive does not. Think of the case of an engine driver who has stopped the train and, on being asked why he has done so, replies "because I saw a red light". Seeing a red light constitutes the *reason* for his action, (putting on the brake); as such it justifies his action. Now what was his motive? May be he had none; may be he pulled the lever automatically on seeing the red light, and when he asks himself afterwards whether he remembers any motive, he cannot. On the other hand, he may have had a motive, e.g. to obey the instructions. But we should not speak of such a motive unless he could recall having said to himself, "Now if I don't stop, at once, there may be an accident", or something to this effect. But the motive, whatever it may have been, does not justify his action. Rather, it is significant that we speak of a "wrong motive" which shows that the motive, *as such*, has no justifying power. An action can only be justified by referring to rules, instructions, etc. which constitute a *reason* for the action.

From this it can be seen that the ground of, or the reason for, an action is something that is generally recognised—for instance, rules of arithmetic, rules of logic, rules of grammar, rules of chess, rules of spelling, rules of a service. That is, a ground is something that operates in a general way; a motive may be different in each individual case. A motive may be charged with personality, a reason can not.

These, then, are some of the differences in the usage of the words "reason" and "motive".

## (12) Criticism of Russell's View

Russell, in *The Analysis of Mind*, gives the following account of desire:

I believe (…) that desire, like force in mechanics, is of the nature of a convenient fiction for describing shortly certain laws of behaviour. A hungry animal is restless until it finds food; then it becomes quiescent. The thing which will bring a restless condition to an end is said to be what is desired. But only experience can show what will have this sedative effect, and it is easy to make mistakes. We feel dissatisfaction, and think that such-and-such a thing would remove it; but in thinking this, we are theorizing, not observing a patent fact. Our theorizing is often mistaken, and when it is mistaken, there is a difference between what we think we desire and what in fact will bring satisfaction.[86]

According to this view, a desire must be considered as "a causal law of our actions, not as something actually existing in our minds".[87] I have already indicated what Russell means by this; I shall further amplify it by quoting some more passages from his book.

We all think that, by watching the behaviour of animals, we can discover more or less what they desire. If this is the case (…) desire must be capable of being exhibited in actions, for it is only the actions of animals that we can observe. They *may* have minds in which all sorts of things take place, but we can know nothing about their minds except by means of inferences from their actions; (…) It would seem, therefore, that actions alone must be the test of the desires of animals. From this it is an easy step to the conclusion that an animal's desire is nothing but a characteristic of a certain series of actions, namely those which would be commonly regarded as inspired by the desire in question. And when it has been shown that this view affords a satisfactory account of animal desires, it is not difficult to see that the same explanation is applicable to the desires of human beings.[88]

We judge easily from the behaviour of an animal of a familiar kind whether it is hungry or thirsty, or pleased or displeased, or inquisitive or terrified. The verification of our judgment, so far as verification is possible, must be derived from the immediately succeeding actions of the animal. Most people would say that they infer first something about the animal's state of mind—whether it is hungry or thirsty and so on—and thence derive their expectations as to its subsequent conduct. But this detour through the animal's supposed mind is wholly unnecessary. We can say simply: The animal's behaviour during the last minute has had those characteristics which distinguish what is called 'hunger', and it is likely that its actions during the next minute will be similar in this respect, unless it finds food, or is interrupted by a stronger impulse, such as fear. An animal which is hungry is restless, it goes to the places where food is often to be found, it sniffs with its nose or peers with its eyes or otherwise increases the sensitiveness of its sense-organs; as soon as it is near enough to food for its sense-organs to be affected, it goes to it with all speed and proceeds to eat; after which (…) its whole demeanour changes: it may very likely lie down and go to sleep (…) The char-

86 Bertrand Russell: *The Analysis of Mind*. London 1921, p. 32.

87 *Ibid*., p. 60

88 *Ibid*., p. 61f.

acteristic mark by which we recognize a series of actions which display hunger is not the animal's mental state, which we cannot observe, but something in its bodily behaviour; it is this observable trait in the bodily behaviour that I am proposing to call 'hunger', not some possibly mystical and certainly unknowable ingredient of the animal's mind.[89]

Generalizing what occurs in the case of hunger, we may say that what we call a desire in an animal is always displayed in a cycle of actions having certain fairly well—marked characteristics."[90]

Coming now to human beings (…), it seems clear that what, with us, sets a behaviour—cycle in motion is some sensation of the sort we call disagreeable. Take the case of hunger: We have first an uncomfortable feeling inside, producing a disinclination to sit still, (…). At any moment during this process we may become aware that we are hungry, in the sense of saying to ourselves, 'I am hungry'; but we may have been acting with reference to food for some time before this moment.[91]

"Conscious desire is made up partly of what is essential to desire, partly of beliefs as to what we want".[92] "The primitive non-cognitive element in desire seems to be a push, not a pull, an impulsion away from the actual, rather than an attraction towards the ideal,"[93] together with a true belief as to its 'purpose', i.e. as to the state of affairs that will bring quiescence with cessation of the discomfort. "If our theory of desire is correct, a belief as to its purpose may very well be erroneous, since *only experience can show* what causes a discomfort to cease".[94]

Now is this view correct? Well, —correct in what sense? Let me ask more exactly, Does the account given accord with our common idea of what a desire is? You will at once notice one queer thing: If it is only through experiences in the future that we learn what it is we are desiring, namely what it is that will bring a discomfort to an end, this implies something *very different* from the common idea of a desire. Suppose I wish to eat an apple. That wish may consist in my saying to myself "I wish I had an apple", or "Oh, how good it would be to have an apple", and the like, or in the rising of an image of a crisp apple accompanied by a feeling of attraction towards the thing, etc. But, I ask, has my wish anything to do with what will actually bring about satisfaction? No; that I wish to eat an apple, is neither confirmed nor refuted by a pleasant or wholesome effect the apple may have. If by saying "I have a desire for an apple", I mean to say that my body is in need of an apple (say, of its vitamins), this statement, being a sort of hypothesis, can of course be confirmed in experience. By the beneficial effect of the apple on my organism. But suppose I take an apple and it *fails* to bring about a state of

89 *Ibid.*, p. 62f.
90 *Ibid.*, p. 63.
91 *Ibid.*, p. 67.
92 *Ibid.*
93 *Ibid.*, p. 68.
94 *Ibid.*, p. 72.

satisfaction, then I should have to say, according to this view, that I did not really wish to get an apple or that I was mistaken in that. On the other hand, if someone were to knock me on the head, and this knock brings quiescence—what then? If the object of my wish were disclosed by the occurrence of a state of gratification, or by some similar phenomenon, then I must in fact *have been wanting a knock on the head*, when I believed I wanted an apple, given that the knock has removed my restlessness.

Russell's mistake becomes still clearer if we put it like this: According to Russell, it is a matter of experience what constitutes the fulfilment of a desire or a wish. In this view a wish would be comparable to the experience of hunger which, as shown by observation, is satisfied by taking a certain sort of food, and which may be called, e.g., hunger for an apple. It might then well happen that this hunger for an apple might sometimes be appeased, not by an apple, but by a pear. That is to say, it may well be that I find myself in a state of restlessness, and only after taking a pear do I become quiescent. In no case is it a *contradiction* to say, "The hunger which (I believed) was hunger for an apple has been satisfied not by an apple but by a pear". It is like saying, "This drug which I believed would cure me did not cure me." Now would I say the same thing of a conscious wish? In other words, How does a person know what it is he desires? Has he to wait and see what it is that will dispel a feeling of discomfort and bring about a state of satisfaction? Far from it! It is *characteristic of the use of the words* "desire" that it is *tautologous* to say, "The wish for an apple is satisfied by taking an apple"—evidence that Russell gives a quite unnatural and perverted interpretation. According to his view the connection between a wish and the object of the wish can only be established *by experience* and, note, *afterwards*, after we have observed what it is that will bring a certain restless or discomfort to an end. *Before* our observation, prior to experience, we *did not know* what it is we were desiring. We could at the most guess at it, and, in making such a guess we might be mistaken. But you see that this does not square at all with the way we are using language. We do not say "*I think* I wish for an apple; but let's wait a bit, then I shall see whether it was an apple or a pear or a knock on the head I am actually desiring." That I cannot be mistaken as to the *object* of my wish, that I *know*, and not only *believe*, what I wish for and am quite positive about it, shows clearly that the connection between my wish and the object of my wish is not an experiential one.

Of what sort, then, is it? Exactly of the same sort as the connection between a belief and the object of the belief. Suppose I entertain a certain belief—say, that there will be no war—next year—can I then say, "I wonder what it is I believe"? That this question makes no sense shows that I cannot be uncertain as to what it is I believe. On the contrary the ordinary use of language is such that it is *tautologous* to say, "My belief that there will be no war next year will be confirmed if there is no war next year", i.e. by the coming true of what is *expressed* in the belief. What this amounts to is that there is no *experiential linkage* between the process of believing such and such a thing and the *happening* of such and such a thing. It is essential to what is called belief, in one sense of the word, that it should be *expressed in words*. Once my belief is expressed—e.g. by saying, "There will be no war in the next year"—I can no longer ask what it is I believe: the object of my belief is determined by the *expression* of the belief. In like manner the object of my wish is determined, fully determined, by the expression of the wish.

That brings to our attention a very important point—that a belief, in the sense in which we commonly understand this word, includes the *expression* of the belief. It is in virtue of this expression, or rather, in virtue of the *meaning of the words* which occur in that expression, that a belief is related to its object. *That is* why it is so silly to pretend "I am at this moment entertaining belief; I wonder what it is I am believing?"

I might express it like this: a belief, a doubt, a fear, a desire, a hope, an expectation etc. cannot exist *without a language* in which it is expressed. *Not* that believing, doubting, fearing, desiring, hoping etc. consist *merely* in uttering the form of words "I believe that", "I doubt whether" etc. by no means. If someone were to say "I doubt whether the world has really existed in the past; may be everything has sprung into being within the last minute"—are we bound to give credence to those utterances? Hardly; if I doubt something really *do* doubt it, it is true I shall form some sentence such as "I doubt …; I wonder … is this really so?" But that will *not be all*: the words may be accompanied by a peculiar state of mind, say, a feeling of uncertainty, or a wavering between two alternatives; or besides the words there may be some pattern of behaviour that is characteristic of doubt, such as being slow in taking action, trying warily, taking all possible precautions, and so on. Thus we may distinguish between *describing* the doubt, that is, going very fully into all the details in which a doubtful state of mind, or a dubious manner of acting consists, and *expressing* the doubt. Now the whole point which I want to make is that a full *description* of the doubt must *include as a part an expression* of the doubt. That is, *without* expression the description would be incomplete it would leave out precisely what establishes the connection between the doubt and what is doubted. For this connection is made in *language*, and without language, without linguistic form of expression, we should have to wait and see what it was that removed the state of uncertainty. (Notice again how absurd it would be to say, "A doubt has struck me; but how am I to know what it is I am doubting?")

What I have said of *doubt* obviously goes for *belief, hope, desire, fear*: in all these cases we may distinguish between *describing* the state in which one is when one is in hope or in fear, etc., and *expressing* the hope, the fear etc. It is only in virtue of the *expression* (in words, or images which can be replaced by words) that a belief, a hope, a desire, a fear are *linked* with their object. It is because of this linkage which is of a *semantic*, not of an experiential nature, that we are so sure—and rightly so—of what it is we believe, or hope, or desire, or are afraid of. This most important point was perfectly misconstrued by Russell when he said: "A desire is 'conscious' when we have told ourselves that we have it (…) But it only differs from an 'unconscious' desire by the presence of appropriate words, which is by no means a fundamental difference,"[95] as if the words spoken were of no consequence whatever,—a sort of accompaniment of a desire which might as well be abolished altogether. Russell *completely failed to understand* what is so relevant to this whole discussion: that we know *for certain*, and *independent of any experience*, what it is we believe, or doubt, or hope for, or desire, etc. It is the words, and nothing but the words, which supply the *semantic elements* by dint of which a state of mind (such as doubt, or belief) "points" beyond itself to its object. How we could ever know what it was we believed or desired, would, if there were *no symbols* whatever to express it, remain a perpetual miracle. And yet Russell speaks of "the presence of appropriate words which is by no means a fundamental difference". Little did he dream that with this disdainful remark he was depriving himself of the key to the whole problem.

I must now correct a remark which I have made. I said that a doubt is *more* than uttering certain words which express it. I must add that this *need* not be so. It may well be that I doubt something (say, the truth of a bit of news I read in the paper) *without* having any specific experience and *without* displaying a certain dubious manner of acting. The words which I form to express my doubt may be the *only* process that is going on in my mind at that time; they need not allude to some other hidden mental process. Similarly in the case of desire. If the desire is not emotional,—as when I go to a book-shop, see there a certain book and say "Oh, I should like to get it",—the desire consists *only* of the words spoken. But this is not quite true either: it will also depend on the whole situation in which I utter the words, e.g. on what I do afterwards. If, for instance, I say a minute later "No, I don't really want it", one may rather suppose that I was only *playing with the thought* of buying the book without really desiring to do so. Further, the *same* words, when spoken by a jocose person, may be a tease, or an ironical remark, and so on. So it is not the *saying alone* which constitutes the desire, but the saying *incorporated* in, or *seen against* the background of, an entire situation. What is true is only that there need not be an *emotion*, or a particular sort of behaviour behind the words.

To sum up: Russell gives an entirely perverted account of what we normally call a desire. A desire in the ordinary sense of the word, a *conscious* desire, is something which includes an expression of the desire as an essential part. *Desire is tied up with language*. A being that is speechless, an animal, or an infant, cannot entertain a desire in this sense. "But do not animals and babies *have* desires?" Of course; but what does having a desire consist in? In a certain restlessness which is followed by a state of quiescence when a certain state of affairs is attained. It is here that Russell's account *does* apply with perfectly good sense: a desire of a speechless being is best regarded as a certain "behaviour-cycle." If so, we should clearly understand that the word "desire" has now taken on a new sense, which must sharply be separated from a desire in the ordinary sense. And now notice

95 *Ibid*., p. 31

another thing: even an animal or a small baby may have a desire in a sense which comes much nearer to what *we* human beings call "desire". For even an animal or a baby can use a sort of *gesture language*. Suppose, for instance, a baby looks constantly at the milk bottle, stretches out his arm towards it and cries, then the baby *expresses* in a way his desire: he *indicates*, by stretching out his arm, what he wants. And notice how in this case the behaviour, made up of the gestures, has the *same multiplicity* as the desire: if the baby stretches out his hand towards the milk-bottle, he desires the milk bottle, if he stretches out his hand towards a play thing, say a ball, he desires the ball. Here again there is a *semantic relationship* between what the baby does and what he desires: that is, the gestures have already a *meaning*, they *express*, or *indicate* something in a way in which the behaviour of an animal in general does not. If an animal runs up and down in a cage, we may "infer" from this that it wants freedom. But there is no means, no language, no symbol for the animal to *express* this desire. Our saying that the animal wants freedom is but a guess which may be confirmed or confuted by its subsequent conduct. Contrast this case with that of a cat which rivets his eyes upon a pot of milk, raises his forepaw towards it and mews. In this case the cat, by using a sort of primitive gestures, *expresses* his desire, and this comes much nearer to what *we* commonly call this name.

Thus we see how *three different senses* of "desire" begin to detach themselves from one another. There is, first, what we commonly call desire, the desire expressed in word-language. Then there is, second, a desire expressible by means of a gesture language covering a much smaller range; and then there is thirdly the desire as described by Russell and the behaviorists, defined as a specifiable pattern of behaviour. These three classes are not quite sharply separated, as there are intermediate or border line cases, as when a little child can only imperfectly express himself and uses gestures besides half-articulated words.

# The Logical Force of Expressions[1]

Friedrich Waismann

## 1. Ramsey

It seems to make perfectly good sense to distinguish between what is expressed and the way in which it is expressed. There is little doubt that there are many different ways of saying the same thing open to us. If I denied this, I would certainly be wrong. And yet a word of caution may not be amiss. Among logicians a tendency has grown up to concentrate their attention on those properties of a statement which make it true or false, what they call a "proposition", and to neglect the form in which it is expressed. I think it is a dangerous tendency as it may lead to overlooking all sorts of differences which are due to the form. Let me give an example taken from F. P. Ramsey. In an article on "Universals" he says:

> Thus in 'Socrates is wise', Socrates is the subject, wisdom the predicate. But supppose we turn the proposition round and say 'Wisdom is a characteristic of Socrates', then wisdom, formally the predicate, is now the subject. Now *it seems to me as clear as anything can be in philosophy* that the two sentences 'Socrates is wise', 'Wisdom is a characteristic of Socrates', *assert the same fact and express the same proposition*. They are not, of course, the same sentence, but they have the same meaning, just as two sentences in two diffferent languages can have the same meaning. Which sentence we use is a mattter either of literary style, or of the point of view from which we approach the fact. If the centre of our interest is Socrates we say 'Socrates is wise', if we are discussing wisdom we may say 'Wisdom is a characteristic of Socrates'; *but whichever we say we mean the same thing.*[2]

So far Ramsey; he concludes that which particular sentence we use to express our proposition, has "nothing to do with the logical naature of Socrates or wisdom, but

1 Editor's Note:
The present paper is taken from Waismann's Nachlass (Bodleian Library, Oxford) in which it is registered as Q 2. It was prepared from a manuscript largely written in shorthand. Apart from minor emendations, the text is that of the original. As may be gathered from its style, the paper must have been presented as a lecture. Although no date is indicated on the manuscript, it is likely that it was commposed in 1948 when Waismann taught a class on the topic of the essay. The detailed lecture notes he prepared for this class are, under the same title, included in his Nachlass (Q 3) and coincide largely with the issues treated of in the present piece.
The editor owes a great debt of gratitude to Friedrich Waismann's literary executors, Sir Isaiah Berlin and Sir Stuart Hampshire, for permission to publish the paper.
Wolfgang Grassl

2 Frank P. Ramsey, "Universals", in *The Foundations of Mathematics and Other Logical Essays* (London 1931), p. 116. [The italics are Waismann's. –W.G.]

B.F. McGuinness (ed.), *Friedrich Waismann – Causality and Logical Positivism*,
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is a matter entirely for grammarians".[3] Here, then, logic is contrasted with grammar, logic being conncerned with propositions which are expressed by sentences whereas grammar is concerned with the form of sentences. I do not claim that such a distinction cannot be made; what I claim is that Ramsey's example won't do and throws doubt upon the whole value of the distinction. For it is really easy to see that the two sentences Ramsey is quoting do not express the same thought. If I say 'Wisdom is a characteristic of Socrates,' this implies that Socrates has other charracteristics as well; for if what I meant was that he has only the characteristic of wisdom and no other characteristics besides this, I should have said 'Wisdom is the characteristic of Socrates.' The use of the indefinite article 'a' is a quite clear indication that wisdom is taken to be only one characteristic amongst others. On the other hand, if I say 'Socrates is wise,' this gives no hint whatever whether there are, or are not, other characteristics as well. We know naturally that a man has not only one characteristic, but this does not concern us here; what we are concerned with is what a sentence expresses and what it implies. And there is a quite clear difference between the two sentences, for the one implies what the other does not imply, namely that Socrates has other characteristics as well. In other words, the logical force of the two expressions is different. In view of this fact it is queer that Ramsey should say "it seems to me as clear as anything can be in philosophy that the two sentences (…) express the same proposition." They plainly do not; it is as clear as anything can be in philosophy that there is a difference between them, and the overlooking of such a simple and striking difference may be due to the habit of modern logicians to pay no heed to the precise linguistic way in which something is being expressed.

We see from this that one of the means to detect the logical force of an expression, or to compare two expressions with regard to their logical force, is to consider what is implied by it (by them). *To study the logical force of expressions is to study the logical relationships – the entailment relations – which hold between them and other expressions.*

## 2. Two Sorts of Inference

In speaking of the "logical force" of an expression we should keep in mind that this force may be due to two distinct factors, and that accordingly there are two sorts of logical relationships, or two sorts of inference. The one sort of inference is based on the form of a sentence: 'all men are mortal' and 'Socrates is a man' entail 'Socrates is mortal.' This inference is due entirely to the form of the two premises and will also hold if 'men,' 'mortal' and 'Socrates' are replaced by any other words, or, indeed, by variables:

3 Ibid.

$$\begin{array}{ccc} \mathrm{M} & a & \mathrm{P} \\ \mathrm{S} & e & \mathrm{M} \\ \hline \mathrm{S} & e & \mathrm{P} \end{array}$$

Such an inference is valid regardless whether the letters are taken to stand for men or propellers or logarithms. Its validity is due to the form. The form is exhibited in part by certain basic locutions—such as 'and,' 'or,' 'not,' 'if – then,' 'unless,' 'if and only if,' 'some,' 'there are,' 'all,' 'every,' 'any' —which are also called logical symbols, and in part by the way in which the terms are arranged in the premise-statements. Traditional logic is the study of inference in so far as it is based on form only.

But there is another class of inferences which cannot be based on form in the sense first explained. 'If a thing is red, it cannot be green at the same time' would be an example of such an inference. 'Being red' no doubt entails 'being not green;' but the entailment is not due to the form, as can be seen from the fact that the relation no longer holds if one changes 'red' to 'coloured': for 'being coloured' in no wise entails 'being not green.' One is rather inclined to say that the validity of such an inference is due to the meaning of the words 'red' and 'green': as soon as I understand what 'red' means and what 'green' means, I see that a thing which is red cannot be green at the same time. Such an inference, it would seem, makes an appeal to the understanding of the words involved, and, therefore, is necessarily bound up with definite, specific words: it is no longer possible to replace the words in the example by any other words, or by variables, and maintain the validity of the inference-scheme. It is desirable to use some term to designate this class of inference and mark it off from those studied in traditional logic. I propose to call the inferences in question *semantic inferences*.

Other examples of this sort are furnished by *relations*. 'A is married to B' entails that 'B is married to A,' but if I change 'married to' into 'parent of,' the entailment no longer holds. Thus we see again that the validity of the inference has something to do with the meaning of the relation-word, and that symmetry, reflexivity, transitivity are tied up with the specific content of the relation.

## 3. V-Inferences

Before going on let me call attention to a third category of inferences: those which occur in laying down the *verification* of a stateement. Suppose the statement is 'There is an electric field in this room.' How can I verify it? Well, I put an electrically charged test-particle there and watch whether it remains at rest or moves. In the latter case we say that the statement has been verified. In what, then, does giving the method of verification consist? Simply in this that we construct the statement 'There is an electric field in this room' (s) with the statement 'A test-particle left to itself moves' (t) and declare that t is to follow from s. In other words, we are laying down a rule of inference which allows us to pass from s (that is about an abstract entity 'electric field') to another one that describes an observable situation. Not quite so, to be sure, because of the words 'electrically charged' which do not stand for anything observable. But the latter statement, in its turn, can be connected with an observation stateement by a further rule of inference; for we may say that the test-particle is charged with electricity if, when connected with an electroscope, the gold leaves of the latter diverge (*n*). Just as the statement 'A test-particle moves' gives the verification of the statement 'There is an electric field in the room', so the statement 'The gold leaves of the instrument diverge' gives the verification of the statement 'The test-particle is charged with electricity.' Thus describing the verification of a statement consists in connecting the statement with other ones by setting up rules of inference, and thus incorporoaating it into the body of language—or determining the way it is to be used. Without laying down such rules the statement under review would be isolated, cut off from the remainder of language, and we should be unable to manipulate it; in short, it would be a sentence without application.

It is clear that inferences of this sort do not flow from the form of the sentences in question; they are rather introduced in the process of *extending* language, namely in the formation of new sorts of sentences with newly coined terms which, in some way, must be linked with the remaining body of language. I shall refer to them as *V-inferences* ('verificational').

From the many interesting and puzzling problems posed by these inferences I shall here mention only one. I think it is pretty clear that we are *essentially free to choose the rules of inference as we like*; for in laying down a rule of inference we determine the way a newly formed sentence is to be used. If we lay down one rule of inference, we fix its use so that it means one thing, if we lay down a different rule of inference, we fix its use so that it means a different thing. How we fix its use lies with us. This means that in laying down rules of inference we can proceed at will: the only consequence will be that if we choose different rules the meaning will be different. In this way, by choosing rules, we chisel out a meaning. Such an expression has not got a logical force unless we give it a force.

In the case I have first considered we are not tempted to say that the rules of inference *follow* from the meaning: it is rather the rules which *determine* the meaning. In this respect the V-inferences are very different from the inferences as studied in formal logic. Given the premises 'All men are mortal' and 'Socrates is a man,' I find that it is *not* in my power to choose which conclusion is to follow from them. The conclusion, whether I like it or not, is predetermined by the form of the premises. On the other hand, in setting up a scientific hypothesis, we have first to choose which sort of sentences are to be consequences; we are, to a certain extent, free to arrange the logical relationships.

This brings out an important antithesis between formal inferences and V-inferences which may be characterized by the catchwords ‘unfree – free.’ What about the second category of our inferences, the semantic inferences such as ‘If something is red, it cannot be green’? Which of the two things shall I now say—that the rules of inference flow from the meaning of the words, or that the meaning of the words is determined by the sort of inferences?

## 4. Body of Meanings

What exactly is the relation that holds between the rules governing the use of a word or a phrase, and the meaning of this word or phrase? Do the rules follow from the meaning, or is it rather that the meaning is determined, chiselled out, by the rules?

Frege, considering the formalist view on the nature of mathematics, i.e. the view according to which mathematics is nothing but a manipulating of symbols in accordance with arbitrarily chosen rules—a sort of a game with ink-marks on paper instead of chessmen on a board—said:

> If there were a meaning to be considered, the rules could not be arbitrarily laid down. On the contrary, the rules follow necessarily from the meaning of the signs.[4]

What Frege had in mind, I think, was this: The formalists give a distorted account of mathematics regarding such signs as ‘1’, ‘2’, ‘3’, ‘+’, ‘=’ as meaningless marks to be manipulated according to arbitrary rules. What can be achieved in this way, at best, is a game which may bear a certain superficial resemblance to mathematics, but which nonetheless differs from it by the lack of any thought-content. Indeed, a person who has merely arithmetic as a sort of game played with signs on a piece of paper, will not be able to see in an equation such as 2+2=4 the expression of *truth*, but just a configuration in a game like a certain configuration of chessmen on a chessboard. What he lacks, and what is so essential to mathematics, is the understanding of the *meaning* of the signs. That is why he has just to accept the rules as such, which are for him entirely unconnected; having learnt merely a game, he cannot see how the rules are actually connected and spring from one central point, the *meaning* of the signs; thus it comes that he fails to understand why it is just these rules and no others which hold. On the other hand, this can be understood by any person who has grasped the meaning which is, as it were, behind the sign. Thus we have to penetrate behind the sign to its actual meaning in order to gain an insight into the necessity of using a sign according to just those rules. He who understands the meaning sees the why; the other is just blind. In this

4 Gottlob Frege, *Grundgesetze der Arithmetik*, Vol. II (Jena 1903), p. 156. [Waismann does not render Frege‘s text literally. – W.G.]

way, disregarding the formalist view, Frege held that the rules cannot be set up independently, but must follow from the meaning of the signs.

If we try to understand Frege, we shall encounter a difficulty. For what does he mean when he says that "the rules *follow* from the meaning of the signs"? In what sense do they follow? I understand perfectly well what is meant by saying that one rule follows from another rule, for instance that the rule ''George' is spelled with a capital' follows from the rule 'Any Christian name is spelled with a capital.' The one rule stands to the other in the relation of ground and consequence, or the general and the particular. But what on earth can be meant by saying that a rule follows from the meaning of a sign? Can the meaning be a ground from which a rule is derived as a logical consequence? Certainly not; so Frege cannot mean by the word 'follow' '*logically* follow.' What else could he have meant? No answer. Strange that so subtle a thinker as Frege should have felt no need to explain himself a bit more on this subject. What he said was: "the rules follow necessarily from the meaning of the signs." If he meant anything at all, it is on us to put on some interpretation. Now in reading the passage quoted in its whole context, there is little doubt that what Frege had in mind seems to have been something like this: It is not so that we have first to learn the rules, we rather *grasp immediately* the meaning of the signs '1', '2', '3', '+', '=', etc. as a whole, and from the meaning thus grasped we come quite naturally to apply such and such rules. *We begin with the meaning and end with the rules*, and not the other way round. This fact he expressed by saying that "the rules follow from the meaning" which may perhaps be reworded by saying, "The rules *flow* from the meaning," thus precluding the interpretation that they are deducible from the meaning.

As a psychological statement this is perfectly true; but it does not help us in the least to clear up the *logical* question: What sort of relaation is it that holds between the meaning of a sign and the rules for its manipulation? For it is this latter question on which our whole interest is centred.

Now there is an exactly analogous question in regard to word-language, and by following it up we may hope to throw some ray of light on the situation. The counterpart to the formalist view in the case of word-language would be the view that we learn all the rules for the use of words without ever penetrating to the meaning which would lie, so to speak, behind all this. A man who has only learnt, say, from a book of grammar, all the single rules for the use of words, is faced with a lot of disconnected rules; he would not be able to see why the words are used just in this and not in some other way. As soon as his eye is opened to the *meaning*, he will understand clearly why the words can be used only in such a way. For, he will say, the use is determined by the meaning: because the word means such and such, it can only be used in this way. He could put his argument like this: If I were to string together words at random, taking first one word out of a dictionary, then another word, and so on, joining them into a sequence, no sense in general will result. It will not, because of the meanings of the words which will not fit together. It is only by taking the meanings into account that we are able to arrange words into meaningful patterns. So the possible word-combinations are determined by their meaning.

The words, when we look at them, e.g., in print, are visible. the meanings are invisible. And yet it is these invisible entities, it would seem, which determine the patterns of the physical signs. We may try to imitate the situation by the following picture: Suppose there are a given number of bodies in space, such as cubes, prisms, pyramids, and the like; now suppose you cannot see these bodies because they are made of glass and are perfectly transparent, except for one face of each cube, or the base of a pyramid, which are coloured. In these circumstances you will see a given number of coloured squares of rectangles distributed in space, all seemingly unconnected. But if you try to join these surfaces together, you will find that this is possible only in certain definite ways, not in others, because of the invisible glass bodies behind the faces which permit some configurations and preclude others.

This, then, seems to be the picture which underlies the idea we make ourselves of the relation of a word to its meaning. We are apt to regard the meaning as an ethereal entity behind the word. Not that we explicitly regard it so. But to admit the use of a crude picture is always better than to use this same picture unadmittedly. To say that the soul is a little man peeping out of a man's eyes is less harmful than to use the same picture in a covert and 'sublimated' form so that it might escape the censorship in Freud's sense. Likewise it is better to confess to ourselves that we are tempted to look at meaning as something that *guides* us in arranging words, though the meaning itself cannot be perceived by any of the senses. These meanings behind the words, we are apt to think, determine whether a series of words fit, or do not fit, together significantly.

A philosophical problem is, at first, nothing but a peculiar kind of uneasiness. The philosopher who is worried tries to get hold of, to seize that which, so far unseizable, has burdened his mind. But the difficulty lies just in this—to penetrate to the core of the uneasiness. It is as if one had a hair on the tongue: one feels it, but cannot seize it, and, therefore, cannot get rid of it. Or, to put it differently, the difficulty is to express the troublesome features in any problem in so characteristic a way that the other says, 'Yes, that is exactly what troubles me.' I do not know whether the picture helps you to seize the hair on your tongue.

Anyhow, let us get clear as to which of two things we fancy is exxplained by the picture. Is the picture meant to explain the causes of our combining words in the way we do, i.e. to explain the mental or physical processes which lead to our combining words in a particular case? Or is it meant to give a reason why words can be combined only in certain, and not in other, ways? Obviously the latter; Frege leaves no doubt whatever that he is not interested in any sort of causal explanation. What he has in mind is to find out a sort of *ultimate sanction* for the rules of grammar. (Remember, giving the reason justifies a use, giving the causes does not). The picture supplies us with a causal explanation: it explains why, as a matter of fact, surfaces can be joined together in certain ways only, and why any other configuurations do not occur. Yet what we wanted to see explained was how it is that only certain configurations *can* occur, in a sense: are *permitted* to occur. For only such an explanation is parallel to that which we were in search of—namely how it is that only certain combinations of words can occur, in the sense of: are permitted to occur. But this is precisely the question to which our picture offers *no* answer. It gives a causal explanation: such and such configurations do not occur because of the glass bodies behind the surfaces which prevent them from occurring. So the picture has at least the advantage of making you see what is lacking in it.

Suppose now we want to change the picture so as to make it give the *reason* why surfaces can only be joined in certain ways. We should then have to say: the surfaces may be joined in such ways only as are permitted by the shapes of the geometrical bodies behind them. We no longer speak of *glass* bodies, but of *geometrical* bodies, we no longer say that they cause, but that they determine the possible configurations. In short, what we are concerned with is not the actual disposition of bodies, but rather a *system of rules* from which the possible configurations can be derived; and this system of rules is embodied in the shape of the geometrical cubes, prisms etc. behind the actual faces. But what are we talking about when we talk of *geometrical bodies*? Here a second confusion comes in. We are inclined to look upon geoometrical bodies as if they were a sort of material bodies, only less crude, less real, more etherealised, more Platonic. We think of an 'ideal cube' as something that we can never get quite realised in this imperfect, paltry world. We do not realise that the 'ideal' or 'geometrical' cube is nothing but a *representation of all the rules characteristic of the concept of a cube*, a representation or embodiment in visual form. In other words, the drawing of a cube, or a model of a cube used in a geometrical demonstration, is not the subject described by geometry, but rather *part of a notation*. This notation enables us to take in at a glance all the rules and relations which otherwise it would be burdensome to express in words. In doing geometry in a purely abstract way (which is quite possible), we would have to write down a lot of rules—what a release to look at a drawing from which we can gather all these rules without any effort!

The danger is only that we do not see that the ideal or geometrical cube is merely a part of a notation and are thus led to contrast the ideal with the material cube made of wood or glass. This confusion is deeply rooted in our mode of expression, e.g. when we talk of *geometrical cubes* and of *wooden* cubes as if they were just two different species of one genus. It is as if one were talking of three men, John, Paul, and Stephen; then of the average man, saying 'I wonder what the average man will have for dinner to-night?' Here one can see how ridiculous it is to speak in this way. And here one can see now how ridiculous it is to contrast the wooden with the geometrical cube in this way.

So the cube is only the expression of certain rules in a visual symbolism. And when I say that I can describe the disposition of surfaces in space by referring to the geometrical bodies behind them, I am not giving a causal explanation, but a picture in which the rules governing the possible arrangements can be derived from other rules, namely from those for which the geometrical bodies stand as a visual representation.

To sum up: We have derived rules from rules—not from the glass bodies or anything else that is not a rule. The rules have not disapppeared from the description and, as it were, retired into the essence of the bodies.

The situation thus cleared up illustrates the confusion in a precisely parallel case. What we want to understand is why it is that words can be joined in certain ways only. The answer we are tempted to give—'because of the meaning of the words'—does not get us any further. For how can the rules for connecting words be derived from something that is not a rule, the amorphous meaning? We are liable to make the same mistake as in the case of the sufaces and the geometrical bodies behind them. No, the law according to which the surfaces can be joined is not 'extracted' from the material bodies; nor are the rules governing the arrangement of words 'extracted' from the meaning. For the meaning of a word is the way it is used; or better, a word has got a meaning when it is used in some definite way. So it is not that the rules for connecting words are *derived* from their meanings; we rather try to *distil* the use of words into rules, to codify the usage.

## 5. 'All men are mortal'

Let me now resume the discussion at the point before I began to speak of the bodies of meaning. The question which concerned us was whether the rules of inference spring from the meaning of expressions, or whether the meaning of expressions is, at least in part, determined by the rules of inference. I think we can now see our way more clearly. What is so misleading is the idea that we need to hold a concept before the mind's eye, gazing at it closely in order to see that such and such does follow from the nature of the concept.

Consider, for instance, the standard example 'All men are mortal.' Is this sentence to be understood as an experiential statement, a generalisation of previous experiences? Or is it a sort of general maxim, being a bit vague (like 'Westwinds bring rain') and allowing of some stray exceptions? Or is it to be taken to mean 'To be a man entails to be mortal,' i.e. as expressing a rule of inference? I think, if I take my own feeling of language for a guide, that the first sense is slightly more in the foreground, although the third sense is in no way excluded. It is by no means easy to find out what exactly we mean by 'All men are mortal.' How, for instance, would we set about to verify or falsify this statement? It would be easier if there was some upper limit to the age of man, say 150 years. 'All men are mortal' would then simply mean: men live no longer than 150 years. Such a statement, to be sure, could never conclusively be *verified* because of the unfortunate 'all' it contains; it would, in this respect, be on a par with other universal statements. But it could at the least be *falsified*. On the other hand, to lay down an age-limit would be unnatural. For suppose a man has been found who, on good authority, is 200 years old, then what shall we say? That the statement ‘All men are mortal’ is *confuted* by the counter-example? But suppose the man dies 10 years later, then one would say, ‘Here you are: man, after all, is mortal.’ The fact that one did take the death of such an extremely old person as a confirmation of man’s mortality goes to show that, at bottom, one was always prepared to disregard an age-limit, if this is desirable in the light of new experiences. To insist on a sharp age-limit won’t do. But now suppose a man has been found in Java of whom authentic well-attested records are handed down to us saying that he was already living 3000 years ago; suppose he is still a youth of radiant vigour, showing not the least sign of ageing—what then? Shall we say that we are in the presence of an immortal, excluding such a being from profane mankind, and seeing in him a kind of superior being? Or shall we say this is a case that defeats the general rule of mortality? Or should we simply say, ‘Wait and see, maybe he will still die in the end’? If we take the first line, this would mean that we include mortality in the concept of man. ‘All men are mortal’ could then be construed as meaning ‘To be a man *entails* to be mortal,’ i.e. it would be self-contradictory to say of someone that he is a man and not mortal. If we take the second line, we should regard the sentence ‘All men are mortal’ as expressing an experiential (and untrue) statement; and so in the other cases. Now what do we mean by the sentence? I think all one can say is that the sentence has not got a precise meaning. If we were called upon to make a decision, we should be irresolute, though we might, in special circumstances, decide in favour of one or the other of the senses. But as long as we are faced with a normal situation, there is no need whatever to decide the issue. Nor does that impair our use of language without splitting hairs and trying to find the precise meaning of our words. I think that most of us are not even aware that there are so many different senses to be read into a little sentence such as that referred to. And if our attention was drawn to this fact, we should become unncertain and wavering. This, however, does in no way impair the use of our language, if it is only applied to *normal* circumstances. The question reaches a critical condition as soon as we are faced with a thoroughly new situation. Then it calls for a decision and mind, the decision will be anything but capricious. Suppose, to change the example, it was possible to put up the duration of life, say, to restore youth and vigour to an aged person through an operation and to repeat this process an indefinite number of times so that natural death would be eliminated. Man would then become potentially immortal. Would this mean that the statement ‘All men are mortal’ is false? This will depend on the whole situation arising out of the disscovery. If it was found that man has remained, in all other respects, essentially the same, we should be ready to give up that statement. If, on the other hand, it was found that, the certainty of death no .longer before his eyes, many of man’s characteristics would undergo a profound change, one might say that a new biological species has come into being, thereby implying that being mortal is an essential characteristic of man. Between these two cases a number of others can be envisaged in which we should be doubtful as to how to decide.

Now the old school logic was under the impression that language is *rigid*, that words *have got* a quite definite meaning, and that, as a consequence, any question as to whether one statement does, or does not, entail another one, can be decided by 'Yes' or 'No.' And even in our time this strange prejudice presents itself in G. E. Moore's well-known question 'What exactly do you *mean* by this?'—as if, when we utter words, we *should* mean something quite definite. But ask yourselves whether you have anything quite definite in mind when you speak of a man or a murderer?

Or, to change the example: In which sense do you understand a metaphor? Take Goethe's saying 'Music is frozen architecture.' Yes, you understand it quite well, don't pretend you don't; but if we were asked, 'What exactly do you mean?', a proper reply would be 'Don't ask so silly questions,' or 'I do not mean anything definite; I see the point.' Considering that there are ever so many expressions which were used first as metaphors one cannot draw a sharp line between metaphorical, figurative and literal uses of words. But if so, there is not much point in insisting that one *should* use words in a quite clear sense. Language is fluid, and so is meaning and understanding.

# A Philosopher Looks at Kafka

Friedrich Waismann

I shall best approach my subject by explaining how it was that I, a non-professional, began to take an interest in Kafka. The first thing of his which I happened to read was *The Trial*. It is difficult to describe my reaction. Certainly I didn't understand the book. At first sight it seemed to be a confused mass, a nightmare, something abstruse, incomprehensible to the utmost degree. One fine morning Joseph K., the junior manager of a bank, is arrested. No grounds are given. He has, we are assured, done nothing wrong. A charge against him is never specified. Though he is under arrest, he can walk about freely and go to his office. In the course of the story we are led on to catch a glimpse of a very strange 'Court', a ridiculous, corrupt, despicable Court that sits in a suburb, in the attics of a building where the poverty-stricken tenants have flung their useless lumber. The Examining Magistrate sits on a kitchen chair, with an old horse-rug doubled under him. The Judges are obsessed with vanity, and run after every woman they see. One of them has been thrown out of no less than five different flats that he managed to worm his way into. The higher officials keep themselves well hidden. What is still more puzzling is that, as the story unfolds, everyone seems to be in the know. K.'s uncle, for instance, and a manufacturer he happens to meet, are fully aware that he is involved in a 'case' that it is a very dangerous thing, that this queer Court really does exist, and so on. Yet the proceedings of the Court are kept secret, not only from the general public, but from the accused as well. Only, of course, within possible limits, but still to a very great extent. All this creates a strange atmosphere. The Defence, though not expressly countenanced, is merely tolerated. Strictly speaking, therefore, no counsel for the defence is recognized by the Court, and all who appear before the Court as Advocates are in reality merely hole-and-corner Advocates or hedge-Advocates. There are fantastic details which show the contempt in which they are held by the Court. The ranks of officials in this judiciary system mount endlessly, so that even adepts cannot survey the hierarchy as a whole. The accused man can be condemned or acquitted. But there are three possibilities of acquittal: definite acquittal, ostensible acquittal, and indefinite postponement. As to the first, we are told by an expert who has listened to countless cases in their most crucial stages and followed them as far as they could be followed, that he has never encountered one case of definite acquittal. There are only legendary accounts of ancient cases. The judge can grant an acquittal, and the accused walks out of the Court: a free man. But he is only ostensibly, or, more exactly, provisionally free. The Judge has not the power to grant a final acquittal; that power is reserved for the highest Court of all, which is quite inaccessible to ordinary men. When the charge is lifted from

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the accused man's shoulders, it therefore continues to hover above him and can, as soon as an order comes from on high, be laid upon him again. In *definite* acquittal the documents relating to the case are completely annulled, they simply vanish from sight, they are destroyed. In the case of ostensible acquittal, all the documents are preserved, with the addition of the record of the acquittal and the grounds for granting it. The whole dossier continues to circulate. One might think that the case had been forgotten, the documents lost and the acquittal made absolute. But this is wrong. No document is ever lost. The Court never forgets anything. One day, quite unexpectedly, some higher Judge may take up the documents, recognize that the charge is still valid, and order an immediate arrest. It is possible for the acquitted man to go straight home from the Court and find officers already waiting to re-arrest him. The case then begins all over again, and again it is possible to secure an ostensible acquittal. This second acquittal may be followed by a third arrest; and so on. Lastly, *postponement* consists in preventing the case from ever getting any further than its first stages. The case must be kept going all the time, although only in the small circle to which it has been artificially restricted.

It all seems patent nonsense. And yet, as I was reading, it came with a curious impact upon me, as if I had known these things before and forgotten all about them. The very absurdities, the fantastic and ridiculous Court, seemed to be reminisscent of—I hardly knew what. I was haunted by the novel. I was sure that there must be something behind it, and yet I was utterly unable to say why.

Then a friend of mine told me of an experiment he had made with this novel. At that time he was living in Prague. He lent the volume to a number of people of quite different standing and education and social position: University students, uneducated people, ladies of the aristocracy, and workers at the Skoda factories. He asked them to read it and to tell him their impressions. What he found was that *The Trial* was understood at once, without previous reflection, regardless of education or social position, by all those who were suffering from the disease that Kafka died of: consumption.

This experiment suggests that *The Trial* narrates the story of an illness, of its inner experiences, its inner states and stages; and this interpretation is confirmed by something significant in the text, the *ambiguity* of most of the expressions Kafka uses. When he speaks of an 'inquiry' into the manager's 'case', you will notice that the term 'inquiry' might refer either to a legal or a *medical* inquiry, just as the word 'case' is used both by lawyers and doctors; and one speaks of an Examining Magistrate and of a medical Examination. This ambiguity is even more obvious in the original German. (Translation, after all, often *re*places the words only to *dis*place the sense.) The very title in German, *Der Prozess*, provides a clue: for *Prozess* means, on the one hand, 'trial', and on the other hand 'process' in the sense of 'pathological process'. Chapter II is headed *Erste Untersuchung*; and *Untersuchung* means both 'legal' and 'medical examination'. In the first talk, K. is told 'proceedings will be instituted against you'. But 'proceedings' fails to render the ambiguity of the German word *Verfahren*, which can mean either 'proceedings' or 'treatment'. Thus a good many of Kafka's terms have the peculiarity that they can be interpreted in two different ways, and the effect is that two distinct planes of meaning begin to detach themselves from each other. There is a literal sense, and, ranged behind it, another, more hidden sense. The reader seems to have a chance to penetrate to a deeper understanding by following up this second layer of meaning, which creates, as it were, a second deeper horizon cutting through the whole novel.

Many things at first sight quite incomprehensible now take on a new and quite clear meaning. We understand now why, to begin with, the hero pays no heed at all to his 'case', whereas, as the story proceeds, he becomes more and more involved in,·and absorbed by it, until he loses all other interests in life, forgets the whole world,and lives only in the hope of toiling along his path until the end of his 'case' comes in sight. He has to think of his 'case' time and time again, until it becomes second nature to him. That is precisely the way a patient feels and behaves, when a disease is slowly and irresistibly 'proceeding'. In the end, he is no longer able to take an interest in his business or even go about it. Now one can see too why Kafka speaks of three possibilities of acquittal: the definite acquittal which is nothing but a myth, a legend (*there is no cure for·this disease*); the ostensible acquittal, when the patient goes home apparently cured, from the sanatorium, but the disease·continues to hover above him, and the case may start again any day (the second·arrest); and postponement, which naturally consists in·preventing the disease from ever getting further than its initial stage.·The *Advocate*, of course, is the *doctor*; and as no doctor can really cure, there are only hedge-advocates—quacks. And one can now see too why the doctor slowly grows in significance in the eyes of the patient until the patient, is absolutely under his spell, becomes his bondsman,so to speak, and looks up to him as the dispenser of mercies. Kafka's advocate is called *Huld*,which suggests this, for *Huld* in German means 'graciousness' or 'favour'.

In this light many of Kafka's details take on an unexpected·significance. The 'first examination' (*erstes Verhör*) is a curious, weird scene, with the right half of the audience which fills the hall growling with disapproval, which the left half·keeps perfect silence. As the word *Verhör* derives·from *hören,* to·hear, the hidden sense may well be that in the examination of the two lobes of the lung; one half gives out noises while the other is quiet. There is another scene in which Titorelli, the painter, obviously a quack consulted by the hero, offers him an unframed canvas. 'It showed', so the text runs, 'two stunted trees standing far apart from each other in darkish grass.' (Kafka always complained about his body 'drawn out from a lumber room', as he once put it.) Is it too fanciful to see in the painter a radiologist, in his painting an X-ray photo, and in the two stunted trees the two lobes again of the patient's lungs? In this context it gains significance that the painter's studio *belongs* to the Law-Court offices, for everything connected with the Court has a bearing on this mysterious case. Moreover, I think that this has something to do with Kafka's own consumption. I take it that the initial 'K' in the novel stands for Kafka, and that Kafka speaks of himself. (A mediocre author speaks of the world, a good author always of himself.) Kafka wrote the novel in 1915, and fell ill in 1917. His novel is therefore an anticipation of his disease. Other features of the novel fit this picture. Whenever K. comes up to the Law-Court offices, he feels dizzy. The people in the office, Kafka says,

> were actually gazing at him as if they expected some immense transformation to happen to him in the next moment … K.'s behaviour was really caused by a slight feeling of faintness … 'You feel a little dizzy, don't you?' she asked. 'Don't worry, that is nothing out of the common here, almost everybody has an attack of that kind the first time they come here. This is your first visit? Well, then, it's nothing to be surprised at. The sun beats on the roof here and the hot roof-beams make the air dull and heavy. The air, well, on days when there is a great number of clients to be attended to, it is hardly breathable.' … He would have been glad to sit down. He felt as if he were seasick.

Again, when he comes up to the studio 'the feeling of being completely cut off from the fresh air made his head swim. He was prepared to gulp down even mouthfuls of fog if he could only get air.' No one sho has suffered from tuberculosis can miss what this means. In another passage K. says, 'I was seized in bed before I could get up'; elsewhere he speaks of a room 'which was polluted not by any fault of mine'. These are details which point strongly to only one interpretation; and they could easily be multiplied.

One might, then, be tempted to say that *The Trial* is a symbolic, veiled representation of a case of consumption. But to regard it in this way alone would be somewhat superficial. This interpretation, it is true, throws light on many things in the book; but other things remain completely obscure. For instance, this account does nothing to explain the idea of the Court, or why it presents itself in such a disgusting and ridiculous manner, or why, though apparently well known to everyone, it is unseen, or even what the real charge is. I do not wish to say now that the consumption-interpretation is wrong; but it is too narrow, it is incomplete. It is a first interpretation, and behind it other, deeper meanings are probably concealed.

I consider *The Trial* a document of literary self-punishment, in other words, an imaginative act of atonement. Max Brod, Kafka's biographer, tells us that Kafka when he started *The Trial* was in a terrible crisis, deeply divided against·himself. This is borne out by a passage from his Diary where he says, 'Should I die in the near future—in the last two nights I had an expectoration of blood—I may say that I have *rent* myself. The world and my Ego tear my body to pieces.'

This seems to explain another detail. The author's full name was *Franz Kafka*. Now the warder who arrests 'K' is called Franz; and names in Kafka are never accidental, but, like *Huld,* are always symbolic condensations. Thus the beginning·of the story seems to mean that Kafka arrests *himself*; in other words, he drives himself into the disease, and does so because he wants to punish himself. And in Kafka's private diary we find:

I always have had a certain suspicion against myself. But I had it only here and there, temporarily, with long breaks between, sufficient to forget … There is some suspicion there, which may manifest itself only on unimportant chance occasions, but which is yet alive. I feel how it moves; and I know moreover that I shall not survive its real birth.

These were prophetic words. We are not told what the suspicion is, but Kafka's suspicion against himself is of much the same kind·as K's guilt in *The Trial*. Here, too, there is not the slightest indication of what the charge amounts to. On the contrary, the story opens with the sentence that Joseph K., without having done anything wrong, was arrested. And yet some dark guilt really seems to hang over him, perhaps not in virtue of what he *does*, but in virtue of what he *is*. In a fragment written in 1914 (one year before·*The Trial*) a stranger comes to a village. Everywhere he goes, he arouses distrust and hostility (just as in *The Castle*); and this is what Kafka makes him say: 'I felt *some* kind of justification for *some* kind of fault found with me, not because I had spoken too much, but for some different reasons which touch my existence very closely.' It seems to be a sort of guilt which cannot be pinned down in words. The man in the fragment seems to suffer from a feeling of guilt, although he is not conscious of any concrete guilt, of any guilty thing he has done. Maybe, like this man, Kafka himself had such a feeling of guilt, and 'instituted proceedings' against himself. I do not say it *is* so, but it may have been so.

Anyhow, it opens up a possible new and deeper interpretaation. In this light, we shall no longer be tempted to regard the Court as a·power which acts from outside on K., and never releases him from its clutches, a sort of barbaric and gruesome institution whose bailiffs go, hunting for crime among the population. We shall rather assume that the 'Court' is *within* K., that the story, in actual fact, is about some part of his inner life to which, as artist, he has given an externalized form. There is some part of himself—conscience, the Super-ego, or whatever one may choose to call it—from which the charge proceeds. This idea makes some passages in the novel take on a new light. 'You know the Court much better than I do,' says K. to the painter. 'I don't know much more about it than what I have heard from all sorts of people. But they all agree on one thing, that charges are never made frivolously, and that the Court, once it has brought a charge against some one, is firmly convinced of the guilt of the accused and can be dislodged from that conviction only with the greatest difficulty.' 'The greatest difficulty?' the painter replies, '*Never in any case* can the Court be dislodged from that conviction.' If the interpretation I am using for the moment is correct, this means: When a man feels guilty, there is a voice in him that cannot be persuaded by any possible argument that he is innocent. The dialogue in the opening chapter, 'The Arrest' , between K. and the warder Franz (the two, remember, are identical), now takes on an almost eerie aspect:

'We are humble subordinates and have nothing to do with your case. But we are quite capable of grasping the fact that the high authorities we serve, before they would order such an arrest as this, must be quite well informed about the reasons for the arrest and the person of the prisoner. *There can be no mistake about that*. Our officials are *drawn towards the guilty* and must then send out us warders. That is the Law.'
'I don't know this Law,' said K.
'All the worse for you,' replied the warder.
'And it probably exists nowhere but in your own head,' said K.

These words, meant ironically, have a weird significance, for they are literally true. The Law does exist nowhere but in the warder's head, because this is K.'s head. The Law is inside him; and the authority in him knows that he is guilty, there can be no mistake about that. The charge is never formulated. The verdict of the Court is never promulgated, but it is executed in life, disease and death. And in this novel Kafka anticipated, as if in a state of clairvoyance, what fate had·in store for him.

This second interpretation in no wise comes in conflict with, or rules out, the first. But it supplies something more comprehensive into which the first interpretation fits smoothly.

So we arrive at something like a multiple interpretation. The 'inquiry' into the manager's 'case' alludes at the same time·to·a medical and to a legal inquiry, the latter by the inner Court of conscience; and the same applies to the word *Prozess*. The definite acquittal has both a medical and a legal (or, if you·like, an·.ethical) sense. Definite acquittal from the voice of conscience never comes, any more than complete release.from illness. The ostensible acquittal stands in this context for a temporary escape, when conscience is for a time·lulled and assuaged. (The passage from the Diary seemed to confirm this) And postponement obviously also means a postponing of the decision, a Hamlet-like hesitation to act. We begin to understand the peculiar effect this novel produces: we seem at times to glimpse behind this or that word another sense, deeper and half hidden, and to hear faintly the entry of a new meaning, in and with which others begin to sound, so that all accompany the original meaning of the word like the sympathetic chimes of a bell. Hence that deep and sonorous ring in Kafka's words; hence also the multiplicity of meaning, the indefiniteness, the strange suggestiveness and evasiveness of his writing. Thus behind the literal· meaning of the word 'inquiry' is ranged another meaning which we guess at half-blindly, but which patients suffering from Kafka's disease do not fail to catch; and behind this second meaning is apparently a third one, which is metaphorical and derived from the first. But we never lose sight of the proper and original sense; they are all present and seem to interpenetrate; and often, as we contemplate a word or a context, we hesitate in our perceptive awareness between the particular reality which it signifies directly, and another mysterious, half hidden reality which it symbolizes.

We are now in a position to understand another feature of *The Trial* which is puzzling and baffling—the queer *appearance* of the Court. The Advocate tells

K. that he 'must have discovered from experience that the very lowest grade of the Court organization was by no means perfect, and contained venal and corrupt elements, which to some extent made a breach in the watertight system of justice. This was where most of the petty Advocates tried to push their way in, by bribing and listening to gossip. In fact there had actually been cases of purloining documents.' Why is that so? Why are the lower ranks of the Court, where they make contact with the public, contaminated and defiled? The answer is quite plain: the waves of the inner voice are deflected when they come in contact with our vanity and our likes or dislikes; or, to change the metaphor, the ray from the inner world darkens and changes when it enters our conscious being. In the language of Kafka, the Judges (those of the lower grade, of course) are immensely vain and incorrigible philanderers. Or in other words, that part of our unconscious Self which breaks through to conscioussness is venal and corrupt; and so the plane separating the two takes on a sort of gappiness or loose texture. Hence that part of the Court which is accessible to us (or to K.) is thoroughly disgusting.

My last remark touched upon the topic of the *unconscious*, but I want to emphasize that it is not curiosity about psychology which actuates Kafka. Indeed, he shows an active dislike for pyschology. There is a passionate outburst in his Diary for December. 9th, 1913:

> Hatred of active introspection. Explanation of one's soul, such as: Yesterday I was so, and for this reason; today I am so, and for this reason. It is not true, not for this reason and not for that reason, and therefore also not so and so.

Kafka's problem is, to understand how the part in our Self that acts as Judge, or Court, interferes and is interlocked with life and consciousness. He is neither a representative of 'deep psychology' in the sense of Freud, Jung and others, nor of the other psychology which, for the sake of contrast, I shall call 'shallow psychology'. He is not interested in the strata of the unconscious; true, he pictures part of it as the organization of the Court. He drops only a hint by saying that its ranks mount endlessly, but he makes no attempt to penetrate to the higher ranks, i.e. the deeper layers of our Self. The picture which seems to underlie his vision is that of a conscious Self which is, surrounded by regions of the *half*- and the *un*-conscious which become more and more dark, and recede, like the ranks of the Court, to infinity.

In *The Castle* the story is told of the land surveyor K. (again Kafka), a man who makes all imaginable efforts to intrude into the Castle (the realm of the inner world), but is frustrated every time, and comes up against a sort of invisible barrier which brings all, his endeavours to nothing: the Castle recedes before; him, it is for ever inaccessible. As we read the novel, we are filled with a sense that it is impossible ever to bridge the gulf between village and Castle, between the conscious and the depths of the unconscious. And the officials of the Castle, like those of the Court, are defiled: they behave, or seem to behave, in a scandalous, even an obscene manner. On the other hand, as one reads on, everything becomes so uncertain that in the end one cannot tell whether Frieda's lover, a high official called Klamm, is really her lover, or even whether the man who may perhaps be her lover is really Klamm at all—for the officials change in appearance when they come down from the Castle to the village, which makes it very hard (if not impossible) to recognize them and not to mix up one official with others. Something that stirs us, coming from that inner world, turns into a different thing when it translates itself into consciousness: it no longer is what it was before. (This is the old gnostic theme of the messenger who becomes transformed on his way as he passes from one realm of being to another.) I shall make no attempt at all to interpret *The Castle*, for there are many other sides to it into which I cannot go here; but I mention it simply to illustrate Kafka's attitude to psychology.

In reading Kafka, in fact, one cannot but be struck by one very remarkable thing: the degree to which motives are conspicuous by their absence. He may not exclude motives altogether, but in the main his characters act not according to a premeditated plan, or for a definite reason, but are just carried away by some dark impulse, rush precipitately on, and plunge themselves headlong into adventures. (For example, K. and Fräulein Bürstner, or K. and Leni; and, in *The Castle*, K. and Frieda.) Are motives only a sort of rationalization? However that may be, it means a radical shifting of the centre of gravity. The great writers of the nineteenth century tried to explain the actions and behaviour of their characters by laying bare their inmost life and illuminating their motives. Their writings, of course, must be seen against the background of a whole world picture, against a tacit assumption that, at least in principle, the inner world is accessible to us. That, however, is precisely the point at which Kafka and the authors of the nineteenth century part company. His view of mankind made a complete account of what goes on in the Inner World unthinkable. Conscious motives may exist, but even so they are unimportant; what decides the issues of life is something quite different and far·stronger—it is everything which is symbolized by the Court or the Castle.These forces elude our efforts to describe them; all we can hope for, is, here and there, to catch a glimpse of them. This is one of the reasons why I am inclined to think that the psychological novel, as we know it, say, from the time of Stendhal on, comes to an end with Kafka, and something new looms there—heralding, perhaps, a new era.

But, important as it is, the question of motives is tied up in Kafka with another still more important problem. A story which permitted one interpretation, and one only, would be what is called an allegory, or a figurative story. There are many things, like *Don Quixote* or the Fables of La Fontaine, for example, which can be understood in this sense; and Kafka wrote this way too. The 'eleven sons' he speaks of, for example, are simply eleven stories which he was working on at the time. The *Metamorphosis* is the same: what Kafka tried to convey in this short story is the feeling one has at times in one's own family of being distant from others—shut off from them·as much as if one belonged to an altogether different species of being, like the insects—and of having to hide one's being different as carefully as one can, just as the insect hides itself under the sofa.

But it seems rather as if, with Kafka's great novels, *The Trial* and *The Castle*, such a translation, such a clear-cut interpretation is no longer possible. The different layers of interpretation·are finally lost in indefiniteness. What is the Supreme Court in that judiciary system? Who are the people in charge of the Castle? There is no single, definite, down-to-earth answer possible. The edges of Kafka's world are lost in darkness; or, to put it differently, we come up here against the ineffable. Had Kafka been able to say straightforwardly that he wanted to say, he would have had no need to write *The Trial*. As it is, he had no other means of expressing himself than in this dark symbolic way only.

Kafka is trying to express what, in the proper sense of the word is inexpressible; and he seems to do this by speaking of everyday things in a peculiar way, by accumulating absurdities, and at the same time describing them in minute detail, until he compels us to seek out another meaning beyond the literal one. But strangely there is no clear-cut meaning—the deeper, layers of the interpretation vanish into the incomprehensible. The absurd, the ridiculous in his work is a *leitmotiv* to point beyond what can be said. By using language in a very particular way, by constructing seeming absurdities, he tries, perhaps not so much to express, as to convey, to make us divine, what it is impossible to say straightforwardly. Perhaps, after all, that is the most interesting thing about Kafka: his attempt to say something for which we have no proper language. This, I take it, is one of the fundamental differences between him and the great masters of the nineteenth century. Whereas they apply psychological categories such as motives, what distinguishes Kafka is the fact that he no longer uses them, that he can no longer make use of them. In this sense Kafka *discontinues* the tradition of the psychological novel. If, as it has been said, German literature consists of a few great exceptions, Kafka is certainly one of them.

I should perhaps add a last point. The world of Kafka is enigmatic, unaccountable, unfathomable—just the diametrically opposite pole to the clear world envisaged by the rationalist thinkers. And yet we see that the world of a rationalist like Spinoza and the world of Kafka have both of them the character of being strange, eerie. Spinoza was a man who was weighed down by the mere fact of there being a world. This hangs over him like a dark, inscrutable fate into which Reason cannot penetrate. Now the whole aim of his philosophy is to transform this dark, inscrutable fate into something rational, so as to rid himself of the pressure which is weighing him down. So he sets himself to prove that the existence of the world is a logical necessity; and so on. But the characteristic of being eerie had transferred itself to Spinoza's world which is just as lifeless, deprived of freedom, constrained and unnatural as marionettes, which are in fact another symbolic expression of that eeriness. (It's not just an accident that the rationalistic period had such a predilection for automata, like marionettes, etc., it was its particular form of expressing the feeling of weirdness.)

The eerie often seems to arise on the edge of the field of vision where there is a clearly organized world picture. In the Middle Ages everything outside the field of vision of the orthodox, such as the infidels, the Moslems, or the Jews appeared uncanny. Perhaps the Western powers now appear uncanny to the Bolsheviks?

On the other hand, it must be admitted that Kafka has nothing like a strictly organized picture of the world. The weirdness of the atmosphere which strikes us so much in his novels clearly derives from the absence of firm psychological categories. One of the forms it takes is the demonic, or grotesque, such as that exemplified by K.'s two assistants in *The Castle*, or the two gentlemen in black who, grimacing, execute the man in *The Trial*. I am not sure why it is that these demonic people—who, in a way, stand, so to speak, on the edge of human nature—are so shockingly clownish? Nor is it clear why they always appear in twos, e.g., the bank clerks, or in threes, but never alone. (One thinks of Rosenkrantz and Guildenstern.) There seems to be some rule which demands it, but I am far from understanding what it is. In *Blumfeld*, a short novel by Kafka, the role is even taken over by two balls which behave in the same silly, ridiculous and annoying way, as if this element was not confined to human beings.

Alexander Bird

# Waismann Versus Ewing on Causality

## 1. Introduction

Friedrich Waismann's typescript "Causality" (2010) dates from the late 1940s or early 1950s, and derives from lectures he gave at Oxford in 1947–8, where he was then university lecturer (and later reader in the philosophy of mathematics).[1] The typescript is divided into twelve sections, and Waismann devotes much of one section to an engagement with A. C. Ewing's paper "A Defence of Causality" (1933).

In this paper I look at Ewing's views and Waismann's criticisms of them, relating their exchange to recent developments of their differing viewpoints. As we shall see, Waismann's approach is typical of his logical positivist heritage, with its emphasis on what is observable and demanding a strong connection between the meaning of an assertion and its empirical verifiability. Ewing's views, which must have begun to look a little outdated to some in the 1930s, have recently had a significant revival, although the current discussions owe little directly to Ewing.

Ewing's principal aim is to argue against the regularity theory of causation, and at the centre of his argument is that claim that the regularity theory does not accommodate certain features central to the ordinary concept of causation. These are (Ewing1933, 98; my headings):

(1) *Intrinsic connection* "The effect is held to be continuous with, dependent on something in the cause so that the two do not merely happen in regular succession, but are intrinsically connected with each other."

(2) *Explanation* "The cause is held to explain the effect, to answer not only the question—how?—but the question—why? so that the demand for causes is primarily a demand for reasons, which implies that there is a logical or quasi-logical connexion between the two such that the cause is at least part of the reason for the effect and helps to make the occurrence of the latter intelligible."

(3) *Production* "The cause is held actively to produce or determine the effect in a sense in which the effect cannot be said to produce or determine the cause."

(4) *Necessity* "Causality involves necessity. If there is a causal law connecting A and B, it is not only the case that B does follow, but that it must follow."

1 For a detailed commentary on Waismann's typescript, see Marion (2010).

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In the following, I shall discuss each of these points in turn, even though they raise closely related issues, together with Waismann's criticisms. To contrast with Waismann's regularity view, I call Ewing's view the *production* account. To conclude I shall show the contemporary *causal powers* view satisfies the most important claims expressed in the production account of the ordinary concept of cause.

## 2. Intrinsic connectedness

Ewing's first-mentioned feature of the ordinary concept of cause asserts an 'intrinsic connection' between cause and effect. This claim is the one to which Waismann devotes least attention in his discussion. This may in part be because while Ewing repeats the point several times, he is unable to articulate what exactly he means by 'intrinsic'. Indeed, he admits that he cannot find a meaning for it other than 'logically implies', although he does acknowledge that others have a conception of an intrinsic relation that is distinct from any logical relation. If intrinsicness is a matter of a logical relation, then point (1) just becomes point (2).

There are two things that Ewing might mean by 'intrinsic':

(a) property F is intrinsic to object **a** iff **a**'s being F is independent of the existence of any object fully distinct from **a**;
(b) property F is intrinsic to object **a** iff F is essential to **a**.

Contemporary philosophy almost always limits use of 'intrinsic' to (a), although there are exceptions to this.[2] The idea in (b) is sometime expressed as the idea that being F is part of **a**'s nature, although that could well be understood as encompassing a wider extension than the essential properties of **a**. I shall examine that interpretation of Ewing's claim in Section 6.

That Ewing may have (a) in mind is suggested by the fact that he repeatedly contrasts the following as accounts of what it is for individual event A to cause individual event B:

(I) A-type events are always followed by B-type events;
(II) event A is intrinsically connected to event B.

The principal difference between these is the following. According to (II) the causal relationship between A and B depends only on those two events. Whereas according to (I) whether A and B are causally related depends on entities other than those events. For whether this event A causes that event B depends not only on whether A is followed by B but also on whether another A-type event, A′, is followed by another B-type event, B′, and so on for all other A-type events. If causality is an intrinsic relation between two events the fact that it holds between A and B depends only on how A and B are and not on any other event or entity. But

2 See Humberstone (1996), Yablo (1999), Langton and Lewis (1998), and Weatherson (2006) for discussions of this conception of intrinsicness.

the regularity theory of causation does make it so depend. The idea of intrinsicness here is that idea that F is an intrinsic property of A if A's being F does not depend on the existence of any object other than A, and a relation R between two objects A and B is intrinsic to that pair of objects if it does not require the existence of any third object.

In this sense an intrinsic relation between A and B does not entail any logical relation between A and B. This may be too weak an understanding of intrinsicness for Ewing's purposes. For in this sense the fact that London and Vienna are 1,233 km apart is an intrinsic relation, for that relationship can hold independently of what else exists. But the distance of two entities can vary too easily for that relation to model what Ewing has in mind by intrinsicness. Ewing (1933, 99) mentions that "there might be different views on the point, e.g., Prof. [Samuel] Alexander, when he speaks of the cause as 'passing into' the effect, might possibly be interpreted as asserting intrinsic connexion, and yet he emphatically denies that causality involves any sort of logical connexion." This suggests that the intrinsic connection between A and B depends only on A, that is, the relation is not intrinsic to A and B together but intrinsic to A alone.

There is a problem with such an idea. If the relation is intrinsic to A then it ought not depend on the existence of any other object. But clearly the causal relation between A and B depends on B's existence. If B is distinct from A then A should be able to exist without B existing. So the causal relation between A and B cannot be intrinsic to A if A and B are distinct events.

So the claim of intrinsicness can be maintained if we deny that A and B are really distinct—B is a part of A. If B is part of A then the relationship between them can be intrinsic to A. The action of my clenching my fist is a relationship between me and my fist and is intrinsic to me, because my fist is part of me.

This response leads to monism. For if every effect is not distinct from its cause but is a part of it, then there is really only one big event.[3] Although Waismann does not consider monism, he does repeatedly insist on the distinctness of cause and effect. Ewing does refer to an "exaggerated monism" that was a feature of the earlier view against which Russell and others were reacting. In his view that reaction has produced an opposite (and, Ewing seems to imply, greater) evil to the one it rejected.

A different response notes that the idea of intrinsicness runs into difficulty when dealing with entities that are necessarily related. Let us say that the existence of object A necessarily implies the existence of (non-overlapping) object B. Then *none* of A's properties will be intrinsic to it, because for any such property F, A's being F will require (necessarily imply) the existence of B. But that seems not a good reason for denying that A can have intrinsic properties. One might think that the singleton set of Socrates has some intrinsic properties, such as having Socrates

3 There could be more than one big event, if there are parts of the universe that are completely causally isolated from other parts, with no common causes or effects. Current physics suggests that this is not in fact the case. This monism suggests that all current events are merely parts of the Big Bang.

as a member, even though singleton Socrates entails the existence of Socrates.[4] So one might adjust the notion of intrinsicness to apply to a property F of X such that: there is no object Y such that X's being F requires the existence of Y and X's existence does not require the existence of Y. So A's being a cause of B might be thought to be an intrinsic property of A, so long as A necessitates the existence of B.

This reference to necessitation relates feature (1), intrinsic connection, to feature (4), necessity. We will consider (4) in due course. We obtain a relation to (2), Ewing's understanding of explanation, if all necessity is logical necessity. That would be a point of view that he would share with Waismann. But note that he refers to a 'quasi-logical' connexion. What that might be is not clear, although the part-whole relation might be regarded as quasi-logical. But note also that he does not reject Alexander's idea out of hand—if that is a form of necessitating, then it seems that Ewing does not immediately reject the idea of non-logical necessary connections.

For this solution to be different from monism requires that distinct existences can be necessarily connected. This is to deny another Humean dictum to which Waismann would be committed. It is true that the related idea that elementary propositions are logically independent is one of the first elements of the *Tractatus* that Wittgenstein was willing to drop. But nonetheless Wittgenstein and Waismann retained the thought that all necessity is logical necessity, where 'logical necessity' includes the rules of grammar.

## 3. Explanation

Ewing bundles several features together under the label of explanation:

- the cause explains the effect;
- causes answer 'why?' questions as well as 'how?' questions;
- the demand for causes is a demand for reasons—a cause is a reason for its effect occurring;
- causes make the occurrence of the effect intelligible;
- there is a logical or quasi-logical connection between cause and effect.

Ewing doesn't make clear what all these requirements amount to, nor how they connect together. However, a connection can be made if we think of causation as akin to agency. For when seeking the explanation of an action, we are typically looking for someone's reasons for acting, to cite a reason is to explain *why* they did something, and such an explanation should make the action intelligible in a certain special way, a case of understanding—*Verstehen*—that comes with the hermeneutic or interpretative approach to explanation. Furthermore, it is often thought that

4 This claim of necessary connection between distinct entities assumes that to be a member of a set is not to be part of it.

there is a something akin to a logical relation between a reason and the action it explains. A reason in this sense must be appreciated as rationalizing the action for any rational being.

The model of rational action for causation is one that is at the heart of what Ewing is saying, but without him saying so explicitly, and it is correspondingly this feature that figures centrally in Waismann's rejection of the view. While Waismann would agree that the above requirements form part of a satisfactory account of reasons, he strongly rejects any connection between reasons and causes. Waismann makes a thee-way distinction between motive, reason, and cause. Waismann agrees that citing a reason (or ground) provides a logical connection between reason and action: for example, someone might explain their utterance of ‘red’ when looking at a red light by declaring that the colour of the light is that which is called ‘red’, and in so doing refers to the ostensive definition of the term he uttered. This Waismann contrasts with a *causal* explanation of the same utterance: that in childhood he went through a process of conditioning that associates the sound “red” with the colour through neural pathways, which thus established a habit that disposes towards the utterance “red” when in the presence of a salient red object. In this case there is no logical connection between the cause and the effect. This distinction and the distinction between these two (cause, reason) and motive form a major part of the rest of Waismann's typescript. For example, the final section rejects Russell's causal account of desire. Whereas Russell wants to assimilate the explanation of action to causal explanation, Ewing, in effect wants to do the reverse, to regard the explanation of action by reasons as a model for causal explanation. Waismann wants to reject both views by keeping causes and reasons firmly apart.

The view that Russell promotes, that desires are causes of actions, is now the dominant view in the philosophy of mind, especially after Davidson's ‘Actions, Reasons and Causes’ (1963) in which he argues that reason-giving (and motive-citing) explanations are causal explanations, thus bundling together the three kinds of explanation Waismann is keen to differentiate. The Russell–Davidson view is resisted by Wittgensteinians, among others, for much the same reason as Waismann, that there is a logical (or ‘internal’) connection between reasons and actions whereas the relation between cause and effect is hypothetical.[5] Davidson argues that this distinction depends on how the events are described. Causal theories of the mind (Smart 1959; Lewis 1966; Armstrong 1968) explicitly identify reason-giving attributes and causal states. Ewing, however, seems to be arguing in the opposite direction: not so much thinking of reasons as like non-mental causes, but rather thinking of causes as like mental reasons. Although this would therefore seem to go quite against the flow of contemporary thinking about causes and their relation to reasons, in Section 6 we shall see that this is not entirely the case.

5 See Wittgenstein's *Blue and Brown Books* (1958) for his articulation of the distinction between reasons and causes; Schroeder (2001) and Tanney (1995, 2009) among others provide a defence of the Wittgensteinian point of view.

Ewing's demand for a cause to explain its effect is also a demand, as he articulates it, for there to be a logical or quasi-logical connection between that cause and the effect. The relationship that Ewing sees between explanation and a logical connection would be established by adopting something like the deductive nomological model of explanation. According to the latter, the relationship between explanans and explanandum is one of deduction: the explanans provides a combination of laws and conditions from which the explanandum is deducible. Consider a case where the volume of gas in a syringe is halved, under isothermal conditions, and the pressure consequently doubles. The following is the explanation of the increased pressure:

$$\frac{PV = nRT\,\text{(law)};\ V\textit{ is reduced by 50\%}\ \text{(condition)};\ n, T\textit{ are constant}\ \text{(condition)}}{P\textit{ is increased by 100\%}}$$

Although the D-N model provides a link between explanation and a logical relation, it does place a strong requirement on what it is to be a cause. For a plausible, first-pass account of causation would claim that to be a cause is to be a (non-redundant) condition in a D-N explanation, not the whole explanans. For example, in the above D-N explanation, we might say that the cause of the increase in pressure was the reduction in volume, i.e. just the fact that $V$ was reduced by 50%. One might argue that the constant temperature and quantity of gas are causally relevant and so part of some total cause (a point that Mill makes). But it would be unusual to regard the *law* also as a cause.

It is an interesting feature of the debate between Ewing and Waismann that neither is particularly concerned to make a sharp distinction between cause and law. For one might think that a regularity account of causation was always going to be a non-starter. Consider the claim that the Treaty of Versailles caused the rise of the far right in Germany; or that the damp odour in the study in February was caused by the blocking of the gutter on the roof in November. The plausibility of such claims is not undermined by there being no regularities of which these are instances, nor by the fact that there is no logical connection between the causes and effects. Furthermore, in both cases there is no contiguity, spatial or temporal, between cause and effect. But neither Waismann nor Ewing regard the Humean claims of contiguity to be refuted for that reason. Waismann does regard the contiguity claim as problematic, but for more sophisticated reasons concerning the divisibility of space, as he discusses in his section on "The Scientific Scheme of Causality". He quotes Georg Simmel as proposing to loosen the connection between cause and law, exemplifying a position he rejects. When Ewing says that he is concerned with the common sense idea of causality, he does not mean that he is interested in providing a precise, extensionally correct analysis of our ordinary concept of cause. Rather, he is interested in capturing the common sense idea of what metaphysical characteristics causation involves. And to a large extent the same is true of Waismann. Neither man was interested in providing necessary and sufficient conditions for the truth of statements of the form 'A caused B'. Rather both men were interested in what sorts of metaphysical features the world must have in order to make such statements true. Of course, Waismann would not have liked the term ‘metaphysical’, but that is a terminological point. He has a minimalist metaphysics, seeking to make do with just what Lewis would later call the ‘Humean mosaic’. From this perspective the distinction between laws and causes is not especially germane, for the underlying metaphysics, one might suppose, will be the same for both.

One mark of positivism is that it rejects the demand for explanation. Comte (1892) regarded this as a feature of the second, metaphysical stage of human intellectual development, which would be eliminated in its third, positivistic stage. In the sense that Comte rejected explanation, it signifies a relationship of a certain kind. Alternatively, one can attempt a reduction of explanation. And indeed Hempel’s D-N model is part of such a reduction when one understands the notion of law that it employs in terms of regularity. Taking laws and causes to be regularities allows them to be understood in a positivistically acceptable way. Likewise, that fact together with the D-N model allows explanation to be understood in a positivistically acceptable way. Although the D-N model meets Ewing’s demands for a logical or quasi-logical connection between explanans and explanandum, it is clear that he requires more from explanation than it plus the regularity theory can supply.

### *Waismann’s conjecture concerning animism*

Before considering Ewing’s claims concerning the productive nature of causation, I shall consider Waismann’s explanation of how it is that the theory he criticizes came to have any attraction for philosophers. His account, justified by what Ewing says about the explanatory aspect of causation, is that we take our voluntary acting as a prototype for all causation. In effect, we naturally take an animist approach to causation, seeing agency in all causal relations between things. Waismann adduces two pieces of evidence for this: the fact that children impute agency and moral culpability to inanimate objects, and the fact that primitive people have animist beliefs. Waismann says that this is reflected in the fact that we use active verbs with inanimate objects, whereas, he supposes (2010), the grammatical distinction between the active and passive voices of verbs was “intended to distinguish what is really active from what is merely passive.”

Whatever the plausibiility (or otherwise) of this hypothesis, there is an internal problem for Waismann with this proposal. For, he says, the case of first person agency does *not* reveal itself as a special case of causation, one where we can see production/necessitation at work. Waismann quotes the famous passage from the *Treatise* where Hume denies that we are able to perceive any connection between ‘an act of volition and a motion of the body’, citing the fact an amputee will find himself trying to use his lost limb. Waismann also quotes passages from Mill and Hamilton to the same effect. But if Waismann, Hume, Mill, and Hamilton are right, then we cannot suppose that we impute productive power to causation in general by extrapolating from the particular case of voluntary action, because even the latter is not a case where we detect productive power.

One might save Waismann's hypothesis by claiming that while we do not perceive any productive power in the case of first person voluntary action, this is nonetheless the case where we first make the error of finding productive (or 'active') power, and then we extrapolate to the other cases. But then Waismann is left with still needing to explain why we make the error in the first person case. Perhaps Hume's account in terms of habit may be appealed to here, but we would then ask why that account cannot be applied directly to the case of inanimate objects as explaining the source of our fallaciously seeing active power in objects.

## 4. Production

In introducing the idea of *production*, Ewing points to an asymmetry between cause and effect. Causes produce their effect, but effects do not produce their causes. One might respond by noting that cause and effect are logically asymmetric on the regularity account: if A causes B, then A is always (or usually) followed by B, but the converse does not hold. And this asymmetry might be responsible for the asymmetry found in intuitions concerning production.

Oddly, that asymmetry is not one that Ewing dwells on. For he quotes Russell as declaring that to say C causes E "means simply that C is a set of conditions such that whenever they are all fulfilled E happens and whenever E happens they have all been fulfilled." And goes on himself to say "An alternative formulation that I should prefer would be that E can be analyzed into a set of factors each of which is always, wherever it occurs, preceded by some factor in C." Waismann, on the other hand, is clear that the *logical* regularity in causation is always from cause to effect, not vice versa.

Ewing's point has some force, even if we acknowledge the logical asymmetry. For one could introduce a concept of causality*, such that A causes* B is like A causes B, except that the (universal) logical implication is reversed: instead of 'whenever A, B also' ($\forall x[\mathrm{A}x \rightarrow \mathrm{B}x]$) we have 'whenever B, A also' ($\forall x[\mathrm{B}x \rightarrow \mathrm{A}x]$). So causality* holds between A and B when A is followed by B and B-like events are always *preceded* by A-like events. If the asymmetry of production were the asymmetry of material implication then we would want to say that just as in causality A produces B, in causality* B produces A. But that is clearly not correct, which suggests that the production idea is not related to the asymmetry of material implication.

The other source of asymmetry in the regularity account is the temporal asymmetry, that causes always precede their effects. Could this be the source of whatever intuitive force there is in the production idea? It is difficult to show that it is not, except to observe that temporal precedence on its own doesn't imply anything about production. It does not seem that we say: A produced B because B followed A; rather, B followed A because A produced B. Insofar as there is a temporal asymmetry in causality, that asymmetry is not part of a definition of causality, but rather follows, if at all, from the nature of causality.

Waismann rejects the production idea. Insofar as there is anything to it, it can be regarded as a near synonym for 'cause'. One might distinguish the notion by relating it to the idea of 'activity'. But the latter term cannot be understood in the sense that implies volition. Furthermore, empirical investigation does not reveal to us anything like activity or production, just regular succession, as Waismann (2010) holds is shown by the following thought experiment:

> Suppose there was a region of the world, say A, in which everything held good that Ewing and other philosophers of the same school tell us—that is, in which the events were "intrinsically" connected with each other, so that the cause "actively produced" the effect; imagine another region of the world B in which the events merely follow each other, without being connected in this way; and imagine that the observable laws are the same in A as in B. What then, I ask, could be the difference between these two regions of the world, as far as their causal structure is concerned? Or how can we tell whether this world of ours is more like the part A or the part B? There is no way in which we can tell; for there is no conceivable observation which is relevant to establishing the existence of such a relation. The two worlds which we have described would appear exactly alike in all respects to any observer: what, then, is the dispute about?

Here Waismann appeals to the verification principle; as he says here "A statement has meaning for us only if it makes some kind of difference to us whether it is true or false". (In *The Principles of Linguistic Philosophy* (1965, 326) he holds that a verification principle can be used to determine meaninglessness. Ewing rejects this in his paper "Meaninglessness" (1937), where he introduces the now well-known criticism that the verification principle, in a strong formulation, shows itself to be meaningless, or, in a weak formulation, cannot be used to reject metaphysics, as the positivists intend.)[6] The proposal that "A produces B" has significance that goes beyond "A is regularly followed by B" implies, according to verificationism, that there is some investigation or experience that would distinguish between a case of the former and a case of the latter and so would provide evidence for one or the other. But in this case there is no perceptible difference, and so there cannot be any difference in meaning.

## 5. Necessity

Ewing suggests that while the regularity account takes A causes B to be a matter of A simply being followed by B (and likewise for other A-type events), our common sense idea of causation takes it to be the case that A *must* be followed by B. The force, however, of this *must* is unclear. It is logical or metaphysical necessity? Or is there some other kind of relation between two events that has some kind of modal character that is short of metaphysical necessity?

6 Waismann articulates his version of the principle in further detail in his (1945) although rejects a natural formulation—'There is no more to the content of a statement than the total evidence which would warrant its assertion'—suggested and criticized by D. M. Mckinnon (1945).

The idea of necessity, in some form or other, crops up in the preceding three alleged elements of the common sense notion of cause. We considered that (1), intrinsicness, might refer to the fact that the effect is part of the essence of the cause, to which we will return in Section 6. We looked at intrinsicness as referring to the ability of the relation to hold independently of what else exists. But this needs supplementing to do the work Ewing wants it to do, and the natural supplementation, I suggested, was that the existence of the cause entails the existence of the effect. The idea of a cause explaining its effect, as Ewing articulates it, involves a logical or quasi-logical connection. One way to achieve that would be to appeal to the D-N model of explanation (although Waismann would be able to argue that the D-N model makes explanation a logical relation even for the positivist). All these kinds of necessity are metaphysical or stronger.

The idea of production, however, does not of itself suggest metaphysical necessity. Indeed Waismann challenges it to show how it differs at all from regularity. The fact that we distinguish mere regularity from causal relations suggests that there is indeed a difference, but does not show whether that difference has a modal character.

It is worth noting that in the contemporary discussions of causation, the distinction is sometimes made between difference-making views of causation and production views of causation. Indeed, Ned Hall (2004) thinks that there are two concepts of causation, a difference-making concept and a production concept. Difference-making views, of which David Lewis's (1973) account is the best known, are those that take 'A causes B' to be founded on the the counterfactual relation:

$$\neg A \mathrel{\Box\!\!\rightarrow} \neg B$$

While the modality of difference-making is clear, the nature of production has not been articulated nearly as clearly. However, a natural alternative is to consider the subjunctive conditional:

$$A \mathrel{\Box\!\!\rightarrow} B$$

This relationship between A and B we may call *subjunctive sufficiency*. The virtues of the subjunctive account of causation have not been satisfactorily explored. One reason is that since causation is factive ('A causes B' entails 'A exists/occurs' and 'B exists/occurs'), the subjunctive account implies that every fact causes every other facts, under the standard Lewis–Stalnaker interpretation of '$\Box\!\!\rightarrow$'. However, that is not the only interpretation of '$\Box\!\!\rightarrow$', and one can understand the subjunctive relation in a way that drops Lewis's centering requirement:

$$A \wedge B \Rightarrow A \mathrel{\Box\!\!\rightarrow} B$$

Without the centering requirement we are free to understand '$\Box\!\!\rightarrow$' thus:

$A \mathrel{\Box\!\!\rightarrow} B$ iff in all the nearby worlds where A occurs, B occurs.

Once one does this, as Robert Nozick (1981) does in his account of knowledge, one gets an account of causation that is roughly this:

A causes B iff in all the nearby worlds where A occurs, B occurs.

Whatever deficiencies this view has, it might be held to provide the sense of 'must' that Ewing is looking for—while also accounting for the idea of production. It is weaker that the implausibly strong idea that causes entail (i.e. metaphysically necessitate) their effects, but rather stronger than simple regularity.

## 6. Causal powers

In this section I show how Ewing's view might be updated and rendered rather more plausible than he himself presented it, Ewing contrasts the regularity view of laws and causes with a predecessor which he characterizes thus:

> The older schools of philosophy almost invariably assumed that the connexion between cause and effect was identical with or closely allied to that between ground and consequent; and from this it would follow that, since everything in the world we know is, directly or indirectly, causally related to everything else, the world was in some marked degree a logically intelligible system and the nature of any one thing taken by itself was incomplete and internally incoherent apart from the system on which it depended. On that view different things by their very essence belong together, and their connexion is capable of a rational explanation deducible *a priori*, if not by our mind, at any rate by a mind which possessed real insight into their nature.

Although Ewing notes that the regularity account is a reaction against certain unjustifiable excesses of this view, such as its tendency to monism, it is clear that it is not far from his own preference. While the reference to 'ground and consequent' indicates Kantian views on causality, also notable in this account is the reference to things being connected via their essences and the claim that such connections are in principle knowable by someone who has insight into their natures. While such things were anathema for much of the twentieth century, it is significant that since the 1970s they have become a familiar part of the discourse, thanks to Saul Kripke (1980), Kit Fine (1994), and others. Turning to causation in particular, Mathieu Marion (2010, 14) rightly remarks "One should note that the view here is not completely unrelated with recent views about 'causal powers' or 'capacities'."[7] In this section I shall explore Marion's suggestion. According to such causal powers views, the source of of causality and nomicity is to be found in the essential natures of natural properties. Such natures are sometimes said to be dispositional or have the character of capacities. For example, it is part of the essence of the property *charge* that oppositely charged bodies are disposed to exert an attractive force on one another.

7 Marion refers to Harré and Madden1975, which is a minor irony, given that Harré was the editor of Waismann's best known work, *The Principles of Linguistic Philosophy*. Marion also cites Cartwright (1989), although it is unclear whether Cartwright supplies the modal features that Ewing requires. Developments of this view are found in Shoemaker (1980), Ellis and Lierse (1994), and Bird (2007).

The general form of a causal relation on this view will be as follows. An object **a** will possess some natural property P, which will have a dispositional nature, $D_{S,M}$—the disposition to yield manifestation M in response to stimulus S; **a** receives stimulus S, activating the disposition; consequently manifestation M occurs. While this may characterize the metaphysics underlying causation, it is a further question whether the *cause* of M should be regarded as **a**'s possessing $D_{S,M}$, or **a**'s receiving stimulus S, or both together. Stephen Mumford prefers the former, while I prefer the latter. Different cases may seem to go in either direction:

(a) the balloon's being positively charged caused it to stick to the wall

supports the former, whereas

(b) the striking of the (fragile) glass caused it to break

supports the latter. However, either way we are referring *only* to the objects involved and their properties or the events involving them.

The following features of the view are worth noting:

(i) It is a matter of essence, and so of necessity, that one property is related to another by causal law.
(ii) Consequently, the laws of nature are metaphysically necessary.
(iii) It is possible to identify the necessity that is associated with causation with this necessity.
(iv) However, that necessity can be reconciled with the idea that causal laws are sometimes *ceteris paribus*.

How well does this view meet Ewing's requirements? And how well does it resist Waismann's criticisms?

*Intrinsic connection* On the causal power view, it can be the case that causes and effects are intrinsically connected in that the relation need not depend on the existence of further entities. When two charged objects cause one another to accelerate, that can hold independently of what else exists. Of course, that cannot always be the case, for example, when there are causal intermediaries. But in contrast to Waismann's regularity account, what is significant is finding any case of causation that is intrinsic.

Our discussion moved on to a further proposal, that the cause necessitates the existence of the effect. This can be accommodated if one includes both the disposition and the stimulus as components of the cause. Let it be part of the essence of P and hence necessary that P involves the disposition $D_{S,M}$ to manifest M in response to stimulus S:

(I) $\Box\,(\mathrm{P}\mathbf{a} \leftrightarrow D_{S,M}\mathbf{a})$

Let us assume for argument's sake that the conditional analysis of dispositions is necessarily correct:

(II) $\Box\, D_{S,M}\mathbf{a} \leftrightarrow S \Box\!\!\rightarrow M)$

From the above follows:

(III) $\Box\, ((P\mathbf{a} \wedge S) \rightarrow M)$

Thus *necessarily*s if **a** has the causal property P and receive stimulus S it will bring about the effect M.

Earlier I noted that sometimes ‘intrinsic property’ is used to refer to an essential property. Sometimes the term is used to refer to what is part of something’s nature in contrast to what is an accidental change, as when, for example, it is asked whether people are intrinsically good or bad. Although a thing’s essence and its nature need not be identified, it is a common part of the new essentialism to do so. When he refers to an intrinsic connection, it might well be that Ewing has something like ‘essence’ in mind. In which case (i) immediately supplies that connection as regards the properties involved in the causal relation. That does not mean that it is part of the essence of some particular cause that it does in fact have its particular effect. However, if the cause is an event of fact of the kind (a), which involves the dispositional property itself, then it will be essential to that event/fact that it would have that effect, were certain circumstances also to occur (e.g. in the case of (a), that the balloon is placed in contact with the wall).

*Explanation* The dispositional explanations (a) and (b) given above are clearly adequate explanations. As we have just seen, the cause, considered as the disposition plus its stimulus necessitate the effect. Furthermore, if, in Ewing’s words, one has insight into the (dispositional) nature of the property P, then one will know the truth of (I), and one can deduce that the effect will occur, as he suggests. That does not make causal relations all apriori, because such insight is not generally available apriori. Waismann is quite right that there is no logical knowledge of causal powers. Waismann concludes that there are two things to which we might apply the term ‘insight’. The first is logical knowledge, and this does not tell us anything about powers. The second is a knowledge of causal chains and process, which may be gained by experiment and observation of correlation, and this is entirely consistent with the regularity theory. Nonetheless, there is arguably a third kind of insight, which is a *conditional* insight into essences. Kripke argues that we know apriori that if water is $H_2O$, then water is necessarily $H_2O$. To detach the necessity, we must first have purely empirical knowledge that water is $H_2O$.

Note, additionally, that although on this account the total cause necessitates the effect (as we saw above), it does so without having to include, implausibly, within the total cause, the law of nature as a distinct component. Of course, laws of nature are present implicitly, since they are reflections (consequences) of the dispositional natures of the natural properties. (Mumford (2004) argues that this view shows that laws of nature may be eliminated.)

Ewing’s view that the regularity account fails to allow for adequate explanation by laws and causes has something to be said for it. Consider a D-N explanation employing the law that As are Bs ($\forall x[Ax \rightarrow Bx]$), to explain why if some particular object **o** is A it is also B (A**o** → B**o**). The explanandum here is deducible from the law as the D-N model requires, and the law is a regularity, as the regularity theory requires. Now let us accept Wittgenstein's equation of universal generalization with conjunction. In which case the explanans, $\forall x[\mathrm{A}x \rightarrow \mathrm{B}x]$, is equivalent to $\ldots[\mathrm{A}\mathbf{m} \rightarrow \mathrm{B}\mathbf{m}] \wedge [\mathrm{A}\mathbf{n} \rightarrow \mathrm{B}\mathbf{n}] \wedge [\mathrm{A}\mathbf{o} \rightarrow \mathrm{B}\mathbf{o}] \wedge [\mathrm{A}\mathbf{p} \rightarrow \mathrm{B}\mathbf{p}]\ldots$. Note that the explanandum is one of these conjuncts. However, nothing may explain itself, so the conjunct [A**o** → B**o**] doesn't explain the explanandum. But if this conjunct doesn't explain the explanandum concerning **o**, how can any of the other conjuncts, which concern other objects, explain that explanandum? Nor, in this case, can we see how these conjuncts working together can explain what they cannot explain individually. So, the conjunction does not explain the explanandum, and so neither does the law with which it is equivalent—according to the regularity theory. Consequently, if the regularity theory is correct, a law cannot explain without violating the requirement that nothing can explain itself.[8]

My discussion of Ewing on explanation started by suggesting that he had in mind something like the assimilation of causation to agency. This aspect is absent from my characterization of the causal powers view. It is not, however, absent from all discussions of the causal powers view. Several philosophers regard dispositions as having something akin to the intentionality of desires and beliefs. This is because a causal power 'points to' its possible manifestation: the fragility of a glass indicates its possible breaking; the positive charge on a proton holds within it the possibility of attraction to a negatively charged object. These are held to be very close to the way in which a desire points to the things that is desired. Thus U. T. Place (1996) reframes Brentano's dictum that intentionality is the mark of the mental to state that intentionality is the mark of the dispositional. Brian Ellis (2002) thinks that 'physical intentionality' of causal powers helps reconcile the manifest and scientific images. David Armstrong (1997) also sees intentionality in powers, and takes this to be a reason to reject their existence. I, on the other hand, do not think that causal powers have anything like intentionality; nor would it be explanatorily helpful if they did (Bird 2007, 114–31). So Waismann was correct, in my view, to reject the agency aspect of Ewing's view of causation. But not because reasons and causes cannot be the same, but because powers, despite superficial appearances, do not have anything like intentionality.

*Production* If it is part of the essence of charge, as this view holds, that oppositely charged bodies are disposed to attract one another, then it is a small step to arguing that it is the charge on these bodies and their being brought into proximity that produced their attraction to one another. It was part of their pre-existing state that they were disposed so to do, and this was triggered by their being brought together. And clearly this is a conception of production that is not equivalent to regularity.

We saw that Waismann rejected the idea of production because the difference between regularity and production would lead to no observable difference and so

8 See Armstrong (1983) for an argument of this kind against the regularity theory of laws, and also Bird (2007).

cannot constitute a real difference at all. One response to this objection would be to reject the verificationism on which this objection rests. Another would be to reject just part of it, the claim that the evidence to distinguish the propositions must be observational in nature. This equation of evidence and observation (or experience or perception) is one of the components of logical empiricism that has lasted the longest. Nonetheless, it too is open to objection. If one sees a sequence of As followed by Bs, one might accept that this is just a coincidence. One might also think that that this arises because A produces B. These are competing, incompatible hypotheses. As the number of As increases, each time followed by a B, then so one shifts one's epistemic preference away from the coincidence hypothesis towards the production hypothesis. Thus a purely coincidental relationship between As and Bs and a productive relationship between As and Bs may predict exactly the same experiences, but they are nonetheless epistemically distinct. Indeed, distinguishing such hypotheses is a common feature of science, and for many kinds of data, can be formalised by significance tests. A significance test estimates how likely it is that a certain statistic (such as the mean value of a measured parameter) comes about purely by chance. If an outcome is significant at the $p$=0.05 level, then the evidence favours the causal hypothesis over the chance hypothesis by a factor of 19 to 1.

This point may be expanded in a way the draws upon Ewing's preceding point about explanation. Why is it that the productive, causal hypothesis is better supported by the evidence of regular succession than the coincidence hypothesis? The simple answer is that the former provides a better explanation of the evidence than the former. Of course, such explanationist epistemology would be rejected by Waismann and the positivists. Yet it is today very much at the heart of scientific epistemology, challenged only by Bayesian thinking.

*Necessity* We have seen that the total cause necessitates the effect. Let us now consider the view that only part thereof needs to be regarded as a cause, as in (b) above for example. There the striking is the cause, and the fragility of the glass, though causally relevant, is not itself a cause. Can the cause thus restricted still necessitate its effect? Yes it can, but not in the sense of (III) above.

Above I suggested that a suitable sense of necessity, that would provide some formal content to the notion of 'production' is subjunctive sufficiency. We can see immediately that the causal powers view gives us a relationship of subjunctive sufficiency between cause and effect. Consider a case of an object with some disposition $D_{S,M}$. According to (II) the following is true (but not necessarily true):

$$S \Box\!\!\rightarrow M$$

which, as we discussed above, suffices to provide an account of necessitation weaker than entailment.

## 7. Conclusion

Waismann is reported to have said, "Poor Causality, to have Ewing for her defender." It is difficult to not contrast Waismann's clear and cogent criticisms of Ewing's views and exposition of the regularity alternative with the vague and impressionistic articulation of the production view presented by Ewing. As far as their exchange is concerned, Waismann may be held to have come out ahead. Nonetheless, it is possible to reconstruct an account of causality that meets many of Ewing's requirements and which can respond well to Waismann's criticism.

It is sometimes said that philosophy does not make progress.[9] Insofar as the Waismann–Ewing debate continues today without a definitive resolution, that opinion appears to be confirmed. At the same time, our understanding of the issues is now so much more sophisticated than it was in the 1930s–40s that it cannot be denied that we now know much better what is involved in adhering to one side of the argument or the other.

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9 See Gerber (1973) for a discussion.

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# Waismann as Spokesman for Wittgenstein

In 1929 Wittgenstein left Vienna for Cambridge, and Waismann grew into the role of spokesman for his absent hero. The story of his relation with the man so greatly esteemed by his much-admired mentor Schlick contains dramatic elements: there were moments of friction and of coldness, announcements of withdrawal from a shared project, accusations of plagiarism or, at least, insufficient acknowledgement. What we know of this story has been told by Brian McGuinness and Gordon Baker.[1] If one wishes to gauge the extent to which Waismann succeeded in fulfilling his task as spokesman for Wittgenstein, one must start from the basic fact that between 1929 and 1936 the two men collaborated, trying to realize the common plan of producing a systematic exposition of Wittgenstein's philosophy.

As we know, the only constant element in Wittgenstein's thought was its tendency to evolve continuously. Moreover, it would be hopeless to try to describe the development of his thought in linear fashion. For what was discarded today could turn into an important insight tomorrow, while yesterday's self-evident truths could become today's obvious falsehoods. No one has characterized the attitude behind this more vividly than Waismann, who wrote in a letter to Schlick dated 9 August 1934:

> [Wittgenstein] has the marvellous gift of always seeing everything as if for the first time. But I think it's obvious how difficult any collaboration is, since he always follows the inspiration of the moment and demolishes what he has previously planned.[2]

We as readers of this tale may wonder whether the people involved should not have understood early on that the enterprise was doomed from the beginning. But whatever the correct answer to this question may be, the results of this collabora-

1 McGuinness, 'Vorwort des Herausgebers' (Friedrich Waismann, *Wittgenstein und der Wiener Kreis*, Oxford: Blackwell, 1967, pp. 11-31; tr. by Joachim Schulte and Brian McGuinness, *Ludwig Wittgenstein and the Vienna Circle*, Oxford: Blackwell, 1979); McGuinness and Baker, ‚Nachwort' (Friedrich Waismann: *Logik, Sprache, Philosophie*, Stuttgart: Reclam, 1976, pp. 647-662); Baker, 'Preface' (Ludwig Wittgenstein and Friedrich Waismann, *The Voices of Wittgenstein: The Vienna Circle*, London: Routledge, 2003, pp. xvi-xlviii); Baker, '*Verehrung und Verkehrung*: Waismann and Wittgenstein' (*Wittgenstein: Sources and Perspectives*, ed. by C. G. Luckhardt, Ithaca, NY: Cornell University Press, 1979, pp. 243-85); cf. McGuinness, 'Wittgenstein and the Vienna Circle' and 'Relations with and within the Circle' (*Approaches to Wittgenstein*, London: Routledge, 2002, pp. 177-183, 184-200).

2 Quoted in McGuinness, 'Editor's Preface' (*Ludwig Wittgenstein and the Vienna Circle*, p. 26).

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tion – Waismann's posthumous book *Logik, Sprache, Philosophie* and the astonishing number of drafts of its chapters and sections – may count as a sufficient reward for all the difficulties and troubles borne by those concerned.

In this paper, I want to examine certain aspects of Waismann's role as a spokesman for Wittgenstein. My method is simple: I shall look at one specific example, Wittgenstein's notion of meaning-bodies, and compare it with what became of it at Waismann's hands. Comparison, I expect, will help us understand the nature of Waismann's contribution to the picture we have come to draw of Wittgenstein's thought. Naturally, given the space at my disposal and the complexity of the task, the significance of whatever conclusions will be reached is bound to be limited. But they may nevertheless be of some assistance in forming a judicious conception of the value of Waismann's work.

## I

Readers of Waismann's *Logik, Sprache, Philosophie* or Gordon Baker's compilation *Voices of Wittgenstein* (VoW) will be familiar with the idea of *Bedeutungskörper* (meaning-bodies). To quote one of Waismann's formulations:

> … let us imagine a number of bodies: cubes, prisms, pyramids, made of glass and thus invisible[3] in space, except for the bases of the pyramids and one surface – say a square – of each prism, which are to be coloured and therefore visible. We shall then perceive only a number of coloured surfaces distributed in space. These surfaces cannot be arbitrarily joined together, because the invisible objects of which they are parts prevent certain configurations. The laws according to which surfaces can be joined seem to be embodied in those invisible objects. This simile brings out, I think, what we have at the back of our mind when we say that the rules should conform to the meaning of the words. It seems as if behind the word there is an imperceptible body which constitutes the meaning of the word, and determines whether a series of words fit meaningfully together or not.[4]

The idea goes back to Wittgenstein, but as regards discursiveness and clarity, there is no discussion in his writings that could compete with Waismann's exposition. To appreciate Waismann's contribution it will be helpful to proceed in two stages: as a first step, we shall take a brief look at the development of Wittgenstein's own

3 The fact that in his middle-period writings Wittgenstein himself mentions *Glaskörper* and stresses their invisibility fits his principle of the 'transparency' of grammatical rules, as I have called it in my paper 'Phenomenology and Grammar' (*Le Ragioni del Conoscere de dell'Agire: Scritti in onore di Rosaria Egidi*, ed. by Rosa M. Calcaterra, Milano: Franco Angeli, 2006, pp. 228-240). This paper contains an attempt to describe at least some of the features of Wittgenstein's notion of grammar, whose general intractability is deplored below.

4 Waismann: *The Principles of Linguistic Philosophy*, ed. by Rom Harré, London: Macmillan, 1965, p. 235 (*Logik, Sprache, Philosophie*, ed. by Gordon Baker and Brian McGuinness, Stuttgart: Reclam, 1976, p. 340-1).

ideas; and only after we have gained an impression of what Wittgenstein may have been up to shall we go on to compare his ideas with Waismann's account.

In view of the relative popularity of the notion of meaning-bodies[5] it may come as a surprise to realize that there are only a few occurrences of the word '*Bedeutungskörper*' in Wittgenstein's writings. The earliest of these seems to be his handwritten change, or correction, of the word '*Wortkörper*' into '*Bedeutungskörper*' in a passage of TS 213 (published under the title *Big Typescript* [BT]) where he discusses his standard example of the word 'is', which can be seen as having (at least) two meanings (= and ∈), whose distinct roles may be represented as different transparent bodies (e.g. a prism and a tetrahedron) with a single coloured and hence visible side, which would be a triangle in both cases.[6] The different bodies would then permit certain combinations and preclude certain others, where these bodies are intuitive ways of symbolising different rules or sets of rules of permissible word combination.

As the change of wording in TS 213 indicates, in Wittgenstein's manuscripts '*Bedeutungskörper*' was a – perhaps stylistic – variant of '*Wortkörper*', which is indeed the expression used in two earlier manuscript passages. The first of these (MS 110, p. 112) was written on 25 February 1931, and hence roughly two years earlier than the BT correction, the second (MS 112, p. 111v) is dated 22 November 1931 and comments on the previous one. Apart from straightforward repetitions in typescripts, there seem to be no further occurrences of '*Wortkörper*'.

In a way, however, these are not the earliest examples of the idea which interests us in Wittgenstein's writings. The notion of a *Wortkörper* is anticipated by the earlier idea of a *Beweiskörper*, which figures in two manuscripts written shortly after Wittgenstein's return to Cambridge in 1929. In the first of these manuscripts he says that a mathematical proposition is 'only the immediately visible surface of a whole proof-body' and that 'this surface is the boundary facing us'. In the second passage he claims that a mathematical proposition 'is related to its proof as the outer surface of a body is to the body itself. One might talk of the proof-body belonging to the proposition. Only on the assumption that there's a body behind the surface, has the proposition any significance for us'.[7] In their context,

5 In Hanjo Glock's *Wittgenstein Dictionary* (Oxford: Blackwell, 1996) an entire entry is dedicated to meaning-bodies (pp. 239-41), and in the commentary to *Philosophical Investigations* by Baker and Hacker the notion crops up again and again, even though the word is mentioned only once in the whole book – and there (§559) it occurs in double brackets, thus signalling a gap rather than anything that might fill it.

6 BT, p. 166 (§39). *The Big Typescript* has been edited and translated by C. Grant Luckhardt and Maximilian A. E. Aue (Oxford: Blackwell, 2005; page references are to the original typescript). The numbers of Wittgenstein's manuscripts (MS) and typescripts (TS) are given in accordance with the catalogue of Wittgenstein's papers by Georg Henrik von Wright. See his book *Wittgenstein*, Oxford: Blackwell, 1982, pp. 35-62.

7 MSS 105, p. 60, and 106, p. 98. Both passages were later transferred to TSS 208 and 209 (published as *Philosophische Bemerkungen*). The translations are taken from *Philosophical Remarks*, ed. by Rush Rhees, trans. by Raymond Hargreaves and Roger

these remarks can be seen to foreshadow a number of points coming to the fore in later passages involving word-bodies or meaning-bodies. For present purposes it is sufficient to acknowledge that the relevant body image was playing a role in Wittgenstein's thought as early as spring 1929.[8]

There are various ways of connecting the first remark on word-bodies (MS 110, p. 112) with different strands of Wittgenstein's discussions of grammatical rules – an extremely tangled matter about which nothing helpful can be said in a few sentences, so we have to make the completely unrealistic assumption that this notion is sufficiently well-understood. What Wittgenstein adds to the by now familiar image of meaning-bodies is the question whether such a body could serve as a notation for a rule and in this capacity enshrine an account of how to use the word whose body is in question.

One may well want to argue that this is not quite the question Wittgenstein articulates in these pages, but it seems to be what he took himself to have said when he returned to word-bodies ten months later.[9] For in the later passage he claims that what he had then written about word-bodies is a clear expression of an error he has just been discussing. And the error he has just been discussing is spelled out as follows:

> … it can easily seem as if the sign contained the whole of the grammar; as if the grammar were contained in the sign like a string of pearls in a box and we had only to pull it out. (But this kind of picture is just what is misleading us.) As if understanding were an instantaneous grasping of something from which later we only draw consequences which already exist in an ideal sense before they are drawn …[10]

White, Oxford: Blackwell, 1975, p. 192. A fruitful understanding of these passages would require a good deal of contextualisation and an open mind for various possibilities of interpretation, and hence translation. In the last sentence, for instance, it might be better to replace 'on the assumption that' by 'if' and 'significance' by 'meaning'. But that would make it difficult to retain 'proposition', as the latter term seems to imply the meaningfulness of the relevant signs. It appears that however one chooses to translate these passages, one will prejudge some questions of interpretation. So, the only change I decided to make was to substitute 'proof-body' for 'body of proof'.

8 At this point I leave aside another interesting use of the body idea. In MS 108, p. 190 (the only entry written down on 20 June 1930) Wittgenstein says that there must be some kind of continuity between an order and its execution: the execution cannot be more than the front surface of the command or command-body. [*Die Ausführung muß, sozusagen, nur die Endfläche des Befehls (Befehlskörpers) sein*.]) Here the image of a command-body is used in a way which suggests that in Wittgenstein's eyes there was nothing strange about it. In particular, there is not even a hint of anything in the nature of a criticism.

9 Of course, there is no guarantee that in MS 112, p. 111v, he is really referring back to MS 110, p. 112, but as far as I can tell this is the only earlier passage in his extant papers containing the expression '*Wortkörper*', and it is a passage which *can* be read as being exposed to the criticism formulated later.

10 MS 112, p. 111v, also in *Philosophical Grammar*, ed. by Rush Rhees, tr. by Anthony Kenny, Oxford: Blackwell, 1974, p. 55.

Again, I am simplifying matters to make things clearer than they really are in the manuscripts, but it needs to be understood that we are dealing with two different points here. On the one hand there is the image of word-bodies which Wittgenstein has been writing about, and on the other hand there are the things he has said about this image. And these things, he now claims, contained an 'error' – the error of attributing to the word-body *qua* sign the capacity to epitomize all the rules ('the whole grammar') governing the correct use of the word in question in such a way that these rules can be extracted from the body by contemplating it.[11] What he does *not* say in this passage is that this (or any other) error is inherent in the image of word-bodies itself.

After this (that is, after the end of 1933) we find practically[12] no further mention of the idea of meaning-bodies until we reach the last stage of the composition of *Philosophical Investigations* and the first two manuscript volumes chiefly dedicated to what Wittgenstein himself used to call the philosophy of psychology. As a matter of fact, there are more occurrences of the actual term '*Bedeutungskörper*' in his writings from 1945-6 than in his earlier manuscripts. These later remarks are of some, but chiefly of indirect relevance to our story. For this reason I shall content myself with a brief description.

(1) There is the addition of the word '((*Bedeutungskörper*))' to the main body of §559 of *Philosophical Investigations* (TS 227). This addition must have been made at the very last stage of the composition of the typescript, as it does not occur in either *Bemerkungen I* (TS 228) or *Bemerkungen II* (TS 230) both of which contain the rest of §559, the first version of which was written much earlier (1933). Double brackets were presumably meant to indicate that Wittgenstein considered adding a remark, or part of a remark, on the subject alluded to by the expression enclosed in brackets.[13]

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11 The criticism mentioned above is retained in the revised BT version of our remark, but the typed reference to the passage criticised is crossed out at this stage. The reasons for this are not obvious, as the (slightly corrected and expanded) substance of the earlier material (i.e. MS 110, pp. 112ff.) is kept both in the typescript and in the subsequent manuscript version (MS $114_{ii}$, pp. 32-3, cf. *Philosophical Grammar*, pp. 54-5). This part of the story is likely to be particularly relevant to our present concerns, as Waismann can be assumed to have had access not only to the (revised?) Big Typescript, but also to parts or the whole of the revision contained in MSS $114_{ii}$-$115_{i}$.

12 At some stage, probably in the autumn of 1937, Wittgenstein copied one remark on *Bedeutungskörper* from the Big Typescript (TS 213, pp. 166-7) into MS 116. But that is not significant: it was simply part of the task he had set himself at that time of selecting a number of remarks from what he called his *alte Maschinschrift*.

13 Peter Hacker, in Volume 4 of his (and Gordon Baker's) commentary on the *Investigations*, suggests that Wittgenstein may have had in mind adding part of remark (2) below, from MS 130, pp. 68-70 (= *Remarks on the Philosophy of Psychology*, Vol. I, §42-3). This may be so, if TS 227 was completed late enough for Wittgenstein to have thought of this remark.

(2) In MS 130, p. 69, the image of meaning-bodies is used to illustrate our feeling that certain combinations of word-meanings cannot be held in thought because these meanings have a kind of shape that does not permit fitting them together (as in a puzzle). The image can be misleading if it goes together with an erroneous picture of meaning something by individual word-meanings in such a way that they add up to a sense expressed by a corresponding sentence.

(3) A similar point is made in MS 131, p. 40, where Wittgenstein comments on the idea that 'These meaning-bodies don't yield – or result in – a sense-body' (*Diese Bedeutungskörper geben keinen Sinnkörper*). He writes that this idea is not suitable for explaining what claims to the effect that certain combinations of signs make no sense may amount to. Again, it is the amalgamation of the meaning-body image with a psychological account of the impossibility to think certain expressions (*Denkunmöglichkeit*) which is criticized.

(4) In MS 131, p. 166,[14] Wittgenstein points out that the inclination to think in terms of meaning-bodies may simply be a feature of our way of thinking, just like the tendency to assume that there is a locus of thought ('thinking [or calculating] in the head'). At any rate, the inclination may be there – independently of how it has come about.

(5) The last occurrence of the image (MS 131, p. 182-3) is different from all previous ones. Here, Wittgenstein talks about a poet's or a painter's ability to suffuse his work with a certain atmosphere and our capacity to pick up on this and describe it. Such a description, he insists, cannot be given in a few words, for here we are dealing with a connection with a whole way of living 'which as it were forms its meaning-body' (p. 183). Obviously, this is not the same notion of a meaning-body as in the earlier passages mentioned, but it is a related idea which also involves the image of a firmly attached body with a certain shape determining the position of the object in question and thereby our chances to understand the work.

Not all the uses of the image[15] of meaning-bodies are of the same kind. In the earliest cases, in particular where he employs the notion of a proof-body (and that of a command-body), he uses it to illustrate a feature of his own view. In some of the later ones he suggests that, together with erroneous ideas about thinking or meaning something, it may actually mislead us. But as far as I can see, there is not a single passage in his writings where he says that there is something inherently wrong with the image, or that this image taken by itself is bound to lead us astray.

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14 *Remarks on the Philosophy of Psychology*, Vol. I, §349.

15 Wittgenstein speaks of a 'metaphor' or a 'comparison', and he uses a standard formulation to indicate an analogy: *es verhält sich wie* … (MS 106, p. 98).

## II

Against the background of this brief account of Wittgenstein's remarks on meaning-bodies I should now like to take a glance at some passages in Waismann's writings where he makes use of Wittgenstein's ideas. I shall first look at a short section from Notebook I in Gordon Baker's *Voices of Wittgenstein* and then briefly move on to a longer section (entitled *Bedeutungskörper*). Much of this material can be found in *Logik, Sprache, Philosophie* as well, but I shall generally refrain from pointing out such parallels. Basically, I shall try to list a number of differences between Wittgenstein's remarks and the story told by Waismann.

The first mention of the meaning-body theme to which I want to draw attention occurs in a section on rules and meaning (VoW, p. 132). Here, the image of meaning-bodies is not simply introduced as a separate topic; it is supplied with a context that is supposed to give its introduction a certain point. The beginning of the section is written in the first person, and the narrator is clearly supposed to be Wittgenstein (not Waismann).[16] In words reminiscent of the well-known conversation on dogmatism[17] he says that he used to hold a mistaken conception of analysis, according to which he believed that sense, or meaning, is hidden behind our linguistic expressions. And it is this hidden meaning which can, as he continues to say, usefully be compared with transparent prisms, pyramids, etc.

Of course, this is an elegant way of introducing the meaning-body analogy, but we should remember that it is not Wittgenstein's. We are given a reason for reflecting on the analogy, and the reason is from the very beginning embedded in a context of criticism – in this case, self-criticism. The reported self-criticism is strongly exaggerated. Even from the point of view of his later self, or selves, the early Wittgenstein surely did not think that *sense* was hidden. The view really ascribed to him by his later self amounts to the claim that elementary propositions (and hence, one might say, the ultimate determinants of sense) are not known but may one day be discovered. To be sure, this is a much weaker view than the one described by Waismann's Wittgenstein; and in particular it is a view which would not easily lend itself to making the transition Waismann wants to make in order to motivate the introduction of our analogy.

So, the few sentences prefacing Waismann's introduction of the meaning-body image serve to accomplish at least two things: (1) they supply a context which to some extent sets the agenda and the tone of what is to follow; (2) they link the image with a certain motive or reason for mentioning the image and thus make its introduction appear more natural.

Another noticeable feature of Waismann's account is constituted by some seemingly minimal additions which help the reader to understand Wittgenstein's

16 This *may* indicate that the passage goes back to one of their meetings where Waismann took down Wittgenstein's words. On the other hand, Waismann's use of the first person may be conventional and hence just another expression for 'Wittgenstein'.

17 *Wittgenstein and the Vienna Circle*, pp. 182-6 (9 December 1931).

analogy as part of a familiar story. Where Wittgenstein speaks of a rule in accordance with which the visible shapes may be arranged,[18] Waismann has a *law* (his italics) determining the possible ways of fitting the bodies together. The rules of grammar, Waismann continues, would then be ways of describing the invisible bodies behind their visible surfaces and a kind of unfolding of the nature or essence of these bodies.[19]

What is admirable about Waismann's version is the way he succeeds in spelling out the analogy: on the one hand there are the rules of grammar, and on the other hand there are the bodies with their various shapes. Their possible arrangements are governed by certain laws, which in their turn correspond to the rules of grammar. And so it is made (fairly) clear which elements of the image we are supposed to map onto which elements of the criticized conception of meaning.

As a matter of fact, the words chosen by Waismann come quite close to Wittgenstein's, who in a characteristic formulation writes that the rule would anticipate the nature or essence of the shape (*in ihr wäre doch bereits das Wesen der Würfelform präjudiziert*). But if one compares the two versions, one finds that, in spite of the similarity of the words used, what is going on at one end is completely different from what is happening at the other. If you look closely at the sequence of remarks in Wittgenstein's manuscript or typescript, you will notice that he has simply changed his subject.[20] He began by considering the metaphor of word-bodies, or meaning-bodies, but now he goes on to contrast physical bodies with geometrical forms and wonders whether geometrical rules can somehow be seen as encapsulated by a given shape. And it is in this sense that he asks: 'Can I read the geometry of a cube off a cube?'[21] This question gives rise to a discussion of the problem whether a cube, or a drawing of a cube, can be regarded as a sign encompassing the relevant geometrical rules. As a tentative answer, Wittgenstein then says that it (the cube, or its drawing) can serve as a notation of geometrical rules only if it belongs to a system; and in a later revision the condition is put much more strongly: it (the cube) can serve this purpose only if it functions as a sentence belonging to an entire system of sentences.

Evidently, these remarks of Wittgenstein's were an inspiration for Waismann to spell out the meaning-body analogy and its point in a way which in his view agreed with the spirit of those remarks. And to be sure, Waismann's account is much neater than Wittgenstein's: he does not stray from his original course, nor

18 See for instance BT, pp. 166-7. It is interesting to note that in the next remark (BT, p. 167), which however seems to belong to a different context, Wittgenstein *does* use the word '*Gesetz*'.

19 VoW, p. 132: ,'Ich dachte dann, dass das Wort gleichsam einen "Bedeutungskörper" hinter sich habe, und dieser Bedeutungskörper sollte durch die grammatischen Regeln, die für das Wort gelten, beschrieben sein. Es wären dann die grammatischen Regeln gleichsam eine Auseinanderbreitung des *Wesens* des Bedeutungskörpers.'

20 MS 110, pp. 112-114; TS 211, pp. 203-5; TS 213, pp. 166-7.

21 BT, p. 167, tr. p. 126e.

does he wander off in directions not announced at the beginning of the journey. What remains unclear, however, is the extent to which Waismann's story tallies with Wittgenstein's train of thought. Surely the answer to the question implicit in this description of the situation will largely depend on how we read Wittgenstein's own remarks on meaning-bodies and how well we succeed in this effort without being influenced by Waismann's persuasive account of the matter.

Though the fourth feature of Waismann's version will be described only briefly, it is of great importance but often difficult to trace. What I mean is the addition of truly Wittgensteinian ideas to a line of thought which in Wittgenstein's own manuscripts and typescripts is developed without drawing on these ideas. The short section on rules and meaning contains at least two unmistakable examples of this sort of move. The first of these is the idea of *being guided* by a model in doing something. This idea is familiar from various passages in Wittgenstein's writings, especially from the long section on reading in his *Philosophical Investigations* (see in particular §§170-8).[22] Waismann brings it in to illustrate a certain aspect of the feeling that a model of the kind of a geometrical figure already contains all the rules that apply to it. In this way it can serve to lead the discussion of geometrical figures and rules back to the notion of meanings as spatial objects standing behind our words (VoW, p. 134). And this is a move which (as far as I can see) has no counterpart in Wittgenstein's writings.[23]

The second example of the fourth feature I want to mention concerns the idea of a surveyable or perspicuous representation (*übersichtliche Darstellung*) famously discussed in §122 of the *Philosophical Investigations* – a remark whose earliest version was written in the summer of 1931. In the section on rules and meaning which we are considering here, Waismann mentions the idea in order to connect geometrical figures with rules of grammar, thus associating these kinds of figures and rules with the discussion of meaning-bodies and their role as ostensible determinants of word-meaning. As Waismann points out, the colour octahedron, which after all is a geometrical figure or body, can serve as a surveyable representation of certain rules of grammar. And while it does not simply usurp the place of these rules, it manages to render them more perspicuous than they would be without this means of representation.

22 Of course, there are earlier sources. Among these, see Wittgenstein's German revision of his *Brown Book* (ed. by Rush Rhees and published under the title *Eine philosophische Betrachtung* [Frankfurt am Main: Suhrkamp, 1984]), pp. 183-9. Cf. my paper 'Reading-machines, Feelings of Influence, Experiences of being Guided: Wittgenstein on Reading', in: *Wittgenstein: Mind, Meaning and Metaphilosophy*, edited by Pasquale Frascolla, Diego Marconi and Alberto Voltolini, Houndmills: Palgrave Macmillan, 2010, pp. 31-45.

23 To repeat, all or most of the individual steps of the argument as developed by Waismann can be traced back to Wittgenstein's writings, but the specific sequence of moves indicated is Waismann's.

Again, this is a truly Wittgensteinian idea: in manuscripts and typescripts written in 1930 Wittgenstein explicitly says that the colour octahedron is a perspicuous means of representation, and that it serves to elucidate grammar. He even claims that it *is* grammar.[24] Some of these remarks are repeated in writings as late as the Big Typescript, but apart from a problematic mention of the octahedron in his late manuscripts on the philosophy of psychology the whole idea seems to vanish from his thought. The other notion of a surveyable representation, however, the one discussed in PI §122, is retained. But this passage employs a peculiar sense of *übersichtlich* and is closely connected with Goethe's and Spengler's morphological ideas and insights gained through a critical examination of Frazer's *Golden Bough*. The earlier notion, on the other hand, relies on the ordinary meaning of '*übersichtlich*', but connects it with a notion of grammar which arguably disappeared from Wittgenstein's writings together with the role ascribed to the colour octahedron in 1930.

But be that as it may, the idea of an *übersichtliche Darstellung* brought into the discussion of meaning-bodies by Waismann is surely the earlier, transitory, idea connected with the colour octahedron and its alleged relevance to grammar, and should not be confused with the somewhat later morphological conception of surveyable representation.[25] And we should also remember that the idea is added by Waismann to his version of the meaning-body analogy. I am not aware of any passage where Wittgenstein himself employs it in this context.

Comparison of Waismann's writings with Wittgenstein's typescripts and manuscripts goes to show that Waismann had a great knack for coming up with general and at the same time precise reformulations of Wittgenstein's groping attempts at finding a natural expression of his inchoate and often schematic thoughts. (This is the fifth feature I want to mention.) In the pages we are concerned with this comes out in Waismann's repeated statement of the leading question in terms of which the notion of meaning-bodies should be seen. This question amounts to asking whether rules yield meaning or meaning determines rules. This is a neat alternative which surely articulates a contrast that can be identified in Wittgenstein's manuscripts, but it would not be easy to read Wittgenstein as framing his remarks in exactly this way. And it is this generality and precision of Waismann's reformu-

24 *Philosophical Remarks*, p. 52. Much is made of this passage by Gordon Baker in his article '*Philosophical Investigations* §122: Neglected Aspects' (1991), reprinted in Baker, *Wittgenstein's Method: Neglected Aspects*, ed. by Katherine Morris, Oxford: Blackwell, 2004, pp. 22-51.

25 In a way it is possible to attribute the most important changes in Wittgenstein's outlook (as he himself observed in a well-known passage [*Culture and Value*, 2nd edition by G. H. von Wright, Heikki Nyman and Alois Pichler, tr. by Peter Winch, Oxford: Blackwell, 1998, p. 16]) to the influence of Spengler and Sraffa, and these changes can be described by drawing attention to morphological and 'ethnological' elements in Wittgenstein's later approach. My feeling is that these are the chief characteristics of Wittgenstein's thought in the first half of the 1930s that are (almost) absent from Waismann's adaptation.

lations which makes it possible for him to express Wittgenstein's sketchy contrasts in words that make for clear-cut opposition and a rewriting of the original tale in terms of a conflict of antagonistic views and correct vs. incorrect opinions or theses.

This talent of Waismann's manifests itself in another distinctive feature of his account, viz. in his ability to coin persuasive slogans. An example from the relevant part of his writings is the following italicized statement, which (as we must not forget) is meant to reproduce Wittgenstein's views on the matter. Waismann writes: *'The rule is discursive and cannot be replaced by something amorphous – by the meaning*.' Not only is this a memorable claim setting discursive rule against amorphous meaning; it also manages to suggest a reason for giving priority to rules, for 'discursive' sounds like a good thing while 'amorphous' surely points the other way and makes one think of something lumpy, primitive, uncivilized. And what this part of the slogan suggests can easily be seen to hang together with the meaning-body analogy if one takes to heart Waismann's version of a truly Wittgensteinian idea: 'We do not extract rules from meanings, as if meanings stood *behind* words like objects in space.'[26]

Now, readers of Wittgenstein will not be astonished to learn that nothing really corresponding to this slogan can be discovered in Wittgenstein's writings. There are indeed a few remarks about the discursiveness of thinking and understanding, and in some of these passages the more conventional contrastive term 'intuitive' is used. But the general slogan itself does not occur in the manuscripts.[27] What does occur, however, and may come as a bit of a surprise is a related way of contrasting discursiveness and amorphousness (as well as intuitiveness). In a passage from TS 211, which is known to have been in Waismann's hands, Wittgenstein writes as follows:

> … we labour under the error that believing, meaning something, knowing, desiring, looking for something, thinking etc. are *states* and that this is why something of a different kind must be hidden behind the symbolic processes of thinking – something which contains the sense of a sentence in an as it were amorphous form; that is, it would be intuitive, similar to an unchanging picture, not discursive and hence comparable to an activity (like washing).[28]

26 VoW, p. 135 ('Wir holen die Regeln nicht aus der Bedeutung heraus, als ob die Bedeutung wie ein räumlicher Gegenstand *hinter* dem Worte stünde').

27 It goes without saying that this way of putting the matter is short for 'I have not found any such occurrence'.

28 This passage originates in MS 111, p. 5 (7 July 1931). My translation is based on the version given in TS 211, p. 3: 'Wir laborieren nämlich unter //an// dem Irrtum, dass Glauben, Meinen, Wissen, Wünschen, Suchen, Denken etc. *Zustände* sind, und dass daher hinter den symbolischen Prozessen im Denken etwas von andrer Art verborgen sein muss, das den Sinn eines Satzes gleichsam in amorpher Form enthalte, d.h. intuitiv, dem Sehen eines gleichbleibenden Bildes ähnlich, nicht diskursiv, also einer Tätigkeit (wie dem Waschen) vergleichbar.' There are remarks on the next page of the typescript which repeat the words 'intuitive' and 'amorphous'.

So it is likely that Waismann got his inspiration from this passage and applied the contrast alluded to there to his task of producing an elegant version of Wittgenstein's ideas involving the image of meaning-bodies. And what he came up with was a general slogan which at the same time serves to justify a certain (critical) view of the notion of meaning-bodies.

The seventh and last item on my list of distinctive features of Waismann's (as opposed to Wittgenstein's) account of meaning-bodies is of great significance. In the first paragraph of the section entitled *Bedeutungskörper*[29] Waismann proceeds in a way which has no counterpart in Wittgenstein's manuscripts or typescripts. He traces the meaning-body idea back to Frege and quotes various passages from the second volume of *Grundgesetze der Arithmetik* to support this attribution. Now, as far as I can see, neither the meaning-body analogy nor the view it has been turned into by Waismann is ever attributed to another author in Wittgenstein's own writings, nor is it ever connected with the three quotations supplied by Waismann (from *Grundgesetze* §§91, 158, 207). None of the quoted passages from Frege is mentioned in Wittgenstein's manuscripts. So, in all likelihood we are dealing with a genuine addition by Waismann to Wittgenstein's own discussion.

Of course, readers may find Waismann's attribution convincing and helpful, just as they may think that Waismann's much more critical description of the meaning-body image does more justice to the whole idea than Wittgenstein's own attitude towards it. Three eminent readers who willingly go along with Waismann's version are Hanjo Glock, Gordon Baker and Peter Hacker. Thus, Glock claims that 'Wittgenstein uses this term ["meaning-body"] to characterize the idea that behind each sign there is a non-linguistic entity, its meaning, which determines how it is to be used correctly. […] Such a view is prominent in Frege, who […] insisted […] that the rules for the use of mathematical symbols must "follow from what they stand for", their meanings.'[30] In their commentary on Wittgenstein's *Philosophical Investigation*, Baker and Hacker heavily rely on Waismann's version of the meaning-body analogy and repeat the attribution to Frege as well as Waismann's supporting quotations. One example is their exegesis of §138. There they write that 'This picture of meanings fitting or failing to fit each other […] informs Frege's philosophy of language and mathematics […]. It is as if each word had behind it a "*meaning-body*" […].'[31]

29 VoW, p. 134, see also the following pages. Cf. *Principles of Linguistic Philosophy*, pp. 234-7 (*Logik, Sprache, Philosophy*, pp. 339-45).

30 *Wittgenstein Dictionary*, p. 239. Glock goes on to cite various passages from *Grundgesetze* to support the attribution to Frege and repeatedly states his indebtedness to Waismann.

31 Baker and Hacker, *Wittgenstein: Understanding and Meaning*, Part II, revised edition, Oxford: Blackwell, 2005, p. 294. (Notwithstanding the great number of changes made by Hacker in the second edition, practically the same words can be read in the first edition of 1980, p. 577.) Unsurprisingly, most of the relevant references are to Waismann's writings. All three quotations supplied by Waismann are also given in the commentary.

In a sense, Wittgenstein's original version or versions of the meaning-body analogy have been put in the shade by Waismann's account. There can be no doubt that this is at least partly due to the features described above, and I am sure that my list of features could be extended.

Before I move on to the concluding part of my paper I shall briefly summarize the features that characterize Waismann's account and help to explain the success of his story:

1. A context is supplied to set the tone of the following remarks
2. A motive or reason is given to lend plausibility to what follows
3. Supplementing the account by adding new details helps to round off the story
4. Filling in of Wittgensteinian ideas where they are not used by Wittgenstein himself
5. More general and more precise reformulation of issues
6. Coining of slogans
7. Attribution to specific authors

Here, these features have been extracted from a couple of pages of Waismann's text. I am certain that all of them (as well as some further features) can be detected in many other of his writings.

## III

I am sure that some readers would want to object to what I have been saying by pointing out that there is one text by Wittgenstein which I have forgotten to take into account, and that this work not only contains the story of meaning-bodies told along Waismann's lines but quite generally displays many of the features that I have listed as typical of Waismann's way of rewriting Wittgenstein. This is the so-called *Diktat für Schlick* (D 302). This dictation has come down to us in various shapes and copies, and most scholars have not hesitated to accept it as a *bona fide* work by Wittgenstein.

As a matter of fact, I have not forgotten about these pages, nor about the fact that they contain a passage on meaning-bodies which tallies much better with Waismann's story than with anything in Wittgenstein.[32] But I see no reason to regard the *Diktat für Schlick* as a work by Wittgenstein, not even in the most extenuated sense still compatible with the notion of a dictation.

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A different attribution of the meaning-body idea can be found in Stephen Hilmy's book *The Later Wittgenstein* (Oxford: Blackwell, 1987, pp. 122-5). He claims that this notion comes from William James's and Russell's views on psychology. In my view, Hilmy's argument is unconvincing, even though he gives a good account of the story as it unfolds in the manuscripts (see note 348, pp. 282-3).

32 The passage on meaning-bodies can be found on p. 12 of the printing in VoW. The title of this section (*Bedeutungskörper*) has been added by the editor, Gordon Baker.

Since Gordon Baker and Brian McGuinness have clarified a number of aspects of the collaboration between Wittgenstein and Waismann, some studies have appeared in which their authors have tried to give a convincing account of the *Diktat*, its genesis and its bearing on our picture of Wittgenstein's philosophical development.[33] Georg Henrik von Wright, in his article on the Wittgenstein papers, writes that the Diktat cannot be 'earlier than 1926' and adds that 'It is improbable that any of the listed typescripts is later than 1933'.[34] Gordon Baker opts for December 1932,[35] and Iven tells an admirably coherent story dating the dictation September 1933, when Wittgenstein and Schlick spent some time together in Istria. In Iven's view, the stenographic record in the Schlick Nachlass is the *Urschrift* on which all the known typescripts that were found among Waismann's and Schlick's papers are based. In addition to these writings, a further stenographic record has turned up, or rather been unearthed by Juha Manninen from among the papers of Rose Rand.

So, on the basis of these data one may think of various scenarios, but I want to suggest that we should start at the other end, as it were, and begin with an examination of the text of the *Diktat* itself. It is quite obvious that it looks like nothing else in Wittgenstein's *Nachlass*. This has been noted by several authors, perhaps most recently by Josef Rothhaupt, who carefully lists some of the evident differences – no separate paragraphs or 'remarks', an amazing degree of discursiveness, several specific references to other authors – but does not find these uncharacteristic features sufficient to doubt Wittgenstein's authorship.[36]

Here, I should like to mention a few points that may help readers to make up their minds about the question whether or not this document is likely to be authentic:

With the help of the Bergen Electronic Edition (BEE) of Wittgenstein's *Nachlass* I attempted to trace parallel passages in Wittgenstein's manuscripts. Half a day's work resulted in the identification of several dozen parallels. A few of these

33 See Matthias Iven, 'Wittgenstein und Schlick: Zur Geschichte eines Diktats' (*Schlick Studien*, Vol. I [2008]), pp. 63-80; Peter Keicher, 'Untersuchungen zu Wittgensteins "Diktat für Schlick"' (*Arbeiten zu Wittgenstein*, ed. by Wilhelm Krüger and Alois Pichler, *Working Papers from the Wittgenstein Archives at the University of Bergen*, No 15 [1998]), pp. 43-90; *id.*, 'Aspekte musikalischer Komposition bei Ludwig Wittgenstein: Studienfragmente zu D 302 und Opus MS 114ii/115i' (*Wittgenstein Studien* 1 [2000]), pp. 199-255. Juha Manninen has been kind enough to make the fruits of his researches into many aspects of the collaboration between Waismann and Wittgenstein available to me. I have greatly profited from his generosity and hope that his results will soon be published.

34 G. H. von Wright, *Wittgenstein*, p. 56.

35 VoW, Preface, pp. xv and xvi. In his preface, however, Baker does not mention any reasons for his decision to fix on this exact date.

36 See Josef G. F. Rothhaupt, 'Wittgensteins "philosophisches Akupunktieren" mit "Bemerkungen"' (*"Krakau zugeteilt"*, ed. By Józef Bremer and Josef Rothhaupt, Cracow: Universum philosophiae, 2009), pp. 243-293, especially 262-3.

remarks occur in relatively early manuscripts from around 1930, but practically all of them can be found in TS 211 and/or TS 213 and/or MSS 114ii-115i-140 (the last stage of the revision of TS 213 [BT]).[37] Since a fair number of particularly close parallels come from this last *Umarbeitung*, it appears likely that the *Diktat* – however it may have been produced – was not completed before 1934.[38]

The most important result of this comparison between Wittgenstein's writings and the *Diktat* is this: that the parallels are a wild mixture – one sentence comes from BT, the next from MS 115, another from TS 211 and so on. That is, if one tried to assemble the *Diktat* from Wittgenstein's writings, one would have to produce a mosaic by fitting together little snippets taken in the most discontinuous way from a large number of these writings. As far as I can see, there is no explanation for this that could be compatible with Wittgenstein's usual way of working (about which we know a lot, after all).[39] What makes Wittgenstein's authorship even more unlikely is the extraordinarily smooth discursiveness of the *Diktat*, which has no parallel anywhere among his writings and does not even seem to have been a goal he ever strove to achieve.

Another point to consider are direct and indirect references to other authors. Neither the reference to Nietzsche (VoW, p. 12) nor the discussion of Heidegger's "Das Nichts nichtet" (p. 72) are in Wittgenstein's usual style. The awkward statement about the influence of Adolf Loos (p. 76) was certainly not phrased by Wittgenstein himself. And it is unthinkable that Wittgenstein should have referred to the *Logisch-philosophische Abhandlung* by the absurd name '*Traktat*'[40] (which on the other hand *was* used by Waismann).

Another game facilitated by the resources of BEE can be played by searching for expressions essentially occurring in the *Diktat* but never or only exceptionally in Wittgenstein's writings. Some of these are quite striking, as for example the word '*Woge*' (wave), which is used twice in the *Diktat* (once as a noun and once in its verbal form) but never in Wittgenstein's *Nachlass*. Another example is the word '*entladen*' as used in the sense of 'express (an emotion etc.)'. Apparently, this use can be found only once in the entire *Nachlass* (MS 140!) but three times in the *Diktat für Schlick*. A third example is the word '*psychomechanisch*', which is used on one of the first pages of the *Diktat* but never in Wittgenstein's writings.[41]

37 I don't count the extracts from TS 213 copied into MS 116 and from there into later typescripts as stages of a *revision* of whatever work Wittgenstein may have had in mind around 1933–4.

38 The first entry in MS 115 is dated 14 December 1933.

39 As a matter of fact, we know about a few cases (e.g. the assembling of TS 213 or the last third of *Philosophical Investigations*) where Wittgenstein designed and prepared fairly complicated rearrangements of his remarks. In these cases, however, we are dealing with (only partially successful) attempts at reordering relatively self-contained *remarks* – not the production of an uninterrupted and relatively well-organized exposition of claims and arguments.

40 VoW, p. 6. Another occurrence of '*Traktat*' can be found on p. 128 of VoW.

41 VoW, p. 8. The inspiration for this may come from one of those passages where Witt-

Other striking expressions that occur in the *Diktat* but not in Wittgenstein's writings are the nouns '*Epigramm*'[42] and '*Weltkrieg*' as well as the common phrase '*über einen Leisten schlagen*'. I am sure that this list could be extended, but for present purposes this should be enough.

The significance of these observations should be obvious: it is extremely unlikely that Wittgenstein was the author of D 302. As we know from our discussion of meaning-bodies, this does not in the least weaken the claim that the *Diktat* should be seen as a summary of Wittgensteinian ideas. Only that this summary was not put together by Wittgenstein himself.

I have no story to tell that could serve as a scenario describing how the extant copies of D 302 were produced. I do have a hunch, however, as to the identity of the author of the *Diktat*. In view of the comprehensiveness, the coherence and the faithfulness to much of the spirit of Wittgenstein's writings I cannot see anyone in the role of author except for Wittgenstein's ablest spokesman – Friedrich Waismann. If you read the text of the *Diktat* with the seven features I listed in mind, you will find that they can be detected everywhere in this document. My guess is, however, that it was not drafted as one continuous account. The most likely course of events can be conjectured from an observation in Gordon Baker's preface to *Voices of Wittgenstein*. There he says that Wittgenstein's thoughts 'can be studied in some detail by comparing the full text of "Diktat für Schlick" with the sequence of short typescripts that Waismann excerpted from this source' (p. xxxi).[43] I think we should turn this around and say that those short typescripts were based on Waismann's knowledge of Wittgenstein's writings and his discussions with him; and at a later stage these typescripts were used to assemble the comprehensive account contained in the *Diktat*. This story has the advantage of explaining the fact that the parallels between this document and Wittgenstein's writings can be found in such extremely diverse parts of the source material: as a first step, Waismann used a fair but surveyable number of different passages to piece together short texts; and as a second step these short texts were employed to produce the *Diktat*, thus stirring up the elements of the earlier mixtures to manufacture a new patchwork.

Of course, this is just a conjecture, but it helps to explain the actual form and content of the *Diktat für Schlick* and it throws additional light on Waismann's usual practice of dealing with Wittgenstein's work as described in the previous section of my paper. Altogether this goes to show that no one was as qualified as Waismann was to act as spokesman for Wittgenstein. His accounts are faithful to much of the spirit of the latter's writings; in many cases they are more coherent

genstein speaks of a *psychophysischen Mechanismus* (MS 114, p. 163; MS 140, p. 27 – to mention two passages that Waismann may well have seen).

42 The adjective '*epigrammatisch*' occurs once (MS 137, p. 140a, apropos of Buffon's famous observation on style, printed in *Culture and Value*, 2nd edition, p. 89).

43 It was Brian McGuinness who drew my attention to this passage and its potential significance.

than the original they are based on; the reasoning is lucid; the language is clear; and arguments as well as metaphors are spelled out in a way which readers can be expected to take in at first glance.

On the other hand, such virtues have their defects – or one defect, at any rate. While the changes effected by Waismann's interventions are apt to render Wittgenstein's ideas more palatable, they proportionately diminish their value as sources giving unfiltered information about Wittgenstein's thought at a given time. If you want to find out about this, you will have to turn to Wittgenstein's own writings, whereas Waismann's accounts can, precisely because of their many virtues, easily distract your attention from essential qualifications, self-doubts and incongruities. In sum, while I disagree with Gordon Baker's conclusion that Waismann's accounts have 'a very good claim to being treated as authoritative in the exposition and critical analysis of Wittgenstein's philosophy in the period 1928–36',[44] I do share his admiration for Waismann's qualities as spokesman for Wittgenstein.

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44 VoW, Preface, p. xxxiii. Cf. the following passage from the same Preface (p. xl): 'There seems no room for doubt that much of this material [that is, Waismann's writings] consists of larger or smaller fragments of Wittgenstein's own exposition of his ideas in the period 1929–36, principally in the period 1931–34. Consequently, it constitutes what is perhaps the only remaining hitherto-unknown *primary* source material for clarifying Wittgenstein's thinking (and its development during the early 1930s).'

Juha Manninen

# Waismann's Testimony of Wittgenstein's Fresh Starts in 1931–35

## 1

In the Vienna Circle archives in Haarlem, NL, there are a great number of protocols connected with Moritz Schlick's philosophical chair – manuscripts, typescripts and shorthand manuscripts.[1] They contain extensive and detailed information about Schlick's seminars and also about the elementary seminars, so-called proseminars, which were held, as the documents explain: "bei Prof. Schlick", but actually after 1929 not by him. Since his arrival in Vienna, Schlick was responsible for these both types of seminars and they were under his supervision. They are documented mainly by students. The different participants had the task of producing a handwritten report of the meeting, later also typescripts. It is not mentioned in the protocols who was responsible for the elementary seminars. The professor was the person who mattered. It is interesting to observe that while Schlick's seminars mainly contained descriptions of chapters in philosophical books, the proseminars were more ambitious.

Beginning with 1929, they were in fact based on lectures by the person who held them, Friedrich Waismann. Officially, they could not be called lectures. Waismann had not filled the precondition for lecturing, i.e. presented a grand thesis called *Habilitationsschrift*. He had officially only the humble task of a librarian. Waismann had originally chosen as his research subject the phenomenological problem of space, a critical examination of Edmund Husserl's philosophy. When Schlick became enthusiastic about Ludwig Wittgenstein's *Tractatus* and succeeded in establishing contact with the author, Waismann with his clear mind and excellent presentational abilities seemed to be the ideal person to write a more readily intelligible book about the *Tractatus* than the *Tractatus* itself.

Waismann was much appreciated for his presentation of Wittgenstein's new ideas within the Vienna Circle. Waismann's proseminars were held in high esteem, as evidenced by a visiting student from Berlin, Carl G. Hempel:

> Im Carnapschen Seminar wird „Unser Wissen von der Außenwelt" gelesen, und Carnap benützt dann häufig die Gelegenheit, in sehr instruktiver Weise die Abweichungen der

1 See the catalogue by R. Fabian, Inventory of the papers of the Vienna Circle Movement (1924–1938) in particular of the scientific papers of the philosophers Moritz Schlick (1882–1936) and Otto Neurath (1882–1945). It is available on the internet pages of the Vienna Circle Archive (=VCA).

B.F. McGuinness (ed.), *Friedrich Waismann – Causality and Logical Positivism*,
Vienna Circle Institute Yearbook 15, DOI 10.1007/978-94-007-1751-0_10,

> Wittgensteinschen Einstellung von der Russellschen zu erläutern. Hierdurch und durch Waismanns Seminar hoffe ich allmählich zu lernen, was Wittgenstein mit seinen Fundamentalthesen überhaupt meint, die ja für die Wiener Philosophie eine ganz erstaunliche Rolle spielen. […]
>
> Schließlich wird das Schlicksche „Proseminar" von Waismann abgehalten. Ich bin froh, durch Zufall davon gehört zu haben, denn gerade bei Waismann scheint man sehr viel über die Wittgensteinsche Auffassung der Logik lernen zu können. Im Seminar finde ich Waismann ganz wesentlich angenehmer als damals in Prag. Seine Vorsicht in der Argumentation und die straffe Art, wie er den Gang der Diskussion fördert, gefallen mir sehr.[2]

Waismann was presenting Wittgenstein's new ideas to the Circle, while Wittgenstein soon elected to conduct discussions with only Schlick and Waismann whom he found the most congenial partners. The discussions with these two are documented in the book *Wittgenstein und der Wiener Kreis*, edited by Brian McGuinness.[3] In that book the story actually ends with a meeting on 9 December 1931, following Waismann's records. There is one unconnected later fragment, nothing more.

Some interpreters have drawn the conclusion that after this single meeting Wittgenstein must have lost faith in Waismann.[4] However, Wittgenstein during his Christmas vacation of 1931 had several meetings with Waismann and the meetings continued, even intensified, next Easter. Schlick, however, did not participate in these meetings. He was a visiting professor in the U.S.A. The meetings did not end, but Waismann changed his method of recording them. The hypothesis of "lost faith" should be rejected. Without Waismann's testimony of his interaction with Wittgenstein it remains difficult to understand the development of Wittgenstein's ideas during and following this specific stage.

Serious collaboration between Wittgenstein and Waismann began immediately following the meeting on 9 December 1931, not a few years later, as is usually thought. Gordon Baker's edition *The Voices of Wittgenstein* is a first guide to this joint work, but its artificially constructed thematic structure is misleading without studies of the original archives.[5] In addition to the sources in the Vienna Circle archive, a great number of relevant shorthand notes by Waismann still remain untranscribed in his notebooks which are preserved in the Bodleian Library, Oxford. Baker's edition is based on typewritten items only and from this single archive.

Even without knowledge of the untranscribed notes it is possible to see that there was a much greater accord between Wittgenstein and Waismann than is usu-

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2 C. G. Hempel to H. Reichenbach, 15.12.1929. Archives for Scientific Philosophy (=ASP), Pittsburgh, HR 014-28-12.

3 F. Waismann, *Wittgenstein und der Wiener Kreis*. Aus dem Nachlass herausgegeben von B. F. McGuinness. Basil Blackwell, Oxford 1967 (=*WWK*).

4 For the best known, see R. Monk, *Ludwig Wittgenstein. The Duty of Genius*. Vintage, London 1991, p. 321.

5 G. Baker (ed.), *The Voices of Wittgenstein. The Vienna Circle. Ludwig Wittgenstein and Friedrich Waismann*. Routledge, London and New York 2003 (=*VoW*).

ally presupposed. For instance, did Waismann, in contrast to Wittgenstein, persist in advancing "theses"? In his typewritten but unpublished teaching materials from the year 1932 Waismann explained:

> Man sieht also, dass Fragen, die auf „Beunruhigung" beruhen, nicht sachlich, d.h. durch Eingehen auf ihren Inhalt und Partei-Ergreifen zu lösen sind, sondern dass es vielmehr nötig ist, sie durch methodische Kritik zu entwirren. Es gehört also gar nicht in den Stil unserer Auffassung, etwas zu behaupten, d.h. irgendwelche Thesen aufzustellen.[6]

## 2

Wittgenstein had promised Schlick a book, or, at least, so the matter was understood by Waismann who gave this news to his master in Vienna. Schlick wrote to Wittgenstein about this on 8 May 1930, absolutely happy: "Sie tun damit wirklich ein gutes Werk." Much had happened in Wittgenstein's development after that date, but Wittgenstein had not forgotten his promise to the professor in his native city, even if he may have had only a partial fulfilment in mind.

At the end of October 1931, Wittgenstein felt that he should inform Schlick in some appropriate manner about the *c.* 400 pages, prepared on the basis of his manuscript volumes, which he had now in typescript. This was the main part of TS 211, as it is known today, but some of the manuscripts to be included into it were still missing, not yet written. Wittgenstein mentioned to Schlick that he had no time to write any short exposition of his views, but he believed that he had now gone through all important topics. A book would be finished within one year. That proved to be an illusion, of course. Wittgenstein was soon writing further notebooks, and, when he came back to Vienna, dictating his new ideas to Waismann.

Schlick had urged Wittgenstein to help Waismann to finish the book on the *Tractatus.* He still expected it for his series. Wittgenstein agreed that it was a pity that Waismann's book had been so long in the making, but at the same time he explained that he was not interested in a book on that early work of his, containing, as it did, several errors. He wrote to Schlick on 20 November 1931:

> […] auch ich kann mein Versprechen – wenn es ein's war – Ihnen lieber H. Professor einen vernünftigen, oder verständlichen, Auszug aus meinen Manuskripten zu schicken, nicht halten. Nebenbei: alles oder doch das meiste was „Elementarsätze" oder „Gegenstände" betrifft hat sich nun als fehlerhaft erwiesen, & mußte gänzlich umgearbeitet werden. […] Nur eine Bemerkung möchte ich machen, obwohl ich nicht weiß, ob sie Ihnen helfen kann: vielleicht den Hauptunterschied zwischen der Auffassung des Buches [*Tractatus*] & meiner jetzigen ist, daß ich einsah, daß die *Analyse* des Satzes nicht im Auffinden verborgener Dinge liegt, sondern im *Tabulieren*, in der **übersichtlichen Darstellung**, der Grammatik, d.h. des grammatischen Gebrauchs, der Wörter. Damit fällt alles Dogmatische, was ich über

6 I am editing this and a number of related documents for publication in a book preliminarily entitled *Innenansichten des Wiener Kreises*.

„Gegenstand", „Elementarsatz" etc. gesagt habe. Will man z.B. das Wort „Gegenstand" verstehen, so sehe man nach wie es tatsächlich gebraucht wird.[7]

With that letter Wittgenstein killed off all hope for *two* books, Waismann's planned exposition of the *Tractatus* and his own promised one—whatever his promise had exactly been. New ideas were growing in his mind after he had completed the c. 400 pages for the TS 211.

Schlick knew that Wittgenstein had rejected the independence of elementary sentences. This had been the topic of Wittgenstein's discussion with Schlick and Waismann already on 2 January 1930. At that date Wittgenstein had still held the view that analysis must lead to an "immediate connection of objects". Now Wittgenstein thought that his conception of logical analysis had been a misleading attempt to find something hidden. On 28 November 1931, he wrote in MS 112, p. 133v-134r:

Die Idee Elementarsätze zu konstruieren (wie dies z.B. Carnap versucht hat) beruht auf einer falschen Auffassung der logischen Analyse. Sie betrachtet das Problem dieser Analyse als das, eine *Theorie* der Elementarsätze zu finden. Sie lehnt sich an das an was in der Mechanik geschieht wenn eine Anzahl von Grundgesetzen gefunden wird aus denen das ganze System hervorgeht.

Meine eigene Auffassung war falsch: teils, weil ich mir über den Sinn der Worte „in einem Satz ist ein logisches Produkt *versteckt*" (und ähnlicher) nicht klar war, zweitens weil auch ich dachte die logische Analyse müsse verborgene Dinge an den Tag bringen (wie es die chemische und physikalische tut).

A similar fate befell the "objects", supposedly discovered when analysis was continued far enough. Wittgenstein was not only concerned with "objects", now in the everyday sense in contrast to the Tractarian dreams, but with objects, samples, labels and names and everything relevant to their context, including the role of ostension. On 15 July 1931 Wittgenstein had written in his MS 111, p. 15-17, something which would prove to be uniquely important for him when he had developed his tools further:

Augustinus, wenn er vom Lernen der Sprache redet, redet ausschließlich davon wie wir den Dingen Namen beilegen, oder die Namen der Dinge verstehen.
Hier scheint also das Benennen Fundament und Um-und-Auf der Sprache zu sein. (Und was Augustinus sagt ist für uns wichtig weil es die Auffassung eines natürlich-klar denkenden Mannes ist, der von uns zeitlich weit entfernt gewiß nicht zu unserem besonderen Gedankenkreis gehört.) Diese Auffassung des Fundaments der Sprache ist offenbar äquivalent mit der, die die Erklärungsform „das ist …" als fundamental auffaßt. – Von einem Unterschied der Wörter redet Augustinus nicht, meint also mit „Namen" offenbar Wörter wie „Baum", „Tisch", „Brot" und gewiß die Eigennamen der Personen, dann aber wohl auch „essen", „gehen", „hier", „dort"; kurz, alle Wörter.

7 L. Wittgenstein to M. Schlick, 20.11.1931. VCA 123/Wittg-15.

Gewiß aber denkt er *zunächst an Hauptwörter* und die übrigen als etwas, was sich finden wird. (Und Plato sagt, daß der Satz aus Haupt und Zeitwörtern besteht.)
Sie beschreiben also das Spiel einfacher, als es ist …
Dieses Spiel kommt aber wohl in der Wirklichkeit vor. Nehmen wir etwa an ich wolle aus Bausteinen ein Haus bauen, die mir ein Andrer zureichen soll, so könnten wir erst ein Übereinkommen treffen, daß ich auf einen Stein zeigend sagte „Das ist eine Säule", auf einen andern zeigend „das ist ein Würfel", – „das ist eine Platte" u.s.w.
Und nun bestünde die Anwendung im Ausrufen jener Wörter „Säule", „Platte" etc. In der Reihenfolge wie ich sie brauche. […]
Ich will damit sagen: Augustinus beschreibt wirklich einen Kalkül; nur ist nicht alles was wir Sprache nennen dieser Kalkül.

Every reader knows the story of Wittgenstein's builders, but it did not as yet have the prominent place which Wittgenstein later allotted to it, beginning in Cambridge with his dictation of the *Brown Book* from October 1934 on. Wittgenstein was toying with the analogy of language and games, but he had not yet at this stage coined the term "language-game" for the primitive uses of language and for his invented but quite ordinary objects of comparison. This had to wait until the spring of 1932, when it occurs in notes immediately before his Easter vacation in Vienna.

When Wittgenstein began writing MS 112, he still believed in the distinction between "primary" and "secondary" signs. Approaching the end of the manuscript, he had rejected the distinction, together with the similes connected with it. He was now free to understand language as "autonomous" in a sense, but with an important restriction which was indicated, for instance, in MS 112, p. 16r: "Die Regel in dieser Form bringt das Spiel schon mit dem Leben in Zusammenhang."

This could be described as Wittgenstein's fresh start. By a "fresh start" I do not mean any sharp dividing line. No single line divides the earlier Wittgenstein from the later, despite the fascination such an idea has had on many writers. There were many dividing lines, as well as connecting points, and perhaps more turbulence than on any other period. The fresh starts, sometimes also false starts, about which I speak, were painfully prolonged ones and did not come out of the blue.

In his pocket notebook MS 153a, p. 76v, Wittgenstein expressed forcefully: "Es ist wahr: Namen können Dinge vertreten; aber sie vertreten nicht ihre Bedeutungen und die Dinge (etwa räumliche Gegenstände) die Bedeutungen der Wörter zu nennen ist absurd." He accepted that he was the bearer of the name "Ludwig Wittgenstein", but rejected the idea that he was the meaning of that name. "Aber zeigen wir nicht zur Erklärung der Bedeutung auf den Gegenstand den der Name vertritt? Ja; aber dieser Gegenstand ist nicht die Bedeutung obwohl sie durch das Zeigen auf diesen Gegenstand bestimmt wird." He copied soon these notes to MS 111 and the distinction between the meaning of a name and its bearer remained a presupposition of his discussions in the MS 112.

Wittgenstein began his first "big typescript", the TS 211, with the MS 111, but then, curiously, he went back to his older manuscripts. In September 1931, he had already shown Waismann the first 90 pages, actually the most up to date portions of it. When he informed Schlick about the typewritten "c. 400" pages, most of this consisted of rewritings of the older manuscripts. Only when he began to write new manuscripts beginning with the MS 112, did he reach fundamentally new insights. He could not lend his manuscript books to Waismann, because he was working on them. But he could dictate his new conceptions to Waismann.

I will here draw attention to one example, because it stands for many more. This is the example of "Moses", which first appeared in Wittgenstein's writings in MS 112 on 15 November 1931, and soon again, p. 93v-94v and 97r etc. It was preceded by examples concerning the names "Napoleon" and "Ludwig Wittgenstein". The problem was how Russellian descriptions apply in such special cases. Wittgenstein was prepared to leave the case more open than Russell had done. In Wittgenstein's opinion, this did not distort the use of language. We used names and many other words without a "fixed meaning".

Waismann often rushed to explain Wittgenstein's new ideas to his colleagues and students. He did so this time too, now in his proseminar. Although the notes made by the student delegated to this task are not extensive, they demonstrate that Waismann declared grammatical clarification as the task of philosophy, referring to Wittgenstein, and continuing early in February 1932: "Begriffe haben keine scharfe Grenzen, deshalb muss man für wissenschaftliche Zwecke willkürliche Grenzen ziehen." He commented the many different ways of describing or defining the name: "Moses hat, wie viele andere Begriffe, eine schwankende Bedeutung."[8] Thus, two months after the first appearance of "Moses" in Wittgenstein's MS 112 it appeared in Waismann's teaching.

## 3

Wittgenstein met Waismann seven times during his Christmas vacation—on 9, 11, 17, 24, 26 and 28 December 1931 and on 9 January 1932—and eleven times during his Easter vacation—on 14, 16, 25, 26, 28, 29, 30 March and 4, 5, 6 and 7 April (including a telephone contact with Waismann on 28 March)—in order to dictate his most recent ideas.[9] Wittgenstein left Vienna only on 16 April 1932.

Wittgenstein was not simply reading from completed manuscripts in order to obtain a typescript—that would have been done for him by the office of his family in Vienna. It was not a task for Waismann, his trusted collaborator in philosophical matters. Rather he was searching ambitiously good and new expressions and formulations for his rapidly developing thought. He could not conceal from Wais-

8 Proseminar-Protokoll WS 1931/32. Thema: Aufgabe der Philosophie. Beginn: 4.11.1931. VCA 074/B.56.

9 I am indebted to B. F. McGuinness who has given me the dates of these and many other meetings between Wittgenstein and Waismann, which he learnt from Wittgenstein's pocket-diaries, when they were in his possession.

mann, who was familiar with the material and had many criticisms of his own to make, that completely new explorations were necessary.

Something of this can be seen from Wittgenstein's own words in his two letters to Schlick in America. The first one was from March 1932: "Haben Sie Waismann's Aufzeichnungen, die ich zu Weihnachten diktierte, erhalten?"[10] The news about this had even reached Herbert Feigl in the U.S.A. He wrote to Schlick on 21 February 1932: "Dass Sie von Waismann gute Nachrichten haben, freut uns sehr. Auf Umwegen hörten wir, dass Wittgenstein ihm zu Weihnachten wieder viel diktiert hat. Wir sind leicht gespannt."[11]

Wittgenstein's second letter to Schlick, dated 6 May 1932, was more informative. It concerned Waismann's *new* book plan, certainly it was no longer to be a book on the *Tractatus*, in Wittgenstein's eyes, by now discredited: "Wie Sie bemerkt haben werden, habe ich Waismann bei der Abfassung seines Buches zu Ostern wieder Aufgehalten und er hat mit größter Geduld gewartet, wenn ich unter Druck, tropfenweise, Erklärungen aus mir heraus gepreßt habe."[12]

The results of these sessions were impressive. On 29 May 1932 Schlick warned Carnap that he should not make his forthcoming book on "metalogic" (i.e., his *Logische Syntax der Sprache*) too big, because the size of Waismann's work in progress would predictably already be making trouble with the publisher:

> […] zumal jetzt nach Waismann's neuestem Plane auch sein Buch wegen der Fülle des neuen Materials in zwei Teilen erscheinen soll. Dies neue Material stammt von Wittgenstein selbst, der zu Weihnachten und Ostern ungeheuer eingehend mit Waismann diskutiert hat und in dem Buche seinen jetzigen Standpunkt dargestellt zu sehen wünscht, den er selbst nur in einer großen Menge von Aphorismenbüchern dargestellt hat. Seiner Hilfe ist es zu danken, wenn der 1. Teil jetzt im Sommer fertig wird.[13]

The two volumes were intended to be about Wittgenstein's current philosophy. Wittgenstein was relying on Waismann more than ever before. One of Wittgenstein's correspondents was worried about the development. W. H.Watson wrote to Wittgenstein on 25 May 1932: "I was very disappointed to read in your last letter you have been despondent about ever preparing your work for the press yourself, and hope that this has been a merely temporary phase occasioned by influenza from which I hope you have now quite recovered."

The news from the meetings on the Easter were spreading. Otto Neurath had been the staunchest critic of Tractarian philosophy within the Vienna Circle. Now, on 10 May 1932, he commented to Carnap: "Angeblich gedeiht Waismann's Buch, aber Wittgenstein gebärt ständig rätselhafte Thesen, die sich schwer immer voll umformen lassen. Eine komische Art Bücher zu verfassen. Wittgenstein sollte das

10 Quoted according to WWK, p. 24.
11 H. Feigl to M. Schlick, 21.2.1932. VCA 099/Fei-21.
12 L. Wittgenstein to M. Schlick, 6.5.1932. VCA 123/Wittg-16.
13 M. Schlick to R. Carnap, 29.5.1932. ASP RC 029-29-12.

Buch schreiben und Waismann ihm assistieren. Aber bei Sektierern ist alles anders. Die Hauptsache ist, dass ihre Sektenlehre sich der wahren Lehre, wie wir sie jetzt vertreten, immer mehr annähert."[14]

Carnap had been happy already on 24 March 1932, remarking in his diary: "Waismann. Sein Buch wird bald fertig. Wittgenstein nähert sich uns sehr. Kein Vergleich mehr zwischen Sätzen und Wirklichkeit, alles ‚grammatisch'? Aber trotzdem noch allerhand beinahe phänomenologisch anmutende Überlegungen, was ‚Bedeutung' sei."[15]

Neurath did not say anything more than what is quoted above about the content of the presupposed convergence. A joint work, in some sense, seemed to be forthcoming. But who would be its author? Wittgenstein would have been satisfied, the evidence suggests, if Waismann had succeeded in giving the systematic presentation of his new ideas for the Vienna Circle series. He did his best to promote this goal. Waismann was at that stage his assistant in every possible sense and Schlick, Waismann's well-meaning superior, also with the access to a reliable publisher. Schlick was prepared to do everything, even to abbreviate Carnap's work, in order to secure the publication of Wittgenstein's – or Waismann's – book, or books, or the joint book, whatever the outcome might finally be.

The plan that Wittgenstein had in the late spring of 1932 was that he would organize his remarks and his groupings of them into a dictionary. He wrote in his pocket notebook MS 154, p. 1r: "Der Titel meines Buches: ‚Philosophische Betrachtungen. Alphabetisch nach ihren Gegenständen/Themen/geordnet/aneinandergereiht/nach Stichwörtern geordnet." The note on p. 9v-10r of that manuscript changed the title to "Philosophische *Bemerkungen*".

Josef Rothhaupt has convincingly demonstrated that this was not just a fancy occasional idea. As time went on, Wittgenstein really did this work with his complicated and previously unexplained numbering system, explained by Rothhaupt.[16] A Wittgenstein dictionary by Wittgenstein himself could really be published, unfortunately covering only a part of the transitional period. In the end, Wittgenstein apparently felt that this experiment was a failure. A typescript along these lines was never produced. TS 213 replaced it.

Meanwhile, Waismann had received Wittgenstein's freshest ideas through discussions and dictations. There is no reason to think that all of them found their way to Wittgenstein's own manuscripts or typescripts.

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14 O. Neurath to R. Carnap, 10.5.1932. ASP RC 029-12-49.

15 R. Carnap, Tagebücher 1.6.1930–30.6.1933. Transcription. VCA 585/X.42-2.

16 J. Rothhaupt, *Kreation und Komposition: Philologisch-philosophische Studien zu Wittgensteins Nachlass* (1929-1933). Habilitationsarbeit. Ludwig-Maximilians-Universität München 2008, p. 258-297 and 593-662.

## 4

Waismann left behind him documents both deriving from Wittgenstein's Christmas vacation 1931 and the Easter vacation 1932. Unfortunately, almost all of Waismann's documents are undated, and the ones most immediately recording Wittgenstein can be found only in shorthand form. This may sound like a "mission impossible", but a number of interesting observations are still possible even without transcriptions of the shorthand notes.

The texts following after Wittgenstein's and Waismann's meeting on 9 December 1931 have not been studied, because they have not been identified as such. There is a simple reason to suspect that they can be found among Waismann's typescripts, in addition to the original shorthand notes. Schlick was using the Stolze-Schrey shorthand current in North Germany, Waismann the older Gabelsberger shorthand current in the former Austria-Hungary. Only specialists in shorthand systems could read both. It was necessary for Waismann to produce a typescript of his shorthand notes to make them available to Schlick, either dictating them to a student or typewriting them himself. In both cases he was responsible for the result, but it is important to observe that his task was *not* to produce an interpretation or advance his own views.

As already noted, Wittgenstein wrote to Schlick: "Haben Sie Waismanns Aufzeichnungen, die ich zu Weihnachten diktierte, erhalten?" Certainly, both Wittgenstein and Schlick were expecting that Waismann would do the job. For a person so dependent on the two as Waismann, it was impossible to leave the job undone, especially as it also was extremely interesting for Waismann himself to see the product in a clear form. It can be supposed that it contained a number of reasons for rejecting plans to write a book on the *Tractatus*, in addition to what Wittgenstein had said about "theses" in philosophy during the first discussion. Has the document survived?

Some initially fascinating hypotheses must be rejected. I believe that Joachim Schulte is correct in seeing the so-called *Diktat für Schlick* as not delivered by Wittgenstein.[17] It does have connections with Wittgenstein's conceptions dating already from 1931, but it also draws heavily on Wittgenstein's manuscripts and ideas from much later dates. I see it as a presentation by Waismann for Schlick's seminar early in 1935, in this sense a dictation *für Schlick*, alhough not by Wittgenstein.[18] It was written down on this occasion in shorthand by Schlick and also by Rose Rand, both of them listening to Waismann's presentation.[19]

However, there is a source which fits perfectly to the context. At first sight, it cannot be easily observed, although it is printed in the *VoW*. This sounds strange,

17 In this volume.

18 I will go into the details of this elsewhere.

19 M. Schlick, Wittgenstein, VCA 183/D.1 and R. Rand, Wittg. Manuscript [Title added later], ASP RR 11-16-3.

and in a sense it is. In order to explain such a curious phenomenon we need some facts concerning the original archive materials, both in Oxford and in Haarlem. First, Waismann's report to Schlick about Wittgenstein's dictations can only be found *among* other texts of different origins. Second, despite this it is possible to identify the texts belonging to the original report. This can be done simply by taking a look at Waismann's original notebook. Third, presupposing that Waismann's report was given to Schlick and it has survived, we should be able to find the the typescripts belonging to it also among Schlick's papers in the Vienna Circle archive, not only in Oxford. In fact, they are in Haarlem, but classified falsely.

The example concerning the name of Moses appeared in Wittgenstein's manuscripts for the first time shortly before his Christmas vacation in Vienna 1931 and in Waismann's teaching soon after that. If we follow the track of "Moses", we will find this example discussed in *VoW*, p. 212-216, in an item called 'Vagheit'. The text belongs to Waismann's *Notizbuch I.* Already the first pages of that notebook begin with a consideration of "names" in this new sense. Quite similar items can be found in another series of Waismann's shorthand leaflets, called *Vorarbeiten 1-8*, more exactly in *Vorarbeit 3*, where the items picked up from the *WWK* notes end and new titles begin.

We could continue such observations, but a solution can be found directly in Haarlem. There is a series of typewritten pieces, most of them containing 1-4 pages. Each of such "remarks" is paginated individually and has its own title. No author is mentioned and there are no dates. In the Vienna Circle archive there is a collection of papers classified as Schlick's "Wittgensteiniana", but these pieces are in another collection, entitled "Aufzeichnungen über Sitzungen des Wiener Kreises". They have the archive numbers 188/W.17-36, that is to say twenty items in all. The main bulk under this classification consists of minutes from the meetings of the Circle, covering the period from 4 December 1930 to 25 June 1931. Rose Rand had made shorthand notes from these meetings. Many years later Otto Neurath asked her to produce typescripts of the material and also paid her for the job when it was done. These minutes have now been published by Friedrich Stadler.[20]

Neurath sent a similar query to Waismann, who replied that he might have some minutes from the Circle. Rand was using the same Gabelsberger shorthand as Waismann. Now she had arrived in England as a penniless refugee, and Neurath was searching some job for her. When Neurath received the documents, he commented: "Vielen Dank für die Zirkelprotokolle, die Sie bald zurückbekommen. Sind das alle, die Sie haben?"[21] Waismann: "Die Zirkelprotokolle, die ich Ihnen

20 F. Stadler, *Studien zum Wiener Kreis. Ursprung, Entwicklung und Wirkung des Logischen Empirismus im Kontext*. Suhrkamp, Frankfurt am Main 1997, p. 275-334.

21 O. Neurath to F. Waismann, 10.7.1939. VCA 318.

sandte, sind alles, was ich davon besitze."[22] The number was apparently small.[23] This does not fit well to the collection of some twenty items mentioned above. In fact, Waismann had only a few protocols from the Circle, published by Stadler. If Neurath had received the larger collection, he would immediately have observed that they cannot be *Zirkelprotokolle* at all.

Back to Oxford and to Waismann's collection there. There is an exact correspondence between Haarlem's 188/WK.17ff and Waismann's typescripts numbered as F.2, 3, 21, 27-41 in the Bodleian Library. These and only these, classified into the huge collection *Ältere Reste* among Waismann's papers, are carbon copies. Where are the top copies? Back to Haarlem … Of course, there should be copies in exactly these two different archives, if *this* is the report to Schlick about Wittgenstein's dictations produced by Waismann.

When we look at the original shorthand notebook *NB I*, we can see that all these items are derived from it. The items to be typewritten are marked by Waismann with asterisks. In his edition *VoW*, Baker observed these asterisks, but he had no explanation for them.[24]

But then another problem emerges. Baker announced in the introduction that he had followed the "order" of the items in *NB I*. This is true. However, while following the order he attached to the texts of the notebook a great number of items, somehow thematically connected to the originals. This is not mentioned, at least in the posthumous English edition. What is published in the *VoW* as *NB I* is a construction made by the editor from quite different elements in Waismann's legacy, not the real, historically true *NB I*.

Let us take as an example the small study entitled "Folgen die Regeln aus der Bedeutung?". It defends the idea that the meaning of a word is constituted by the rules for its employment; rules do not follow from meanings. The paper consists of a criticism of four arguments by which Gottlob Frege defended the contrary opinion, complete with quotations from Frege and with references to the relevant paragraphs. The basic idea is certainly Wittgenstein's, but not the scholarly use of apparatus. In fact, when we compare the text to the last chapter of Waismann's book *Einführung in das mathematische Denken*, we will find the very same reasoning there, often word for word.[25] At the end of the book, Waismann indicated his sources and in particular said that he had taken the first of the four criticisms from Wittgenstein's unpublished manuscript on the foundations of mathematics.[26] But does not *all* of it sound like Wittgenstein? For the present, it is enough for us to see that the discussion belongs to the context of preparing a book which appeared

22 F. Waismann to O. Neurath, 12.7.1939. VCA 318.

23 Cf. Stadler, ib., p. 357-360.

24 Baker, ib., p. xlv.

25 F. Waismann, *Einführung in das mathematische Denken. Die Begriffsbildung der modernen Mathematik*. Mit einem Vorwort von Professor Dr. Karl Menger. 2. Auflage. Gerold & Co., Wien 1947 [1936], p. 162-167.

26 Waismann, ib., p. 168.

in 1936. If we look at the original manuscript of the *NB I*, the text cannot be found there. There are items which are rewritings or elaborations of Wittgenstein's ideas and there are even small studies which apparently have nothing to do with Wittgenstein.

The typewritten report to Schlick contained the following items from the original *NB I*, in this order: ‚Kausale Auffassung der Sprache', ‚Was ist ein Befehl?', ‚Verifikation 1', ‚Philosophie', ‚Das Folgen und die W-F-Notation', ‚(Ein Gleichnis) Regel und Bedeutung', ‚Allgemeinheit 1', ‚Das Hineinsehen der Allgemeinheit', ‚Russells Logik', ‚Tautologie', ‚Vagheit', ‚Verbindung der Sprache mit der Wirklichkeit', ‚Rechtfertigung der Grammatik', ‚Allgemeinheit 2', ‚Elementarsätze', ‚Zusammengesetztheit', ‚Was ist eine Regel?', ‚Verifikation 2', ‚Hypothese', ‚Philosophie'.

Waismann certainly did some editing in producing the typescripts, but one should keep in mind that his purpose in producing these typescripts was to produce a readable presentation of Wittgenstein's recent dictations. To stay as close to the original dictations as possible seems to have been his principle, but it must be admitted he was not making a historico-critical edition of these dictations. Still, following this key, it is possible to read Waismann's typescript as report to Schlick about Wittgenstein's fresh thoughts.

By the way, some of the last items, which are not mentioned in the catalogue above, were omitted in the typescripts, similarly a few earlier ones entitled 'Strukturbeschreibung', 'Interne Relation' and 'Russells Klasse von Aspekten'. For instance, the one on structural descriptions was designed as a counter-argument to Schlick and Carnap. It was probably written by Waismann, who had free time between the dictations, but whatever it was, Waismann did not want to present it to Schlick.

It is an interesting observation that in these and only in these of Wasimann's typescripts we find Wittgenstein speaking about his changing views in the first person. This fits perfectly the purpose of these typescripts, if they were intended for Schlick as a report of Wittgenstein's dictations. Normally we see only impersonal references, but in these passages Waismann had no reason to transform the first person use.

## 5

It was Wittgenstein who was speaking in Waismann's report to Schlick, not Waismann himself. Consider the following passages:

*‚Verifikation'*: Ich kann durch die Angabe der Verifikation nicht die Sprache mit der Welt verbinden. Das kann ich überhaupt nicht. […] Ich kann mich nur an den Gebrauch der Sprache *erinnern*.

*‚Das Folgen und die W-F-Notation'*: Daß alle Schlussfolgerung in der W-F-Notation geschieht und ein Übergang in der W-F-Notation ist, darin habe ich ganz richtig gesehen. Doch habe ich einen anderen grundlegenden Fehler gemacht: Wenn aus dem Satz p der Satz q folgt, so meinte ich, dass p.~q ein Widerspruch sein müsse und darin sah ich ganz recht. Daraus glaubte ich weiter schließen zu müssen, dass, wenn ~p aus q folgt, ~p in irgendeinem Sinn in q enthalten sein müsste. In welchem Sinn der eine Satz in dem anderen enthalten ist, habe ich nicht klar angesehen.
*‚Regel und Bedeutung'*: Mein Irrtum war die falsche Auffassung der Analyse, nämlich die Auffassung, dass im Satz etwas verborgen ist, eine Struktur, die man ans Licht ziehen müsse. Ich hatte die Auffassung – die durch unseren irreführenden Sprachgebrauch erzeugt wird – als sei der Sinn eines Ausdrucks gleichsam *hinter dem Ausdruck* versteckt. […] Die Auffassung gegen die ich mich hier wenden möchte, ist nun die: Man holt die Regeln der Grammatik aus den Figuren heraus. Das ist irreführend.
*‚Rechtfertigung der Grammatik'*: Ich sah nun, dass die Schlüsse: Wenn etwas rot ist, ist es nicht grün, wenn es 6h ist, ist es nicht 8h, wenn ein Mensch 1.8 m groß ist, dann ist er nicht 1.5 m groß […].
*‚Elementarsätze'*: Meine Auffassung der Elementarsätze hängt zusammen mit meiner früheren falschen Auffassung von der Analyse. Ich war mir nicht im klaren darüber, was es heißen soll, ein Satz müsse sich in Elementarsätze auflösen lassen, wenn er nicht aufgelöst ist. […] Allerdings gilt dann nicht, dass die Elementarsätze unabhängig voneinander sind.
*‚Zusammengesetzheit'*: Ich hatte früher gemeint, daß jeder Satz zusammengesetzt sei. Diese Meinung hing mit meiner damaligen Auffassung von dem Komplex zusammen. […] Aber diese Analogie ist irreführend und hat mich tatsächlich irregeführt, wenn ich nämlich von Komplexen geredet habe, welche den Sätzen entsprächen. Man kann, wie gesagt, den Sessel einen Komplex nennen. Man könnte auch einen angestrichenen Sessel und den braunen Farbüberzug (das Pigment) einen Komplex nennen. Dagegen Sessel und braun (die Farbe braun) einen Komplex nennen, wie ich es früher getan habe, ist ein Mißbrauch. Subjekt und Prädikat geben keinen Komplex.

It is here presupposed that the intended reader knows who the "ich" is. Schlick certainly did know. When Waismann produced the report, there was no need to distance the text from Wittgenstein's first person usage.

Could it even have been possible for Waismann to do so? It would have presupposed a huge job of interpretation, something like what was indicated above in the quotation from Neurath. But that quotation is from the period after the Easter meetings. On that occasion Waismann already had a different purpose, a book about the "new Wittgenstein". The hard decision to reject years of work with the *Tractatus* was softened by Wittgenstein's openness about his present thoughts.

The possible hypothesis that in the report to Schlick Waismann took it on himself to play the part of Wittgenstein must be rejected. There is no reason to suspect that the first person quotations by Wittgenstein are not genuine. They are the only ones of that kind in Waismann's *Nachlass* outside the *WWK*, as far as I can see. What makes Waismann's report fascinating is its character as a unique snapshot, blurred or not. It is possible to make a number of inferences concerning this stage of Wittgenstein's and Waismann's careers and their interaction, but a more detailed study urgently needs a transcription of Waismann's shorthand notes.

## 6

The report to Schlick did not contain “language-games”. This expression was coined by Wittgenstein only during the next spring, but he had been playing with the analogy of games, mathematics and language already for a while.

In *Vorarbeit 6* we find an item called ‘Sprachspiele’, similarly in *NB III*, where there is a text entitled ‘Welche Rolle spielen falsche Sätze in einem Sprachspiel?’ Wittgenstein’s earliest opportunity to teach Waismann language-games as an object of comparison seems to have been the Easter of 1932.

Waismann’s activities after he had taken notes of Wittgenstein’s dictations on the Easter of 1932 are excellently documented in three different sources. First, there are the handwritten protocols made by his students from his lecturing in the proseminars and the following discussions. Second, there is Waismann’s own typewritten report from this proseminar, presented to Schlick and probably to the students participating in his teaching. Third, there is a typewritten ”continuation” by Waismann to this proseminar, apparently never held, because of the emotionally loaded plagiarism controversy between Wittgenstein and Carnap.[27]

Despite his good intentions, Waismann was not an outsider in this confrontation, as recognized even by Wittgenstein. Waismann was responsible for transmitting the new ideas to the Vienna Circle and his students. The controversy created a distance between Wittgenstein and Waismann, and Waismann could no longer present Wittgenstein’s ideas as openly as he had done earlier. This was in not the end of the collaboration between the two, but there was going to be a long pause in Waismann’s explanations of Wittgenstein’s ideas for Schlick’s Circle.

In his proseminar during the summer term of 1932, Waismann did not yet have these restrictions. He spoke quite freely about what he had recently learned from Wittgenstein, beginning with the genre pictures. The proseminar had nine sessions—on 2, 9, 23 and 30 May, 6, 13, 20 and 27 June and 4 July.

In Wittgenstein’s relevant manuscript MS 113 there is a gap as concerns the period of his Easter vacation. Afterwards in Cambridge, on 18 April, Wittgenstein seems to have briefly reflected what had been going on: “Glauben. Hiermit verwandt: erwarten, hoffen, fürchten, wünschen. Aber auch zweifeln, suchen, etc.”

The Easter meetings were a series of discussions and dictations on intentionality. Wittgenstein and Waismann had discussed this topic for the first time on the

27 A recent summary of the studies concerning this controversy is D. Stern, ‘Wittgenstein, the Vienna Circle, and Physicalism: A Reassessment’, in A. Richardson and T. Uebel (eds.), *The Cambridge Companion to Logical Empiricism*. Cambridge University Press, Cambridge 2007. However, Carnap’s point of view was earlier that of two universal languages, rudiments of which survived, see J. Manninen, ‘Beginning the Logical Construction of Cognition’, in S. Pihlström et al. (eds.), *Approaching Truth. Essays in Honour of Ilkka Niiniluoto*. College Publications, London 2007, and T. Uebel, *Empiricism at the Crossroads. The Vienna Circle’s Protocol-Sentence Debate*. Open Court, Chicago and La Salle, Illinois 2007.

streets of Vienna in September 1931, as can be read from *WWK*, p. 166ff. It was certainly Waismann who introduced to Wittgenstein Brentano's thesis, as was but natural for a person who had earlier planned an examination of Husserl's phenomenology. The comments to it were Wittgenstein's. In the Easter meetings of 1932 Wittgenstein had a sophisticated way of dealing with it, which he conveyed to Waismann.

Waismann finished his proseminar protocol as follows: "Wenn man keine Sprache hat, dann lebt man in einer anderen Welt. Die Frage, ob ein Wesen ohne Sprache überhaupt einen Wunsch haben könnte, dürfte verneinend entschieden werden."[28] In his continuation to the protocol, Waismann wrote:

> Hier muss erst ein sehr tief wurzelndes Vorurteil ausgerottet werden, nämlich das Urteil, dass die Sprache nur eine äussere Begleiterscheinung sei […]. Inwiefern kann ein sprachloses Wesen etwas wünschen, etwas hoffen, sich vor etwas fürchten, eine Absicht haben, etc.? Wenn man sagen wollte, der Wunsch ist ein seelischer Vorgang, so würde sich die Frage erheben, wie dieser seelische Vorgang anfängt über sich hinaus zu weisen auf den Gegenstand hin – und wir stünden vor dem alten Problem der Intention![29]

Consequently:

> Es handelt hier eben nicht um gewisse psychische Vorgänge, sondern um diese und um einen Kalkül mit Worten: die intentionale Beziehung des Wunsches liegt ganz in der Sprache. Zieht man das Gewebe der Sprache weg, so bleibt nicht etwa noch ein seelischer Vorgang übrig, der der ‚Wunsch' wäre, einen Apfel zu bekommen, sondern nur ein dumpfes Gefühl oder ein körperliches Verlangen, vergleichbar dem Hunger, der Müdigkeit etc., d.h. mit dem Ausdruck des Wunsches verschwindet auch der Wunsch. Ein Wunsch ohne Ausdruck wäre wie ein Gedanke ohne Worte […].

A part of the background of Wittgenstein's new interest in intentionality and for the linguistic twist he gave it was a long felt dissatisfaction with the causal theory of language, represented by Russell and others. It may have gained new momentum from the fact that the German translation of Russell's *The Analysis of Mind* was discussed in Schlick's seminar early during the summer term of 1929[30] – news that will have annoyed Wittgenstein. There was hardly anyone who had greater success in discrediting his own teachers than Wittgenstein.

At the beginning of the proseminar on 2 May 1932 one of Waismann's students wrote, recording Waismann's words, that Franz Brentano's conception of "intentional inexistence" had been attacked by Dewey, Russell, the behaviorist Watson and representatives of the Vienna Circle, for instance Carnap. The last references were left out from the corresponding typescript, which mentioned here

28 Proseminar-Protokoll SS 1932. Thema: „Die intentionale Inexistenz". VCA 076/B.58.

29 Über ‚Denken', ‚Meinen', ‚Intention'. VCA 078/B.60.

30 Seminar-Protokoll SS 1929. Thema: Russell, Analyse des Geistes. Beginn: 3.3.1929. VCA 056/B.36-1.

only “die Meinungen des Behaviorismus, insbesondere die von Watson (Dewey) und Russell”.

I will not go here into the details of the typescript as it deserves to be published as a whole, which I will do in a series of the Vienna Circle Institute. Waismann was the the person with whom Wittgenstein first discussed language-games, as witnessed by the continuation script to Waismann’s proseminar held in the summer of 1932, a collection of separate pieces which Waismann entitled as *Worin besteht das, was man „Denken“, „Meinen“, „Intention“ nennt? (Fortsetzung vom Sommer-Semester 1932)*. No date is given, but the text seems to consist of discussions based on Wittgenstein’s recent dictations. There is no reason to think that the date was *much* later.

It was the first appearance of Wittgenstein’s language-games outside of his own manuscripts – or, to be more exact, outside of Waismann’s notes of Wittgenstein’s dictations. Waismann was the first one to discuss this topic at some length in his continuation typescript. In Wittgenstein’s teaching in Cambridge, the topic had still to wait for a while. In MS 113, we shall find Wittgenstein’s first notes described as ”language-games”, immediately before his Easter vacation in Vienna and before the gap in his manuscripts during the period in Vienna. The subsequent explosion of dictations to Waismann, covering the ”silent” period, most probably included a more detailed discussion of the language-games, among others. Waismann quoted later a passage that could even have been from an earlier date, but that must be left undecided.

## 7

On 1 March 1932, Wittgenstein wrote into his MS 113, p. 45r, the first remarks about a “primitive language-game”, that of light/dark. He was using the conceptual innovation as if it were already known.[31] According to it, a child learns to connect the words to a light and dark show and express them in new conditions following the changes. Were these words propositions? Was there a correspondence to reality? The first question could be answered with a reference to conventions. The second one deserved to be answered negatively. In the discussion Wittgenstein reminded of the manifold uses of language, but he did not reject the Tractarian idea of the bipolarity of propositions. He imbedded it into the framework of language-games.

Wittgenstein expressed his attitude towards primitive language-games as follows:

31 A. Pichler, *Wittgensteins Philosophische Untersuchungen. Vom Buch zum Album*. Rodopi, Amsterdam and New York 2004, p. 119.

> Wenn ich bestimmte einfache Spiele beschreibe, so geschieht es nicht um mit ihnen nach und nach die wirklichen Vorgänge der Sprache – oder des Denkens – aufzubauen, was nur zu Ungerechtigkeiten führt, – sondern ich stelle die Spiele als solche hin, und lasse sie ihre aufklärende Wirkung auf die besonderen Probleme ausstrahlen.

We know that he would later reject the first part of the methodological remark when he wrote the *Brown Book*, as far as language was concerned, only to reject the whole project of that book when rewriting it. The rest of the remark remained his attitude always.

Then, on 10 March, there followed a further example:

> Welche Rolle spielen falsche Sätze in einem Sprachspiel? Ich glaube, es gibt verschiedene Fälle. I. Einer hat die Signallaternen an einer Straßenkreuzung zu beobachten und einem anderen zu sagen welche Farben sie zeigen. Er verspricht sich dabei und sagt die falsche Farbe.
>
> II. Es werden meteorologische Beobachtungen gemacht und nach gewissen Regeln aus ihnen das Wetter für den nächsten Tag vorhergesagt. Die Vorhersage trifft ein oder nicht.
>
> Im ersten Fall kann man sagen, er spielt falsch; im zweiten nicht –. Man wird hier (nämlich) von einer Frage geplagt die etwa so lautet: gehört die Verifikation noch mit zum Sprachspiel?
>
> Wie schaut die Verifikation aus, – wie geht sie vor sich?

The person who next summer produced Wittgenstein's typescript TS 211 based on these passages went directly from the sentence "Gehört die Verifikation noch (mit (?)) zum Sprachspiel?" to the one following after the Vienna period in MS 113: "Glauben. Hiermit Verwandt: erwarten, hoffen, fürchten, wünschen. Aber auch: zweifeln, suchen, etc."

Was there nothing in between? Wittgenstein expressly left out a passage where he had been wondering about his former school teacher in English and French, Jonas Groag, "a Jew", as he mentioned, not remembering the first name correctly, and this was the person who had been teaching him that "ich [Wittgenstein] die Methode, eine sprachliche Betrachtung mit einer Gruppe von Beispielen zu beginnen [habe]".

Such biographical information may sound very banal. On the other hand it shows that Wittgenstein was very uncertain at this moment, returning to memories of his earliest education. This was his mood when he came to Vienna again, Easter 1932, and had a long record of discussions and dictations with Waismann. At that specific moment, and even afterwards, it was as unclear for Wittgenstein, whether he or Waismann would write "his" book, as it was for Waismann. It was a period in which Wittgenstein was dissatisfied with the turbulence of his own thought, or as he wrote in MS 155, p. 65v-66r: "The first sign of your understanding would be if I began to have your cooperation and this would alter the tone of these discussions which would become that of a quiet search."

One isolated remark, probably referring to the encounter with Waismann or at least to one of the topics discussed can be found in Wittgenstein's pocket notebook MS 154, p. 52v: "Erinnere Dich hier an die Sprachspiele mit grünen und roten [unclear] und den Sinn von wahr und falsch." Wittgenstein was here remembering something that was not documented in his own manuscripts or typescripts, but there are strong reasons to affirm that it was documented by another person, Waismann. It can be found in a typescript of Waismann's and hence in *VoW*, p. 466-472. It is not written in Waismann's academic style. The text looks much more like a record of Wittgenstein's speech.

There is no reason to suspect that Waismann invented these clarifying examples without Wittgenstein's help, simply making use of the short remarks in MS 133. But it is certainly possible that they both were active in their own ways.

The language-game of true and false, presented here, was not designed as a theory of truth, and it did not go beyond everyday language. Actually, a group of three language-games was needed for this purpose, and not all of them were "primitive". In Waismann's "continuation" to the proseminar protocol, the three games are easy to discern from each other.

For the first, a child is asked to pick up a scrap of paper from a pile, following our advice of the game. We say "red" and the child picks up green. This is an error that has nothing to do with "true" and "false". For the second, a lamp is giving red and green light irregularly. We teach the rules for using these two words. The child acts contrary to the rules, but this has still nothing to do with true and false. Those acts do not belong to this game.

The third case is different. The child has to guess whether the lamp will give red or green light. Now and only now one can consider the result to be "true" or "false". One could say that it expresses a thought and means something, but a psychological study is not what is desired. One should remain with the game and consider only it. Next Waismann excluded agreement with reality. The crucial difference was this: "[…] im zweiten Spiel ist die unrichtige Angabe verboten, im dritten Spiel ist sie erlaubt."

Waismann concluded:

> Unsere wirkliche Sprache gleicht nun diesem dritten Spiel: wenn z.B. ein Meteorologe das Wetter vorhersagt und dann seine Prognose eintrifft oder nicht, so ist dieser Fall ganz ähnlich dem Spiel mit dem Erraten; d.h. auch die falsche Prognose war nicht etwa durch die logische Grammatik verboten; der Witz ist gerade der, dass unsere Sprache sowohl die Bildung von wahren als von falschen Sätzen erlaubt und daher ein falscher Satz auf ganz anderer logischer Stufe steht als ein verbotener. (Verbotener Satz = unsinniger Satz.)
>
> Unsere wirkliche Sprache unterscheidet sich von einem primitiven Sprachspiel (wie unser zweites Spiel) dadurch, dass sie zwei Möglichkeiten vorsieht, die in der Sprache selbst als „wahr" und „falsch" unterschieden werden.

This explained also why there was no ostensive definition for a proposition: "[W]enn man einen Satz durch eine hinweisende Definition erklärt, so hat man den Satz gar nicht als Satz verwendet, sondern ihn zu einem Eigennamen gemacht: er benennt eine Situation, aber er beschreibt sie nicht. Ein Satz, den man als Satz versteht, kann wahr oder falsch sein, er beschreibt." The child could be lying, but this was a different, more complicated game again.

It is not possible here to go in greater detail into the rich content of Waismann's teaching material for 1932. The nearest correspondence to it can be found in *VoW*, p. 436-472. I mean the items 'Intention', 'Brentano', 'Bedeutung als seelischer Akt', 'Ist die Bedeutung etwas Einheitliches?', 'Begriff und Vorstellung', 'Sprachspiele zur Aufklärung psychologischer Begriffe' and 'Logik und Psychologie'. More research is needed on this. In which way however these pieces in *VoW* may have been edited by Waismann, there are reasons to believe that the topics had their origin during the Easter of that year.

For Waismann, the language-games of true and false remained a main element in his discussion of propositions in the posthumous *Logik, Sprache, Philosophie*.[32] One could remark that it was built on a presupposition that was later made problematic by Wittgenstein, that of following a rule. On the other hand, the manifold of uses of language was readily acknowledged by Waismann. He was discussing assertions, but he was not blind to the other uses.

Did Wittgenstein forget the language-game of true and false? Two paragraphs in the *PI* could be used to claim the contrary. In § 136, Wittgenstein considered it a bad picture to see "true" as "fitting" to a proposition, as if it were somehow external to the sense of this word and not constitutive of it: "And the use of the words 'true' and 'false' may be among the constituent parts of this game; and if so it *belongs* to our concept 'proposition' but does not 'fit' it." In § 225, Wittgenstein commented: "The use of the word 'rule' and the use of the word 'same' are interwoven. (As are the use of 'proposition' and the use of 'true'.)"

## 8

As time went on, Wittgenstein returned to a game which he had formulated already in MS 111, p. 15-17: the builders. Its first occurrence did not yet have the name "language-game" and it was scrutinized only briefly. However, Wittgenstein became convinced that for the beginning of his book it would help to raise ques-

32 For a reconstruction of the German original, see F. Waismann, *Logik, Sprache, Philosophie*, ed. by G. P. Baker and B. McGuinness with J. Schulte. Philipp Reclam Jun., Stuttgart 1976. The English translation *The Principles of Linguistic Philosophy*, Macmillan, London 1965, edited by R. Harré from proofs corrected by Waismann, makes no attempt to explain the historical context.

tions about names, commands and different types of words. He did not forget to inform Waismann.

During the summer term of 1935, Waismann held a "private seminar", or actually lectured privately on the *Logische Grammatik der Sprache*.[33] And how did he begin? With the builders. Wittgenstein had long pressed Waismann to begin with the simplest grammatical examples and now Waismann was doing exactly that.

By Easter of 1934, Waismann's book had been nearly finished but apparently not yet following this new conception. One may surmise that it was he who produced the typescript of Wittgenstein's MS 140 in this context. When Wittgenstein saw the result, he was unhappy with it. And it was still unclear who would be mentioned on the cover as the author or authors of the joint work. Wittgenstein, in any case, felt that his advice should decide how the book should be written and structured.

Waismann lost his job at the university, despite Schlick's protest and attempt to get him a better position, but soon after Waismann's dismissal Schlick was murdered.[34] Waismann was now without the steady support which Schlick had provided him. Schlick had also been planning a book about Wittgenstein's new philosophy, but it was not written nor were students' notes from his lectures. Schlick's sympathy with Wittgenstein's recent ideas, dating from their happy days together at Istria in September 1933, is reflected especially in the final part of Schlick's lectures during the winter term 1933/34.[35] It was unclear whether a new book by Wittgenstein would ever appear, as Wittgenstein had his plans to move into Russia and leave philosophy altogether. Waismann felt completely alone with his book plans, but even under these conditions there was no other possibility than to try to go on. This was expressed in a letter to Carnap: "Da Schlick leider nicht mehr zur Ausarbeitung der neuen Erkenntnistheorie gekommen ist, wenn ferner auch Wittgenstein, wie er mir wiederholt sagte, seine Ideen nicht publizieren wird, so bin ich heute der einzige, der diese Gedanken kennt und sie zu entwickeln vermag."[36] His book would probably be the only source on Wittgenstein's new philosophy, so he thought, produced with Wittgenstein's permission and indeed extensive help.

In an undated letter to Karl Menger, apparently from late summer of 1936, Waismann explained the changes of the book plan. At Easter 1934 Waismann had shown Wittgenstein the text he then had. He received in return a radical new suggestion:

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33 To be published in *Innenansichten des Wiener Kreises*.

34 Stadler, ib., p. 578-580.

35 See M. Schlick, *Die Probleme der Philosophie in ihrem Zusammenhang. Vorlesung aus dem Wintersemester 1933/34*, ed. by H. Mulder, A. J. Kox and R. Hegselmann. Suhrkamp, Frankfurt am Main 1986.

36 F. Waismann to R. Carnap, 4. 3.1937. Quoted according to G. P. Baker, 'Verehrung und Verkehrung: Waismann und Wittgenstein', in C. G. Luckhardt (ed.), *Wittgenstein. Sources and Perspectives*. Thoemmes, Bristol 1996, p. 257.

Er war mit Inhalt u[nd] Form der Darstellung zwar einverstanden (er nannte sie eine „wertvolle Arbeit"), beschwor aber Schlick u[nd] mich, das Buch in dieser Form nicht zu veröffentlichen, da seiner Meinung nach das Buch ungeheuer gewinnen würde, wenn es einen ganz anderen Weg einschlagen würde. Ich war in meiner Darstellung darauf ausgegangen, philos[ophische] Probleme Klärung zuzuführen, indem ich eine sprachliche Untersuchung der Ausdrücke anstellte, in denen das Problem formuliert wird. Die Lösung des philos[ophischen] Problems war die Hauptsache, die grammatische Untersuchung das Mittel hierzu. W[ittgenstein] aber schlug vor, ein Buch zu schreiben, das nichts von Philosophie enthält, sondern das die Grammatik systematisch aufbaut; ich sollte, so meinte er, nachdenken, wie ich eine Reihe von Beispielen bilden kann, die von den einfachsten Begriffen bis zu den schwierigsten der Philosophie führen. Wenn eine solche Darstellung gelingt, so braucht man sich um die Lösung der philos[ophischen] Fragen gar nicht mehr zu bemühen: sie fallen einem wie die reifen Früchte in den Schoss. Ich sagte damals, dass mir dieser Gedanke sehr gut gefalle, dass ich aber seine Durchführung für enorm schwierig halte; u[nd] dass ich mich einer solchen Aufgabe nicht gewachsen fühle. Auch fand ich, dass meine ursprüngliche Darstellung gewisse Vorzüge habe, die bei der Umarbeitung verloren gehen würden. Kurz, ich wehrte mich heftig gegen diesen Vorschlag. Schließlich bat ich Schlick, meine Sache gegen W[ittgenstein] zu vertreten, da ich mit ihm nicht verhandeln wollte. Schlick ließ sich durch W[ittgenstein] umstimmen, ich kam damals mitten in der Nacht zu S[chlick], wo auch W[ittgenstein] war, u[nd] beide baten mich, ich möge doch eine Umarbeitung in diesem Sinne *versuchen*. Schließlich willigte ich ein, mit der Bedingung, dass ich die Arbeit einstellen könne u[nd] dass ich nicht Schuld sei, wenn das Buch entweder spät oder gar nicht erscheine.[37]

After much suspense and drama during his summer visit in Vienna 1934, Wittgenstein promised Waismann that he would give him a plan of the intended new form of the book. Wittgenstein promised to work on it in August. The two did not meet again before Wittgenstein left for Cambridge. According to Waismann's letter to Menger, no plan was forthcoming. However, Wittgenstein and Waismann met five times during the Christmas vacation of 1934—on 21, 28, 29, 31 December and 2 January—and still once at Easter 1935—9 April. It is hard to believe that these meetings were used for anything else than planning the new book. There is actually something amongst Wittgenstein's papers that would fit the description of a plan or at least an attempt towards it, the MS 141. Of course, Wittgenstein had already dictated the first part of the *Brown Book* in Cambridge. MS 141 could be even earlier than that.

I do not know how Wittgenstein communicated to Waismann his new idea of beginning the book exactly with the builders, but in any case this was the example with which Waismann began his private seminar of summer 1935. After introducing a number of different steps and kinds of words, Waismann accomplished his task certainly without any knowledge of the *Brown Book*. It was an independent attempt to follow Wittgenstein's advice, although thoroughly informed by Wittgenstein's many preparatory dictations for the book.

37 I am indebted to F. Stadler for this previously unknown letter, discovered by Bernhard Beham in Karl Menger's collection in the Duke University Library.

One indication that MS 141 may be involved here would be Waismann's use of the word *Quader* in his proseminar during the winter term 1935, when he explained the example of builders and the different kinds of words that could be teached in connection with this example. A *Quader* is a square stone block, a cubic block, an ashlar. The word is most commonly used in Austria. Waismann certainly got it from MS 141, where Wittgenstein used it in several places, unlike his other writings until and including the *PI* where he spoke in this connection of a *Würfel*.

## 9

Incidentally, this proseminar by Waismann, nearly his last in Vienna, provides two summaries which are worth quoting. About "meaning" he discussed four views. Only the fourth was the one he adopted as his own: 1) "Die Ansicht Freges: Der Gegenstand ist die Bedeutung eines Wortes." 2) "Die Vorstellung ist die Bedeutung eines Wortes." 3) "Die Bedeutung ist die Wirkung, die auf das Hören eines Wortes erfolgt." 4) "Die Bedeutung ist die Verwendung eines Wortes, allerdings nicht von einer einzelnen Person, sondern beherrscht von allgemeinen Normen."[38]

Many of Waismann's writings after his earlier fresh starts can be read as defending the autonomy of language, at least "in a sense", not unlike Wittgenstein's teaching to him. But this was not the whole story, and, in the proseminar of 1935/36, Waismann emphasized several times the following: "Kann man den Unterschied zwischen einem Spiel mit Worten und der Sprache mit Worten innerhalb der Sprache immanent erfassen? Nein! Erst wenn man aus der Sprache heraustritt und diese Worte in ihrem ganzen Zusammenhang mit dem Leben betrachtet, ist dies möglich."[39]

The case of builders did not remain as the beginning of Waismann's book. In the letter to Menger, Waismann expressed his commitment, explaining:

> Meine alte Idee wollte ich nicht ganz aufgeben, sondern dachte mir die Anlage des Buches so: In einer ausführlich geschriebenen Einleitung – die ca ein Fünftel des Buches umfasst – soll an einer Zahl von Beispielen gezeigt werden, wie philosophische Probleme durch Unklarheit des Denkens entstehen u[nd] wie sie sich auflösen, sobald man sich den Sinn der verwendeten Ausdrücke deutlich vergegenwärtigt. Nachdem auf diese Weise die Wichtigkeit grammatischer Untersuchungen klar geworden ist, sollte der systematische Teil beginnen.

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38 Proseminar bei Prof. Schlick WS 1935. Thema: Die Lehre von den Bedeutungen der Worte. VCA 080/B.62-5. Meeting X.

39 Ib., meeting IX.

with Martin Kusch, Alois Pichler, Josef Rothhaupt, Joachim Schulte and Friedrich Stadler. As usual, they are not responsible for my suggestions. I want to thank the Archives for Scientific Philosophy, the Vienna Circle Archive and the Duke University Archive for the permission to quote from their materials.

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# Otto Neurath's 'Encyclopedia of the World War': A Contextualisation

A hitherto unknown document, Otto Neurath's outline for an 'Enzyklopädie des Weltkrieges' (Encyclopedia of the World War, EWW) presumably of the early 1920s, has been recovered and invites contextualisation.[1] Why did the initiative for this encyclopedia develop? What was the contemporary background? What was done by others, maybe in a similar vein? We can speculate who the EWW was meant for, and learn from the introductory section that its intended audience was the general public.[2] That Neurath should be involved in such an undertaking seems timely and appropriate, as he had experience with the topic, and the capacity and connections essential for carrying out such a long-term scheme.

This essay considers the EWW-document from three perspectives. In the first section, Corinna Oesch describes the historical background. In the second part, Günther Sandner embeds the EWW in the intellectual and political biography of Otto Neurath (1882–1945). Finally, Hadwig Kraeutler looks at Neurath's use of specific enlightenment instruments as efforts at democratically furthering social development.

The First World War—with roughly 16 million deaths and 21 million casualties—had shattered the high expectations of progress, especially of the turn-of-the-century internationalists. However, it was also crucial in giving rise to various efforts aiming at the development of international law and dispute settlement to secure peace, international understanding and cooperation (League of Nations, founded in 1919/20; Permanent Court of International Justice, 1922). Besides these fragile official structures, there were various initiatives dedicated to the study of the causes and impact of war, and to the prevention of future similar events. One was to provide, in a collection, a complete, systematic, and objective picture of the war experiences such as, among others,[3] the Vienna Group of the

1 Otto Neurath, EWW, Special Archive Moscow, Inventory of the Women's International League for Peace and Freedom (WILPF Papers) 523-2-9, pp. 26-31.

2 Cf. Otto Neurath, EWW, loc. cit., TS p. III.

3 Cf. the Carnegie Endowment for International Peace (which had sponsored Neurath and had been referred to in the WILPF's correspondences) commissioned a seminal 150-volume 'Economic and Social History of the World War', published in 1924 with James T. Shotwell as its general editor (http://carnegieendowment.org/; http://app.cul.columbia.edu:8080/findingaids/results.html?q=Carnegie; acc.: 2010-07-20).

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Women's International League for Peace and Freedom (WILPF) intended to procure.[4]

For those familiar with Otto Neurath,[5] the title of the EWW-typescript immediately brings to mind his pre-eminent work as an 'encyclopedist', as well as his various attempts at reaching, communicating with, and involving society at large. Together these reflect Neurath's utopian concerns as a philosopher, planner, a 'social engineer', a museum and education organiser and advocate.[6] The idea of compiling and providing a systematic overview over social developments was akin to many of his lifelong occupations. Neurath's war-time experiences included collecting data on, and publishing about warfare, war conditions and economics. This, together with his work with exhibitions,[7] the short-time directorship (1918) of the German Museum of War Economy in Leipzig (MWE, Deutsches Kriegswirtschaftsmuseum) and the work for the Bavarian Soviet Republic—i.e. Neurath's intellectual background and professional interests—are reflected in the themes sketched out in the introductory paragraphs for the EWW. They witness that he was seriously engaged in a profound reflection of the underlying questions, political, sociological, and cultural.

Already as a young man Neurath considered social policy, women's rights, and peace as the most important political issues.[8] Neurath, however, was not primarily a political activist, but a social engineer. This self image determined his approach towards social, economic and political questions. Thus, also the EWW-outline is less a political manifesto than a document that reflects, from the perspective of a more or less neutral expert, the economic and societal significance and consequences of war. After the introduction, Neurath's text is subdivided into three parts in which a large number of war-related topics are systematically addressed. In the first part (Before the World War) he focuses on different types of wars, their causes and conditions as well as their preparations and predictions. The second part (The World War) deals with the outbreak and course of the world

4 Confer the opening sentence of the EWW document.

5 Cf. Elisabeth Nemeth/Friedrich Stadler (Eds.), *Encyclopedia and Utopia. The Life and Work of Otto Neurath (1882-1945).* Vienna Circle Institute, Yearbook, 1996, Vol. 4. Dordrecht/Boston/London: Kluwer Academic Publishers 1996.

6 Cf. Marie Neurath/Robert S. Cohen (Eds.), *Otto Neurath. Empiricism and Sociology*. Dordrecht, Boston: D. Reidel 1973; Otto Neurath, "Visual Education—Humanisation versus Popularisation" (1945), edited by Juha Manninen, in: Nemeth/Stadler (Eds., 1996), loc. cit., pp. 245-335.

7 Cf. Nader Vossoughian, "The Museum of War Economy" in: Nader Vossoughian, *Otto Neurath. The Language of the Global Polis*. Amsterdam: NAI-Publishers 2008, pp. 49-54, p. 52.

8 "Drei Fragen treten im alten Griechenland sowohl als heute in den Vordergrund: die soziale Frage, die Frauenfrage und die Friedensfrage". Otto Neurath, "Sozialwissenschaftliches von den Ferial-Hochschulkursen" in Salzburg (1903), in: Rudolf Haller/Ulf Höfer (Eds.), *Otto Neurath. Gesammelte ökonomische, soziologische und sozialpolitische Schriften (Band 1)*. Wien: Hölder-Pichler-Tempsky 1998, pp. 1-7, 5.

war, differentiates between a large number of sociological and organisational elements (such as international and trans-national relations, national phenomena, etc) and finally makes the end of the war the subject of discussion. The third and last part (After the World War) includes topics such as the order of states and nations, production and consumption, the inner structure of the state, mass psychology and world peace.

## Otto Neurath and the Women's International League for Peace and Freedom (WILPF)

Otto Neurath's outline for an EWW was found in the "Center for Preservation of Historico-Documentary Collections", the former "Central State Special Archive", known under the name of "Special Archive" in Moscow.[9] Though there exists a small inventory of Otto Neurath papers in the Special Archive, the document in question forms part of another inventory, namely the papers of the Austrian Section of the WILPF. The WILPF originated in a 1915 congress in The Hague assembling over 1100 women from belligerent and neutral states to protest against war. It was officially established at the Congress of Women in Zurich in 1919 that took place during the Conference of the Allied Powers in Paris. The organisation was among the first to protest against the punitive peace treaties, to disapprove officially of the League of Nations's Covenant not being in accordance with Woodrow Wilson's Fourteen Points, and to send suggestions for its improvement. The WILPF members regarded their organisation as the most radical and avant-garde among international women's and peace organisations in the interwar years, and considered international politics as their primary field of action.

While Neurath indicated that he wrote the EWW-outline in "Wien", he did not specify date or year. However, a single document originating from the WILPF papers provides some information as to time and context of its origin. Marie Wettstein, secretary of the WILPF Austrian section, mentions in a letter

9 The Central State Special Archive (renamed in "Center for Preservation of Historico-Documentary Collections" in 1992) was founded in 1946 to preserve documents captured by the Red Army from various countries between 1941 and 1945 as well as different files concerning war prisoners. In 1938/1939 the national socialists transferred most of the files later becoming the Austrian documents in the Special Archive to Berlin or other central locations of NS-administration. These papers, relocated several times, towards the end of WW II and after the retreat of German soldiers, were taken over by the Red Army. A smaller part of the Austrian files were confiscated in Vienna or in the East of Austria and directly brought to Moscow. Cf. Gerhard Jagschitz/ Stefan Karner, *"Beuteakten aus Österreich". Der Österreichbestand im russischen "Sonderarchiv" Moskau.* Graz/Vienna: Selbstverlag des Ludwig-Boltzmann-Instituts für Kriegsfolgen-Forschung 1996. In 2009 the bulk of papers related to Austria was given back by representatives of Russia and turned over to the Austrian State Archive in Vienna.

to the secretary of the WILPF headquarters in Geneva that Mrs. Dr. Friedmann[10] had propagated the plan to edit a so-called “Enzyklopädie des Weltkrieges” and to ask the Carnegie Foundation for financing. At the invitation of Rosa Mayreder and [Marie] Goldscheid[11] various ‘learned’ men had joined the meeting of the WILPF Austrian section for preliminary talks about the project.[12] Probably Neurath’s outline for the EWW arose from these discussions at the end of 1921 and thus could have been written in the following months, in 1922.[13] Otto Neurath had participated in the Bavarian Soviet Republic, as had Anita Augspurg and Lida Gustava Heymann—both leading WILPF activists. This may be the background for Neurath’s acquaintance (in 1920) with Yella Hertzka (1873–1948), appointed representative in WILPF international executive committee and protagonist of the Austrian WILPF branch in Vienna. A few weeks after Neurath’s release from Bavarian imprisonment, Hertzka wrote to Lida Gustava Heymann that she had discussed with Neurath the plan for a so-called Bureau of Statistical Preparatory Work to World Economic Planning (“Büro für statistische Vorarbeit zum Weltwirtschaftsplan”). Yella Hertzka asked for this item to be included in the programme of the WILPF. She also asked for Heymann’s opinion, if they could afford to finance, and if the WILPF German section was already campaigning for Neurath’s project.[14] A few months later Hertzka transmitted an appeal of Neurath’s Research Institute of Social Economy (“Forschungsinstitut für Gemeinwirtschaft”) to Emily Greene Balch, at that time secretary-treasurer of the WILPF in Geneva.[15] For both projects—the Research Institute and the EWW—Neurath had found allies in members of the WILPF. For lack of further documents we do not know if WILPF members contributed further to Otto Neurath’s EWW-outline. In 1919, Augspurg and Heymann from the WILPF German Branch likewise had initiated a project on the World War that might refer to the EWW-outline. Due to their appeal, a commission was formed to investigate responsibilities for the war and to inform the public.[16] Two WILPF activists and Albert Einstein were among the members of the commission and editors of the subsequent publication.[17] As Albert Einstein is sup-

10 Presumably Dr. Constanze Friedmann, mentioned in another context in the sources of the WILPF Austrian section around 1921.

11 Presumably Marie Goldscheid, another WILPF activist.

12 Cf. Marie Wettstein to Anny Wössner, Vienna, December 20, 1921. United Nations Office at Geneva Archives (UNOG), WILPF Papers, Reel 55.

13 The fact that Otto Neurath refers at the beginning of the outline to the Vienna group of the WILPF may indicate, that he wrote it not much later than at the end of 1922, when the organisation in Vienna split and the new groups adopted special names.

14 Cf. Yella Hertzka to Lida Gustava Heymann, March 25 [26?], 1920. UNOG, WILPF Papers, Reel 55.

15 Cf. Yella Hertzka to Emily Greene Balch (American economist fired from Wellesley College due to her pacifist activities, Peace Nobel Prize winner in 1946), July 19, 1920. UNOG, loc.cit.

16 Cf. WILPF News-Sheet No. 4, Sept. 17, 1919.

17 Cf. Georg von Arco/Albert Einstein/Walburga Geiger/Hellmut von Gerlach/Maxi-

posed to have discussed a plan for an encyclopedia with Otto Neurath in 1921,[18] the question arises if, at that time, such could have referred to the World War.

## Struggles for Social Transformation—Links between Yella Hertzka and Otto Neurath

Protagonists of the WILPF considered their political activities as part of a wider struggle for socio-political transformation. Before and parallel to their commitment to the League they were also involved in other social reform movements. Like Neurath, Yella Hertzka and her close friend Helene Scheu-Riesz, another WILPF member, were activists in the garden city and settlement movement. In 1913, Yella Hertzka and her husband Emil Hertzka, managing director of the music publishing house Universal-Edition, founded a colony, composed of eight semi-detached houses in Vienna, designed by architect Josef Hoffmann according to principles of the garden city movement.[19] Helene Scheu-Riesz and her husband Gustav Scheu, lawyer and social democrat, were prominent mentors of the garden city and settlement movement. Both were activists in the Central Office for Housing Reform in Austria ("Zentralstelle für Wohnungsreform in Österreich") before World War I. From 1919 to 1923 Gustav Scheu acted as advisor in matters of housing for the Vienna municipality, and after his term as a city councillor, until 1934, he ran the legal protection agency ("Rechtsschutzstelle") of the Austrian Union for Settlements and Allotment Gardens ("Österreichischer Verband für Siedlungs- und Kleingartenwesen"), co-founded in 1921 and headed by Otto Neurath, as General Secretary until 1925.[20] Though the WILPF defined itself as a non-party organisation, many of its leading members were socialists, and several women were prominent activists in the labour party of their respective countries. Yella Hertzka tried, partly successfully, to engage the Austrian social democrats Martha Tausk and Emmy Freundlich in WILPF's activities. WILPF members of-

milian Harden/Max Hodann/Luise Kautsky/Elisabeth Rotten/Erich Schlesinger/Helene Stöcker, *Lille. Beiträge zur Naturgeschichte des Krieges*. Berlin: Engelmann 1919.

18 Cf. Hans-Joachim Dahms, "Die 'Encyclopedia of Unified Science' (IEUS). Ihre Vorgeschichte und ihre Bedeutung für den Logischen Empirismus", in: Elisabeth Nemeth/Nicolas Roudet (Eds.), *Paris–Wien. Enzyklopädien im Vergleich*, (Veröffentlichungen des Instituts Wiener Kreis, Bd. 13, hg. von Friedrich Stadler). Vienna/New York: Springer-Verlag 2005, pp. 105-120, pp.108-109.

19 Cf. Eduard F. Sekler, *Josef Hoffmann. Das architektonische Werk. Monographie und Werkverzeichnis*. Salzburg–Vienna: Residenz Verlag 1982, 140-142.

20 Cf.: Eve Blau, *The Architecture of Red Vienna, 1919-1934*. Cambridge, MA–London: The MIT Press 1999, 90-98; Robert Hoffmann, "Proletarisches Siedeln – Otto Neuraths Engagement für die Wiener Siedlungsbewegung und den Gildensozialismus von 1920 bis 1925", in: Friedrich Stadler (Ed.), *Arbeiterbildung in der Zwischenkriegszeit: Otto Neurath – Gerd Arntz*. Vienna–Munich: Löcker Verlag 1982, pp. 140-148, 142.

ten campaigned among workers for peace, internationalism and disarmament. The League also obtained the Vienna School Board's permission for peace education projects for teachers and pupils. Red Vienna's general agreement with WILPF activities is highlighted by municipal subsidies for educational and publishing projects to WILPF groups in Vienna.[21] Nonetheless, women in the WILPF regarded their organisation first and foremost as an independent and autonomous agency for social reform, not as a socialist or any other political association. The transformation of the economic system was an integral part of WILPF's proposals for a new world order even though its orientation was contested among the activists. Yella Hertzka never referred to Otto Neurath's term 'war economy'. But other approaches of hers show similarities to Neurath's concepts. She always stressed economic questions in relation to peace, advocated the concept of cooperative socialism, and promoted planned economy. Presiding over the WILPF's Economic Commission, Yella Hertzka interacted with members of the International Labour Office, and organised an Economic Conference in Paris in 1931 in the wake of the world economic crises.

In the mid 1920s, Yella Hertzka and Otto Neurath met again in the Austrian Society for the advancement of intellectual and economic relations with the USSR ("Österreichische Gesellschaft zur Förderung der geistigen und wirtschaftlichen Beziehungen mit der UdSSR").[22] Activists got involved with this association for several reasons. Membership in the Society was motivated by interest in the Soviet system, in trade relations and cultural exchange, intentions to undertake scientific research and journeys in the USSR as well as a desire for direct communication and information beyond the anti-Soviet propaganda of the time. Prominent members in the "Society" were (among others) Moritz Schlick, Hans Hahn and Hans Kampffmeyer. The latter had met Otto Neurath at the time of the "Research Institute of Social Economy" and had cooperated with him in the settlement movement.[23] Yella Hertzka's participation in the Society was founded on various grounds ranging from peace policy, women's emancipation policy to business and cultural relations (in favour of the Universal-Edition). Otto Neurath, who had been invited to work at the ISOSTAT in Moscow in the beginning of the 1930s,[24] was mentioned in connection with the WILPF for the last time in 1932. Yella Hertzka proposed to another WILPF group leader in Vienna to publish a joint annual re-

21 Cf. Marie Wettstein to Madeleine Doty, March 2, 1926; Yella Hertzka to Mary Sheepshanks, November 18, 1929. UNOG, WILPF Papers, Reel 55.

22 Cf. Julia Köstenberger, "Österreichisch-Sowjetische Kulturkontakte", Paper held at the conference *"Kulturelle Transfers – Mythen der Erneuerung",* Wien Museum, Vienna, January 29, 2010 (unpublished).

23 Cf. Blau (1999), 98.

24 Cf. Friedrich Stadler, "Wiener Methode der Bildstatistik und politische Grafik des Konstruktivismus. Wien – Moskau 1931–1934", in: Historikersektion der Österreichisch-Sowjetischen Gesellschaft (Ed.), Österreich und die Sowjetunion 1918–1945. Vienna 1984, pp. 220-249.

port designed by "Dr. Neurath".[25] Similiar interests, transnational relationships, an international perspective and networking strategies formed the basis of various connections between Otto Neurath and Yella Hertzka. However, their discussions on world economic planning, the plans for an annual report "designed by" and the EWW did not develop further.

## Otto Neurath on War and Peace

> For the sake of annihilation it was shown what human energy can achieve. Is it then so incomprehensible that more and more men raise the question whether one might not in a similar way strive for peaceful goals just as one had for so long striven for warlike ones? [26]

The outbreak of World War I was a crucial incident for many European scholars and intellectuals. Many of them, even those who had been socialists converted into aggressive nationalists, and only a few opposed the war from its beginning.[27] Although war had become an important theme in Otto Neurath's work long before 1914, we do not know much about his *political* position of that time.[28] Thus, reflecting on the plan for the EWW in the context of Neurath's numerous writings and activities in the field of war economy may help clarify this question.

"Growing up in the intellectual atmosphere of my father", Neurath remembered, "I was from early youth filled with the notion that the traditional economic order with its crises and misery was in principle unable to make men happy".[29] This conviction came to directly influence Neurath's research on war economy which had started no later than 1909. Already in 1907, however, he planned an edition of two volumes on the "philosophy of war"[30] and not much later he had started collecting documents and materials for his habilitation on "War and the Principles of Moral"[31] which he did not complete. A grant of the 'Carnegie Endowment for

25 Cf. Yella Hertzka to "Fr. Professor" [Marianne Zycha?], March 19, 1932. Special Archive Moscow, 523-1-8, 86.

26 Otto Neurath, "Utopia as a Social Engineer's Construction" (1919), in: Marie Neurath/Robert S. Cohen (Eds., 1973), loc. cit., pp. 150-157, p. 153.

27 Cf. Kurt Flasch, *Geistige Mobilmachung, Die deutschen Intellektuellen und der Erste Weltkrieg. Ein Versuch*. Berlin: Alexander Fest Verlag 2000; Wolfgang Mommsen (Ed), *Kultur und Krieg. Die Rolle der Intellektuellen, Künstler und Schriftsteller im Ersten Weltkrieg*. Munich, Vienna: Oldenbourg 1996.

28 For Neurath's political biography, cf. Günther Sandner, "Demokratisierung des Wissens. Otto Neuraths politisches Projekt", in: *Österreichische Zeitschrift für Politikwissenschaft* 38 (2009) 2, pp. 231-248.

29 Otto Neurath, "Through War Economy to Economy in Kind (Preface)", in: Marie Neurath/Robert S. Cohen (Eds., 1973), loc. cit., pp. 123-124, 124.

30 Otto Neurath to Ferdinand Tönnies, July 16, 1907, Schleswig-Holsteinische Landesbibliothek (Kiel), Estate of Friedrich Tönnies.

31 "Der Krieg und die Moralprincipien". Cf. Otto Neurath to Ferdinand Tönnies, August

International Peace' enabled Neurath to study the Balkan Wars (1912–13) which again influenced his research. He developed the theory that war experience might even offer a possibility of transforming the economic system in order to promote human happiness. Neurath propagated this concept of war economy in a number of books, essays and lectures and elicited criticism by economists such as Franz Eulenburg and Ludwig von Mises. While the former rejected Neurath's idea of "war economy as a separate discipline",[32] the latter argued strictly against any form of planning and state interventionism from a liberal point of view.[33]

But what was Neurath's approach? War economy, he put it in 1913, was the systematic analysis of advantages and disadvantages of war.[34] In his concept[35] three different questions were systematically addressed. Firstly he focused on the question of how war influences the economy and social and individual phenomena as well (including standard of living). He was convinced that war does *not* necessarily mean economic decline and that its social and economic effects ought to be scrutinised very carefully. Secondly, he analysed how a war economy is organised, e.g. on which institutions and principles it is based. In this context phenomena such as economy in kind, calculation in kind, barter and planning were introduced and analysed. And thirdly, he raised the question of how far war economy can serve as a model for peace time. For Neurath, war economy was rather efficient as it released productive capacity inhibited by capitalism. War economy in the end, as he put it, provides the instruments for satisfying human needs while the capitalist economy, in contrast, is based upon private profit motifs.

The war experience has deeply influenced Otto Neurath. In a letter to Ferdinand Tönnies[36] he wrote that he had seen a world full of cruelty that had influenced his image of human beings in a very negative way. However, as a representative of war economy he became a widely acknowledged scholar and expert, particularly in the time of World War 1. After having received military decorations in 1915,[37] Neurath became head of the 'General War and Economics Section' which was part

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27, 1908, ibid.

32 Otto Neurath, "The Theory of War Economy as a Separate Discipline" (1913), in: Marie Neurath/Robert S. Cohen (Eds., 1973), loc. cit., pp. 125-130.

33 Wolfgang Pircher, "Der Krieg der Vernunft. Bemerkungen zur ‚Kriegswirtschaftslehre' von Otto Neurath", in: Elisabeth Nemeth/Richard Heinrich (Eds.): *Otto Neurath: Rationalität, Planung, Vielfalt*, Vienna: Oldenbourg 1999, pp. 96-122.

34 Otto Neurath, "Probleme der Kriegswirtschaftslehre" (1913), in: Rudolf Haller/Ulf Höfer (Eds.), *Otto Neurath. Gesammelte ökonomische, soziologische und sozialpolitische Schriften (Band 1).* Vienna: Hölder-Pichler-Tempsky 1998, pp. 201-249, 202.

35 Among the many contributions of Otto Neurath towards war economy, cf.: Through War Economy to Economy in Kind, in: Marie Neurath/Robert S. Cohen (Eds., 1973), loc.cit.; War Economy, in: Thomas Uebel (Ed.), *Otto Neurath. Economic Writings. Selections 1904-1945*, Dordrecht–Boston–London: Kluwer 2004, pp. 153-199.

36 Otto Neurath to Ferdinand Tönnies, July 20, 1916, ibid.

37 Offiziersbelohnungsanträge (OBA), Dr. Neurath Otto, Nr. 27 922 (Kt.31), Austrian State Archive, (Kriegsarchiv).

of the 'Scientific Committee of War Economy', founded in April 1916 by, and in itself part of the Austro-Hungarian Ministry of War. Its aim was research on war economy based on a historical-statistical method. There were ten different sections that focused on different war economy related topics. Among other tasks the committee was to write a publication for the General Staff ("Generalstabswerk"), to edit the yearbook for military statistics, and to build up a library of war economy.[38] Among the Committee's collaborators were personalities such as the socialist economist Henryk Grossmann (later: Frankfurt Institute of Social Research), Otto Bauer, who later became the intellectual leader of Austrian social democracy, and Othmar Spann, right wing economist, philosopher and intellectual forerunner of Austro-Fascism (that forced Neurath to emigrate in 1934).

In 1918, Otto Neurath was appointed director of the newly founded MWE,[39] a post which he held for only a few months. Initiated and supported by several public institutions such as the Leipzig Chamber of Commerce, the museum planned several activities including conferences, lectures and publications. However, "World Blockade and War Economy" in August 1918 remained the only exhibition organised by the MWE. Although Neurath, assisted by his friend Wolfgang Schumann (general secretary), underlined the pedagogical character of the museum,[40] there is no doubt that the MWE aimed to support German stamina in the world war.[41]

The Encyclopedia of the World War, in contrast, was intended for the WILPF. So, what was Neurath's position towards the peace movement in general? He had worked for the Austro-Hungarian and German military powers that were ultimately responsible for the war. No doubt Neurath, the expert and scholar, worked scientifically. However, it was science in the context of military strategy and propaganda. Neurath, for instance, wished to propagate the German war museum by

38 Wissenschaftliches Komitee für Kriegswirtschaft, Entwurf eines Arbeitsplanes dann Personaleinteilung. Austrian State Archive (Kriegsarchiv), k.u.k. Kriegsministerium/ Intern Akten, Karton 74.

39 Cf. "Führer durch die Sonderausstellung Weltblockade und Kriegswirtschaft des deutschen Kriegswirtschaftsmuseums. Leipzig 1918". (Bundesarchiv Berlin, Papers of the German War Museum, Band 1/R 3101].

40 Otto Neurath, "Die Kriegswirtschaftslehre und ihre Bedeutung für die Zukunft" (1918), in: Rudolf Haller / Ulf Höfer (Eds.), *Otto Neurath. Gesammelte ökonomische, soziologische und sozialpolitische Schriften (Band 2),* Vienna: Hölder-Pichler-Tempsky 1998, pp. 588-598, 595-599; Entwurf einer Niederschrift über die am 16. Mai stattgefundene Besprechung wegen des Deutschen Kriegswirtschaftsmuseums und des Reichskriegsmuseums im Ministerium des Inneren (Akten, betreffend das deutsche Kriegswirtschaftsmuseums in Leipzig, Band 1/R 3101, 182-199, Bundesarchiv Berlin).

41 "Führer durch die Sonderausstellung "Weltblockade und Kriegswirtschaft" des deutschen Kriegswirtschaftsmuseums. Leipzig 1918", loc. cit., pp. 182-199.

highlighting its important popular function in preparing for war,[42] at the same time trying to promote human happiness.[43] This may seem contradictory.

For Neurath, a social engineer was someone who worked independently of political parties and their propaganda.[44] He wrote the EWW-draft at about the same time as he published his social engineer's reports on socialisation in Bavaria (1919)[45] and on Jewish planning economy in Palestine (1921).[46] There, he underlined the 'unpolitical' character of his expertise. In this time Neurath often stressed the fact that his scientific investigations were neither 'pro' nor 'against' because such a statement could never be the task of science.[47] However, the social engineer Neurath was definitely not uncritical towards the peace movement. In the EWW he rejected an approach that tried to prevent war only by describing its horrors. The peace movement, he put it, must become part of a larger movement that aims to construct a new social and economic order of life. However, it "has hitherto shown itself rather sterile in the field of economics,"[48] he stated. Neurath, in contrast, freely discussed the economical advantages and disadvantages of war and warned the peace movement against one-sidedness. He repeatedly accused 'friends of peace' of their naivety, as, in his opinion, the peace movement should address its criticism towards the economic order as such—and not only attack the military.

In Neurath's view, it was the dominant economic order (or system) which, not only promotes but even entails war. Those who are interested in peace should therefore first and foremost combat the economic system, which consequently would eliminate war indirectly, but most successfully.[49] As he put it:

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42 Otto Neurath, "Die Kriegswirtschafslehre und ihre Bedeutung für die Zukunft" (1918), loc.cit. pp. 588-598.

43 Cf. Otto Neurath, "Menschheit" (1920), in: Rudolf Haller/Heiner Rutte (Eds.), *Otto Neurath. Gesammelte philosophische und methodologische Schriften (Band 1)*, Hölder-Pichler-Tempsky: Vienna 1981.

44 Cf. for a typology of the social engineer: Thomas Etzemüller, "Social engineering als Verhaltenslehre des kühlen Kopfes. Eine einleitende Skizze", in: Thomas Etzemüller (Ed.), *Die Ordnung der Moderne. Social Engineering im 20. Jahrhundert*. Bielefeldt: transcript 2009, pp. 11-39.

45 Otto Neurath, *Wesen und Wege der Sozialisierung. Gesellschaftstechnisches Gutachten, vorgetragen in der 8. Vollsitzung des Münchner Arbeiterrates am 25. Januar 1919*. Callwey: Munich 1919.

46 Karl Wilhelm (= Otto Neurath), *Jüdische Planwirtschaft in Palästina. Ein gesellschaftstechnisches Gutachten.* Welt-Verlag: Berlin 1921.

47 Otto Neurath, "Einführung in die Kriegswirtschaftslehre" (1914), in: Otto Neurath, *Durch die Kriegswirtschaft zur Naturalwirtschaft*". Munich: Callwey 1919, pp. 132-133.

48 Otto Neurath, "War Economy as a Separate Discipline", loc.cit., p. 128.

49 Otto Neurath, "Probleme der Kriegswirtschaftslehre", in: Rudolf Haller/Ulf Höfer (Eds.), *Otto Neurath. Gesammelte ökonomische, soziologische und sozialpolitische Schriften (Band 2),* Vienna: Hölder-Pichler-Tempsky 1998, pp. 201-249, 248.

The best success therefore may possibly be achieved by struggling not directly against war, but instead against certain deficiencies of our economic order which have the effect of reducing the horror of war and increasing its advantages.[50]

After World War I, when the EWW-draft was written Neurath had transformed his concept of war economy into plans for socialisation and, around this time, had also become a member of the socialist party. With this, his idea of war economy, however, could serve as a guide for a future world order. Thus, the role of the social engineer, as he pointed out, was to transform economy and society:

The hesitations and vacillations of those called upon to act, the advice of my friends and sundry accidental circumstances, finally moved me, after much reflection, to conclude my life of contemplation and to begin one of action, to help to introduce an administrative economy that will bring happiness.[51]

In this context, Neurath's concept of utopia had become politically relevant. He was convinced that utopias "one might with full justice call (…) constructions of social engineers"[52] and from 1919 onwards, peace more and more was seen as part of a future order of life. In 1922, together with Rudolf Goldscheid, Alfred Adler, Josef Luitpold Stern and others, he co-founded the Vienna section of the international pacifist association "Clarté".[53] Looking at Neurath's political essays of these years it seems clear that for him questions such as peace and war were becoming more closely connected to socialism (and even Marxist theory). "World-socialism", he was convinced, would ultimately result in a world of peace.[54]

## Otto Neurath—Utopias, Encyclopedias, Museum Work

The last part of this essay considers Otto Neurath's EWW-document in two ways: One points at related 'utopias' and refers to the notion of encyclopedic re-presentations/models (i.e. also museums) as tools for societal development, and to their position within Neurath's cosmos.[55] The other suggests parallels, in the EWW and

50 Otto Neurath, "War Economy", loc.cit., p. 194.
51 Otto Neurath, "Through War Economy to Economy in Kind (Preface)" (1919), loc. cit., p. 124.
52 Otto Neurath, "Utopia as a Social Engineers Construction" (1919), loc. cit., p. 151.
53 Cf. Wolfgang Fritz/Gertraude Mikl-Horte, *Rudolf Goldscheid – Finanzsoziologie und ethische Sozialwissenschaft.* Vienna–Berlin: Lit Verlag 2007, pp. 76-77.
54 Otto Neurath, "Weltsozialismus" (1922), in: Rudolf Haller/Heiner Rutte (Eds., 1981), loc.cit., pp. 203-208, 208.
55 Cf.: Elisabeth Nemeth/Stephan W. Schmitz/Thomas Uebel (Eds.), *Otto Neurath's Economics in Context*, VCI-Yearbook, 2007, Vol. 13. Vienna/New York: Springer 2007; for the term 'Encyclopedist', cf. Hans-Joachim Dahms, "Die 'Encyclopedia of Unified Science' (IEUS). Ihre Vorgeschichte und ihre Bedeutung für den Logischen

early museum work and in the developmental changes leading to his unfinished projects 'Museums of the Future'[56] and 'International Encyclopedia of Unified Science' (IEUS, 1935 ff.).[57]

### *Utopias*

> There was a moment in the history of Otto Neurath's International Encyclopedia of Unified Science during which the project actually began to fulfil some of the Enlightenment-ideals it shared with its older French counterpart. (…) (as) a successful, international forum for philosophers who believed that science, as organized, collective inquiry into the nature of the world and of society was the supreme tool with which civilization could possibly build a world, more humanistic, peaceful and economically just.[58]

With the involvement in encyclopedia enterprises (the EWW being an early example) and museum and exhibition projects—the MWE prefiguring the Social and Economic Museum in Vienna (SEM, Gesellschafts- und Wirtschaftsmuseum, 1925–1934; SEM)—Neurath aimed to provide instruments which would demonstrate the impact of economic, political and organizational measures on living conditions (the basis for comparing alternatives, and for rationally deciding on unavoidably insecure steps). This would deliver a format "to communicate the results, (and) (…) to promote an informed discourse among those whose living conditions are affected by the measures, the general public."[59] In 1933, Neurath saw a twofold task for such instruments, "to show social processes, and to bring all the facts of life into some recognisable relation with social processes" as, "How to organize human life socially (…) (was) the great question which people are asking with ever greater intensity". These instruments should be seen as diverse, but *orchestrated* media

> (w)ith the identical purpose of making (…) less afraid of the world. If previously (…) oppressed by the complexity of facts, the visitor to the museum should leave it with the feeling that, after all, "one can find a way through".[60]

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Empirismus", in: Elisabeth Nemeth/Nicolas Roudet (Eds. , 2005), loc. cit., p. 108, p. 114.

56 Cf.: Hadwig Kraeutler, *Otto Neurath: Museum and Exhibition Work—Spaces (Designed) for Communication*. Frankfurt/Main: Peter Lang 2008; Otto Neurath, "Museums of the Future" (1933), in: *Survey Graphic,* Vol. 22, Nr. 9, New York 1933, pp. 458-463.

57 Cf.: Otto Neurath/Rudolf Carnap/Charles Morris (Eds.), *Foundations of the Unity of Science, Toward an International Encyclopedia of Unified Science*, Chicago and London: The University of Chicago Press 1971.

58 George A. Reisch, "Doomed in advance to defeat? John Dewey on logical empiricism, reductionism, and values" in: Elisabeth Nemeth et al. (Eds., 2005), loc. cit., pp. 241-251, p. 241.

59 Cf. Elisabeth Nemeth/ Stephan W. Schmitz/Thomas Uebel (Eds., 2007), loc. cit., p. 4.

60 Otto Neurath, "Museums of the Future" (1933), loc. cit., p. 463.

Official representations of warfare usually oscillate between the depiction of horror, of the fascination of instruments—'auratic' objects (in museums, exhibitions) or heroes (accounts in various media)—and an impetus to explain the causes underlying destruction and violence, in order to render them manageable. How centrally a pedagogic/didactic impetus is driving the leading notions, and shaping interfaces and structures, is decisive for the communicatory potential and impact (be this of an encyclopedia, exhibition, museum).

In the introductory paragraphs of the EWW-document, Neurath referred to the complex contingencies involved, leading to the catastrophic experiences of the years 1914–1918. Soon after WWI, Neurath, who doubted that "bourgeois pacifism which expects salvation from a League of Nation and Courts of Arbitration" could contribute effectively to preventing future wars,[61] was searching for a more satisfactory strategy. He turned from an academic career and mostly peer-audiences to political activism. For the 'social engineer' this required involving the public at large—ultimately the target audience of his utopian economic and societal visions—and democratic procedures which Neurath defended as not naïve, but rather *as the only way forward*.[62]

The tasks outlined for the EWW were: to gather and analyze as factually, objectively and encompassingly as possible the war-related phenomena, to find methods for presenting and transferring this specific knowledge/information, with the goal to reach a better future—engendered by a long-term utopia of world-peace.

### *Encyclopedias, Museum Work*

The EWW-document, the MWE (1917),[63] and the SEM in Vienna,[64] show similarities in the factual, systematic approach to subject matters, as well as in pedagogical intentions expressed. All three were meant to furnish the public with an understanding of the social, administrative, and economic dimensions of social phenomena. But in addition to a 'Gesamtbild' (complete overview) and lasting memory, Neurath's stated aim was to contextualise the historic developments in

61 Cf. Otto Neurath, "Personal Life and Class Struggle" (1928), in: Marie Neurath/Robert S. Cohen (Eds., 1973), pp. 249-298, p. 270.

62 Cf. Otto Neurath, "Utopia as a Social Engineer's Construction", loc. cit., pp. 150-157.

63 The MWE-guide emphasised scientific and educational (popular education) orientation, cf. "Grundsätzlich geht es … um die Frage, wie aus kriegswirtschaftlichen Erfahrungen gelernt werden kann...") [Bundesarchiv Berlin, Bestandssignatur: R/3101, Archivsignatur: 617].

64 Cf. Otto Neurath's related writings, especially of the mid 1920's, propagating the GWM-plans; the introduction to the 'Gruppe: *Weltkrieg'* (Group World War), "Gesellschafts- und Wirtschaftsmuseum in Wien", 1925 in: Österreichische Gemeinde-Zeitung, 2. Jahrgang, Nummer 16, (reprint) in: Rudolf Haller/Robin Kinross, Eds., *Otto Neurath. Gesammelte bildpädagogische Schriften*, Vienna: Hölder/Pichler/Tempsky 1991, pp. 1-17, p. 12.

order to encourage complexity in the discussions and with this, to deliver the basis for reflection and informed decisions.

In the early 1920s the overriding concern had been that the projects were scientifically sound, sufficiently attractive, and viable. Around 1928, Neurath's plans for museums and exhibitions became more functional (information organisation, approachability),[65] and later developed into systematic visualisations.[66] This was the result of a decisive communicational reorientation appropriate to the 'era of the eye', that ultimately led to his proposal for a global, egalitarian educational scheme.[67]

Such developments seem paralleled by changes in Neurath's notion of 'encyclopedia'. In a detailed 'reconstruction of the pre-history' of the IEUS, Hans-Joachim Dahms points at the obvious relation to the classical Encyclopédie of 18th century France, and traced it back to earlier plans of Neurath's. Dahms describes an unrealized 'Volksbibliothek' of 1921—likened to an encyclopedia by Albert Einstein[68]—(and a further 1928 'Leselexikon') as precursors of the IEUS. This, a complex and plurally structured model, was to represent the sciences in a genuinely international scope, an adaptable framework for open-ended, interdisciplinary collaboration.

By 1933 Neurath had introduced metaphoric concepts such as a collage, or mosaic-like 'orchestration' of the sciences, which would ensure the progression of the *common play* and at the same time, respect the provisional nature of scientific knowledge, engaging the users in 'argumentations', in dialogic processes and partnership relations, encouraging exploration in an active rather than passive stance. With this approach to enlightenment tools, characterised by tedious democratic processes and the corresponding practical political structures, Neurath definitely placed social responsibility with the institution, with the team of scientists and planners.[69]

With more experience in museum and exhibition work, and the engagement in the IEUS, Neurath came to consider team work, with self-reflexive exchange

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65 Cf. Otto Neurath ,"Die neue Zeit, Köln 1932", in: Die Form, vol. 4 (21), Berlin 1929, pp. 588-590.

66 Cf. Elisabeth Nemeth "Gesellschaftliche Tatbestände sichtbar machen. Otto Neurath über den Gegenstand der Wirtschaftswissenschaft und seine Visualisierung", in: Ewa Czerwinska-Schupp (Ed., 2003), *Philosophie an der Schwelle des 21.Jahrhunderts*, Frankfurt/Main: Peter Lang, pp. 181-207.

67 Cf. Otto Neurath's and Paul Otlet's plans for a global information network Mundaneum (1932 onwards); Otto Neurath, "Bildstatistik—ein internationales Problem" (Original in English, Pictorial Statistics—An International Problem, in: *The Listener,* London 1933, pp. 471-472), (reprint) in: Rudolf Haller/Robin Kinross, (Eds. 1991), loc. cit., pp. 258-264; Otto Neurath (1944, 1996): "Visual Education: Humanisation versus Popularisation", in: Elisabeth Nemeth/Friedrich Stadler (Eds., 1996), loc. cit., pp. 245-335; Hadwig Kraeutler, loc. cit., pp. 135-139.

68 Cf. Hans-Joachim Dahms, loc. cit., p. 110.

69 Cf. Hadwig Kraeutler, "Strategies for Enlightenment", loc . cit., pp. 175-192, p. 185.

and approximations of statements, as a precondition for achieving acceptable (intersubjective, temporarily valid) propositions, and to prefer transmitting information to non-expert audiences visually (more egalitarian than word-based communication).[70] In this vein, he had proposed an accompanying visual thesaurus to make the IEUS approachable. In his last years, towards the end of WWII, Neurath regarded visual education as an appropriate tool for international communication. In this, museums and exhibitions would figure as specifically well-suited media, with designed (finely attuned) visual narratives, spaces, and interactivities to engage the public at large as well as the researchers in 'argumentations'.[71]

Already in the EWW-document of the early 1920s, Neurath had hinted at underlying methodological challenges—however far from the refinement to be reached in the next decades[72]—and acknowledged the difficulties involved:

> While we know how to tackle such a task from the point of view of war history or political history, we lack experience on how to best analyze (…) for the development of future universal peace (…).[73]

There were crucial questions: How to reach the many with fact-based argumentation which should not become a thrilling story or nightmare "oppressing (…) [the] defenceless"?[74] How to patently strive for objectivity—the basis for a more reflected stance—as one-sidedness, emotionally coloured clichés,[75] absolutes or anecdotes would "only deter the calm ones who we aim[ed] to win"? Neurath emphasized that no answers or progress could be founded on merely enumerating the phenomena and effects of war-fare.[76]

Comparable to this essential request for contextualizations, Neurath sharply criticised museum presentations which were not used analytically, i.e. to enable comparisons, or to open up discussions in a "scientific attitude".[77] Referring to literary accounts of warfare as mostly speaking to emotions and feelings,[78] he implied that the EWW should not be the work of an artist, as this would involve a different concept.

We can only speculate if this encyclopedia, planned as an elaborate work and intended as programmatic tool for reaching the broad public, would not rather have been addressing the academically trained. However, we may also acknowl-

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70 Cf. Otto Neurath, "Unified Science as Encyclopedic Integration", in: Otto Neurath / Rudolf Carnap / Charles Morris, (Eds. 1971), loc.cit., pp. 1-27.

71 Cf.: Hadwig Kraeutler, "Otto Neurath's concern with the visual", loc. cit., pp. 193-206.

72 Cf. Marie Neurath / Robin Kinross, *The Transformer*, London: Hyphen Press, 2009.

73 EWW, loc.cit. p. I.

74 ibid.

75 Cf. EWW, loc.cit., p. III.

76 Cf. EWW, loc. cit., p. I.

77 Cf. Otto Neurath, "Museums of the Future", loc. cit., p. 459.

78 Cf. EWW, loc. cit., p. I.

edge that Otto Neurath, who rejected the concept 'truth', even in this sketchy outline for the EWW, pointed out that this, "however incomplete it may be" is suggesting one possible structure, and that there might be other schemes and "better solutions"[79].

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79 EWW, loc.cit., p. III.

Translation of the "Encyclopedia of the World War" by Otto Neurath[80]

Encyclopedia of the World War.

by Otto Neurath—Vienna.

The Vienna group of the International Women's League for Peace and Freedom is planning to start a collective work about the World War. A huge and significant task. Difficulties of all kinds arise, not to mention the financial ones. It will not be easy to find really objective people to work on it; it will be even more challenging to subject them to a uniform editorial standard, especially because not sufficient preliminary systematic work has been done so far that might help us to give room to each single problem. While we know how to tackle such a task from the point of view of war history or political history, we lack experience on how to best analyze the World War in its significance for the development of future universal peace or in its influence on institutions that are supposed to further or inhibit wars or on inclinations as well as other states of affairs. The mere enumeration of appearances and effects of the World War is able to awake horror and repulsion to a large degree. But that is not enough if we want to further the peace movement in a decisive way. The horror of the Thirty Years' War has been made known sufficiently by Grimmelshausen, Gustav Freytag and other writers, and so have the Napoleonic Wars. Berta Suttner's gripping depictions have been repulsive enough. But all that does not suffice without other driving forces which will give us a glimpse of hope for a peaceful future. Without such forces the horror becomes a gripping story about Indians or a nightmare oppressing us defenceless.

II

Yes, when the forces that press towards a war are alive, such depictions of horror can only dull the mind of courageous souls. Who believes that he can turn an inquisitor away from his sad work by describing the suffering of his victims to him? The genuine inquisitor had to learn through permanent self-training to despise all pity as vain play of the moment. And when socialists think of a bloody revolution, not because they wish for it, but because they consider it inevitable, must they not go against their nature and harden themselves?

If one describes the terror of an uprising to them, in the style of the great French Revolution or the Soviet fights, then they often come up with the follow-

80 The original German text of this document follows, starting on page 289. Handwritten comments had been added to Otto Neurath's typescript. They probably do not stem from the author himself, since they partly represent a contradiction, and partly they ask questions about the meaning of individual words. All handwritten comments are retained or commented on in the editorial footnotes.

ing thought: We must come to terms with all this in order to serve the cause for which we are ready to make sacrifices. The depiction of all suffering, the depiction of all corruption is only a small impediment when the masses begin the struggle for power! On the contrary, horror which already existed will be considered less so—this is the fate of man!

The depiction of the World War as deterrent is also meaningful, but only as a subordinate means of propaganda. Leaders, wide circles of thoughtful and energetic people, who are very necessary for the peace movement, have not been decisively influenced through this. Who didn't know that wars cause pain? Who didn't know that robbery, violation, brutality of any kind occur in all wars? We know how little the sheer fear of venereal diseases deters young people from haphazard sexual intercourse. It is quite different things that come into play here, especially spontaneous instincts which stem from a feeling of duty and similar sentiments, as

III

religious communities, political parties and other organizations are able to show. The Quakers, who heroically resisted to be drafted for military service, didn't do so because of their pity with the victims, but because of their general world view. Pity towards others and care for one's own well-being are not the decisive motivators in a person's life, as all brutal sports, mountaineering, tournaments, fencing bouts and many other activities prove. The peace movement will only sink into the consciousness of wide circles when it it understood as part of a development of humankind which is reflected in everything, so also in the World War, and which carries with it the promise of a future which will bring happiness. The creation of a new order of life which doesn't know any wars must appear as historical necessity and as an aim. This would be the aim of an encyclopedia of the World War, if it should be more than a collection of anecdotes in a higher sense. Also the collection of literary creations only has a permanent value if it represents the direction of the mental development. The mere fact that there is a literature which depicts the war as terrible is of little importance! The task is a huge one. But even the weakest attempt has to be undertaken so that at least everyone becomes aware of what insights are necessary. Below we will sketch the structure of such an encyclopedia so that we can serve such an attempt, however incomplete it may be. There is more than one solution, there are better ones than the one introduced here.

IV

It goes without saying that all the facts listed in the encyclopedia must be presented truthfully. It is equally imperative that the selection must be made objectively. The full scope of the war must be presented, from all the magnanimity, dedication, loyalty, all the sublime[81], which the war brought with it, to all the horrible, the cruel, the pain, the vileness. One-sidedness will only deter the calm ones who we aim to win. Nobody wants to be deceived. The argument that one's opponents lied and cheated doesn't convince.

Scholarly experts shall proceed in a scholarly spirit; however, the overall idea which guides every individual cannot evolve from scholarship; it rests on conviction, on belief. One can only demand that it doesn't conflict with the facts. It is of the utmost importance how the experts are chosen. Selection only on grounds of knowledge of facts would mean that the encyclopedia project will be usurped by opponents of war [the author probably meant opponents of peace; editors' note]. The experts are in the majority of cases dependent on the governments; in many cases they have been made professors, heads of departments, civil servants etc. because of their views going in a certain direction, however without doing research against their conviction. That doesn't mean that the authors of the encyclopedia must be supporters of the peace movement, least in its haphazard present state, in order to fulfil this task adequately.

## V

### Organization of the content

A. Before the World War
- 1. Types of great wars in various cultures and eras[82]
  - a. Raids, migration[83]
  - b. Other
  - c. Thirty Years' War, Napoleonic Wars
- 2. Origins and conditions
  - a. Geographic
  - b. Governmental
  - c. Economic
  - d. National
  - e. Religious
  - f. Emotional
  - g. Other
- 3. Preparations
  - a. Governmental (alliances, laws concerning contributions to wars etc.)
  - b. Military (mobilizing plans, armament etc.)
  - c. Economic
  - d. Other (revenge propaganda, philosophical tenets etc.)
- 4. Predictions (scholarship and literature)

81 Comment of five lines in the left margin, probably: "This has been depicted enough. Objectivity now calls for the negative side."

82 Indecipherable comment of one line, probably in shorthand, in the left margin.

83 Handwritten amendment at this position: "Migration of the peoples to Djingis Khan."

a. Scope and outcome
b. War strategies
c. Organization of the economy
d. Other

B. The World War
1. Outbreak and development in outlines
2. Sociology and organization
a. Interstate relations
aa. Type of war strategy
aaa. Military
bbb. Administrative
ccc. Other
bb. Position of the neutral states
b. Supranational relations
aa. Formation and strengthening of international relations (Panturanic[84] Movement etc.)
bb. Weakening and destruction of supranational relations (e.g. in the case of the Austro-Hungarian Monarchy a supranational unity on a dynastic basis)
c. Phenomena within a state
aa. Mass motives
aaa. Defense
bbb. Fight for freedom, justice, culture etc.[85]
ccc. Pugnacity etc.
bb. Peace forces
aaa. Internationalism[86]
aaaa. Socialism
bbbb. Catholicism[87]
cccc. Freemasonry
dddd. Peace movement
eeee. Other
cc. Economic centralism and planning
dd. Economy and wastefulness
ee. Boost and reduction of a sense of community
ff. Subjugation and liberation of individual groups

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84 "Panturanic" is underlined by hand; in the left margin of the text a handwritten question-mark is added.

85 Here "Freemasonry!" is added in handwriting.

86 In the right-hand margin next to the category "Internationalism" the hand-written amendment: "4f [ffff] surfeit a[nd] war-weariness 4 g [gggg] Nazarenes and followers of Tolstoi."

87 A question-mark added by hand in the right-hand margin might refer to the word "catholicism" also marked slightly by hand.

gg. Moving of masses of people
hh. Administration, jurisdiction, the military etc.
ii. Destructions
aaa. Killings, mutilations, wounding, disease
bbb. Substances and powers
ccc. Comparison with peaceful state
aaaa. Crises, suffering, unemployment etc.
bbbb. Other
ddd. Comparison with other wars

VI

kk. Social phenomena
aaa. Social hygiene
bbb. Marriages, births, deaths etc.
ccc. Upbringing, education
ddd. Crime, prostitution etc.
eee. Other
d. Individual items
aa. Love and hate (pity, revenge, cruelty etc.)
bb. Forgetting and remembering
cc. Enjoying nature, physical strength
dd. Sorrow and pleasure in general
ee. Knowledge, studies, narrowing and widening etc.
ff. Ability to act
gg. Change of fate as a possibility
e. Expressive culture
aa. Literature and writers
bb. Fine art and artists
cc. Habitation, nourishment, clothing etc.
dd. Other
f. Scholarship and scholars (Wissenschaft und Gelehrte)
g. Family, marriage, sexuality etc., friendship etc.
3. Conclusion of the World War, the peace negotiations and the peace treaty in outline (gemeint
C. After the World War
1. Governmental and national organization
a. General state
b. Alliances, economic relations etc.
c. Courts of arbitration etc.
d. Stability and instability (causes)
2. Production and consumption
a. Comparisons with the pre-war era
b. Crises among the winners, want among the losers

3. Inner structure of the state
    a. Situation of the individual classes
    b. Revolutionary tendencies
    c. Creation of a system of government
4. Mass psychology
    a. War atmosphere
    b. Revolutionary atmosphere
5. Ideas for a universal peace
    a. Antimilitary movements
    b. Universal peace as a special aim
    c. Universal peace as a subsidiary aim
        aa. Liberalism, analysis and criticism
        bb. Socialism, analysis and criticism
    d. Peace movement
        aa. Views
        bb. Political importance
        cc. Possibilities
            aaa. Inadequacy of the courts of arbitration
            bbb. Necessity of a wold authority and a surmounting of states
            ccc. Future revolutions and wars
            ddd. Conditions for universal peace
                aaaa. Geographic conditions
                bbbb. Diversity of the members of a peace community
                cccc. Spiritual, economic etc. conditions
            eee. The overall organization of life in the future

*Translation: Wiltrud Steinacker (Innsbruck/Budapest)*

Transcript of the "Encyclopedia of the World War" by Otto Neurath[88]

| | |
|---|---|
| xxx | passages which have been made illegible through overwriting them with "xxx" |
| / / | passage inserted later above the line |
| ___ | underlined words were printed in double space in the original |
| [ ] | editors' amendments |

Enzyklopädie des Weltkrieges.

von Otto Neurath – Wien.

Die Wiener Gruppe der Internationalen Frauenliga für Friede und Freiheit hat den xxx[89] Plan gefasst ein Sammelwerk über den Weltkrieg ins Leben zu rufen. Eine gewaltige und bedeutsame Aufgabe. Schwierigkeiten aller Art ergeben sich. Von den finanziellen soll ganz abgesehen werden. Es wird nicht leicht sein, für alle Fragen wirklich objektive Mitarbeiter zu finden, noch schwieriger sie einer einheitlichen Redaktion zu unterwerfen. Vor allem auch deshalb, weil wir bis jetzt über keine ausreichenden[90] xxx[91] systematischen Vorarbeiten verfügen, die uns gestatten würden, jedem Einzelproblem seinen Platz zuzuweisen. Während xxx wir wissen, wie man unter dem Gesichtspunkt der Kriegsgeschichte oder der politischen Geschichte eine solche Aufgabe anpackt, fehlt Erfahrung darüber, wie man den Weltkrieg in seiner Bedeutung für die Entwicklung eines zukünftigen Weltfriedens analysieren soll, wie in seinem Einfluss auf kriegsfördernde und kriegshemmende Einrichtungen, Neigungen sowie sonstige Tatbestände. Die blosse Aufzählung der xxx[92] Erscheinungen und Wirkungen des Weltkrieges kann Grauen und Abscheu in reichem Masse erwecken. Das genügt aber nicht, wenn man der Friedensbewegung xxx[93] /in entscheidender Weise /nützen will. Man hat die Schrecken des Dreissigjährigen Krieges durch Grimmelshausen, durch Gustav Freytag und andere Schriftsteller ausreichend kennen gelernt, auch die der napoleonischen Kriege

88 Explanations of the transcription: Overwritten and illegible passages in Otto Neurath's text were marked with xxx by the editors. The footnotes give the original version, if it could be deciphered. Overwritten and illegible passages which could not be deciphered for sure are labelled as "probably" in the footnotes. Obvious typing errors were corrected by the editors without comment. Handwritten comments had been added to Otto Neurath's typescript. They probably do not stem from the author himself, since they partly represent a contradiction, and partly they ask questions about the meaning of individual words. All handwritten comments are retained or commented on in the editorial footnotes.

89 Probably "Gedanken an" (replaced by "Plan gefasst")

90 The flectional "n" was added later.

91 Probably "ausreichende gedankliche Vorarbeiten verfügen" (replaced by "ausreichenden systematischen Vorarbeiten verfügen").

92 "Wirk[ungen]" (replaced by "Erscheinungen und Wirkungen").

93 "irgendwie" (replaced by "in entscheidender Weise").

sind allgemein bekannt geworden. Berta Suttners packende Schilderungen haben wahrlich genug des Abschreckenden gebracht. Das xxx[94] hilft alles nichts, wenn nicht andere treibende Kräfte da sind, die uns eine xxx[95] Friedenszukunft erhoffen lassen. Ohne solche Kräfte werden die Greuel zur packenden Indianergeschichte oder zum Nachtmahr, das uns Wehrlose bedrückt.

II

Ja[,] wenn zum Krieg drängende Kräfte wahrhaft lebendig sind, kann die Schilderung der Schrecken mutige Seelen nur dazu veranlassen, rechtzeitig sich[96] innerlich gegen derlei abzustumpfen! Wer glaubte, wohl einen Inquisitor von seiner traurigen Aufgabe durch die Beschreibung des Leidens seiner Opfer abhalten zu können? Der echte Inquisitor musste durch dauernde Selbsterziehung geradezu lernen, alles Mitleid als nichtiges Spiel des Augenblicks / zu[97] / verachten xxx[98]. Und wenn Sozialisten an blutige Revolution denken, nicht weil sie sie wünschen, sondern weil sie sie für unausweichlich halten, müssen sie nicht oft wider ihre Natur ihr Inneres geradezu verhärten? Schildert man ihnen die Schrecken eines Aufstandes, xxx / nach Art der / grossen französischen Revolution oder der Sowjetkämpfe, dann wird in ihnen vielfach der Gedanke lebendig: also mit all dem müssen wir uns abfinden, um der Sache zu dienen, für die wir Opfer zu bringen bereit sind. Die Schilderung aller Leiden, die Schilderung aller Korruption übt nur eine geringe Hemmung aus, wenn die breiten Massen[99] den Kampf um die Macht beginnen! Ja, im Gegenteil, Schrecken, die bereits da waren, werden vielfach geringer eingeschätzt – Menschenlos!

Die Darstellung des Weltkrieges als Abschreckungsmittel[100] hat auch eine Bedeutung, aber doch nur als ein mehr untergeordnetes Propagandamittel. Führende Männer, xxx, breite Kreise besonnener und energischer Menschen, deren die Friedensbewegung dringend bedarf, waren dadurch xxx wahrlich nicht / wesentlich / beeinflusst. Wer wusste nicht, dass Kriege Jammer bringen? Wer wusste nicht, dass Raub, Schändung, Brutalität jeder Art in xxx[101] allen Kriegen auftreten? xxx[102] Wir wissen, wie wenig die blosse Angst vor Geschlechtskrankheiten, die Jugend von ungeregeltem Geschlechtsverkehr zurückhält. Da sind ganz andere

94 “genügt nicht” (replaced by “hilft alles nichts”).
95 “Zukunft” (replaced by “Friedenszukunft”).
96 The reflexive usage of the verb “abstumpfen” is not common in German.
97 “zu” was added in handwriting.
98 Probably “verachten zu lernen” (replaced by “verachten”).
99 The original contains a comma here.
100 The original contains a comma here.
101 Probably “jedem” (replaced by “allen”).
102 Probably “Es genügen Furcht und”.

Dinge wirksam. Vor allem unmittelbare Antriebe, die auf Pflichtgefühl und verwandten Gefühlen beruhen, wie sie xxx[103]

III

Religionsgemeinschaften, politische Parteien und andere Organisationen zu zeigen vermögen. Die Quaecker [sic!], welche heldenmütig sich geweigert haben Kriegsdienst zu leisten, haben das nicht getan, weil sie Mitleid mit den Kriegsopfern hatten, sondern aus einer umfassenden Gesamtanschauung heraus. Mitleid mit anderen und Sorge um eigenes xxx[104] /Behagen sind nicht die entscheidenden /Triebfedern im Menschenleben, wie jedem brutaler Sport, Bergbesteigungen, Tourniere, Mensuren, und vieles andere beweisen. Die Friedensbewegung wird erst dann im Bewusstsein grosser Kreise wirklich Fuss fassen, wenn sie als Teil einer Menschheitsentwicklung erscheint, die sich in allem, so <u>auch im Weltkrieg[,] offenbart</u> und eine <u>Zukunft</u> verspricht, die heraufzuführen beglückend ist. Die Gestaltung einer neuen Lebensordnung, welche keine Kriege kennt, muss als <u>geschichtliche Notwendigkeit</u> und als <u>Ziel</u> angeschaut werden können. Das hätte eine Enzyklopädie des Weltkrieges zu leisten, soll sie mehr sein, als eine Sammlung xxx[105] von Anekdoten in höherem Sinne. Auch die Sammlung von Literaturerzeugnissen ist nur dann von bleibendem Wert, wenn dadurch eine Richtung der geistigen Entwicklung gekennzeichnet wird. Die blosse Tatsache, dass es eine Literatur gibt, welche den Krieg schreckhaft schildert, hat wenig zu bedeuten! Die Aufgabe ist eine ungeheuere. Aber selbst der schwächste Versuch muss so unternommen werden, dass mindestens allen bewusst wird, um welche Einsicht gerungen wird. Es sei im folgenden angedeutet, wie eine solche Enzyklopädie aufgebaut werden könnte, um bei aller Mangelhaftigkeit, doch solchem Bestreben zu dienen. Es gibt mehr als eine Lösung, bessere, als die hier vorgeführte.

IV

Dass alle in der Enzyklopädie mitgeteilten Tatsachen wahrheitgemäss wiedergegeben werden, ist selbstverständliche Forderung, dass sogar die Auswahl objektiv getroffen wird, ist ebenso berechtigte Forderung. Es muss ebenso berichtet werden von aller Hochherzigkeit, von aller Hingabe, von aller Treue, von allem Erhebenden,[106] das der Krieg xxx gebracht hat, wie von allem Scheusslichen, aller Grausamkeit, allem Jammer, aller Niedertracht. Einseitigkeit hat nur zur Folge, dass die ruhig gesinnten, um deren Gewinnung wir ringen, sich enttäuscht ab-

103 "insbesondere Religionsgenossenschaf" [sic!] replaced by "Religionsgemeinschaften".

104 Probably "Heil und Leben sind verhältnismässig schwache".

105 Probably "von ziellos".

106 Comment of five lines in the left margin, probably: "Das ist genug geschildert worden. Die Objektivität verlangt nun die Schattenseite."

wenden. Niemand lässt sich gerne hinters Licht führen. Es ist ein verfehltes Argument, darauf hinzuweisen, die Gegenpartei habe genug gelogen und betrogen.

Wissenschaftlich geschulte Sachverständige sollen in wissenschaftlichem Geiste darstellen. Die Gesammtidee [sic!], die jeden Einzelnen aber leitet[,] kann nicht selbst aus der Wissenschaft entnommen werden, sie beruht auf Ueberzeugung, auf Glauben. Es kann nur gefordert werden, dass sie nirgends mit den Tatsachen in Widerspruch tritt. Es ist von grösster Wichtigkeit, wie man die Sachverständigen auswählt. Blosse Auswahl auf Grund des Sachverständnisses, bedeutet Auslieferung der Enzyklopädie an die Kriegsgegner [gemeint war vermutlich „Friedensgegner“, Anm. der HerausgeberInnen]. Die Sachverständigen sind in der Mehrzahl der Fälle[107] abhängig von den Regierungen, sind ohne wider ihre eigene Ueberzeugung zu forschen, dennoch in sehr vielen Fällen, um ihrer in bestimmte[108] Richtung gehenden Ueberzeugungen willen in die Lage versetzt worden, als Professoren, Institutsvorstände, Beamte usw. wissenschaftlich tätig zu sein. xxx[109] Das bedeutet noch lange nicht, dass die Verfasser der Enzyklopädie Anhänger der Friedensbewegung oder gar in ihrer zufälligen gegenwärtigen Form sein müssen, um diese Aufgabe entsprechend lösen zu können.

## V
## Gliederung des Stoffes[110]

A. Vor dem Weltkriege
  1. Grosskriegtypen verschiedener Kulturen und Zeiten[111]
    a. Kriegszüge, Wanderungen usw.[112]
    b. Sonstiges
    c. Dreissigj[ähriger] Krieg, Napoleonische Kriege
  2. Ursachen und Bedingungen
    a. geographische
    b. staatliche
    c. wirtschaftliche
    d. nationale
    e. religiöse
    f. gefühlsmässige
    g. sonstige
  3. Vorbereitungen
    a. staatliche (Bündnisse, Kriegsleistungsgesetze usw.)

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107 The comma in the original changes the meaning and has probably been put erroneously.

108 A flectional “r” at the end of the word was subsequently overwritten.

109 “Unbedingte Gegner des Krieges”.

110 The punctuation in the overview was tacitly standardised by the editors.

111 Indecipherable comment of one line, probably in shorthand, in the left margin.

112 Handwritten amendment at this position: “Völkerwanderung bis Dschingis-Chan”.

b. militärische (Mobilisier[ungs]pläne, Rüstungen usw.)
c. wirtschaftliche
d. sonstige (Revanchepropaganda usw., Weltanschauung usw.)
4. Voraussagen (Wissenschaft und Dichtung)
a. Umfang und Ausgang
b. Methoden der Kriegsführung
c. Wirtschaftsorganisation
d. Sonstiges.
B. Der Weltkrieg
1. Ausbruch und Verlauf in Umrissen
2. Soziologisches und organisatorisches
a. zwischenstaatliche Beziehungen
aa. Art der Kriegsführung
aaa. Militärisch
bbb. Verwaltung
ccc. Sonstiges
bb. Stellung der Neutralen
b. Uebernationale Beziehungen
aa. Neuentstehung und Stärkung übernationaler Beziehungen (Panturanische[113] Bewegung usw. usw.)
bb. Schwächung und Zerstörung übernationaler Beziehungen (z.B. inOesterr[eich-]Ungarn einer übernat[ionalen] Einheit auf dynastischer Grundlage)
c. Innerstaatliche Erscheinungen
aa. Massenmotive
aaa. Verteidigung
bbb. Kampf für Freiheit, Recht, Kultur usw.[114]
ccc. Rauflust usw.
bb. Friedenskräfte
aaa. Internationalismus[115]
aaaa. Sozialismus
bbbb. Katholizismus[116]
cccc. Freimaurerei
dddd. Friedensbewegung
eeee. Sonstigem

113 "Panturanisch" is underlined by hand; in the left margin of the text a handwritten question-mark is added.

114 Here "Freimaurerei!" is added in handwriting.

115 In the right-hand margin next to the category "Internationalismus" the hand-written amendment: "4 f [ffff] Überdruss u[nd] Kr[iegs-]Müdigkeit 4 g [gggg] Nazarener + Tolstoianer".

116 A question-mark added by hand in the right-hand margin might refer to the word "Katholizismus" also marked slightly by hand.

cc. Wirtschaftszentralismus und Planmässigkeit
dd. Sparsamkeit und Verschwendung
ee. Steigerung und Verringerung des Gemeinsinns
ff. Unterjochung und Befreiung einzelner Gruppen
gg. Verschiebung von Menschenmassen
hh. Verwaltung, Rechtsprechung, Heerwesen usw.
ii. Zerstörungen
aaa. Tötungen, Verstümmelungen, Verwundungen, Erkrankungen
bbb. Stoffe und Kräfte
ccc. Vergleich mit Friedenszustand
aaaa. Krisen, Leiden, Arbeitslosigkeit usw.
bbbb. Sonstiges
ddd. Vergleich mit anderen Kriegen

VI

kk. Soziale Erscheinungen
aaa. Sozialhygiene
bbb. Ehen, Geburten, Todesfälle usw.
ccc. Erziehung, Unterricht
ddd. Verbrechen, Prostitution usw.
eee. Sonstiges
d. Individuelles
aa. Liebe und Hass (Mitleid, Rache, Grausamkeit usw.)
bb. Vergessen und Erinnern
cc. Naturfreude, Körperliche Tüchtigkeit usw.
dd. Leid und Lust im allgemeinen
ee. Kenntnisse, Studien, Verengung, Erweiterung usw.
ff. Aktionsfähigkeit
gg. Aenderung des Lebensschicksals als Möglichkeiten
e. Ausdruckskultur
aa. Dichtung und Dichter
bb. Bildende Kunst und Künstler
cc. Wohnung, Nahrung, Kleidung usw.
dd. Sonstiges
f. Wissenschaft und Gelehrte
g. Familie, Ehe, Geschlechtsleben usw., Freundschaft usw.
3. Abschluss des Weltkrieges, der Friedensverhandlungen und des Friedens in Umrissen
C. Nach dem Weltkriege
1. Staaten- und Völkerordnung
a. Gesamtlage

b. Bündnisse, Wirtschaftsbeziehungen usw.
c. Schiedsgerichte usw.
d. Stabilität und Instabilität (Ursachen)
2. Produktions- und Konsumverhältnisse
a. Vergleiche mit Vorkriegszeit
b. Krisen bei Siegern, Not bei Besiegten
3. Innere Struktur des Staates
a. Lage der einzelnen Klassen
b. Revolutionäres
c. Aufbau der Staatsordnung
4. Massenpsychologie
a. Kriegsstimmung
b. Revolutionsstimmung
5. Weltfriedensideen
a. Antimilitaristische Bewegungen
b. Weltfrieden als Sonderziel
c. Weltfrieden als Nebenziel
aa. Lieberalismus [sic!], Analyse und Kritik
bb. Sozialimus, Analyse und Kritik
d. Friedensbewegung
aa. Anschauungen
bb. politische Bedeutung
cc. Möglichkeiten
aaa. Unzulänglichkeit der Schiedsgerichtsidee
bbb. Notwendigkeit einer Weltinstanz und Ueberwindung der Staaten
ccc. Zukunftsrevolutionen und Zukunftskriege
ddd. Voraussetzungen des Weltfriedens
aaaa. geographische Voraussetzungen
bbbb. Mannigfaltigkeit der Glieder einer Friedensgemeinschaft
cccc. seelische, wirtschaftliche usw. Vorauss[e]tz[ung]
eee. die Gesamtlebensordnung der Zukunft

Thomas Mormann

# One Hundred Years of Philosophy of Science: The View from Munich

Carlos Ulises Moulines, 2008, *Die Entwicklung der modernen Wissenschafts-theorie (1890–2000): Eine historische Einführung*, Lit Verlag, Hamburg, 210 pp.

These days, a number of philosophers of science indulge in lamenting about a crisis of their discipline. They complain about its loss of relevance, and bemoan the marginalization of their discipline in the philosophical community and in the wider academia (cf. Howard (2003, 75), Hardcastle and Richardson (2003)). The Munich take on the philosophy of science does not succumb to this temptation. According to it, philosophy of science is well and alive. In Carlos Ulises Moulines's *Die Entwicklung der modernen Wissenschaftstheorie (1890–2000) Eine historische Einführung* (henceforth *Einführung*) the word "crisis" is used only in reference to the 1940s when classical logical positivism encountered some difficulties in dealing with problems concerning verification, the analytic/synthetic distinction, and similar conundrums. For Moulines, "crisis" is not a word that applies to contemporary philosophy of science. My expectations to find an encouraging piece of philosophy of science, something one doesn't come across so often today, grew, when I hit upon a Mexican review of the French version of *Einführung* that concluded with the enthusiastic verdict that "the community of philosophers of science may congratulate themselves for the publication of this book" (*Crítica* 38 (2006), 120). Not only the French but also the German version of Moulines's book has found an extremally positive reception in certain quarters. A recent review of *Einführung* closes with the following acolades: "Without overstatement we claim that *[Einführung]* is the best historical overview of modern philosophy of science that has been published in German", and, the enthusiastic Austrian reviewers felt obliged to add, "[it is] the one and only existing book of this kind" (*Journal of General Philosophy of Science* (2010), DOI 10.1007/s10838-010-9133-x). So it seems well worth our time to take a closer look at Moulines's achievements.

The view from Munich is not just any view. On a map of contemporary philosophy Munich is, if one happens to consult a German atlas, the home of the *Münchner Schule*, founded by the Austro-German philosopher Wolfgang Stegmüller some forty years ago (cf. Stadler 2010). In 1993 Carlos Ulises Moulines followed Stegmüller on the chair of philosophy, logic, and philosophy of science at the University of Munich. Together with Wolfgang Balzer he is a leading figure of the so-called structuralist philosophy of science founded by Joseph Sneed and

B.F. McGuinness (ed.), *Friedrich Waismann – Causality and Logical Positivism*,
Vienna Circle Institute Yearbook 15, DOI 10.1007/978-94-007-1751-0_12,

Stegmüller in the 1970s, building on the work of Patrick Suppes in the 1950s. Thus, someone interested in the German scene of philosophy of science should take notice of how one of the protagonists of the *Münchner Schule* describes the evolution and the present state of his discipline at the beginning of a new century.[1]

A French version of *Einführung* was published in 2006 under the title *La philosophie des sciences. L'invention d'une discipline*. This title more clearly expresses the basic intention of the book than the blander German terms "*Entwicklung*" and "*Einführung*". The French and the German version are not identical. Some extra pieces have been added to the German version. The most significant changes are an expanded preface, and two new sections on more recent developments in philosophy of science, among them a half-section on the "New Experimentalism" of Ian Hacking (175ff.) and a section on the "Structural Realism" of John Worrall (188ff).

*Einführung* is not only intended to be an historical introduction to philosophy of science, it is also supposed to be a work of history of philosophy of science that presents a substantial thesis on how this discipline developed and how the results of this development are to be assessed. Due to its panoramic character the book deals with a variety of different issues related in one sense or other to philosophy of science. I do not intend to discuss them all in this review. Instead, I'll concentrate on a few that may be particularly interesting for assessing *Einführung*.

As Moulines rightly remarks, *Einführung* does not offer novelties to the expert in matters of history of philosophy of science, rather it seeks to give an overall account of the evolution of the discipline during the last century. According to the author, this has been a desideratum until now, since, according to his knowledge, *Einführung* is the first panoramic survey of this kind in any language (ibid., 7):

> Seit ihrer Geburtsstunde in den 80er Jahren des 19. Jahrhunderts hat unsere Disziplin (die Wissenschaftstheorie, T.M.) eine … Entwicklung erfahren, die … als globaler Prozess noch sehr ungenügend erforscht worden ist.
>
> In den letzten Jahren habe ich mich verstärkt der ideengeschichtlichen Problematik meines eigenen Fachs gewidmet, in der Hoffnung einen Beitrag zur Schließung dieser historiographischen Lücke zu leisten. Ein erstes Ergebnis dieser Bemühungen ist … *La Philosophie des sciences. L'invention d'une discipline*. (*Die Entstehung der Wissenschaftstheorie als interdisziplinäres Fach*, 3).[2]

1 Since up to now there is no English translation of *Einführung* I think it is appropriate to give the quotes in German. This may enable the reader to get a feeling of the original that otherwise may be lost.

2 *Die Entstehung der Wissenschaftstheorie als interdisziplinäres Fach* is a lecture that was given at a meeting of the *Bavarian Academy of Sciences*. It is essentially a version of the first chapter of *Einführung*. This evidences that the author considers *Einführung* not merely as an introductory text but also as a serious contribution to the history of philosophy of science.

The basic thesis of *Einführung* is that the evolution of philosophy of science follows a "dialectical" pattern of different phases such that the structuralist theory of science, favored by the author, is to be considered as a kind of dialectical synthesis of earlier phases; in particular, structuralism is the only existing account of philosophy of science that does justice to the historicist (diachronic) *and* structural (synchronic) aspects of scientific knowledge. This is non-trivial and probably controversial thesis, but not quite new—already Stegmüller in *A Combined Approach to the Dynamics of Theories. How to Improve Historical Interpretations of Theory Change by Applying Set Theoretical Structures* (Stegmüller 1979) had put forward a similar claim:

> I even dare to predict that at present his ideas (i.e. Sneed's, T.M.) form the best foundation in order to bridge the systematically oriented and the historically oriented philosophy of science. (Stegmüller 1979, 152)
>
> …
>
> It is my *hope* that some of the logical reconstruction sketches given in this paper will contribute to a better understanding of the dynamic aspects of theories and to the erection of a stable bridge between the systematically and the historically (as well as psychologically) oriented philosophy of science. (ibid. 181)

Indeed, as we shall see, *Einführung* remains faithful to the spirit and the style of the founder of the *Münchner Schule*. This holds also for the blind spots to be found in both accounts.

In chapter I the author sketches the different phases of the evolution of philosophy of science that in the subsequent chapters II – VI are treated in greater detail. From its beginnings in the last decades of the 19th century to the end of 20th century he distinguishes five "phases" in the development of philosophy of science:

II. Preformation (1890–1918)
III. Unfolding (1918–1935)
IV. Crisis and Consolidation (1935–1970)
V. Historicism (1960–1985)
VI. The Model-theoretic Account (1985–2000)

Moulines's "phases" are not just historical periods. Rather, phases are characterized by the specific aspect of science that they emphasize at the expense of others. For instance, the preformative phase is characterized by emphasizing the historical evolution of scientific knowledge, while it has not much to say about the logical structure of scientific knowledge. In contrast, the subsequent phase of unfolding, which historically may be roughly identified with the heyday of the Logical Empiricism of the Vienna Circle, is said to have laid more emphasis on the investigation of the logical structure of theories while ignoring the historical development. Indeed, the author contends, there is a sort of dialectics between subsequent phases. The ultimate phase of the model-theoretic account is distinguished from the earlier ones through the fact that here at last a kind of synthesis is reached in which the achievements of the earlier phases are “sublated” (*aufgehoben*) in a Hegelian sense. This is, of course, the merit of the structuralism of the *Münchner Schule*.

The history of philosophy of science is not a virgin field (cf. Uebel 2010). On the contrary. In the last decades, perhaps due to a certain stagnation of philosophy of science proper, a plethora of articles, monographs, and anthologies has been published on a wide variety of issues dealing with the history of philosophy of science. One may contend that today we understand the past of the philosophy of science better than ever before. This holds in particular for Logical Empiricism and Neokantianism whose contributions to the evolution of the discipline have been seriously misunderstood or were simply ignored in the past. For readers of the *Yearbook* it is hardly necessary to give a complete list of authors who contributed to this development. But for the sake of clarity, let us mention authors such as Coffa, Creath, Ferrari, Friedman, Haller, Reisch, Ryckman, Stadler, Uebel, to name but a few. *Einführung* does not mention one of them.

In the preface of *Einführung* Moulines offers a kind of explanation of this remarkable fact. According to him, *Einführung* does not intend to compete with “Einzelstudien” primarily written for professional philosophers. Rather, *Einführung* has been written with non-specialized readers in mind who “might have heard that there is a discipline named ‘Wissenschaftstheorie’ and wish to learn something about its general development (8)”.[3] In other words, he contends that the “Einzelstudien” do not contribute anything to our understanding of the global development of philosophy of science. This is a highly questionable thesis. I think that the distinction between “Einzelstudien” concerned with historical details and technicalities on the one hand, and general treatises that deal with the global picture and the broad lines of the evolution does not hold water. Rather, many of the works that have to be characterized as “Einzelstudien” brought about profound revisions of the conventional wisdom and the traditional pictures that dominated the discourse of history of philosophy for decades. This holds, as we shall see, for the received view of standard Logical Empiricism, but also for the role of Kant and Kantian philosophy for 20th century philosophy of science, and many other issues. In other words, by leaving aside the research on history of philosophy of science of the last twenty years *Einführung* ends up seriously distorting

3 A side remark on the bibliography of *Einführung*: A natural requirement for the bibliography of an introductory treatise is to mention the most accessible editions of the literature used. For some twenty years or so cheap and accessible German translations or editions of the works of Bachelard, Kuhn, Lakatos, Neurath, Popper, Schlick are available. *Einführung* mentions none of them. Instead, the German reader is advised to consult Pierre Wagner’s anthology *Les Philosophes et la Science*. Wagner’s anthology is certainly useful for French readers but is of limited use for German-speaking beginners.

some of the essential features of the evolution of philosophy of science in the last century, or so I claim.

Let us consider a handful of examples of how key episodes and issues are treated in *Einführung*. As many authors before him, Moulines chooses Kant as the starting point of the prehistory of modern philosophy of science. In keeping with his general strategy described above none of them is mentioned in *Einführung*. I don't think that such a strategy can be justified in the case of Kant. As evidence we may take the role of Neokantianism. Moulines makes short shrift with the role of Neokantianism for the new emerging discipline of philosophy of science:

> Man muß jedoch zugeben, daß der Neukantianismus kaum zur Bildung der spezifischen Thematik der modenen Wissenschaftstheorie beigetragen hat. (22)

This claim directly flies into the face of much solid work on history of philosophy of science that has been carried out in the last twenty years. Take, for instance, Coffa's trail-blazing monography *From Kant to Carnap. To the Vienna Station* (Coffa 1991). There, Coffa gave the following report on the relation between Neokantianism and "Viennese positivism":

> All of the leaders of Viennese positivism began their philosophical path as neokantians, in particular Schlick. The particular brand of neo-kantianism [Schlick] endorsed had been inaugurated in the writings of Helmholtz and developed by other great scientists, including Planck. Indeed, Helmholtz himself considered his philosophy of science as sort of a scientifically improved Kantianism. (Coffa 1991, 171)

Coffa's *From Kant to Carnap* spawned a wealth of further studies investigating the role of (Neo)-kantianism in the evolution of modern philosophy of science. Here, *e pluribus unum*, the work of Michael Friedman may be mentioned. As many studies Friedman's confirm that Moulines's verdict on the unimportance of Neokantian philosophy for philosophy of science is hardly tenable.

Chapter II of *Einführung* deals with the "preformative" phase at the end of the 19th and early 20th century when the new discipline unfolded. Its most interesting section examines the role of Ernst Mach for the fledgling philosophy of science. Today, Mach is a relatively unknown figure outside the circle of professional philosophers of science. Hence it is to be highly welcomed that he is treated in an introductory treatise. *Einführung* concentrates on Mach as one of the founding fathers of monism. According to Machian monism there are nothing but "sensations" which are the common elements of all possible physical and psychical experiences, which merely consist in the different kinds of ways in which these elements are combined, or in their dependence on one another. Mach's theory of "elements" (sensations) sought to connect physics, physiology and psychophysics and to provide a solid, non-metaphysical base for all of science. More generally, he conceived the language of "elements" and their relations as a medium usable for all the sciences. This Machian program of a unified science was not only theoretically motivated, Mach characteristically conceived science as a part of a progressive enlightenment and gradual emelioration of human life. Although these "political" aspects of Mach's philosophy of science played an important role for his later influence, in particular on the Logical Empiricism of the Vienna Circle, they are hardly mentioned in *Einführung*.

Let us have now a look on the central chapter III – the phase of "unfolding" (40-59). It primarily deals with the contribution of the Vienna Circle and its branches to modern philosophy of science. Without any doubt, the Circle played a crucial role for the development of our discipline, and an introductory text should get it right. In *Einführung* one finds the following description of the *Verein Ernst Mach* for the logical empiricist philosophy of science:

> … 1928 [wurde] unter [Schlicks] Vorsitz der *Ernst-Mach-Verein* (sic) gegründet, ein Zusammenschluss wissenschaftlich gebildeter Philosophen und Fachwissenschaftler mit philosophischen Interessen, die sich regelmäßig trafen, um alle Arten philosophischer Fragen in wissenschaftlichem Geist zu diskutieren. Weniger offiziell war die Gründung des Wiener Kreises … durch die Mehrzahl der Mitglieder jener Vereinigung im Jahr 1929. (47)

In earlier writings the author even maintained that Schlick founded the *Verein Ernst Mach* and that this society, to be considered as the first institutionalized group of philosophers of science, gradually "changed into the Vienna Circle" (Moulines 2000, 486). Actually, things were quite different. The *Verein* was part of the Vienna system of adult education closely related to what can be succinctly described as the cultural and political network of *Red Vienna*. Originally the *Verein* was founded as *Allgemeiner Naturwissenschaftlicher Bildungsverein Ernst Mach* by the *Österreichischer Freidenkerbund* (*Austrian Freethinkers' Association*). According to the statutes, the aim of the *Verein* was

> to promote the ideas and findings of natural science by offering courses, presenting lectures and papers, organizing guided tours and excursions and providing scientific literature. (Stadler 1997, 364)

The first official lecture in the *Verein* was given by Philipp Frank on "Travel Impressions of the Scientific World Conception in Russia", later, his brother, the architect Josef Frank, gave a lecture on "The Modern World Conception and Modern Architecture", and the notorious Wilhelm Reich delivered a talk dealing with a topic of psychoanalysis (cf. Stadler 1982, 1997).

Characterizing the *Verein Ernst-Mach* as an institutionalized group of philosophers and scientists interested in academic discussions on issues of philosophy of science misses the point. It plays down the political aspects of the Viennese Logical Empiricism. The Vienna Circle appears as an academic and unpolitical discussion circle which renders the Circle's conception of philosophy of science rather similar to that propagated by the *Münchner Schule* some decades later.

In *Einführung* only thin traces of the political dimension of the Vienna Circle's philosophy of science survive when, for instance, Neurath's account of philosophy of science is described as driven by strong "social-pedagogical motifs" (55). Rather cryptically, the author asserts that the positions of the Vienna Circle philosophers influenced "the Social-democratic program". Here are the plain facts: Neurath was a confessed although non-orthodox marxist, Hahn, Frank, Carnap and others characterized themselves as socialists (cf. Hegselmann 1979, Stadler 1997).

Today it is rather unanimously recognized that the Logical Empiricism of the Vienna Circle had strong ties to politically "progressive" currents in a quite similar vein as Machian positivism was almost universally understood to be liberal and progressive in its political implications. The shift to a socially disengaged, depolitized philosophy of science took place in the US in the 1950s. Ignoring this difference many philosophers and historians of philosophy tended to consider logical empiricism as an apolitical philosophical movement. As a result, after the Second World War *Wissenschaftstheorie* in Germany was generally associated with conservative or reactionary political currents. This held in particular for the *Münchner Schule*. Its founder Stegmüller was explicitly against any kind of "engaged" philosophy of science. When he was asked why he did not include a chapter on marxism in his *Hauptströmungen der Gegenwartsphilosophie* he replied that instead of "including a strange piece of contemporary theology he would rather prefer to include some honest piece of contemporary science." Since then, a disengaged and depolitized conception has been a characteristic feature of the philosophy of science of the *Münchner Schule*. Moulines's (structuralist) philosophy of science faithfully follows the founder in this respect:

> [S]tructuralism is a theory about science. But, of course, it is *not* a theory about *every* aspect of science. For example, it is not a theory about the ethical or political aspects of science, … (Moulines 1996, 2)

Let us now have a closer look on how *Einführung* deals with some key themes of 20th century philosophy of science. *Einführung* rightly emphasizes the importance the so-called protocol-sentence debate for the development of Logical Empiricism and for 20th-century philosophy of science as a whole (51f). In the last twenty years or so, some book-length treatises have been dedicated to this issue, see for instance Uebel's *Overcoming Logical Positivism from within: the Emergence of Neurath's Naturalism from the Vienna Circle's Protocol-Sentence Debate* (Uebel 1991). Of course, the subtleties of Uebel's detailed reconstruction have no place in a short introductory book like *Einführung*. But it would have been helpful for the beginner if some references to the secondary literature on the protocol-sentence debate had been given.

An analogous remark applies to the discussion of Carnap's *The Logical Construction of the World* (*Aufbau*) that Moulines offers in his book. In the last decades a profusion of different, in many aspects diverging interpretations of Carnap's *opus magnum* have been put forward. It goes without saying that they cannot be mentioned in a short introductory book. But the reader of *Einführung* may expect at least to find some references to the existing literature in the bibliography.

As a final example that a short paper ("Einzelstudie") may challenge some deeply ingrained opinions about the global structure of the historical development of philosophy of science let us mention George Reisch's article *Did Kuhn Kill Logical Positivism?* (Reisch 1991) that showed the accepted conventional wisdom concerning the relationship between Kuhn's historicist account of philosophy of science and logical empiricist philosophy of science was seriously oversimplified. Reisch pointed out that Carnap and other logical empiricists did not see Kuhn as an enemy. Rather, in a letter to Kuhn he confessed: "I very much like your ideas". This flexible attitude is in stark contrast with Stegmüller's claim that virtually all traditional (logical empiricist) philosophers of science considered Kuhn's historicism as a threat of philosophy of science.

In sum, Moulines's peculiar strategy in *Einführung* of not taking into account many results of the recent detailed research on history of philosophy of science makes his global picture of the development of the discipline problematic, to put it mildly.

Let us move now to a more general level. Philosophy of science, as well as philosophy in general, is beset with lots of "-isms". Every philosophical position has to explain where it is located with respect to the most important "-isms" of the field to which it belongs. So it seems expedient to describe the position of *Einführung* with respect to "-isms" that played a more or less important role for philosophy of science in the past century. Let us begin with relativism. In *Einführung* the author shows a deep-seated aversion to any kind of <u>relativism</u>. According to him, cultural relativists put forward absurd theses like the following one:

> Die Aussage „Die Erde ist flach" kann in unserer westlichen Kultur gut und gerne falsch sein; wenn eine Gruppe Ureinwohner in Neuguinea oder sonstwo glaubt, sie sei wahr, dann ist sie auch wahr, punktum. (122/123)

One need not be a partisan of philosophical relativism to feel a certain unease about this brusque way of dismissing this philosophical stance. After all, relativistic positions of various kinds have survived in philosophy since antiquity. According to Moulines, a source of the relativist evil in 20$^{\text{th}}$ century philosophy has been Marxism:

> Das marxistische Postulat, wonach die philosophischen oder sogar die wissenschaftlichen Ideen im wesentlichen von der sozialen Schicht abhängen, der die sie unterstützenden Individuen angehören, ist dabei eine stillschweigende, aber offensichtliche Quelle der Inspiration für den modernen Relativismus. (123)

Not only Marxists will disagree with this blunt dismissal. Even for an introductory text one would ask for a more sophisticated discussion. Issues concerning relativist and Marxist positions in philosophy of science are a bit more complex than these harsh remarks suggest.

Pragmatism has a mixed appearance in *Einführung*. On the one hand, Peirce's philosophy of science is mentioned as one of two "seeds for the future" at the end of the preformative phase (the other "seed of the future" is formal logic) (38). Moulines succinctly formulates a simplified version of Peirce's famous "pragmatic maxim" (without mentioning this term). Surprisingly, there is no further reference to Peirce in the rest of the book and the bibliography. This makes it impossible for the unversed reader to figure out what the short remarks on the Peircean "seed" really amount to. This is a pity, since Peircean themes as the pragmatic maxim, the method of abduction, and the problem in what sense scientific knowledge may be conceptualized as converging to a "final theory", have been important issues on the agenda of the 20th century philosophy of science. Peirce's fellow pragmatist William James is dealt with in *Einführung* only as a partisan of monism (30f). Other pragmatists such as John Dewey, Clarence I. Lewis or Charles W. Morris are not mentioned at all. This is insofar surprising as in the midst of the 20th century pragmatism was the most important current in American philosophy. The protean figure of Putnam, who considers himself as a pragmatist in James's tradition, is not treated as such.

In line with the Anglo-Saxon meaning of "science", only the empirical sciences are the objects of philosophy of science (Wissenschaftstheorie), as the humanities (Geisteswissenschaften) or the social and cultural sciences are not considered. This may be considered as a bit odd insofar as the structuralist theory of science always proudly contended that it could handle all kinds of theories, irrespectively of whether they belonged to the realms of *Naturwissenschaften* or *Geisteswissenschaften* (cf. Balzer and Moulines 2000, Balzer 2009).

The restriction to the empirical sciences also excludes mathematics from the scope of that brand of philosophy of science that is treated in *Einführung*. According to Moulines, the philosophy of the empirical sciences and the philosophy of mathematics developed quite separately from each other (12). This move may help to keep things simple. But I am not so sure it can be justified. After all, already in his *Intellectual Autobiography* Carnap asserted that "the nature of logic and mathematics can be clearly understood only if close attention is given to their application in non-logical fields, especially empirical science" (Carnap 1963, 12). Similar theses may already be found in Frege and Cassirer. Thus for the phases of "preformation" and "unfolding" the relation between mathematics and empirical knowledge was a more important issue on the agenda of 20th century philosophy of science as *Einführung* would like us to believe.

Although *Einführung* contains a more or less implicit plea for structuralist theory of science, it is not, of course, a piece of structuralist philosophy of science proper. Rather, structuralism is presented as a member of a family of more or less similar approaches that all have their origins in the work of Patrick Suppes and his school (chapter VI). They all are characterized by the fact that the concept of a „model" plays a central role for them. In *Einführung* they are subsumed under the rather ugly name *Modellistische Ansätze* (VI). The author rightly recognizes that it is hardly possible to describe the common features and the most salient differences between them in a succinct and clear-cut way. In any case, for Moulines the best among them is the so-called "metatheoretical structuralism" of Stegmüller and Sneed. It is distinguished from the other members of the family by the fact that it is the only current that takes into account some ideas from the historicist phase of philosophy of science, in particular from Kuhn. Thus it is said to offer a kind of dialectical synthesis of the preceding more or less one-sided phases of philosophy of science. This assessment is rather similar to the one Stegmüller put forward in *A Combined Approach* (1979) where he expressed the hope that the structuralist approach would provide a "bridge between the systematically, the historically, and the psychologically oriented philosophy of science" (ibid., 181). After thirty years have passed there is not much evidence that Stegmüller's hope was more than a pious dream.

To be sure, *Einführung* is not a piece of structuralist philosophy of science proper. We are shown the promised land of structuralist philosophy of science from a distance, so to speak, but, like Moses, we do not enter into the structuralist paradise. What structuralist philosophy of science really amounts to, the reader may learn from the compilation *Structuralist Knowledge Representation. Paradigmatic Examples* (Balzer, Sneed and Moulines 2000), or, on a more elementary level, by Balzer's "textbook of structuralist philosophy of science" *Die Wissenschaft und ihre Methoden. Grundsätze der Wissenschaftstheorie, Ein Lehrbuch* (Balzer 2009).

In these works the pretension of structuralism to be the leading account of contemporary philosophy of science is expressed quite explicitly. For instance, in the preface of *Structuralist Knowledge Representation* the editors contend that the structuralist notation is the best notation for the representation of scientific knowledge available. For this claim they offer an argument that shows up in *Einführung* again:

> Our … argument … is that our representation format has passed the test of general applicability. In the literature, one can find now more than 40 reconstructions and case studies from various disciplines all using the structuralist format … looking at the examples ranging from purely qualitative theories like Freud's theory of the unconsciousness to highly mathematized physical theories in general relativity theory the claim that all scientific theories can be cast into our frame does not seem to be a bold one." (Balzer and Moulines 2000, 9).

As a co-compiler of the *Bibliography of Structuralism* (Diederich, Ibarra, Mormann 1989, 1994) I feel competent to give a more realistic estimation. Since the number of reconstructed theories does not diminish, the herbarium of structuralistically reconstructed theories today comprises at least 100 specimens.

In other words, scarcity of structuralist reconstructions is certainly not the problem. Rather, a major problem of structuralist philosophy of science is to give a convincing answer to the question “What are all these reconstructions good for?” Most simply serve as trophies evidencing the versatility of the structuralist approach. The great majority of philosophers simply ignores them, as well as the practitioners of the reconstructed scientific theories.

Structuralist reconstructions evoke the idea of glass bead games whose apparent precision cannot hide the fact that they have not much to do with real scientific knowledge. Of course, in *Einführung* matters are assessed in a different way. Rehearsing the just mentioned argument of Balzer and Moulines (2000), the author of *Einführung* considers it one of the great merits of structuralism to have “described at least fifty theories from all scientific disciplines … in all their comprehensiveness and with highest precision” (162). Frankly, I have some qualms with this alleged “highest precision”. If we conceive structuralist reconstructions (like Carnap’s constitutional systems) as maps of scientific knowledge (cf. Goodman 1963 and Kitcher 2001) the mapmaker may contend that his maps meet the highest standards of precision whatsoever, the only relevant question being whether his maps are serviceable for the purposes of their users. But who uses the structuralist maps? I don’t think that it is sufficient to answer this question with the remark that the philosophy of science is an autoreferential system whose products are produced only for its own needs. If this were the case, philosophy of science would be doomed to intellectual irrelevance. I think, a global account of philosophy of science should be concerned with articulating a “model” of science that brings into clearer focus the global questions concerning science, among them the question about the role of science in our society and culture.

Time to take stock. In my opinion, *Einführung* does not give a satisfying account of the history of philosophy of science. What it does is to offer a concise presentation of the view of the *Münchner Schule* on the evolution of philosophy of science. This view essentially boils down to the conception that Stegmüller formulated some thirty years ago including some new figures that entered the stage after Stegmüller. With *Einführung* we are back in the Golden Age of the *Münchner Schule* of the 1970s and 1980s when its members began to bless the philosophical community with structuralist reconstructions of all kinds of theories. The guiding idea of the dialectics underlying *Einführung*, namely, that structuralism, as the culmination of philosophy of science, is the only account that successfully synthesized diachronic and synchronic perspectives on science, can already be found in Stegmüller’s *Combined Approach*.

In a similar vein as the founder of the *Münchner Schule Einführung* subscribes to a perspective on science, from which many interesting and important aspects of this multi-facetted object get invisible. In the Munich perspective, science is an autonomous, purely epistemic enterprise. Correspondingly, philosophy of science is a purely “metatheoretical” endeavour. This is, of course, a possible proposal of how to conceive philosophy of science. But today quite a few philosophers of science have come to doubt that this proposal offers a promising prospect for "a philosophy of science for the twenty-first century" (cf. Kouranyi 2003).

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# John T. Blackmore: Two Recent Trilogies on Ernst Mach[1]

What would Mach think of about six (or altogether nine) volumes written on him, his ideas and his life? John T. Blackmore has in his life-work undertaken this scientific effort (together with several authors/editors including his wife Setsuko Tanaka). Before Blackmore's life-work and especially his most recently published work will be reviewed in detail, a brief overview of the perspective of Mach from which the review approaches this question will be given in the following.

> Some forty years ago [~1863], in a society of physicists and physiologists, I proposed for discussion the question, why *geometrically similar figures* were also *optically similar*. I remember quite well the attitude taken with regard to this question, which was accounted not only superfluous, but even ludicrous. Nevertheless, I am now as strongly convinced as I was then that this question involves the whole problem of *gestalt* seeing. That a problem cannot be solved which is not recognized as such is clear. In this non-recognition, however, is manifested, in my opinion, that one-sided mathematico-physical direction of thought […]. (Mach 1886/1919, 90; my translation)

Thus at the end of his scientific career, Mach returns to a question from early in his scientific career. It is the question of the (common) relation between (optical) psycho-physiological and mathematical (geometrical) concept formation. Mach's suggestion for this synthesis is his psychical concept of "gestalt".

This early question seems to have been very productive, if it engrained an idea, which Mach worked out 40 years later. It took another 100 years for us to reflect again on this process and it might take even much longer to understand its

1 The following review provides an overview of John T. Blackmore's life work and takes a more detailed focus on the latest three books in the two-trilogy series by Blackmore, John T., Itagaki, Ryoichi and Tanaka, Setsuko "Ernst Mach's Influence Spreads" (2009, 486 pp., $40.00), "Ernst Mach's Graz" (2010, 245 pp., $25.00) and "Ernst Mach's Prague" (2010, 476 pp., $40.00), Bethesda, MD, and Tokyo: Sentinel Open Press. The books are in the following sometimes abbreviated as "Influence", "Graz" and "Prague". Also some of Ernst Mach's works are abbreviated, such as "The Science of Mechanics" as "Mechanics". Mach citations are translated by the author from the German reference with the exception of the "Mechanics" and the "Conservation of Energy" as they are exceptionally good translations which themselves additionally provide interesting hints on Mach (by the translators T.J. McCormack and P.E.B. Jourdain, but this shall not be further elaborated here).

B.F. McGuinness (ed.), *Friedrich Waismann – Causality and Logical Positivism*,
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scientific implications. But for this, one needs to "recognize the problem" and one has to pursue it as persistently as Mach did[2].

From Mach's perspective, the unsolved central question—then as it is now—is the integration of the (general) concept of evolution into science in general. This requires fundamental transformations within science as well. Mach describes evolution as the (joint) idea of "Lamarck, Spencer, Darwin, Wallace", i.e., that phenomena with biological roots can also be described without an absolute teleology as an anthropomorphic (inherently non-scientific) feature. Mach certainly was one of the first (four years after the publication of Darwin's *Origin*) to have asked what implications a general understanding of evolution (and especially human evolution, the evolution of human knowledge and science as part of human knowledge) has for fundamental scientific concepts such as "causality", what is "empirical", "knowledge" (*Erkenntnis*), "error" or what is the empirical meaning of an "economy of thought". These questions and their implications can still be regarded as an ongoing process of inquiry in science. In this, Mach arguably has elaborated one of the most consistent answers, combining perspectives from as diverse sciences as physics, physiology, psychology, history, philosophy, mathematics, economics, anthropology and others. If Mach's elaboration was consistent is of course a fundamental question addressed in many books on Mach. The question is also central to the six recent volumes of Blackmore, Itagaki and Tanaka. The books are a case in point that Mach's elaboration was (and seemingly still is) very productive, but needs further explanation. In at least this pragmatic understanding, Mach's elaboration is fundamental in understanding "science".

From this perspective, what would Mach think about the six volumes that have been written on him, his ideas and his life? First of all, he would probably ask "what is the central idea" leading to these six volumes and "what is the underlying historical-genetic development"? Regarding these questions, the author-editor(s) describe the following narrative of their life-work in the forewords in the volumes on Prague and Graz. Some 40 years ago (sic!) John T. Blackmore wrote his dissertation at UCLA in "an attempt to research all available source material on Mach". This attempt "seemed to come close for a while, but so much had to be left out of the published work and gradually so much more became available […] that a second book obviously became necessary." (Prague, p. viii). The goal seems to have been one worthy of Sisyphus, as already the published version of Blackmore's dissertation *Ernst Mach—His Life, Work, and Influence* (1972) had to be abridged by nearly half. In this sense, the many following books including the most recent ones are a result of Blackmore's ambitious goal.

But there is a second side to the process. Contrary to the previous works by other authors on Mach, the dissertation was to place its emphasis on "biography and science", but also on a different "manner in which philosophy was treated"

2 See Mach's detailed article on how he develops his concept of "gestalt" from a criticism of Herbart's concept: Mach 1862, 1-5.

(Prague, p. viii). This second goal in a sense can be understood as Blackmore's adaptation of Mach's historical-genetic method of description from Mach's *Mechanics*, now applied to Mach's life.

The second book by Blackmore was published as *Ernst Mach als Aussenseiter* in 1985. The questions of Mach and the questions reverberating around him seem to have then suggested a third book *Ernst Mach—A Deeper Look* (1992). Blackmore thus not only collects biographical facts on Mach, but his understanding of Mach transforms. This process necessitates new books. In a Machian way, the transformed (*erkenntnis*-) theory unearths new facts and new relations between them.

As Mach (1893/1960, p. 298) wrote regarding his critics and his own psychological process:

> I am convinced that my elaborations are relatively deficient in more than one sense. This cannot be otherwise in any more radical processes of change of view. These also never happen completely within one head. I can therefore only feel, but not point out these deficiencies. I would otherwise be much further. But also from the writings of my critiques, this did not become much clearer for me. Let us therefore wait a little more!

Thus for Blackmore and his co-author-editors each of the books has "added [new information] to our understanding of Mach's life, work and influence" (Prague, p. vii). The seeming "disorder" in the six books can thus be seen as a result of Mach's self-described "radical process of changing one's view". In a sense it is about "six Mach's" as well as "one". The reasons Blackmore gives (assuming that he is "this editor") in his introductory overview (to the Prague volume)—though partly suggestive—do not always seem sufficient for his actual change of perspective. The genetic question regarding the six recent volumes shall therefore be regarded as unanswered, and not to be fully elaborated here. This of course remains—in the Machian sense—an interesting knowledge-psychological question (*erkenntnispsychologische Fragestellung*) regarding Mach's reception today.

The two trilogies do not follow a straightforward logic, but a complicated history (which is why the genetic view is probably the most general one in this case). It is for example interesting to note that while the preface to the book on Prague covers all books in their order of publication, the introduction to the book on Mach's time in Graz describes the interrelation of the two trilogies.

The first trilogy "includes the result of research which has taken place from 1965 to the present [2010] and what we have learned since our first book on Mach came out which will soon be forty years ago." (Graz, p. i). It comprises *Ernst Mach's Science* (2006), *Ernst Mach's Philosophy Pro and Con* (2009) and *Ernst Mach's Influence Spreads* (2009).

The second trilogy "is more about Mach's life and how he seems to have developed his philosophy" (Graz, p. i). It comprises *Ernst Mach's Graz* (i.e., his life up to 1867 when he took on his position in Prague), *Ernst Mach's Prague* and

*Ernst Mach's Vienna 1895–1930*. The author-editors here use Mach's professional time in Prague (1867–1895) as the main ordering criterion. The books thus focus on Mach's life (and influence) before, during and after his time in Prague, while bearing the title of Mach's professorship positions. The book titles are thereby slightly misleading in terms of the actual content (especially concerning the book on Graz, which also includes his early life). The first book of the second trilogy was *Ernst Mach's Vienna 1895–1930*, which covers Mach's most "developed" period and the immediate succession of his ideas up to their cultural exodus from Austria. The second book (Graz) and the third book (Prague) were only recently published (2010). They constitute one book project, which eventually became too large. When the original book project was split into two, more material on Graz was added, while Mach's student years in Vienna were given less space, as "a number of scholars have written about [it]" (Graz, p. i).

Most of the books on Mach with Blackmore's participation are meanwhile sufficiently known to scholars (see for instance the review of *Ernst Mach's Philosophy Pro and Con* by Michael and Stadler (2010)). Therefore, the following will only provide a brief overview regarding the most recent works, namely the last book of the first trilogy *Ernst Mach's Influence Spreads* and the two latest books of the second trilogy (Mach's Graz and Prague). Here only the most important of the diverse and numerous sources of the books cited by the author-editors and others can be mentioned.

*Ernst Mach's Influence Spreads* takes a quasi-genetic look at Mach's influence on others. In comparison to *Ernst Mach's Science*, it uses as category the sociological group of people instead of scientific categories for ordering the historical material. But its main methodological point—now becoming clearer than in the previous books—is the focus being placed on the early influence of Mach's ideas on other scientists. This focus is the genetic perspective on the seed of an idea, the *status nascendi*, or in Einstein's terminology as found in his obituary to Mach, the time when people sucked their intellectual, *erkenntnistheoretische* "mother's milk" from Mach. The book contains details on many less prominent examples, previously mainly known to specialized and German-speaking scholars. The book then tries to follow the long-term gestalt-transformation effects, which the influence of Mach's ideas can have. In the words of the author-editors, Machian "influence, which seems minor or secondary, can have major or primary consequences" (Influence, p. ix). The interesting *general* question this volume raises regarding the genesis process is probably, when *seemingly minor* ideas have major "exponential" or transformative long-term consequences instead of minor, rather "linear" or additive consequences and why. For Mach, this is a question of the method of historical genesis and the—partly intuitive—expectations of the historian.

The first examples elaborated are Mach's influence on "Musicians" and modern music through Edward Kulke, Wilhelm Kienzel, Otakar Hostinsky, Richard Wallaschek, David Joseph Bach, Eduard Hanslick and Guido Adler. The chapter includes a translation of Mach's "On the Causes of Harmony" as well as many small pieces of translations from various sources. The second chapter comprises various contributed pieces (R. von Mises, Jerusalem, Weiler) regarding Mach's influence on "Hebrews" (meaning intellectuals of Jewish origin), especially illuminating with respect to his attitude to anti-Semitism and his relations to Josef Popper-Lynkeus, Wilhelm Jerusalem and Fritz Mauthner. Some obvious sources, such as Mach's newspaper article on anti-Semitism or Popper-Lynkeus's obituary to Mach, needed for a thorough study of the topic, are unfortunately not included. But many interesting sources are translated into English, such as the Mach-Mauthner letters. The third chapter comprises "Anthropologists" with a focus on Malinowski and Lowie (with excerpts from Malinowski, Sredniawa, Flis and Lowie and the Mach-Lowie letters). The fourth chapter gives an overview on "Hungarians" influenced by Mach. The Hungarians treated in greater detail are Pikler and the Galileo Circle, Károly Polányi and his family, von Kármán, Prandtl, von Békésy, von Hevesy, von Neumann, Wigner, Szilárd and Teller. The chapter also contains two articles by Endre Kiss ("La Belle Époque") and Katalin Demeter ("Budapest Positivism"). The fifth chapter on "Energeticists" is largely contributed by Robert J. Deltete & Matthias Neuber and comprises parts on Helm, Ostwald and Duhem as well as a translation of the Mach-Duhem correspondence. The sixth and final chapter is written by Makoto Katsumori on "Philosophers". It details the ideas of the Japanese philosopher and Mach translator Wataru Hiromatsu regarding Mach's philosophical ideas.

One important change introduced by the author-editors in the books on Graz and Prague (as they initially were part of the same project, their methodology is relatively similar) is the introduction of "a neutral system of epistemological and ontological classification to replace the use of pejorative categories and words like 'metaphysical', 'psychologistic', 'meaningless', 'nonsense', and 'rubbish' that disgrace much of philosophy of science in general and various kinds of logical positivism and analytical philosophy in particular." (Graz, p. 84). Blackmore here introduces a Machian *erkenntnis*-psychology into his historiography "even if as finite and fallible humans we should emphasize relative and conditional certainty and not claim that anything is absolutely or unconditionally certain, especially if based on extensive idealization or alleged intuition. All of these discoveries and changes have been made in the last few years though there may have been a few anticipations earlier" (Graz, p. iii). Even if not all of the implementation of this principle might be immediately convincing[3], it certainly adds to the general agreeability of the texts.

3 Can one for instance understand Mach's changed concepts of "*Empirie*" and "theory", if one does not use "metaphysical"? Alternatively, one can use "metaphysical" in a "positive", non-pejorative sense, i.e., in the way Mauthner for instance understood it from Mach. "Mach's positivistic philosophy of science—which does not hate the metaphysical words, like Auguste Comte, but psychologically describes, so explains them—had continued to have an effect in my subconsciousness." (Mauthner, cited in Thiele 1978, p. 158; my translation). One might remember that as opposed to Husserl,

*Ernst Mach's Graz (1864–1867)* subtitled inside "where much science and philosophy were developed" is a smaller volume, which comprises different aspects of Mach's time before Prague. The first chapter is on Mach's childhood, including a family tree, a translation on Josef Sajner's article on Mach in his birthplace Chrlice, Marie Mach's account of the time in Untersiebenbrunn and Sajner's account of the time in Kroměříž. The second chapter gives a brief account of Mach's teachers and influences as student at the University of Vienna (specifically including Erdberg and Herbart). The third chapter comprises Mach's early interest in "Optics, Acoustics & Helmholtz" (including a part of Mach as initiator of gestalt psychology). In the fourth chapter, Mach's involvement concerning the Petzval-Doppler debate is elaborated.

Chapter five is a bit of an oddity in the book as it marks the historical line before Mach's times in Graz and serves as a general *erkenntnis*-theoretical reflection of the author-editors on their changed view in both books (Graz and Prague). The chapter elaborates why the book is "dedicated to Victoria, Lady Welby and to her daughter Frances who introduced the term 'semantics' into English and who began the fight for meaning as intent, the root use, for the purpose of maximum communication." (Graz, initial dedication). The idea is to separate (tacit) value judgments from empirical meanings and to first hear the arguments before judging. The integration of this as a guiding principle has certainly improved the general style of the books, especially for those who do not necessarily share Blackmore's philosophical perspective. Mach (1905/1926/2002, 223; my translation) would probably have agreed with this changed view, including the suggestion to take it even a step further:

> A thought is most completely and rigorously founded, when all the motives and paths, which led to it and strengthened it, are clearly exposed. The *logical* connection with older, more familiar, *uncontested* thoughts is only *one part* of this account. A thought, the motives of which have been completely exposed, can *never be lost* as long as the motives are valid, while it can on the other hand be immediately abandoned, as soon as the motives are recognized as outdated.[4]

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Mach used the term "vulgar" in the sense of naïve, common-sensical, as a genetic and integrally necessary step towards science instead of something fundamentally different. In a general sense, it might not be an "either-or", but rather a "how-to" question.

4 If one compares my translation with the one by published in 1976 (by Brian McGuinness), one might find the latter translation slightly misleading. A similar problem can be found in Mach's alleged criticism of theories in science, some of which metaphorically "are like dry leaves which fall away when they have long ceased to be the lungs of the tree of science" (Mach 1872/1911, 17). Contrary to Blackmore's translation, theories in Mach's original version do not "gradually disappear 'like falling leaves'" (Prague, p. ix). Leaves (also metaphorical ones) are continuously replaced. Observing nature, leaves will keep falling from trees and in Heraclitus' sense "science is unfinished, variable" (Mach 1872/1911, 17). Mach's new meaning of the concept of "theory" is genetic in the sense of being a (gestalt) result and process of several closely interlinked co-evolutionary processes. He focuses on a specific part of the whole biological pro-

Chapter six describes Mach's initial problems in Graz and his relations to Helmholtz and Fechner. Chapter seven is titled "Mach Bands & Helmholtz II". The next brief chapter eight "Economy, Functions and Herrmann" includes discussions on Mach's economy of thought concept and how it relates to the biological evolution concept. Chapter nine elaborates the author-editors' new interpretation of Mach's philosophy in "Mach's Three Philosophies & Darwin". Chapter ten, titled "Lab Shocks & Wosyka", seems to include some brief leftovers or rather "trans-overs" to the Prague book, where the issues of Mach's transfer to Prague are dealt with in greater detail. The appendices provide more information on individuals in Mach's family tree and his school record.

*Ernst Mach's Prague 1867–1895 as a Human Adventure* is written in a similar spirit and as a direct continuation of *Mach's Graz*. The chapters are much longer than the last chapters in the Graz book. The book was compiled in quite intense collaboration with Czech scholars.[5] The first chapter comprises "The early years in Prague 1867–1876". It gives a general introduction to the Prague atmosphere and Mach's view of it, details on Mach's lab assistants, especially Mirumil (Clemens) Neumann and Čeněk (Vincenc) Dvořák. There is also the interesting background narrative of three scientific societies (student and professional) that Mach had an influence on and helped to develop. The chapter closes with a part (not mentioned in the table of contents) on "Mach, Boltzmann and Graz" regarding a free position in Graz to where Mach wanted to return. But Mach competed with Boltzmann who was eventually chosen for this position. Chapter two addresses the "Czech Renaissance 1877–1886". Here the part titled "Spark Waves, Students & Tesla" touches on how many students and assistants were influenced by Mach's spark waves experiments. "Rector, Split University and James" deals with the issues of the Czech-German nationalisms (with the new Czech professors discussed in the following part) as well as William James' visit to Mach. The following sections deal with the influence of Mach's books *Mechanics* and the *Analysis of Sensations*. The final part of the chapter focuses on Mach being "Rector again and Anti-Semitism".

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cess (without disregarding the whole). This serves in highlighting a methodological principle of carefulness in historical genesis, i.e. to refrain from judging the value of theories from the anthropomorphism of the current perspective of the historian. With Mach's evolutionary view, his metaphor never meant that trees (and especially the tree of knowledge) should breathe without their "lungs" formed by leaves. He was only referring to dry leaves and hoped for his own "theory" to be a fresh, new leaf, replacing withered ones. Furthermore, from a culture and psychology of science perspective, it is the role of younger researchers to put the gist of their criticism against the tradition they perceive as overcome, while the (perhaps same) established scholars tend to emphasize the opposite. The resulting (paradigm) gestalt is product as well as process and Mach would probably have agreed that he (and his early article) is no exception to this.

5 In a discussion with the reviewer they regretted that the book was not published in Prague as they had initially suggested.

Chapter three comprises "The late years 1886–1895". It elaborates diverse topics, such as "Ballistic Shock waves", "School Reform and Textbooks", "Students Who Became Professors", "Troubled and Problem Students", "Mach as a Late Lecturer" and "Heinrich Mach's Suicide". The final chapter four contains details related to Mach especially on Karl Wittgenstein and František Wald (including the Ostwald-Wald relation). The parts on Wald were written by Gerald Druce and Yoshiyuki Kikuchi and the one on Karl Wittgenstein mainly by his colleague Georg Günther. The Mach-Wald letters are also included. Furthermore, the book contains (as "Additions") an article by Emilie Těšínská on "Ernst Mach, His Prague Physics Students and Their Careers", a list of Mach correspondence by Blackmore from 1994, eight "Appendices" and a section by the editors on "Mach, Einstein, and Philosophy". The "Appendices" include a section from and on Carolina Mach, Jaumann's institute in Brünn, Einstein and Lohr and several sections by Martin Cernohorsky on the hundredth and 150$^{th}$ birthday celebrations of Mach and the history of the plaques commemorating Mach at his birthplace. These final sections also indirectly contain much on Mach's continued influence from his time in Prague up to his currently perceived role (at least in the Czech Republic) as anti-communist liberator.

The books definitely provide a huge variety of sources, some known, but much not previously available or brought together, especially in English. If one would think of a criticism, one could nevertheless mention that the coverage of some topics necessarily has to lack in a few cases. As a result, some sources of the authors' ideas in the literature are unfortunately not provided and some additional sources for further references are not directly cited. These omissions concerns factual as well as *erkenntnis*-theoretical (epistemological) issues. For example, the reflections in the introductions and elsewhere on the genesis of the author's ideas do not take into consideration the connection to Joachim Thiele. Alone the fact that two books of Blackmore were actually dedicated to Thiele, namely *Ernst Mach als Aussenseiter* and *Ernst Mach's Science*, suggests that here has been some influence, probably also in terms of the self-recognized shift in *erkenntnis*-theory by Blackmore.[6]

As a (minor) example of a factual omission of central information, the book in Prague contains (p. 21) an interesting list of Mach's ten "preferred books", which includes some obvious items, such as Hoeffding, Gomperz (*Greek Thinkers*), Tyler, Ribot, Darwin and Mayer, but also rather surprising ones, such as Roskoff's *History of the Devil*. One wonders what "preference" means as a criterion, if the actual question leading to the list was "which books should I read as doctoral student, if Mach is one of my examiners" or "which books led Mach to his ideas" or rather "which books are interesting for readers of general literature, which could lead him/her to Mach's ideas" or other one. The question posed to Mach is unfortunately not quoted in the text (also the footnote to the source is strangely

6 Some of the intellectual roots of the review's author can also be traced to Thiele.

placed several sentences before the topic is brought up). Instead one is left with the speculations of the author(s) as to why Mach did not include the Bible in his list, a mystery which might easily be resolved by knowing the initial question. These omissions are a bit unfortunate, as they unnecessarily devalue the admirable historian's achievement of finding such an interesting document and making it available to the international scientific community.

One problem concerning Blackmore's life-time task of making the Machian sources available in English has to do with translation. As many Mach scholars have noted, Mach uses a rich and precise German, which is very difficult to translate. Furthermore, because of his shift(s) in world view, many empirical meanings of fundamental (including pre-scientific) concepts change, as well as their relation to each other (which also is a problem for many German readers). Many translations, even those officially acknowledged by Mach bear traces of the resulting problems (one can for instance see this in the letters Mach exchanged with his translators). Blackmore's command of German has certainly improved in the more recent works, but this unfortunately does not change many of the older translated sources, which will continue to puzzle the researchers using them. Like shown before, the problem also concerns the choice of abbreviations and omissions of context in quotations.

As a result, the two trilogies and the latest three books in them do not necessarily provide a basis for getting to know Mach. They are rather a rich additional source for Mach scholars, suggesting them "leads" via many interesting ideas and sources hitherto not available, especially in English. Nevertheless, effectively following-up on these sources seems to require quite a bit of scholarly initiative and effort. In Mach's perspective they are *interim* gestalts. The problems regarding publisher, design and layout were already noted by Michael and Stadler (2010). As an alternative possibility of publication would have been allegedly available, the option chosen by the author-editors suggests certain reluctance towards the then-to-be-expected more open review process. In the reviewer's perspective, the book might nevertheless have benefitted from this. Such an approach would not have involved too much additional effort or made it necessary to abandon fundamental points of view.

The critique from a "Machian" perspective (which the author-editors have explicitly refrained from adopting) is probably that the six volumes do not (and no single one of them does) fundamentally capture the "radical change" in Mach's ideas over his life. From the initially suggested Machian perspective, the problem is the understanding of "historical genesis" in the general Machian sense. As the author-editors do not have this view primarily, it is probably difficult to capture by secondary adaptation. In the words of the author-editors (Graz, p. 86), "One of the reasons why we have written so many books on Mach is that we keep finding new assumptions which at least seem to be deeper, more influential, and more informative than the ones we thought were deepest before, even if we still prefer philosophy based largely on the representative theory of perception and its gestalt and other modifications." This preference then has teleological implications in terms of historical genesis. "[…] But since we have a different perspective and think [Mach's phenomenology] has generally had a regrettable influence on science and philosophy as foundation theory, we have decided […] to describe the basic problems and to recommend solutions." (Graz, p. 86). Unfortunately, traditional (pre-Machian) categorizations tend to fundamentally fail in capturing Mach's "plastic" gestalt transformations. One therefore has to be very careful before jumping to the conclusion that one has understood Mach's concepts, especially the fundamental ones. The hypothesis of still being within the process of understanding is always the safer one. A "finished world view" is certainly more mentally ensuring, but an illusion regarding Mach's knowledge theory.

Overall, Blackmore's scientific effort is admirable. Following the same question (one that has certainly been productive) for 40 years and still being able to shed new light on it and change one's fundamental view on it requires a rare character. It is especially thanks to these traits that Blackmore has gathered admirers among eminent and specialist scholars in the scientific community and will have a continuing influence.

Blackmore is certainly right that physics and science since Mach's time has moved on and that therefore many of Mach's views, such as his doubt of the reality of atoms are outdated. And by now, because of marvelous new instruments, such as electron microscopes, we have all become believers in the reality of atoms as we have all seen them on computer screens and television programs. So Mach's question of their gestalt is not a question anymore and his ironical Viennese smile while asking "Ham's eins g'sehn?" (Have you seen one [an atom]?) is a chapter in physics' history books. Maybe, if—as a fiction *au finale*—Mach now would have a look at all this, he would agree that he was all wrong and we are right. And maybe, he would regret that now it is too late for him to see an atom, but still he would like to know from us a little detail, now that we can *see* the atom in reality. He might then want to know "what color does it have?"[7] Maybe that would be one of the many questions to answer for Mach in Blackmore's next book.[8]

7 For details, see Mach (1865).

8 After discussing this question with Czech theoretical physicists, several agreed that what electron microscopes measure are specific properties of electrons (not atoms) and that the wavelength in which one could "see" atoms does not correspond to the wavelength perceived by our eyes, so there is a necessary transformation involved. The principal problem remains that the physical scale of atoms is far away from the anthropomorphic human experience scale. Therefore, many (pre-scientific) empirical meanings, analogies and metaphors on which physics itself is based are not necessarily readily applicable on this scale. One of the Czech physicists suggested that theories might have a "bizarreness" property. Carefully read, Mach mainly states that the more "bizarre" a theory is, the more it might be misleading. Though for Mach the theory might still be currently necessary and thought-economical regarding other properties. It might both psychologically and logically (*erkenntnis*-theoretically) "fit" bizarre empirical observations—such as quantum "jumps"—to ideas closer to human sensual ex-

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(The years as provided in the references indicate respectively the first edition / the last updated edition/the edition used by the reviewer).

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perience. One theory might be regarded as more thought-economical than two theories. But, as for instance the debate around string theory shows, the bizarreness is similarly part of the overall economical considerations. The dominant synthesis should never be regarded as the only possible one. Such a (anthropomorphic-historical) hypothesis unnecessarily limits one’s search *a priori*. Maybe there will be a less “bizarre” theory some day. It might at least help to keep looking for it. In this sense, Mach’s physics is not exhausted. It is still pointing to the future in an open and challenging way.

Christoph Limbeck-Lilienau

# Logical Syntax and the Application of Mathematics

Pierre Wagner, Jacques Bouveresse (eds.), *Mathématiques et expérience. L´empirisme logique à l´épreuve (1918–1940)*, Paris: Odile Jacob, 2008.

Pierre Wagner (ed.), *Carnap's Logical Syntax of Language*, in the series *History of Analytic Philosophy* (series editor: Michael Beaney), Basingstoke: Palgrave Macmillan, 2009.

Moritz Schlick, *Théorie Générale de la Connaissance*, traduit de l'allemand et présenté par Christian Bonnet, Paris: Gallimard, 2009.

French scholars have recently been particularly active in making the texts and ideas of logical empiricism accessible to French readers. But they have also increasingly contributed to the historical research on the development of logical empiricism and to the study of the different positions. I have already considered some of these French contributions to the understanding of the philosophy of the Vienna Circle.[1] Jacques Bouveresse, Christian Bonnet and Pierre Wagner have been particularly important in the recent interest on the part of French philosophers in logical empiricism, a philosophic movement which seemed not to fit so well in the context of French thought. Pierre Wagner has now published two volumes collecting new papers by French and Anglo-American scholars on logical empiricism. The first volume (edited together with Jacques Bouveresse) presents different facets of a central problem in the philosophy of mathematics of logical empiricism: how can mathematics as a formal system without content be applied to experience in the different natural sciences? The second book features papers on different central issues of Carnap's *Logical Syntax of Language*. Both volumes give enlightening insights into the development, the central role and the problems connected to the conception of purely formal systems of mathematics, logic and language in the philosophy of the Vienna Circle. And with the translation of Schlick's *Allgemeine Erkenntnislehre*, Christian Bonnet has filled a major gap in the French edition of writings of the Vienna Circle.

1 In: "A Second Appraisal: New French Literature on Logical Empiricism", in: Elisabeth Nemeth, Stefan W. Schmitz, Thomas Uebel *Otto Neurath´s Economics in Context* (Vienna Circle Yearbook 13), 2007, p. 183-195.

B.F. McGuinness (ed.), *Friedrich Waismann – Causality and Logical Positivism*,
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# I

Sometimes the philosophy of mathematics in logical empiricism has been narrowed down to the following claims: mathematics is reducible to logic (logicism) and logical propositions can be analysed as tautologies. The volume edited by Pierre Wagner and Jacques Bouveresse on the relation of mathematics to experience in logical empiricism seeks to show that the philosophy of mathematics in the Vienna Circle and in the Berlin Group goes far beyond these claims. It is not only the nature and status of mathematical statements which is a central topic for logical empiricists, but also the application of formal systems to experience in the different sciences and the explanation of this applicability. If mathematics is a contentless formal system which says nothing about the world or about our forms of intuition, how can it be applied to various areas of our experience, as it is done in the different fields of the natural sciences? This problem of the applicability of mathematics, a consequence of the logical empiricists' view of mathematics, is analysed in a rich collection of papers that address this issue during the classical inter-war period of logical empiricism (1918–40). Some papers focus on Carnap's and Schlick's philosophy of mathematics and the problem of the applicability as it figures in their theories, while others treat this problem in specific fields of the sciences (mathematical physics, relativity theory, quantum mechanics, probability calculus).

A first part of the book retraces the development of the philosophy of mathematics in logical empiricism, with three papers focussing on the evolution of Carnap's mathematical thought from the *Aufbau* to his discussions with Tarski and Quine during his year at Harvard in 1940–41. Fabrice Pataut deals in his paper with the relation between analyticity and applicability in Carnap's philosophy of mathematics. He presents the different conceptions of analyticity in Carnap and focuses especially on Gödel's criticism of a purely analytical conception of mathematics. The early Carnap has a conventionalist conception of the analytical nature of mathematics and logic. For Carnap these disciplines provide only conventions for the use of symbols. Pataut reviews Gödel's objections against analyticity which are based on the one hand on his incompleteness proof and on the other hand on his rejection of the view that the content of laws of nature derives only from empirical propositions and not from their mathematical formulation. For Gödel it is not possible to draw a strict distinction between the contribution to content from empirical propositions and the contribution to it from mathematics. For the author, although Gödel's criticism is not decisive, Carnap is ultimately unable to give a satisfactory explanation of mathematics considered as a purely syntactical system.

The second paper on Carnap's philosophy of mathematics by Frédéric Patras places his thought in the larger context of the development of mathematics in the mid-century, comparing it to structuralist mathematics (Bourbaki). The author concedes a mutual ignorance between Carnap and the structuralists but underlines some strong similarities. Both hold a syntactic view of mathematics influenced by Hilbert's axiomatic approach. Both share a pragmatic conception of mathematics, which rejects epistemological justification in favour of useful results. And for Patras, Carnap developed since the *Aufbau* a theory of relations and functions which goes beyond classical set theory and which is very similar to the description of structures developed by the Bourbakists. The author furthermore describes Carnap's major influence on the development of category theory (Mac Lane, Eilenberg), a theory which generalized the analysis of structures in mathematics. Besides these similarities major differences remain concerning their respective philosophy of mathematics. Contrary to Carnap, the structuralists still claim a role for mathematical intuition and for the knowledge of mathematical entities. They also emphasize the role of the history of mathematics, which is conceived as a kind of dynamics and a Hegelian development of ideas. Patras correctly points out the influence of the French philosopher Albert Lautman (1908–44) on the philosophic views of the structuralists. Unfortunately the author does not analyze the direct confrontation of Lautman with logical empiricism, which had an important impact on the development of Lautman's realist conception of mathematical knowledge and his dialectical conception of mathematical history (for that see Lautman's contribution to the Paris Congresses for the Unity of Science of 1935 and 1937 and his correspondence with Neurath). Although the paper is very instructive concerning this underestimated relation between Viennese and French mathematical thought, the conclusion has been drawn too quickly. The author sides with Lautman's and the structuralist philosophy of mathematics and endorses without much argument a mathematical Platonism as much more suited for explaining "mathematical activity" than Carnap's philosophy.

In the third paper related to Carnap's philosophy of mathematics, by Paolo Mancosu, the confrontation of Carnap's conception of analyticity with Tarski's and Quine's objections take center stage. In his remarkable dissertation, Gregg Frost-Arnold[2] already insisted on these essential discussions at Harvard in the 1940s. Mancosu carefully describes and interprets the issues discussed, mainly the viability of the analytic/synthetic distinction and the necessity of a finitist language for the logic of science. He emphasizes the early rejection (from 1930 on) of the analytic/synthetic distinction by Tarski and his influence on Quine in this matter. Most of the paper examines the discussions concerning a basic, finitist language for science, limited to first-order logic and to variables for individuals. Tarski and Quine both feared that the introduction of variables for classes or predicates might lead to Platonism. They both favored a finitist language on ontological grounds (there is only a finite number of things) and because such a language remains "understandable". Carnap accepted a finitist language, but did

2 Gregg Frost-Arnold, *Carnap, Tarski, and Quine's Year Together, Conversation on Logic, Mathematics and Science*, to appear in the "Full Circle Series", Chicago–LaSalle: Open Court Press.

not want to limit the construction of a basic language by considerations of ontology or “understandability”. Without taking sides, Mancosu insists on the merely intuitive and unclear notion of “understandability”, which both Tarski and Quine use as an essential argument for the limitation of the conceived basic language. By way of conclusion, Mancosu emphasizes the central importance of this intellectual episode for the development of instrumentalism in mathematics.

After this treatment of the main phases of the philosophy of mathematics in logical empiricism (mainly in Carnap), the second part of the book focuses on the central topic of application in Schlick’s, Carnap’s and Feigl’s philosophy. For Jocelyn Benoist the strong Platonist tradition in the philosophy of mathematics has limited philosophical reflexion to pure mathematics and accorded a minor role to applied mathematics. For logical empiricism, by contrast, mathematics does not express proper knowledge and is only an instrument to be applied in the sciences. If a philosophy of mathematics attributes such a huge role to application, then it must also give a coherent and satisfactory explanation of application. Benoist analyses Schlick’s position and asks if that is the case. His conclusion is negative, because mathematics is conceived in Schlick’s philosophy as a purely axiomatic system. The choice of the system and its interpretation is conventional. Conventions thus have the burden of showing how mathematics is applied. Benoist underlines a shift in Schlick’s conception in the 1930s. Now grammar had to explain the application i.e., the rules which govern the use of mathematical symbols. Benoist asks whether meaning is only given by the rules, as assumed by Schlick, or could also precede them. In the second case we would have an a priori governing the application of mathematics. Benoist does not specify which alternative to take.

In his paper Pierre Wagner analyses the importance of the problem of application for Carnap and describes clearly the internal transformations of this problem in different phases. He also emphasizes the difference of Carnap’s view on this topic to other logical empiricists. Wagner distinguishes two problems of application: on the one hand, the application of logic to the sciences (including mathematics) in order to create a “scientific philosophy” and to give a rational reconstruction of the sciences, and on the other hand the problem of the relation within the sciences between the formal sciences (logic and mathematics) and the factual sciences, between analytic and synthetic propositions. These problems are transformed in Carnap’s syntactic phase. Here the first problem becomes the challenge of finding a logical metalanguage in which the sciences can be reconstructed. The second problem concerns now the relation between analytic and synthetic sentences at the level of the object language. The author underlines the difference between Carnap’s concept of application in the *Logical Syntax* and Schlick’s. Contrary to Schlick, Carnap did not seek application of a system of concepts to reality, but only the translation of one language (e.g., a language formulating axioms) into another language (e.g. a physical language) giving an interpretation to the descriptive terms.

In her paper Delphine Chapuis-Schmitz continues the analysis of the difference between Schlick and Carnap. She focuses on their divergent definitions of concepts and the differences these entail for the application of concepts in physical theories. Schlick disrupts the relation between concepts and intuition; his concepts are defined implicitly in a formal axiomatic system. The system of concepts defined implicitly is applied to objects, if they share the formal structure of the system of concepts and if we give definitions of coordination relating concepts to objects. For Schlick, knowledge consists in the univocal application of concepts, but unfortunately, implicitly defined concepts can only be applied univocally as a whole system of concepts. For Carnap, concepts are given by an explicit definition and an empirical basis. For these concepts the criteria of application are given explicitly. Carnap's concepts can be individually applied while Schlick's concepts can only be applied as a system of concepts. Concluding her paper, Chapuis-Schmitz underlines the difference between Carnap's empiricism from Schlick's scientific realism which accepts purely theoretical concepts. But in the 1950s this difference almost disappeared with Carnap's acceptance of theoretical terms.

There is a special problem of application in probability theory: Is the mathematical law of great numbers applicable to reality? Jacques Bouveresse analyzes this question in Feigl's doctoral dissertation (supervised by Schlick). Feigl accepted the frequentist interpretation of probability. However, he questioned the applicability of the law, which states that the relative frequency of an event converges in infinity to a certain probability measure. The law would only apply if we could verify the frequency, which is impossible in the case of an infinite sequence. Feigl also opposed the solution stating that the law applies only with a certain degree of probability. Feigl opted for a practical solution to the application problem: probability applies practically to experience if the relative frequency corresponds to the value given by probability in a sufficiently big segment of experience. The paper places Feigl's discussion in the context of Schlick's philosophy and of similar work done by students of Schlick (Tscha Hung, Marcel Natkin).

The final part of the publication brings together four papers on the philosophy of physics espoused by the logical empiricists, focusing respectively on Carnap's conception of the formation of physical concepts, on the reception of quantum mechanics by Reichenbach and the Vienna Circle, on Schlick's reaction to the indeterminism in quantum mechanics and on the different interpretations of physical geometry raised by relativity theory. Nadine de Courtenay gives a fine-grained analysis of the formation of physical concepts in Carnap's early book *Physikalische Begriffsbildung* (1926). She describes the rules which permit the transition from pre-scientific qualitative concepts to quantitative concepts. For Carnap, in our formation of physical concepts we eventually arrive at functions correlating quantitative expressions. These functions are considered to be summaries of our observations, an idea Courtenay rejects by considering the procedures of concept formation as described by the British physicist Norman Campbell in the 1920s. There, the quantitative functional expressions of physics are considered as empirical models which are more than just summaries of observations, a view much closer to newer conceptions of models of data (Suppes).

Two papers in particular explore how the logical empiricists reacted to quantum mechanics. Andreas Kamlah gives a general overview of this reception. He contrasts the interested and open-minded reaction of the Berlin Group, especially Reichenbach, with the "dogmatic" reaction of the Vienna Circle, especially as shown by Schlick, Frank and Zilsel. Generally, the positions of logical empiricism were shaped in an essential way by the theory of relativity, but quantum mechanics did not prompt the logical empiricists to engage in any substantial conceptual adaptation, with the exception of Reichenbach. As the author notes the Viennese seemed mainly to fear quantum physics, because it was used by some to defend metaphysical positions (subjectivism in measurement, free-will, neo-vitalism). Kamlah has a somewhat distorted vision of the Vienna Circle, which is described as a group closer to philosophical dogmas than to real science. Therefore he mainly concentrates on Reichenbach whose evolution in that matter is described in detail, from his initial criticism of the uncertainty principle up to his interpretation of quantum mechanics in the 40s. But for Kamlah, even Reichenbach does not succeed in giving a satisfactory philosophical account of quantum mechanics.

Michael Stöltzner completes Kamlah's analysis by focusing on the Viennese reaction to quantum mechanics, especially on Schlick. Stöltzner shows how Schlick revised his conception of probability and causality with his reception of quantum mechanics. Causality becomes for Schlick in the 1930s a rule for the formation of assertions, the principle of causality is not itself an assertion which can be verified. For Schlick, quantum mechanics does not reject the principle of causality, but only sets a limit on the precision of our predictions.

The volume closes with a rich and stimulating paper by Michael Friedman on the different views on the foundation of geometry around 1900 and their impact on Einstein's view of physical geometry. The problem of the application of geometry was strongly transformed with Hilbert's axiomatic conception of geometry. Before that, the relation of pure geometry and physical geometry was mapped on the relation between the form of intuition and its content, or between space and physical bodies. Helmholtz, with his view of geometry as founded on our spatial experience with solid (and rigid) bodies and Felix Klein's "Erlangen program", with its foundation of geometry on rational intuition, share this paradigm. Geometry loses any foundation in intuition with Hilbert's axiomatic treatment of geometry. The application is conceived as a coordination of contentless mathematical symbols to physical facts. As this coordination always implies a choice, physical geometry becomes conventional. The logical empiricists are proponents of this conventionalist view and they interpret Einstein's application of non-Euclidean geometry in that light. Friedman insists on the awkwardness of this interpretation. Einstein did not want to make the application of non-Euclidean geometry in relativity theory a matter of convention, which would leave open the option of keeping Euclidean geometry. Einstein's view of physical geometry is more in keeping with the 19$^{th}$ century view. Friedman concludes that the interaction of experience with mathematical structure and the philosophical reflection on their relation, as exemplified in Einstein, is much more promising than the strict separation of the development of geometries from experience, as exemplified in the logical empiricist's view.

Although the papers in this volume more or less share the central problem of application, they cover a very large area of topics in the philosophy of mathematics and the philosophy of physics in Logical Empiricism. The volume is an excellent complement to and extension of the existing literature in this area.

## II

Perhaps analytic philosophy, including logical empiricism, is the philosophic movement which has most resisted to the history of philosophy, the description of its own historic development and the identification with a historic tradition. A new series on the "History of Analytic Philosophy" edited by Michael Beaney is a welcome project in that it sheds more light on the historical itinerary of analytic philosophy. The series is certainly a sign that the resistance to history is disappearing more and more in analytic philosophy, and it is certainly also a sign of an abandonment of the initial ahistorical and revolutionary ambitions of analytic philosophy, a sign of a crisis of the initial project. After a publication on Russell, the series has now edited an excellent volume on Carnap's *Logical Syntax of Language* (1934/37). Initially, since the 80s, Carnapian scholarship focused more on the *Aufbau*, but the studies by André Carus and Steve Awodey on the genesis of the *Logical Syntax* have shown the centrality of this book for Carnap's whole subsequent development: they have separated the enduring innovations of the book from outdated aspects. For them, the enduring innovation consists in the idea that philosophy makes proposals, governed by the principle of tolerance, for new languages for science and for more precise conceptual explications. They have shown that this idea, which persisted after 1945 in Carnap's central notion of "explication", was developed by Carnap separately of the short-lived idea of a purely syntactic analysis of language and a definition of analyticity on this syntactic basis. The principle of tolerance thus does not fall with the demise of the syntactical method. The present volume can be seen in the spirit of this new reading of the *Logical Syntax*.

The volume, edited by Pierre Wagner, presents papers of the foremost Carnap scholars and is divided in four sections: two sections on the philosophic context of Carnap's book, one on the genesis of the *Syntax* and the other on its subsequent reception in analytic philosophy, particularly by Quine and Tarski, and two sections on central claims of the book, one in the philosophy of logic and mathematics and the other on the radical consequences Carnap drew for traditional philosophy. Wagner introduces the volume with a long and careful exposition of the main concepts and positions of the *Logical Syntax* and the scholarly interpretations which have been given of the book. He points out the two central ideas of the *Logical Syntax*, first the transformation of philosophy into a logic of science in which philosophical theses were replaced by syntactic proposals for language construction and second the introduction of logical pluralism with the principle of tolerance. Wagner then explains how the two syntactic languages developed in the *Logical Syntax* are constructed. Finally he discusses the problematic positions taken by Carnap towards truth, towards the possibility of a unique language which includes the metalanguage in the object language and towards the much debated and contested conventionalism in logic and mathematics. Regarding the semantic notion of truth, Wagner insists that Carnap does not systematically reject semantic notions in the *Logical Syntax*, but accepts in the definition of analyticity for "Language II" a method of evaluation of a sentence which already has semantic elements. The transition to the semantic period is therefore much smoother then often thought. Concerning the single language thesis, Wagner underlines that the definition of analyticity is confronted, as Tarski showed, with the same problem as a definition of truth, namely that the notion of truth for a particular language can be defined consistently only in a metalanguage and not in the object language itself. Wagner insists that Carnap, although he believed in a single language thesis in 1931, concedes in the *Logical Syntax* that in mathematics all the metalogical notions cannot be exposed in the system itself. Concerning conventionalism, Wagner discusses Gödel's objections and agrees with other scholars (Friedman, Awodey, Goldfarb) that these are not sufficient for a rejection of Carnap's conventionalism.

Two papers analyse the context in which Carnap developed his syntactical project. Thomas Uebel focuses more on the interaction of Carnap with the Vienna Circle during his elaboration of the syntactic project, while Steve Awodey and André Carus analyse the internal development of Carnap's position from the first idea of the project (1931) to the publication of the book (1934). Uebel retraces possible influences on the *Logical Syntax* from the Circle and the Circle's reaction to the book. In agreement with Michael Friedman, he rejects Menger's priority claim regarding the principle of tolerance. Menger reacted differently to the foundational debate in mathematics than Carnap, rejecting any attempts to justify a mathematical or logical system, while Carnap dissolved and relativized the question: for him mathematics and logic are "justified" pragmatically by their fruitfulness in applications. Uebel attests a much bigger similarity of Carnap's logical pluralism with Hans Hahn's position. Hahn's attitude towards Wittgenstein's conception of logic corresponds to Carnap's dissatisfactions with the *Tractatus*. Hahn also adopted a conventionalist view of logic against the transcendental conception of logic in the *Tractatus* (i.e. logic reflects formal properties of the world, because sentences can only depict, if they share such a logical form with the world). He rejected also the correspondence theory of truth inherent in the *Tractatus*. The dissatisfaction with these ideas of the *Tractatus* was a central motive for the initiation of Carnap's syntactical project. Concerning the reception of the *Logical Syntax*, Uebel carefully analyses Schlick's skeptical or even negative reaction. Schlick for his part feared an extended conventionalism, namely that the acceptance of P-rules in the syntactical metalanguage implies the view that laws of nature are conventions and more generally that Carnap blurs the difference between conventional definitions and factual assertions. For Uebel, Schlick's skepticism towards the syntax project can be explained more generally by a divergent view of philosophy. Schlick adopted a Wittgensteinian conception of philosophy as analysis of ordinary language against Carnap's view of philosophy as logic of science. On behalf of Neurath's reaction to *Logical Syntax*, Uebel notes justly (against some who want to construct the image of a Neurath generally opposed to logical formalism and the "icy slopes of logic"), that Neurath fully endorsed Carnap's positions, and obviously overlooked some hidden semantic aspects already implicit in the *Logical Syntax.* The split with Carnap over semantics begins only with Carnap's official acceptance of Tarski's definition of truth in 1935.

The remarkable, historically and philosophically precise paper by Awodey and Carus continues (and somewhat repeats) their earlier analysis of the different steps leading to the final positions in the *Logical Syntax*.[3] For the authors there are two different steps which led to the central claims in the book, a step (in January 1931) towards the adoption of a metalanguage which treats language in a purely syntactic way and a step (in October 1932) towards logical conventionalism and the principle of tolerance. These distinct steps have been mixed into one position. Because one aspect of the first step seems untenable, the entire position of the *Syntax* has been rejected in an undifferentiated way. The authors describe in detail how aporias in Wittgenstein's view on language led to a purely syntactic description of language in a metalanguage inspired by Hilbertian meta-mathematics. As a route to the second step they retrace how Carnap proposed, after Gödel's incompleteness theorem, a criterion of validity for mathematics which is larger than provability: analyticity. Gödel showed to Carnap that his definition of analyticity was defective and could only show "analytic in L" relative to another language L' which interprets L. Analyticity as a central notion of Carnap's metalogic was therefore relative to the choice of a language and there was no means to determine the "correctness" of language. This pushed Carnap to logical pluralism. For the authors, the rejection of *Logical Syntax* after the semantic turn was motivated by one aspect of step one, namely the prohibition of any reference to an interpretation of a language. This was abandoned when Carnap accepted that not only syntactic rules but also semantic interpretations can be specified by explicit rules. But other aspects of step one were not abandoned, i.e., the possibility of specifying a language by explicit rules and the separation of the syntactic treatment of language from the semantic one (the separation was maintained but only the prohibition on the second part was abolished). Therefore central aspects of step one and the

3 The paper of Awodey and Carus extends their analyses of the genesis of Logical Syntax given in André Carus, *Carnap and Twentieth-Century Thought. Explication as Enlightenment*, Cambridge University Press, 2007 and Awodey and Carus "Carnap's Dream: "Gödel, Wittgenstein, and *Logical Syntax*", *Synthese* (2007), 159: 23-45.

whole of step two of *Logical Syntax* were preserved in Carnap's later philosophy and are not in contradiction with the general semantic turn in analytic philosophy.

Three papers treat specific problems of the philosophy of logic and mathematics, especially the relation of the *Syntax* to the foundational debate in mathematics (Warren Goldfarb) and two technical papers on Carnap's definition of logical consequence and of logical terms (by Philippe de Rouilhan and Denis Bonnay). Goldfarb follows a similar explanation of the development of the syntactic method and the principle of tolerance as the one given in the previous paper of this volume, emphasizing the central role played by the dissatisfaction with the tractarian conception of language and Gödel's objections to analyticity. But Goldfarb emphasizes especially how Carnap's attempt to find a conciliatory position between the conflicting parties in the mathematical foundational debate led him to *Logical Syntax*. Carnap's logicist extension of Wittgenstein's tautological conception of logic to the whole area of mathematics led to the special problem of a logicist account of impredicative definitions and the question about the admissibility of such impredicative symbols. This central problem for Carnap's defense of logicism in 1930–31 suddenly disappears with Carnap's syntactic method. Logicism did not depend any more on a logical reduction which has as its basis interpreted primitive symbols. With Carnap's constructivist approach to mathematical languages the necessity of conciliation in the foundational debate disappeared as well.

De Rouilhan analyses how Gödel's incompleteness proof led Carnap to define a new criterion for mathematical validity with the notion of logical consequence instead of the insufficient criterion of derivability. De Rouilhan underlines that Carnap was the first, before Tarski, to look for such a notion of consequence distinct from derivability. Unfortunately Carnap's definition of logical consequence is unnecessarily cumbersome, because Carnap wants to avoid any reference to truth, contrary to Tarski's famous later definition of logical consequence. De Rouilhan reconstructs Carnap's definition of logical consequence for Language I and II and gives simpler alternatives for such a definition in language II. Bonnay's paper treats Carnap's distinction between logical and non-logical expressions. A definition of a logical expression is essential for the notion of analyticity. For Carnap an expression is analytic, if it follows from logical rules alone. And logical rules are defined as rules in which no descriptive expressions play an essential role. Therefore Carnap must offer a criterion to distinguish descriptive from logical terms. Bonnay confronts Carnap's definition of logicality with objections by Saunders Mac Lane, Quine and Richard Creath. Although he admits the defects of Carnap's definition, he provides arguments for improving it and countering the mentioned objections.

Three papers on "Carnap's philosophical program" treat the status of philosophy when it becomes the logic of science. Jacques Bouveresse questions Carnap's acceptance of the conventionality of meaning and its relation to philosophic problems. If meaning is just a question of the choice of linguistic conventions, then any philosophic discussion becomes reduced to a discussion over our preferences for different linguistic proposals. Bouveresse considers Alberto Coffa's objection that Carnap's rejection of the factuality of meaning actually implies a second level factual claim: if an axiom is a convention, then the fact that it is a convention cannot be itself conventional. Similarly, if the material mode of speech results in errors and misconceptions, then this position about the material mode is not itself a convention. A second objection (by Bar-Hillel) stated that any practical question about the fruitfulness of a linguistic convention can be transformed into the following theoretical question: Is our experience such that language L is more expedient? Bouveresse opposes that objection, because practical questions need a gradual answer (by more-or-less), contrary to answers by yes-or-no, and practical questions are relative to a task and a context. Although Bouveresse does not clearly reject Carnap's approach, he regrets that the principle of tolerance often leads, in philosophical discussions, to an attitude close to philosophic indifference.

Pierre Wagner complements the previous paper with a clear and detailed analysis of Carnap's arguments for the substitution of philosophical questions by syntactical questions. Wagner emphasizes the shift in Carnap's rejection of metaphysics founded on verification to its rejection as an instance of quasi-syntactic and pseudo-objective sentences. Wagner gives a close analysis of quasi-syntactic sentences which erroneously attribute a syntactic property of a term to the object designated by the term. Wagner underlines that "quasi-syntactic sentences" can be rigorously defined only in a formal system and not in informal ones like a natural language. Therefore quasi-syntactic sentences cannot be clearly identified in such informal systems.

The short paper by Richard Creath defends the position that the principle of tolerance is a central principle for a positive philosophical program and is much stronger than the previously favored principle of verifiability. Creath shows how the uncertain status of the latter principle (it is neither analytic nor synthetic) eventually prompted Carnap to adopt tolerance. Creath notes three stages in this transition. First, Wittgenstein's atomic sentences were interpreted by Carnap as protocol sentences. This implied a strong verificationism which could not be made intelligible in Wittgenstein's framework, because of the cited uncertain status of the verification principle. Second, with the metalogic point of view it becomes possible to formulate in a satisfactory way rules of language and also the principle of verifiability. Third, the introduction of the principle of tolerance offers strong defense for verification because verification loses its self-refuting aspect and can now be defended on pragmatic grounds. For Creath the apparent weakness of tolerance actually offers a strong pragmatic instrument for the defense of philosophic positions.

Two papers on the reaction to the *Syntax*, particularly in Quine's philosophy, form the final section of the volume. Thomas Ricketts focuses on the similarities and differences between Carnap and Quine. For him a fundamental difference between the two philosophers is their divergent conception of the relation between logical notation and colloquial language. Carnap defends a clear cut distinction between language as calculus and any psychological aspects. This is linked to his distinction of philosophy (logic of science) from science itself. Ricketts analyses how, under these premises, formulas of a calculus are correlated to linguistic expressions of scientists and how as a result the logical syntax becomes applicable. For the calculus, Carnap wants to save a notion of (only logical) truth, but he does not extend this criterion of validity to empirical sentences, contrary to Tarski and Quine. Tarski applies his bivalent truth predicate to logico-mathematical and empirical sentences. Quine, for his part, rejects a clear cut distinction between artificial and natural languages and extends the colloquial predicate "true" to logic. He also rejects a Hilbertian view of language with uninterpreted sentences. But for Ricketts, once the Carnapian strict distinctions are softened, the difference between Quine and Carnap remains minimal.

Michael Friedman shows in his paper how the rejection of any notion of intuition in mathematics finally led to the principle of tolerance. In the foundational debate Brouwer's intuitionism was a thread leading to the strict rejection of intuition. The conflict is resolved in the *Logical Syntax* with a notion of logical truth (analyticity) which separates mathematics from any factual content. Friedman analyses two comebacks of intuition, first in Beth's objection to Carnap, according to which the *Logical Syntax* relies on a standard interpretation of arithmetic. This presupposes an intuition against non-standard interpretations. The second comeback can be seen in the already mentioned Harvard discussions between Carnap, Tarski and Quine. In these debates Tarski and Quine endorse a nominalistic, finitary version of arithmetic. This version relies on an intuitive notion of "understandability" which guides the selection of this mathematical framework. In his discussions Carnap rejected such a standard of understandability and the ontological commitments connected to it. Friedman shows how these discussions led Quine (and Goodman) to their project of a foundational Humean empiricism expressed in their common paper "Toward a Constructive Nominalism", a foundationalism where an intuitive notion of understandability plays an essential role. Friedman defends Carnap's free choice of pragmatically justified languages against Quine's foundationalism.

The whole volume is a rich and indispensable guide and instrument for a close reading and understanding of Carnap's notoriously difficult book. Although the papers investigate different sources of the *Logical* Syntax, there is a general agreement upon the main steps of the genesis. The central logical and mathematical positions of the book are carefully explained and evaluated. Perhaps some connections of the *Syntax* to topics outside of the strict logico-mathematical sphere are underrepresented, for example the connection to physicalism. But every reader of the *Logical Syntax* should have this book on her table.

Finally I want to mention the excellent French translation of Schlick's *General Theory of Knowledge*. The volume appears in the prestigious series "Bibliothèque de Philosophie" (a series founded by Sartre and Merleau-Ponty) that has specialized previously in editing numerous byzantine and enigmatic Heidegger translations. Fortunately in the hands of the translator Christian Bonnet, French has once again become the language of clarity and precision. Bonnet has already translated Schlick's *Problems of Ethics* and has co-edited in the same series a collection of the essential papers of logical empiricism. The translation is accompanied by a clear and instructive introduction, where Bonnet retraces the development of the main positions of Schlick's *Erkenntnislehre*.

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# Reviews

Jean Leroux, *Une histoire comparée de la philosophie des sciences*, Volume I: *Aux sources du Cercle de Vienne*, Volume II: *L'empirisme logique en débat*, Les Presses de l'université Laval, Québec (Canada) 2010

As Paul Feyerabend once remarked, philosophy of science is a subject with a great past. Let me for the moment leave aside his disillusioned impression that it had only a sad present and no future and concentrate on its past. It is surprising indeed that much has been published on the history of science in the last few decades, while only very few efforts have been made to give an overall description of the history of philosophy of science. That of course presupposes a definition or at least a rough idea of the subject. And along with that goes an answer to the question when it started and what has been part of it during its development. Some (as for instance John Losee, who in 1972 published the first "Historical Introduction to the Philosophy of Science", an early sort of history of the discipline) seem to think that philosophy of science already began with Aristotle's *Analytica Posteriora*, while others would be inclined to have it start more than 2000 years later, let's say with the Vienna Circle.

Jean Leroux, the author of the two volumes under discussion here, makes a wise decision when he dates its beginning somewhere in between (but not just in the middle). He takes as a starting point the natural scientists (like Helmholtz and Hertz) of the late 19th century discussions on the foundations of their disciplines and then countinues his history with the professionalization of the subject in the Vienna Circle and the Berlin Group. Quebec-based—and thus of course French-speaking—Leroux is known mainly for his logic textbooks and some fine articles in the philosophy of science. Now, however, he brings his knowledge from many years of teaching philosophy of science to bear on the two volumes. This, of course, includes knowledge of the French connections. But he also studied in Germany in the 1970s and thus acquired first-hand knowledge of the German debates, which are otherwise largely neglected as a consequence of language barriers in a field mostly dominated by Anglo-Saxon players.

Leroux divides his history into two parts. The first one (*Aux sources du Cercle de Vienne*) focuses on the forerunners of the Vienna Circle and on the circle itself. By forerunners he has especially in mind active scientists who kept an interest in the philosophical foundations of their disciplines. These are Hermann Helmholtz and Heinrich Hertz on the German side and Henri Poincaré and Pierre Duhem on the French. This strategy seems particularly appropriate given the fact that both Wittgenstein and Schlick were much influenced by Hertz and Helmholtz, and the

B.F. McGuinness (ed.), *Friedrich Waismann – Causality and Logical Positivism*,
Vienna Circle Institute Yearbook 15, DOI 10.1007/978-94-007-1751-0_15,

so-called first Vienna Circle was influenced not only by the local heroes Mach and Boltzmann, but also by Poincaré, Duhem and other French conventionalists.
The chapters dealing with the Vienna Circle itself sum up the vast recent literature on this singularly influential group.

The second volume (*L´empirisme logique en débat*) is devoted to an analysis of two classical examples of philosophy of science in the Logical Empiricist style by Rudolf Carnap and Carl Gustav Hempel. It then continues with a criticism of two contemporaries and critics of the Vienna Circle (Karl Popper and Gaston Bachelard), before it then turns to the historization of philosophy of science in the work of Thomas S. Kuhn and the reactions to that movement by two former Popperians: Imre Lakatos and Paul Feyerabend. In some chapters the description opens up perspectives onto more recent discussions. That happens for example when Leroux describes and discusses the Sneed/Stegmüller-approach to formalizing the Kuhnian picture of scientific revolutions (pp. 111-114). Here Leroux can rely on a series of papers he published in the late eighties. Erhard Scheibe, who died in 2010, was also one of the leading German philosophers of science who contributed to the so-called strucuralist view of scientific theories and who is mentioned and discussed by Leroux.

The two volumes, each comprising approximately 200 pages, are self-contained (including their respective extensive bibliographies) and can serve well as textbooks for courses and seminars in the philosophy of science. But they also are useful for scholars and experts in the field who are interested in gaining an overall picture of the development of their discipline. Of course the new histories of the philosophy of science written by Leroux and Carlos U. Moulines (see the review by Thomas Mormann in this volume) are only a beginning. They both rely exclusively on published material. And as in historiography in general, many surprising insights in the history of philosophy of science are only to be gained by delving into unpublished papers and correspondences.

*Hans-Joachim Dahms (Wien)*

Ilkka Niiniluoto and Heikki J. Koskinen (eds.), 2002, *Wienin piiri*, Helsinki: Gaudeamus (261 pp.).

In Paul Edwards's prestigious *Encyclopedia of Philosophy* (1967) John Passmore wrote that logical positivism is about as dead as a philosophical movement ever becomes. Yet according to a new anthology on the Vienna Circle, written in Finnish by Finnish philosophers, the Circle and its philosophy—logical positivism or empiricism—is currently the subject of growing research, in addition to an extensive reassessment of the history and legacy of the movement. Was Passmore's obituary thus untimely?

The influence of the Vienna Circle on Finnish philosophy originated with Eino Kaila who was in close contact with it, and who introduced the new logic and philosophy of science to students and colleagues at the University of Helsinki. The enormous achievements of G. H. von Wright and Jaakko Hintikka on philosophical logic would hardly have been possible without him. This tradition is continued by the current holder of Kaila's chair, Ilkka Niiniluoto who is strongly influenced by Karl Popper in his studies on the philosophy of science. It is thus quite appropriate that all these authorities have also contributed to this anthology on the Vienna Circle.

Ilkka Niiniluoto has written two articles. The first serves as an introduction to the historical background of the movement and to the new research on its historical significance. The second describes Eino Kaila's philosophical development and career and his relationship to the Vienna Circle. It is interesting to note that Kaila was critical of phenomenalism and the verification thesis. Moreover, he provided the first detailed critique of Carnap's constitution theory, which was presented in *Der Logische Aufbau der Welt*. According to Niiniluoto, Kaila was actually an antireductionist realist who tried to combine his view with empiricist epistemology.

Juha Manninen gives an in-depth treatment of the birth of the circle and its manifest *Wissenschaftliche Weltauffassung: Der Wiener Kreis*. He emphasizes that in spite of the manifesto there never were any common doctrines accepted by all its members. There was in fact a hot dispute between two camps: one composed of Schlick and Waismann, who were close to Wittgenstein, and the other composed of Neurath and Carnap. Neurath in particular was very critical of Wittgenstein, accusing him of being a metaphysician and a mystic. He objected to phenomenalism and the verification thesis advocated by Schlick and Waismann, arguing that it is untenable metaphysics to think that one can compare sentences directly to experience. He was a physicalist and a coherentist. Carnap's views developed more closely to Neurath's. Manninen says that it is wrong to assume that the doctrines of the circle are dead, since there never were any common doctrines.

Leila Haaparanta relates how the philosophical content of Frege's logic changed in the hands of the Viennese philosophers. For Frege, philosophy is Kantian transcendentalism purified of all psychological elements. Logic reveals the pure forms that are the necessary conditions of the possibility of knowledge and experience. For the members of the Vienna Circle, on the other hand, there are no transcendental truths or any other philosophical truths. Logic is a tool for the analysis of language and the form of language. For this reason, epistemology became less significant, and linguistic symbols became the primary object of study.

Gabriel Sandu explicates Carnap's attempt to include the truth predicate in the syntax of language in *Logische Syntax der Sprache* (1934). Unlike Wittgenstein Carnap thought that it was possible to talk about the syntax of language in that very same language. Yet this view makes it problematic to include the truth predicate in the syntax: the important distinction between the formal mode of speaking and the material mode of speaking is erased and paradoxes result. The paradoxes could be avoided if the truth predicate were included in the meta-language, but this was incompatible with Carnap's syntacticism, which attempted to reduce meta-language to object language. Sandu reminds us that the discussion on the definability of truth in the language itself continues in modern logic, in the work of Jaakko Hintikka, Tapani Hyttinen, and Sandu himself.

Arto Siitonen writes on Hans Reichenbach who had his own philosophic-scientific discussion group in Berlin. According to Reichenbach, the task of science is to make predictions on the basis of observed regularities. Unlike Schlick, he thought that knowledge is based on probabilities and cannot attain certainty.

Georg Henrik von Wright reminisces how his original plan to do post-graduate studies in Vienna foundered when Schlick was murdered, and the Circle disbanded. Von Wright subsequently went to Cambridge, in which Wittgenstein, to his surprise, had also settled down. Having been invited by Wittgenstein's sister to visit Vienna in 1952, he participated in a seminar led by Victor Kraft who was retiring at the time and who proclaimed that session to be the last one of the Vienna Circle. Afterwards, von Wright received a letter from Kraft who asked him to become his successor. Von Wright likes to think that the spirit of the Vienna Circle and Wittgenstein might have survived longer in Vienna had he accepted the offer.

Pertti Lindfors writes about Georg Klaus who, according to Lindfors, continued the work of the Vienna Circle, but who, under the pressure of official Marxism in the DDR, was forced to transform his logical positivism into dialectical materialism. Klaus distinguished between dialectical contradiction and logical contradiction, and attempted to develop a general theory of the former. Lindfors does not believe that non-logical contradictions contain any interesting common features, but, due to Klaus, modern logic and cybernetics developed a stronger hold on Marxism.

Sami Pihlström searches for similarities between logical positivism and American pragmatism. He discovers pragmatic features particularly in Carnap's later philosophy and in Neurath. Both had a strong influence on W. V. Quine, who brought the pragmatization of empiricism even further. Pihlström is not just searching for historical connections. He believes that the pragmatic side of positivism deserves to be rehabilitated.

Heikki J. Koskinen relates how the ideas of the Vienna Circle were transformed in the course of their reception by W. V. Quine, perhaps the most influential American philosopher in the 20$^{th}$ century. Quine rejected two central dogmas of logical empiricism: (1) the distinction between analytic and synthetic truths and (2) reductionism. The result was holistic empiricism, in which a demarcation between meaningful science and senseless metaphysics cannot be drawn. Neither are there analytical or conceptual truths that could be studied by *a priori* philosophy. Metaphysics and philosophy become a part of empirical science. Metaphysics is once again, due to Quine, a central subject of Anglo-American philosophy. It differs from traditional *a priori* metaphysics in that it takes the results of empirical science into account.

Metaphysics that was taken to be poetry or cognitively meaningless nonsense is thus returned to philosophy. Metaphysics and the classical problems of philosophy seem to be stronger trends in current philosophy than the pragmatism that Pihlström emphasizes.

Logical positivism had in fact a great deal of the spirit of the Enlightenment. The members of the Circle were inspired by the belief that all genuine problems concerning reality could be identified and solved with the help of new logical tools. Jaakko Hintikka shares this belief. He assures us that the philosophy of the Vienna Circle is not dead, and he attempts to defend it against unjustified criticism.

According to Hintikka, the philosophy of science advocated by Thomas Kuhn and others cannot replace logical positivism, because it does not deal with the same problems. Kuhn's question is how science is in fact done. The positivistic philosophy of science is concerned with the completely different question of the right method of science and its structure. The attempts of neo-positivists were problematic because of underdeveloped tools of logic. Hintikka believes that now we have the logical tools to answer the question of the right scientific method. He also criticizes the exaggerated conclusions that the Kuhnians draw from the theory-ladenness of observation, and defends the positivistic thesis of the analyticity of logic.

Not all would share Hintikka's optimism about the efficacy of logic in solving the problems of the philosophy of science. For them, a third alternative exists between Kuhn's historic-sociological approach and Hintikka's logistic approach: Philip Kitcher's and Alvin Goldman's social epistemology studies science as a social institution and attempts to evaluate its practices from a veritistic point of view. The central question is to what extent these social practices enhance the attainment of truth. Just like the positivists, both also defend the veritistic superiority of science compared to other practices.

The book *Wienin piiri* offers interesting new insights into the activities of the Vienna Circle. The essays are mostly historical. The reader may want more of the kind of assessment of the philosophical significance of logical positivism that Hintikka provides. In any case, the point is well taken. The movement was comprised of many different, even conflicting, doctrines, many of which are far from dead.

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THOMAS UEBEL, *Empiricism at the Crossroads. The Vienna Circle's Protocol-Sentence Debate.* Open Court, Chicago, Ill. 2007.

All of us now agree that the Vienna Circle was a tale of sound and fury. There are only a few diehards who would say that it meant nothing. Within and around the Circle there was a series of explosive intellectual developments and fresh insights into the presuppositions of contemporary science. The most prominent personalities of philosophy of science cannot avoid explaining how their heroes related to the Circle. Even the person whom many consider to be the greatest philosopher of the century, Wittgenstein, was in many ways (only partially studied) involved with the Circle. The ongoing rebirth of studies dedicated to the Circle is fuelled not only by the timely re-orientations of analytic philosophy but also—I would say, mainly— by archival studies concerning the Circle, by discoveries of forgotten sources.

Thomas Uebel has been a prolific writer on the Circle since the publication of his *Overcoming Logical Positivism from within: The Emergence of Neurath's Naturalism in the Vienna Circle's Protocol Sentence Debate* (1992), one of the best informed works on the subject. His new book is much more ambitious than the earlier one. Outwardly, it still contains the clumsy classification of the stages and sub-stages of the protocol-sentence debate, but it is actually both an up-to-date review of recent research on the Vienna Circle and an attempt to reconstruct some of its main arguments and to consider their relevance for contemporary research. Although all the three empiricists that deserve most attention in the book, namely Carnap, Neurath and Schlick, ended up accusing each other of different kinds of betrayals of empiricism, Uebel describes what he calls Carnap's and Neurath's "bipartite metatheory" as the winner of the debates. It consists of Carnap's logic of science with its different frameworks together with Neurath's suggestions for empirical, social studies of science. However, the tensions between the two were never resolved.

In addition to the original group, active already at the beginning of the twentieth century, the Vienna Circle was made possible by two of Moritz Schlick's friends, Carnap and Wittgenstein. Without the two of them, compatible only in the specific Viennese circumstances, the Vienna Circle would not merit such interest today. Schlick was more oriented towards supporting the work of others than revamping his own profile. This is a pity, because the study of the Circle's history urgently needs to focus on Schlick. The edition of complete works of Schlick, now underway, and the accompanying Schlick-Studien are the desired correction to this situation. On the other hand, it was Neurath who was the "big locomotive" of the Unity of Science Movement. Unfortunately, most writers on the Vienna Circle do not know his extensive correspondence with Carnap. In addition, Neurath's archive was lost to the Austrian authorities in 1934, then to Gestapo, and it can now be found in the Moscow War Archives. Nothing about it has been published so far. Crucial shorthand manuscripts by Waismann, illuminating Wittgenstein's connection with the Vienna Circle, still await transcription. The rediscovery of the Vienna

Circle has only reached a halfway point. If it continues there will be a number of surprises. Since the focus is on the legacy of unique philosophical pioneers, there is bound to be an impact on contemporary thought.

It is good to read Uebel's book together with the historical parts of A. W. Carus' *Carnap and Twentieth-Century Thought* (2007). Both of them provide new insights, although the picture is only partially similar. New sources are being uncovered and interpreted. The book by Uebel is the first extensive work drawing on Neurath's and Carnap's unpublished works immediately preceding their well-known writings on physicalism, with observations such as Carnap's short-time belief in two universal languages. Carus, on the other hand, reports on the highly valuable yet thus far completely neglected Carnap collection at UCLA and discusses, among a series of new interpretations, Carnap's Davosian sketch for a new system of logic, which is indebted to Wittgenstein. The time for a definitive book on the Vienna Circle has apparently not arrived.

I will not even try to recapitulate the rich contents of Uebel's book which will be recommended reading for a long time, especially as concerns the development of Neurath's thought, but also for the background of Carnap's physicalism. The book is more than a synthesis of Uebel's many earlier publications. It turns out to be a highly recommendable revision of Uebel's earlier views. But there are continuities, of course. For some reason Uebel's suggestion that private language arguments were quite common during the 1930s and especially important for Neurath has not caught fire among Wittgenstein scholars, although Wittgenstein should be discussed within this context, which is presented clearly by Uebel. And special attention should be given to Uebel's rich discussion of Neurath's theory of testimony.

Uebel is now able to give plausible evidence for Heinrich Neider's suggested defence of inter-subjective controllability in science, which is important for Uebel's interpretation of Neurath's and Carnap's development. Unfortunately, there is still no convincing document, and so the discussion may go on. One could add that Neider's dissertation opposed the idea of "understanding" as a specific cognitive mode. In his evaluation dated 26 June 1930, Schlick praised the work, but he also pointed out what he considered to be a shortcoming: "… when he says that it should actually not be permissible at all to speak about other minds (vom Fremdpsychischen) and derives from this his main argument against Dilthey and his followers." (Archives of the Vienna University, Philosophical Faculty, Rigorosenakt Heinrich Neider, 1930).

This does not mean that Schlick would have been opposed to naturalism or even physicalism. In his *General Theory of Knowledge* Schlick had written: "… spatio-temporal concepts may be used to describe any arbitrary reality, without exception, including the reality of consciousness." Further: "Physics is the system of exact concepts that our knowledge correlates to all reality. I say to all reality, since according to our hypothesis the entire world is in principle open to designation by that conceptual system. Nature is all; all that is real is natural. Mind, the life of consciousness, is not the opposite of nature, but a sector of the totality of the natural." (A. E. Blumberg's translation of the 2nd edition, Open Court 1985, p. 295-6). When Schlick later referred to this as his acceptance of physicalism, Carnap quoted these passages and commented on them in a letter to Neurath on 15 May 1935: "This is not a vague anticipation; this is in itself the thesis of physicalism." (Vienna Circle Archive, Noord-Hollands Archief, Haarlem, Otto Neurath: Korrespondenz, 220).

"Physicalism" was a word used by Schlick's colleague Karl Bühler in his book *Die Krise der Psychologie* (1927) in order to refer to a standpoint which he did not find congenial. Neurath adopted it to replace his earlier self-made Marxist talk about the "materialistic basis" of all science, when Schlick had rejected the manuscript of his book Der wissenschaftliche Gehalt der Geschichte und der Nationalökonomie or the "Proto-Sociology" as Uebel calls it; a more militant draft than the one that was later printed.

Late in his life Neurath received from Carnap a letter commenting the quarrels surrounding the book rejection. On 23 August 1945, Carnap explained: "… since you ask so insistently what I meant when I spoke of your violent emotional reactions, I will mention the two occasions uppermost in my mind: your quarrel with Schlick about your manuscript, the second, your quarrel with me when I was in Prague and you sent the long wires from Moscow." (VCA, Otto Neurath: Korrespondenz, 223). Carnap was especially referring to Neurath's wish not to appear as a plagiarist of Carnap, much like Wittgenstein later on concerning the very same publication on physicalism by Carnap, though for different reasons. Carnap concluded:

> … you deserved credit and I was glad to give it to you. What I minded was only the violent emotional way with outbursts and moral pressure by which you induced me to give you what seemed to me an exaggerated amount of credit. I gave it for the sake of peace and preservation of friendship. But I resent to the present day that this one time in my life I was bullied by another man into saying something not in accord with my conviction. (Ib.).

Uebel has great difficulty in describing Schlick's standpoint in the protocol-sentence debate. In this he is not alone. I believe that the ongoing publication of Schlick's complete works will clarify the matter, although Schlick's views were in transition because of Wittgenstein's continuing influence, as the archives in particular reveal. Still, a longer perspective than the one opened up by the intervention in 1934 is needed to understand what Schlick meant at that moment. Uebel reads Schlick as a foundationalist of some kind, because Schlick introduced to the debate something he called "affirmations". One of Uebel's summaries of this puzzling doctrine is the following: "… the epistemological problems of science cannot be solved by structural means: justification needs appeal to personal experience." (Uebel, ib., p. 450; cf. p. 442-445). The final evidence had to be something immediately given, incorrigible and certain, understood phenomenalistically.

I think that some continuity in Schlick's views can be found. The affirmations were not something new that was introduced in 1934. In his *General Theory of Knowledge* (Blumberg's translation, p. 165) Schlick wrote:

… the pragmatists (Peirce, Dewey in America, F. C. S. Schiller in England and others) did perform a genuine service by pointing out (specifically for assertions about reality) that there is indeed no other way to establish truth except through verification. This is actually of great importance. We add, however, the likewise important finding that verification always ends up in establishing the identity of two judgements. The moment it turns out that in designating a perceived fact we arrive at the same judgment that we had already on logical ground deduced for this fact, we become convinced of the truth of the tested proposition.

The original German expression for "to establish" was "zu konstatieren", i.e. to affirm. Did Schlick talk about "affirmations" in 1934 in a logical or epistemological sense? They were psychological for him. The affirmations were an answer to the question why an individual accepted something as true, but nothing more. This psychological question was different from the question of constructing a system of science and accepting it as true.

What did Schlick mean with the "logical grounds deduced for this fact"? Definitively not anything concerned with the immediately given. As I read him, he meant the very same as already in his dissertation on truth, when he was discussing the discovery of the planet Neptunus:

The inference which led to this discovery was drawn from two groups of premises. The first consisted of the principles of mechanics and the Newtonian law; the second was made up of a series of judgements about the 'perturbed' orbit of Uranus, and thus about observed facts. By purely mathematical transformations it emerged from these premises that as yet unknown attractive forces must be influencing the motion of Uranus, and once Leverrier had added the further premise: this attraction comes from a planet circling the sun beyond the orbit of Uranus, he could draw the conclusion: At a certain time, a planet will be found at a certain point in space; and from this proposition he could appropriately derive a new judgement of the form: "An observer who at a certain time and place looks through a telescope aligned in approximately such and such direction, will have a visual perception of such and such a kind." Some time later, as we know, Galle thereupon made an observation, on the strength of which he was able to assert this same judgement as a judgement of perception. Now because this perceptual judgement was identical with the judgement deduced by Leverrier, the premise he had hypothetically established concerning the existence of the new planet was held to be verified. Since then, by innumerable observations of the same kind, new verifications of the same truth have repeatedly taken place. (M. Schlick, *Philosophical Papers*, Vol. I, ed. by H. L. Mulder and B. F. B. van de Velde-Schlick, D. Reidel 1979, p. 75.)

For Schlick, all of science was hypothetical and corrigible, and the matter was not changed by his view that ultimately perception was also needed, a judgement "whereby an actual experience is immediately expressed". Uebel agrees with this, but then he draws the strange conclusion that in Schlick's epistemology it was "all about the subject" (p. 454). Had Schlick changed his views radically on this matter? No such change can be seen in his book on general epistemology—see for instance p. 163 which actually repeats the early description taken from the history of science, only in more formal terms. When we turn to Schlick's lecture on the foundation of knowledge from the winter term of 1933/34, we find him saying:

> Thus observation plays the part of absolute certain knowledge, not when it is taken as a starting-point and stands at the beginning, but when it is arrived at through science … The essence of science does indeed consist in making predictions. But the foundation and link with reality is not the predicting, but the fact of its success. Science makes contact with the real, not at the base, but at the apex; what matters in science is not what it rests on, but what it leads to. (M. Schlick, *The Problems of Philosophy in their Interconnection*, ed. by H. L. Mulder, A. Kox and R. Hegselmann, Kluwer 1987, p. 92).

This passage is quoted by Uebel, but in my opinion he makes too much of the rhetoric of "absolute certainty", directed against Neurath. Downshifting this terminology we get exactly the same picture as in the earlier works, and actually Schlick said "plays the part", not "is". I agree that Schlick had a difficulty in formulating the affirmations, but this definitively does not mean that all was about the subject. There is no observation of dark matter or energy. It remains a speculative concept until something new is observed—or fundamental theories have changed. Both of these alternatives are possible from Schlick's point of view. Despite the rhetoric of "immediacy" at the apex, Schlick remained somehow a realist in a specific sense. Here is his difference to the other empiricists he was opposing. And what is missing from inter-subjectivity, when "innumerable observations of the same kind" are made?

The strengths of Uebel's book are to be found in his detailed discussions of the evolution of Carnap's views through several different stages and in his reconstruction of Neurath's theory of testimony. He tries to make the best of Schlick, but in my opinion not quite successfully. In any case, the book will be a treasure trove for some time to come.

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*The Cambridge Companion to Carnap*, edited by Michael Friedman and Richard Creath, Cambridge University Press, 2007, 371 pp.

The volume contains a collection of fourteen articles on Carnap's philosophy by leading scholars in the field. The contributions follow (with two exceptions) a chronological order that begins with Carnap's early work on the philosophy of geometry and concludes with his long-term project on inductive logic and probability. Thematically, the essays can be grouped as follows. (1) several papers examining the details of Carnap's formative intellectual influences and his interaction with other philosophers (Carus, Gabriel, Ryckman, Pincock, Uebel, and Creath); (2) several essays concerned with more general philosophical themes in Carnap's work and their theoretical evolution (Mormann, Friedman, and Richardson); (3) several papers discussing more technical details of Carnap's philosophy of logic, mathematics, and general science (Reck, Ricketts, Awodey, Demopoulos, and Zabell). Friedman's excellent introductory essay gives an overview of several received views of Carnap's philosophy (promoted mainly by Ayer and Quine) as well as the more recent renaissance in scholarship that has led to a "more balanced and dispassionate understanding of Carnap's place within twentieth-century philosophy." He portrays Carnap's intellectual development from his early philosophical work on logic and mathematics to his mature Wissenschaftslogik by highlighting several unifying themes: a general anti-metaphysical and, at some point, anti-epistemological spirit, the analytic/synthetic distinction as well as an overall pragmatist orientation in Carnap's work.

Carus' article is closely related to his recent book *Carnap in Twentieth-Century Thought: Explication as Enlightenment* (2007) and presents a general account of Carnap's early intellectual background (in particular the German Youth Movement) as well as of his theoretical development from the "Aufbau project" to his later work on formal semantics and inductive logic. Carus concludes with a discussion of Carnap's "ideal of explication" in his work after 1935. Mormann presents a detailed account of Carnap's early philosophy of geometry with the main focus on latter's dissertation Der Raum (1922). He argues that several of the dominant topics in Carnap's later philosophy—in particular his conventionalism—are already present in the dissertation. With respect to his account of physical space, this "geometrical leitmotif" concerns a conventionalist understanding of the metrical structures of a space (with a given topological structure). Gabriel presents a detailed account of Frege's influence on Carnap's philosophy of logic. Carnap attended several of Frege's seminars on the Begriffsschrift and on Logic in Mathematics in Jena between 1910 and 1914. Gabriel outlines Frege's logic as documented in Carnap's lecture notes and discusses several traces of Frege's influence in Carnap's later work. Ryckman's article draws a number of parallels between Husserl's and Carnap's work on logic and mathematics and the Husserlian background for Carnap's phenomenalistic constitutional system in *Der logische Aufbau der Welt* (1928).

Uebel presents a detailed discussion of Carnap's work from the 1920s and 1930s in the intellectual context of the Vienna Circle. The paper investigates the philosophical differences between Carnap's program of rational reconstruction in *Aufbau* and parallel work by Schlick as well as the protocol sentence debate between Carnap and Neurath. The articles by Pincock and Friedman discuss Carnap's Aufbau. Pincock surveys Carnap's "philosophical relationship" with Russell with respect to the similarities and differences in their accounts of scientific philosophy. He shows convincingly that neither Carnap in the *Aufbau* nor Russell in *The Analysis of Matter* (1927) is a reductive empiricist in any strong sense. Instead both promoted a "structuralist theory" of scientific knowledge according to which scientific concepts refer to purely formal relations between the things of the object domain. Pincock points out that the real bone of contention between Russell and Carnap concerns the former's metaphysical and theoretical realism. Friedman's paper also challenges the traditional reception of Carnap's *Aufbau* as suggesting a kind of empiricist reductionism and as being anti-metaphysical as a consequence of this. Friedman discusses "the *Aufbau*'s critical rejection of metaphysics on its own terms", by focusing on its concluding section V, which is devoted to the extensive discussion of different metaphysical positions and several "point(s) of agreement" with Carnap's constitutional system. Friedman argues that Carnap's anti-metaphysical attitude in Aufbau is motivated not by epistemological reductionism but by a novel conception of "scientific philosophy" according to which the constitutional theory is conceived as a neutral standpoint compatible with realism, idealism, and phenomenalism. Demopoulos examines Carnap's later "Ramsey-sentence reconstruction" of scientific theories. Briefly, for a given theory TC (and given the distinction between observation O-terms and theoretical T-terms used in it), the Ramsey sentence R(TC) is obtained by replacing the theoretical predicates of TC by existentially bound variables. It is supposed to present the factual part of a theory. The so-called Carnap Sentence C(TC) of the form "If R(TC) then TC" then expresses the analytical part of TC. Demopoulos discusses several strengths of Carnap's account and then turns to a "basic difficulty" concerning the "almost analytic" status of the factual (and thus a posteriori and synthetic) T-sentences. This throws into doubt whether the factual theoretical statements of a scientific theory can actually be expressed by Ramsey sentences.

The articles by Reck, Ricketts, and Awodey each investigate a certain aspect of Carnap's evolving views on the philosophy of logic and mathematics. Reck presents a historically sensitive discussion of the intellectual background of Carnap's early contributions to modern logic, in particular Frege's lectures in Jena as well as Carnap's correspondence with Russell on the theory of types. Reck then turns to a detailed survey of Carnap's work on general axiomatics in the late 1920s and his treatment of three metatheoretic notions of completeness in the manuscript "Untersuchungen zur allgemeinen Axiomatik" (2000). Reck holds that Carnap's formal explication of these notions can be viewed as an attempt to reconcile the "universalist" conception of logic of Frege and Russell with a Hilbertian conception of formal axiomatics. He shows that while Carnap's main result in Untersuchungen, the so-called *Gabelbarkeitssatz* (stating the general equivalence of the categoricity, semantic completeness, and syntactic completeness of a given theory) is incorrect, it contains an interesting und still unresolved question concerning the metatheory of axiomatics. Ricketts' paper investigates the intricate connections between Carnap's specific version of logicism, empiricism, and the principle of tolerance in *Logical Syntax of Language* (1934). He argues that the principle of tolerance concerning the choice of logical calculi implies a revised conception of logicism, i.e., a new understanding of the distinctive role of logic and mathematics as analytical and content-free "auxiliary devices" in scientific languages. Ricketts critically discusses an objection against Logical Syntax originally formulated by Gödel, namely that Carnap's syntactic view of mathematics and the need to give a consistency proof for his logical calculi is effectively undermined by Gödel's second incompleteness theorem. Finally, he addresses the question whether Carnap's principle of tolerance is compatible with this revised logicism when applied to the informal syntax language in which different calculi are to be investigated. Awodey's article surveys Carnap's attempts in his subsequent work on formal semantics to find a "satisfactory general characterization" of the notions of analyticity and L-truth (logical truth). Following a discussion of Carnap's essentially semantic definition of "analytic in LII" and of a demarcation criterion for logical and non-logical constants in *Logical Syntax*, the paper investigates in closer detail Carnap's attempts to define L-truth and logical constancy semantically, in particular in the three-volume book project Series in Semantics (Carnap 1942, 1943, 1947). Awodey argues that the attempts given there do not meet the requirements of the modern model-theoretical notion of logical truth mainly due to Carnap's tacit assumption that (logical) languages come equipped with a fixed interpretation. Consequently, the difference between "truth in a particular model" and "truth in all models" (and thus a modern model-theoretic account of model variation) cannot be expressed within his pre-modern conception of semantics.

Zabell presents a detailed and informed discussion of Carnap's work on probability and inductive logic from the 1940s onwards. Richardson discusses Carnap's pragmatism as a "fundamental philosophical commitment" in his scientific philosophy. It is manifest in the principle of tolerance as well as in Carnap's general distinction between "practical decisions" and "theoretical questions" in *Logical Syntax* and in later work, e.g., in "Empiricism, Semantics, and Ontology" (1950). Richardson also surveys the relation and differences of Carnap's philosophy with American pragmatism. Creath's concluding article investigates Carnap's logical pluralism and the notion of analyticity in *Logical Syntax*. Based on this, he presents a critical discussion of Quine's notorious objections to the analytic/synthetic distinction in "Two dogmas of empiricism" (1951), specifically of Quine's demand for an "empiricist criterion of significance" for the term "analytic".

Overall, this is an excellent volume that presents the state of the art in Carnap scholarship. Many of the articles are based on, and comment on, recent research on specialized topics concerning Carnap's work. The companion therefore provides an invaluable source of reference for scholars working in the field. At the same time, the contributions are intended to be accessible to a larger non-expert audience. As such, the volume also functions as a compact and accessible introduction to the broad spectrum of Carnap's work and his central position in twentieth century philosophy.

*Georg Schiemer (Vienna)*

# Obituary

## Stephen Toulmin
(London, 1922 – Los Angeles, 2009)

Stephen Edelston Toulmin, philosopher and historian of science, pioneer in the logical analysis of substantive argumentation, was educated in physics and philosophy at Cambridge, where he studied with Paul Dirac, John Wisdom and Ludwig Wittgenstein. Cambridge, Isaac Newton's university, remained his philosophical home: he always was very critical of the way that philosophy was done at The Other Place, as Oxford is known there. The only philosopher whom he really revered there was John Austin (for a time in the 1950s they were married to sisters) – although it is necessary hastily to add that he deeply respected Gilbert Ryle and Isaiah Berlin. Like the latter, he considered himself a "public intellectual". As such he was delighted to be invited to become a contributor to *Encounter* and later, from the mid-1960s a regular contributor to *The New York Review of Books*. He was fascinated by Wittgenstein, attending as many of his classes as he could, but had no interest in becoming close to him. Both the idea of discipleship and Wittgenstein's dominating personally were uncongenial to him. Like Wittgenstein and Berlin he was never at home among professional philosophers (he scarcely ever attended APA meetings in the USA, for example). On occasion his relationships with philosophers could be stormy indeed as was the case with Sir Karl Popper and Nelson Goodman. He prided himself on being an amateur and was only mildly disturbed when "experts" chided him as a bungler. His deepest belief was that professional philosophers do not determine what the real problems of philosophy are; rather those problems arise out of conundrums in human life. That meant for him engaging in intense dialogues, with physicists, psychologists, psychoanalysts, medical doctors, lawyers, musician artists and, of course, historians of science.

The scion of a well-known English liberal family could trace his roots back to persecuted Huguenots, who sought refuge in England after the evocation of the Edict of Nantes in 1685. Their values of tolerance and dissent as well as their concern for science and industry were mirrored in Stephen Toulmin's life and work. His deep interest in Michel de Montaigne as well as the idea that rationality is immanent in human practices reflects that tradition. One of his forebears, Harry Toulmin, founded the first institution of higher learning west of the Appalachians in the USA, Transylvania College (now a university) in 1780. The American Toulmins intermarried with the family of Joseph Priestley, the discoverer of oxygen, himself a religious dissident and political radical. So it is hardly accidental that Stephen Toulmin's Jefferson Lecture, which is linked to the highest honor that the United States pays to humanists, centrally featured the case of Joseph Priestley as a public intellectual, who, he argued, was an exemplary figure for our

B.F. McGuinness (ed.), *Friedrich Waismann – Causality and Logical Positivism*,
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time. Stephen Toulmin's sense of history was such that figures like Montaigne and Priestley were his contemporaries, sources of inspiration and strength. Stephen Toulmin was a decided champion of The Enlightenment but at the same time a deeply religious Quaker and a highly cultivated man of letters. His philosophical, scientific, religious and aesthetic concerns profoundly conditioned each other and account for the intensely humane character of his philosophizing throughout his career. No wonder that he wanted to dedicate his first book, *An Examination of the Place of Reason in Ethics*, to John Maynard Keynes (Keynes died before it was published).

Stephen Toulmin began his teaching career in philosophy in at Oxford and was later appointed professor at the University of Leeds. In the United State he taught at Brandeis University, The University of Chicago, Northwestern and The University of Southern California as well as having numerous visiting professorships and short appointments throughout the country. He directed the Nuffield Foundation History of Ideas unit in London from 1960 to 1965, where he produced a series of classic studies in the history and philosophy of science together with his second wife June Goodfield. The avowed interdisciplinarian frequently held prestige university professorships not bound to any single department. He seldom enjoyed copasetic relations with the philosophy departments at the universities where he was active, Northwestern being the exception.

A brief glance at the short bibliography of his major works at the end of this notice reveals a great deal about Stephen Toulmin's central concerns. The very titles of his books revel that Stephen Toulmin's career revolved around philosophical investigations of reasoning, rationality and the nature of rational enterprises. Little wonder that the work that won him widest recognition in the scholarly community at large was his development of a model for analysing the practice of reasoning originally suggested by Oliver Wendell Holmes and later sketched by John Dewey and in *The Uses of Argument*. Apart from being a pioneering study of the logic of explanation it is also a masterpiece of English expository prose, which could be employed to teach people the subject. The second noteworthy aspect of the short bibliography (which contains roughly half of his books and none of his numerous articles) is that 5 of the 12 books listed are co-authored. He was a philosopher who thrived in dialogue with others and those dialogues inevitably led to major collaborations in the form of co-authored books. This attitude also extended to his teaching: at the University of Chicago, for example, he frequently taught courses together with Paul Ricoeur (another descendant of Huguenots). Like Paul Ricoeur in philosophy and Joseph Kockelmans in the philosophy of science (another close friend and kindred spirit), he championed the importance of hermeneutics within philosophy.

In many circles Stephen Toulmin is known principally as a pioneer of historically-oriented philosophy of science, which he certainly was. History was to him a repository of examples of scientific practice, to be employed as a corrective to the overemphasis upon scientific theory on the part of the logical empiricists. He belonged to that group of physicist-philosophers, who rebelled against the caricatured view of science that was central to the logical empiricist's program (perhaps best exemplified in the work of Rudolf Carnap, Hans Reichenbach and Carl Hempel as well as their alleged critic, Sir Karl Popper), which included (for all their differences) Thomas Kuhn, Patrick Heelan, Robert Cohen and others, above all, his dear friend Norwood Russell Hanson, who invited him to come to Indiana University to give the lectures that became *Foresight and Understanding*, arguably his best-known contribution to the philosophy of science. Stephen Toulmin's concerns with philosophy of science were continuous with his concern for the place of reason in ethics: how does a physicist decide that an explanation of a given phenomenon is acceptable – *in practice*? In contrast to Popper and the logical empiricists, he was less interested in demarcating science from other (allegedly "irrational") activities than he was in establishing just what it is that scientists do in the course of producing solid explanations of physical phenomena. Like another of Wittgenstein's students, W. H. Watson, in 1938, Stephen Toulmin's 1953 introduction to philosophy of science aimed at producing a non-normative account of physics deeply inspired by the Viennese thinker.

Stephen Toulmin's philosophy of science was the product of a philosopher's reflections upon the problems that physicists face in practicing their discipline. It turned upon two issues: 1) how does a physicist decide that an explanation of a given physical phenomenon is acceptable? and 2) why is it that a physicist can fail to understand his/her own results. The first bears upon criteria for sound reasoning in binding observations to theories; the second upon modes of interpretation; i.e., the logic and hermeneutics of science. Stephen Toulmin incorporated three important ideas from Wittgenstein into his response to these questions: The first bears upon his methodological pluralism and the Wittgensteinian notion of "family resemblances". "Physics" is not one but many different independent problem fields, which are related to each other on the basis of analogies rather than sharing a common definition/fulfilling a common set of necessary and sufficient conditions. Second, bears upon the implications of Wittgenstein's idea that meaning is use for understanding the function of physical theory. In this context he developed the idea that theories should best be understood on the analogy with maps that help us to get around the universe. The third bears upon the role of examples in coming to understand physical phenomena and in designing research upon them. Following the implications of this Wittgenstein-inspired thought led him ultimately to the R.G. Collingwood and the notion that science is guided by "ideals of natural order", which became a central theme in *Foresight and Understanding* and further led him with the help of June Goodfield to pursue a series of studies in the history of criteria with respect to the concept of the matter and time as well as in the study of the heavens.

The idea that the hypothetico-deductive model of explanation, "covering law theory," which was the core of logical empiricist philosophy of science was completely misguided was his point of departure in the philosophy of science. As a physicist, he was convinced from the start that the idea, inspired by Ernst Mach and most clearly developed by Hempel and Oppenheim in their 1948 paper, according to which the simplest mathematical representation of empirical co-ordinates was the best theoretical representation of the facts of the case, was false because it was entirely incapable of producing substantive explanations of anything. His concern for the substantive nature of explanation also led Stephen Toulmin to produce his *The Uses of Argument* in 1958, which has turned out to be his most influential work. It turns on the idea that reasoning in practice is a matter of applying and justifying rules that are implicit in human action. So, no small part of the logicians task is the hermeneutic procedure of establishing just how reasoning in practice involves rules that frequently are not even mentioned explicitly. The so-called Toulmin Model (the Dewey-Toulmin Model would be more accurate) has been largely-ignored by philosophers (Peter Strawson spoke of "Toulmin's anti-logic book") but has been universally received with enormous enthusiasm in the field of communication studies. Stephen Toulmin thought of the book as his Prodigal Son. It later success was a source of gratification to him. It is frequently taken to be his main work; however, that distinction really belongs to *Human Understanding*, which has not had anything like the impact of *Uses*.

*Human Understanding*, originally planned in three volumes of which but the first actually appeared, grew out of what was originally conceived as a monumental effort to deal with the most pressing philosophical problems about language and mind, science and rationality c. 1970. That book should be an answer to Thomas Kuhn with respect to the question of how science develops, to wit, in an evolutionary, rather than a revolutionary way. It was also an answer to Sir Karl Popper with respect to the nature of criticism in science, which utilized the Toulmin Model to explain that criticism bears upon every aspect of scientific reasoning, not merely upon scientific claims. Moreover, it extended the scope of philosophy of science to the history and sociology of disciplines and profession (parts of Stephen Toulmin's work that have, for the most part, been sadly neglected). In the volumes to come (as he told me) he would have submitted Noam Chomsky's "Cartesian" concept of mind and language to a critique inspired by Kant and Wittgenstein in philosophy and Vygotsky and Luria in psychology. In the final volume he would have treated the philosophical role of the sociology of knowledge and presented his own philosophy of history. It was a grandiose project of the proportions of Kant's critical philosophy but it was simply too much for him. Stephen Toulmin's vision outstripped his powers (even in the published volume his very style of writing betrays a monumental struggle with his subject matter). Although he claimed to have finished a single volume condensed version of the last two projected volumes, publishers would not accept it. What should have been his major work remained unfinished.

*Wittgenstein's Vienna*, *The Abuse of Casuistry*, *Cosmopolis* and *The Return to Reason* are clear evidence that he had, nevertheless, lost none of his vigor. The last three are deeply affected by his experience in connection with a US Senate committee investigation into foetal research. In the course of the committee's deliberations he was deeply impressed at how much consensus lurked beneath ideological differences. When asked directly for their opinions, people responded with strident statements about Right and Wrong but when there were good, detailed examples to discuss or when the question became personal: "what would you do if your daughter was pregnant after being raped by a criminal?" there was much more agreement than might be expected. The importance of the philosophically despised individual case, scepticism with respect to "principles", consensus beneath the surface of public debate on morality and an Aristotelian sense that traditions of practice implicitly contain our common criteria for rationality bind all of these studies together. He came to share a deep admiration for the Aristotelian notion of *phronesis*, which I had taken in with my mother's milk in philosophy. Our main difference concerned the question of legitimate differences of opinion (essentially contested concepts) that are genuine obstacles to consensus in a complex society like ours. He was inclined to minimize their importance.

As for me, I was and was not a "student" of Stephen Toulmin (for all that's worth). True, I did a dissertation with him but I had already developed and published about an alternative view of Wittgenstein's *Tractatus* before I met Stephen Toulmin. He had read the article (on "Schopenhauer and the Early Wittgenstein"), which I had enclosed with my application for doctoral studies in History of Ideas at Brandeis University, and asked me if I wanted to continue these studies with him on the day I began my studies there. He was keen on this because he felt that Wittgenstein, as he knew him, was widely misunderstood: having a background in natural science gave him an access to Wittgenstein's thinking that was lacking in his fellow students but crucial for understanding Wittgenstein's central ideas in all phases of his development. That both flattered and confused me greatly. What became *Wittgenstein's Vienna* emerged from our common ruminations upon a conversation I had with G. H von Wright in 1966 almost year before I met Stephen Toulmin (see below). Incidentally, Georg Henrik was a bit disappointed that I did not continue my work with him (which would have been very different in that case). Stephen Toulmin valued von Wright's book *Explanation and Understanding* highly indeed. That was a source of much gratification to the Finnish sage.

My philosophical interest in Stephen Toulmin's work came after reading Ch. 3 of "The Uses of Argument" towards the end of my first semester at Brandeis. The idea that reasoning and formal logic only slightly overlapped at best was one that I had entertained in a manner of speaking. His way of analysing practical reasoning filled a crucial gap in philosophy. His seminar on epistemology (which, like many of his courses, was oriented towards 2 dissertations that he was supervising and seemed to be part of an on-going discussion that we new students were eavesdropping upon) struck all of us first year students as bizarre: there was Ken Kipnis continually talking about Japanese chicken-sexers, who could tell the sex of a chick in their hand but could not explain how they did it. It was not very clear then (as it would become later) what we were supposed to learn from this. In fact, this was my first epistemological encounter with the concept of tacit knowing, which would become an important object of study for me in my work on practical knowledge from the 1980s until today.

Working with Stephen Toulmin was a joy – if you got through the first 2 or 3 discussions with him, which were often strenuous and frustrating. Stephen was a creative – critical – listener, who challenged you to liberate yourself from all forms of obscurity and cliché. He adamantly insisted that we refrain from employing traditional philosophical terminology wherever possible because it "loaded" the dice philosophically. Working with him – be it on a dissertation as a student or a book as a colleague – involved creating a new vocabulary/language for discussing your philosophical problem. The careful series of statements and re-statements of the matter under discussion he demanded of you was emancipating with respect to conventional philosophical perspectives and orthodoxies generally. Finding "neutral", jargon-free and where possible elegant modes of expression also served to open up philosophical discussions to a broader public, which was always one of Stephen Toulmin's goals as well as something he did superlatively. Without being aware of it, he shared certain preoccupations of Otto Neurath (something that probably goes back to their mutual respect for Pierre Duhem) without sharing Neurath's strident attitude to philosophy. He was a really great listener, listening to colleagues in more or less regularly occurring systematic discussions in all sorts of different areas over the years. Everybody I know that participated in them would say that he enriched these discussions immensely. His critical listening became a source of confidence to his interlocutors. Once, when a fellow student compared him to Teilhard de Chardin and C. G. Jung at the end of a seminar in 1967, he shocked us by responding "I don't like the company you put me in!" He complained that Teilhard and Jung were people with a message in the form of a speculative grand synthesis that tended to bowl over their public; his way of doing things was to influence people from behind the scenes.

I think the most impassioned I ever saw him was at one of our last meetings in Stockholm c. 1994, when a group of younger colleagues from England were taking turns belittling C. P. Snow and the "two cultures" thesis. Stephen Toulmin shocked everyone and disrupted proceedings by suddenly shouting at the top of his voice with his face red as a beet that they had no right to speak so condescendingly of such a great man, who, unlike them, knew whereof he spoke. This so disturbed the things that a break was immediately called by the organizers. The two of us walked around a long block so that he could blow off more steam and cool down. He was visibly moved by the injustice that had been done to Snow in his eyes. Here his Quakerism was also at work. I was immensely proud to be his student at that moment, although my student days were far behind me.

He taught me that philosophical activity should help real people with real problems rather than contribute to the growth of a professional discipline. He was increasingly proud that he was an amateur in all that he did. Two further difficult lessons I learned from him bear upon the connective nature of scientific enterprises and the role of rhetoric in philosophy of science and in philosophy generally. Both took a very long time to sink into my head. In the latter case maybe 25 years! To my own surprise I have never really stopped learning from him.

First, last and always Stephen Toulmin was a cultivated gentleman who unfailingly enhanced the lives and activities of the people who surrounded him.

*Allan Janik*
The Brenner Archives Research Institute
The University of Innsbruck

Stephen Toulmin's Major Works

*An Examination of the Place of Reason in Ethics* (1950)
*An Introduction to the Philosophy of Science* (1953)
*The Uses of Argument* (1958)
*Foresight and Understanding: an Enquiry into the Aims of Science* (1961)
*The Architecture of Matter* (1962) with June Goodfield
*The Fabric of the Heavens: the Development of Astronomy and Dynamics* (1963) with June Goodfield
*The Discovery of Time (1966)* with June Goodfield
*Human Understanding: The Collective Use and Evolution of Concepts* (1972)
*Wittgenstein's Vienna* (1973) with Allan Janik
*The Abuse of Casuistry: A History of Moral Reasoning* (1988) with Albert R. Jonsen
*Cosmopolis: The Hidden Agenda of Modernity* (1989)
*The Return to Reason* (2001)

A comprehensive list of Stephen Toulmin's writings can be found at the following website:
http://rjohara.net/darwin/files/toulmin-bibliography

For his contributions to the *New York Review of Books* see
http://www.nybooks.com/authors/1864

For a fine account of Stephen Toumlin's central ideas and a vivid picture of him in dialogue see his interview with Gary Olson
http://www.jacweb.org/Archived_volumes/Text_articles/V13_I2_Olson_Toulmin.htm

# Activities of the Institute Vienna Circle

## Activities 2010

**10th Vienna International Summer University**
**ScientIfic World Conceptions (VISU/SWC)**
"The Science of the Conscious Mind"
Main Lecturers: Uljana Feest (TU Berlin), Owen Flanagan (Duke University), Michael Pauen (HU Berlin)
*Venue:* Campus der Universität Wien
*Date:* July 5–16, 2010
*Together with:* University of Vienna/Institute of Philosophy and Institute of Contemporary History
www.univie.ac.at/ivc/VISU
VISU-SWC is part of the doctoral programme "The Sciences in Philosophical, Cultural and Historical Context" of the University of Vienna

### International Conferences and Workshops

FWF research project, 2009–2011
**History of Science and/or Philosophy of Science?**
*Together with:* Universität Innsbruck, Forschungsinstitut Brenner-Archiv
*Venue:* Institut Wiener Kreis bzw. Brenner Archiv
*Date:* February 1–2 and June 7–8, 2010
www.univie.ac.at/ivc

**The Philosophy of Science in a European Perspective**
Continuation of the 5-year programme of the European Science Foundation (ESF) with 22 countries participating
5 workshops on "Probability and Statistics"
www.pse-esf.org

**Moritz Schlick Project: Critical Edition of the Complete Works and Intellectual Biography**
Phase 3: 2010–2013
www.univie.ac.at/ivc/Schlick-Projekt/
In co-operation with the University of Rostock and the Institute of Philosophy, University of Torino
*Date:* Spring 2010
*Venue:* University of Rostock

B.F. McGuinness (ed.), *Friedrich Waismann – Causality and Logical Positivism*,
Vienna Circle Institute Yearbook 15, DOI 10.1007/978-94-007-1751-0,

International Conference
**Rudolf Carnap – Context of Discovery vs. Context of Justification and the Program of Rational Reconstruction**
In co-operation with the Carnap editing project "The Collected Works of Rudolf Carnap" General Editor: Richard Creath
www.opencourtbooks.com/images/CWRC_brochure.pdf
*Venue:* Institut Wiener Kreis
*Date:* June 28 and 29, 2010

International Meeting (with book presentation)
**Friedrich Waismann – Causality and Logical Positivism**
Together with Brian McGuinness (Siena), Antonia Soulez (Paris) and Juha Manninen (Helsinki)
*Venue:* Institut Wiener Kreis
*Date:* October 1 and 2, 2010

## Lectures

**18. Wiener (Kreis) Vorlesung / 18Th Vienna Circle Lecture**
Dagfinn Føllesdal (Stanford University and Oslo University)
**Gödel and Husserl**
*Venue:* Campus of the University of Vienna, Aula
*Date:* November 16

**Wissenschaftsphilosophisches Kolloquium**
Weekly lectures on the philosophy and theory of science given by scholars from Austria and abroad
www.univie.ac.at/ivc/koll/

## Publications

*The Present Situation in the Philosophy of Science*. General Editor: Friedrich Stadler. Proceedings of the Opening Conference of the Research Networking Programme "The Philosophy of Science in a European Perspective". (Publications of the PSE-Program, Vol. 1.) Dordrecht–Heidelberg–London–New York: Springer 2010

*Vertreibung, Transformation und Rückkehr der Wissenschaftstheorie – Am Beispiel von Rudolf Carnap und Wolfgang Stegmüller*. Hrsg. von Friedrich Stadler. Wien: LIT Verlag 2010

## Research Projects

**Moritz Schlick Project: Critical Edition of the Complete Works and Intellectual Biography**
Phase 3: 2010–2013
Institut Wiener Kreis and Institute of Philosophy of the University of Rostock, Abteilung II
www.univie.ac.at/ivc/Schlick-Projekt/

FWF-Forschungsprojekt, 2009–2011
**History of Science and/or Philosophy of Science?**
Together with: *Universität Innsbruck, Forschungsinstitut Brenner-Archiv*
www.univie.ac.at/ivc

# Activities 2011

## International Conferences and Workshops

**11th Vienna International Summer University Scientific World Conceptions (VISU/SWC)**
Main Topic: "The Nature of Scientific Evidence"
Main Lecturers: Hasok Chang (Cambridge University), David Lagnado (University College London), Tal Golan (University of California, San Diego)
*Venue:* Campus der Universität Wien
*Date:* July 4–15
In co-operation with: University of Vienna, Institute of Philosophy and Institute for Contemporary History
www.univie.ac.at/ivc/VISU

**Theorienstrukturalismus – Eine kritische Neubewertung**
Workshop of the FWF-research project, 2009–2011
In co-operation with: Universität Innsbruck, Forschungsinstitut Brenner-Archiv
Keynote lecture: Carlos Ulises Moulines (München)
*Date:* May 5 and 6
*Venue:* Institut Wiener Kreis
www.univie.ac.at/ivc/haopos

**The Philosophy of Science in a European Perspective**
Continuation of the 5-year programme of the European Science Foundation (ESF) with 22 countries participating
5 workshops on "Explanation, Prediction and Confirmation"
www.pse-esf.org

Workshop
**Moritz Schlick Project: Critical Edition of the Complete Works and Intellectual Biography**
Phase 3: 2011–2013
Publication of the writings from the estate (manuscripts and correspondence)
In co-operation with Moritz Schlick Forschungsstelle, University of Rostock (D) and and the Academy of Sciences Hamburg and the Institute of Philosophy, University of Torino (I)
*Date:* February
*Venue:* University of Rostock
www.moritz-schlick.de
www.univie.ac.at/ivc/Schlick-Projekt

**Wissenschaftsgeschichte und Wissenschaftsphilosophie**
Together with: Deutsche Gesellschaft für Wissenschaftsgeschichte (GWG)
*Date:* May 19–21
*Venue:* University of Vienna

International Symposium celebrating the 20th anniversary of the Institute Vienna Circle
**Wissenschaftsphilosophie in Europa – Das Wiener Erbe**
**Philosophy of Science in Europe – The Viennese Heritage**
*Date:* December 5–7
*Venue:* University of Vienna

## Lectures and lecture series

**19. Wiener (Kreis) Vorlesung / 19th Vienna Circle Lecture**
Part of the anniversary event
Hans Jürgen Wendel (University of Rostock)
**Über Moritz Schlick**
*Date:* December
*Venue:* University of Vienna

**Philosophy of Science**
Lecture series in co-operation with the Institute of Philosophy at the University of Vienna
http://wissenschaftstheorie.univie.ac.at/vortragsreihe/

**Wissenschaftsphilosophisches Kolloquium**
Weekly lectures on the philosophy and theory of science given by scholars from Austria and abroad.
www.univie.ac.at/ivc/koll/

## Publications

Anniversary edition
*Wissenschaftliche Weltauffassung. Der Wiener Kreis* (Wien: Artur Wolf Verlag 1929).
New edition of the manifesto with translations into English, French and Italian
Edited with an Introduction by Friedrich Stadler und Thomas Uebel
Wien–New York: Springer 2011.

Anniversary edition
On the occasion of the 10th anniversary of the Vienna International Summer University / Scientific World Conceptions (VISU/SWC):
Allan Hobson, *The William James Lectures*. With comments of numerous researchers in the Cognitive and Life Sciences. Edited with an Introduction by Owen Flanagan. Dordrecht: Springer 2011

Vienna Circle Institute Yearbook 15
Brian McGuinness (ed.)
*Friedrich Waismann – Causality and Logical Positivism*
Dordrecht–Boston–London: Dordrecht: Springer 2011

Veröffentlichungen des Instituts Wiener Kreis, Bd. 16
András Máté, Miklós Rédei and Friedrich Stadler (eds.)
*The Vienna Circle and Hungary / Der Wiener Kreis und Ungarn*
Wien–New York: Springer 2011

Veröffentlichungen des Instituts Wiener Kreis, Bd. 17
Juha Manninen, *Innenansichten des Wiener Kreises*
Wien–New York: Springer 2011

Veröffentlichungen des Instituts Wiener Kreis, Bd. 18
Anna Brożek, *Kazimierz Twardowski. Die Wiener Jahre*
Wien–New York: Springer 2012

Moritz Schlick Gesamtausgabe, Hrsg. von Friedrich Stadler und Hans Jürgen Wendel. Wien–New York: Springer Verlag
Abt. I, Band 4: *Zürich – Berlin – Rostock. Aufsätze, Beiträge, Rezensionen, 1907–1916*. Hrsg. von F. O. Engler. 2011
Abt. II, Band 6: *Erkenntnistheoretische Schriften 1925–1936*.
Hrsg. von Johannes Friedl.
Schlick-Studien 2: Matthias Neuber, *Die Grenzen des Revisionismus. Schlick, Cassirer und das „Raumproblem".* 2011
www.springer.com/series/7287

Ernst Mach Studienausgabe in 9 Bänden. Berlin: xenomoi Verlag
Band 2. *Erkenntnis und Irrtum*. Hrsg. von Elisabeth Nemeth und Friedrich Stadler. 2011
Band 3: *Die Mechanik*. Hrsg. von Gereon Wolters und Giora Hon. 2011.
Band 4: *Populärwissenschaftlich Vorlesungen*. Hrsg. von Elisabeth Nemeth und Friedrich Stadler
www.xenomoi.de

## Research projects

Forschungs- und Editions-Projekt:
**Moritz Schlick Project: Critical Edition of the Complete Works and Intellectual Biography**
Phase 3: 2010–2013
Schriften aus dem Nachlass (Abteilung II). Gemeinsam mit der Hamburgischen Akademie der Wissenschaften. Institut Wiener Kreis/Institut für Philosophie der Universität Rostock
www.univie.ac.at/ivc/Schlick-Projekt/

**History of Science and/or Philosophy of Science?**
Together with: *Universität Innsbruck, Forschungsinstitut Brenner-Archiv*
www.univie.ac.at/ivc

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