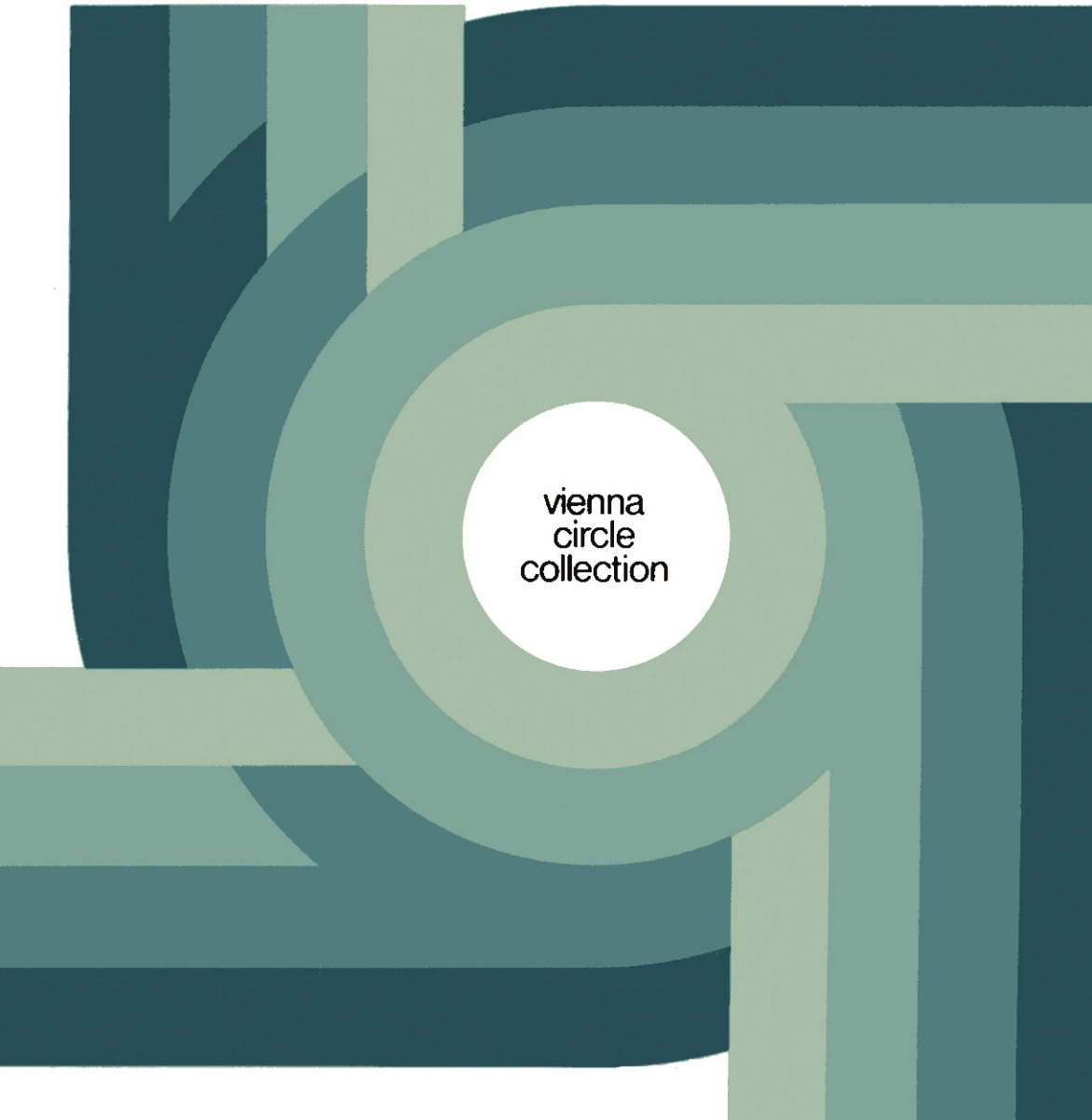
Otto Neurath
Philosophical Papers 1913-1946

Edited by Robert S. Cohen and Marie Neurath

D. Reidel Publishing Company, Dordrecht / Boston / Lancaster

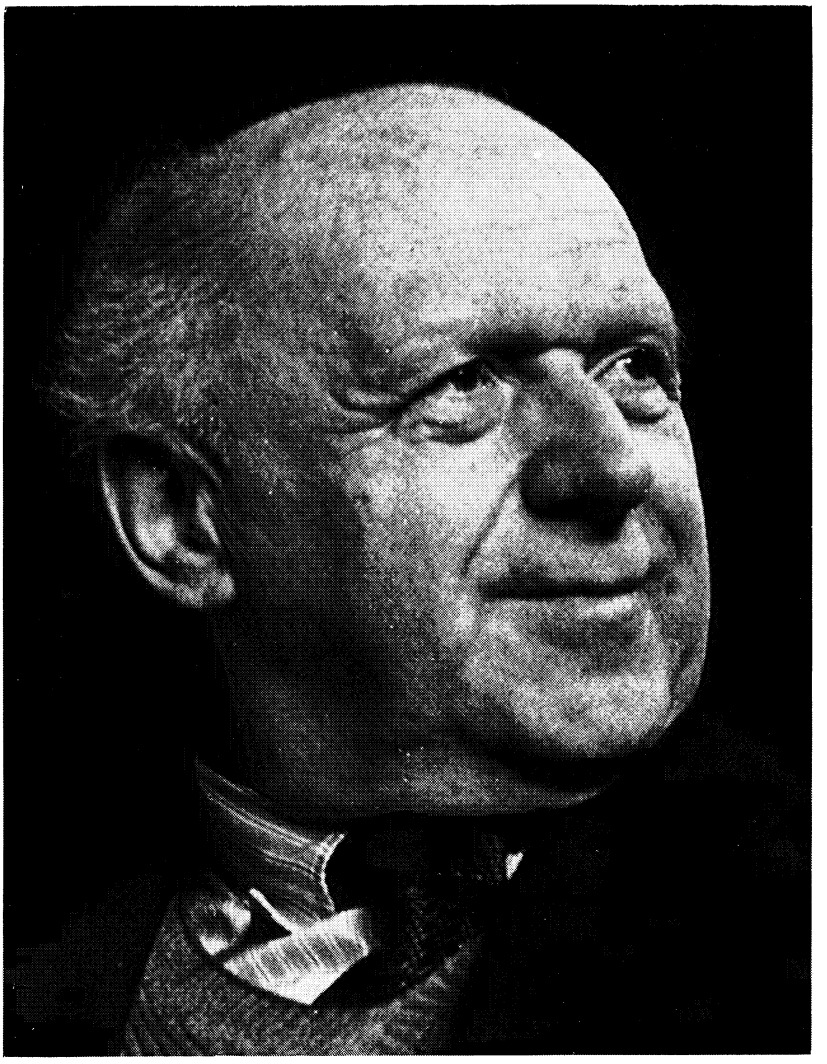


PHILOSOPHICAL PAPERS 1913–1946

VIENNA CIRCLE COLLECTION

VOLUME 16

VOLUME EDITOR: ROBERT S. COHEN



Otto Neurath on December 21, 1945.

OTTO NEURATH

PHILOSOPHICAL PAPERS 1913–1946

With a Bibliography of Neurath in English

Edited and Translated by

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With the editorial assistance of Carolyn R. Fawcett

D. REIDEL PUBLISHING COMPANY

A MEMBER OF THE KLUWER  ACADEMIC PUBLISHERS GROUP

DORDRECHT / BOSTON / LANCASTER

Library of Congress Cataloging in Publication Data

CIP

Philosophical papers, 1913–1946.

(Vienna Circle collection ; v. 16)
Bibliography: p.
Includes index.
1. Philosophy—Addresses, essays, lectures.
2. Science—Philosophy—Addresses, essays, lectures.
3. Social sciences—Addresses, essays, lectures.
I. Cohen, Robert Sonné. II. Neurath, Marie.
III. Fawcett, Carolyn R. IV. Title. V. Series.
B3309.N392E5 1983 193 82–21512
ISBN-13: 978-94-009-6997-1 e-ISBN-13: 978-94-009-6995-7
DOI: 10.1007/978-94-009-6995-7

Published by D. Reidel Publishing Company,
P.O. Box 17, 3300 AA Dordrecht, Holland.

Sold and distributed in the U.S.A. and Canada
by Kluwer Boston Inc.,
190 Old Derby Street, Hingham, MA 02043, U.S.A.

In all other countries, sold and distributed
by Kluwer Academic Publishers Group,
P.O. Box 322, 3300 AH Dordrecht, Holland.

PREFACE

The philosophical writings of Otto Neurath, and their central themes, have been described many times, by Carnap in his authobiographical essay, by Ayer and Morris and Kraft decades ago, by Haller and Hegselmann and Nemeth and others in recent years. How extraordinary Neurath's insights were, even when they perhaps were more to be seen as conjectures, *aperçus*, philosophical hypotheses, tools to be taken up and used in the practical workshop of life; and how prescient he was. A few examples may be helpful:

(1) Neurath's 1912 lecture on the conceptual critique of the idea of a pleasure maximum [ON 50] substantially anticipates the development of aspects of analytical ethics in mid-century.

(2) Neurath's 1915 paper on alternative hypotheses, and systems of hypotheses, within the science of physical optics [ON 81] gives a lucid account of the historically-developed clashing theories of light, their unrealized further possibilities, and the implied contingencies of theory survival in science, all within his framework that antedates not only the quite similar work of Kuhn so many years later but also of the Vienna Circle too.

(3) Neurath's subsequent paper of 1916 investigates the inadequacies of various attempts to classify systems of hypotheses [ON 82, and this volume], and sets forth a pioneering conception of the metatheoretical task of scientific philosophy.

(4) Neurath's lovely meditative paper of 1913 on a passage in Descartes' *Discourse*, [ON 62, and this volume] contains Neurath's sympathetic clarification of the rational decision-making procedure open to all of us in theoretical as in practical life, and of the pseudo-rationalism that so easily deceives when incompleteness and uncertainty in our knowledge are not understood.

(5) Neurath's three papers on physicalism [ON 197, ON 198, ON 218, in this volume] shifted the logical empiricist theory from the 'private' phenomenalism of Carnap's first great re-constitution of the world as known to science and to everyday life over to a 'public' thing-language of communicable and usable technology and social relations; and Neurath stressed the pragmatic reality of common language throughout all responsible discourse, the practical reductionism of a physicalist language rather than any ontological reduction of entities or concepts.

(6) Neurath's work of the early '30s on the basic foundations of knowledge in the elementary statements, the observation reports, the 'protocols' of scientific and everyday life [ON 210 and this volume] established the classical coherence theory of truth in its modern epistemological *and* socio-historical form, argued for the inescapable fallibility of the protocols too, and of entire protocol-classes as well, and stimulated the work of Quine and Kuhn in later years.

(7) Neurath's 1935 critique of the first edition of Popper's *Logik der Forschung* [ON 220, and this volume] foreshadows the confrontations more than a quarter-century later, of Lakatos, Feyerabend, Grünbaum, Kuhn with the Popperian understanding of 'falsificationism', indeed articulates the continuities and the epistemological difficulties with *their* 'paradigms' and 'research programs' and with other ways to honestly characterize the actuality of science.

This book contains new or revised English translations of Otto Neurath's principal philosophical writings, except for those in our *Empiricism and Sociology* [ON 277]. In addition, we have been able to provide an extensive 'Supplementary List' of Neurath's published writings as Bibliography B, comprising 63 individual items and two new German-language collections. The collected philosophical and methodological writings now under publication, and edited by Haller and Rutte, are of particular value [ON 279] and we look forward to completion of that edition with the social, historical, economic and educational writings.

We have also included a full list of 'Neurath in English' as Bibliography C. For this, as for the other apparatus, we would be grateful for further notes and information from readers.

We, and the reader, owe Carolyn Fawcett gratitude for her help at every stage in preparing this book, for the preparation of the bibliographies in particular, and for her careful and intelligent criticism throughout.

ROBERT S. COHEN

ACKNOWLEDGMENTS

The editors and the publisher gratefully acknowledge the permission given by the editors, publishers, and copyright holders of the following publications to reproduce in this book, in the original or in amended form, the articles mentioned below:

'Unified Science and its Encyclopedia'. *Philosophy of Science*, vol. 4, pp. 265–277, 1937. © 1937 The Williams and Wilkins Co., Baltimore.

'The Orchestration of the Sciences by the Encyclopedia of Logical Empiricism'. *Philosophy and Phenomenological Research*, vol. 6, pp. 469–508, 1946.

'Interpretation of Dreams', by S. Freud. George Allen & Unwin, England.

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CHAPTER 1

THE LOST WANDERERS OF DESCARTES AND THE AUXILIARY MOTIVE

(*On the Psychology of Decision*)

I want to take a remarkable passage in the *Discourse on Method* of Descartes as the starting point of my paper. In this work the author, in addition to the rules of theoretical research, also discusses rules of practical action which are for the most part insufficiently appreciated in representations of Cartesian ethics. Among others Descartes puts forward the following principle:

> My second maxim was to be as unwavering and as resolute in my actions as possible, and having once adopted opinions to adhere to them, however in themselves open to doubt, no less steadfastly than if they had been amply confirmed. In this I am following the example of travelers who, on finding themselves astray in some forest, realize that they ought not to vacillate, turning now in one direction and now in another, and still less to stop moving, but to keep always in as straight a line as possible, never for any minor reason changing direction, even though at the start it may have been chance alone which determined them in their choice of direction. If, in thus proceeding, they do not advance in the direction they expected, they will at least, in the final outcome, find themselves better located than in mid-forest. In the same way, since often, in actual living, the requirements of action allow of no delay, it is very certain that when it is not in our power to determine which opinions are truest, we ought to follow those seemingly most likely; and that in those cases in which we fail to observe any greater likelihood in some than in others, we should nevertheless give our adherence to certain of them, and thereafter (since this was our motive for adhering to them) consider them, in their bearing on action, as no longer doubtful, but very true and certain. This decision was sufficient to deliver me from all the repentings and feelings of remorse which are wont to disturb the consciences of those weak, unstable beings who in a vacillating manner abandon themselves to the acting out, as if it were good, what the next moment they are prepared to recognize as being evil (Descartes 1958, pp. 112–113).

With these words Descartes formulates his resignation in the field of practical action. He acknowledges, in principle, the necessity that we must act with insufficient insight. How does this train of thought fit into his world-view? In the second part of the *Discourse on Method* he puts forward his well-known four rules for theoretical investigation: One should assume as true only what is clearly known, dissect all problems into separate questions, arrange the problems according to their complexity, and attempt to make a complete survey of them within an investigation.

Translation of Neurath 1913a [ON 62].

Descartes was of the opinion that, in the field of theory, by forming successive series of statements that one has recognised as definitely true, one could reach a complete picture of the world. He places great confidence in this endeavour which is in sharp contrast to the resignation mentioned above. "Nothing is so difficult that one could not reach it in the end, nothing so hidden that one could not discover it." But how should the man act who has not yet attained complete insight? For this purpose Descartes formulates preliminary rules for practical action which have to be applied as long as one has not reached complete insight. For those who are of the opinion that complete insight can never be reached, these preliminary rules become definitive ones. The necessity that action must take place even if insight is incomplete already follows from the fact that 'non-action' is also an action – the result of a decision. It is precisely this that matters, that the course of events depends on our decision. Descartes does not count theoretical thinking among actions. This view could be supported if one points out that thinking can, as it were, be suspended for a time, whereas with action in the narrower sense this is not possible, since also non-action has to be considered as action, as just mentioned. Against this the objection can be made that there are a whole series of occupations which are similar to thinking. For example, we can interrupt the construction of a house for a time and we can hesitate as long as we want about continuing it. However, the most favourable time for construction may pass and the partly finished building may suffer – but the same is certainly true of thinking. Of thinking it can only be claimed that it belongs to those activities that are relatively independent of the point in time at which they are begun and of the speed with which they are carried out; in any case, the differences between thinking and action are only of degree, not kind. In the *Principles of Philosophy*, Descartes makes a sharp separation between thinking and action.

> . . . we are to make use of this doubt only when we are engaged in contemplating the truth. For, as regards the conduct of our life, we are frequently obliged to follow opinions which are merely probable, because the opportunities for action would in most cases pass away before we could deliver ourselves from our doubts. And when, as frequently happens with two courses of action, we do not perceive the probability of the one more than the other, we must yet select one of them (Descartes 1911, pp. 219–220).

In this sense three provisional moral rules are formulated; one should adapt oneself to the usual laws, customs and religious views; act energetically even if insight is insufficient; and change oneself rather than the order of the world – a view which is, on the whole, of a stoical character.

It was a fundamental error of Descartes that he believed that only in the practical field could he not dispense with provisional rules. Thinking, too, needs preliminary rules in more than one respect. The limited span of life already urges us ahead. The wish that in a foreseeable time the picture of the world could be rounded off makes provisional rules a necessity. But there are fundamental objections to the Cartesian view. Whoever wants to create a world-view or a scientific system must operate with doubtful premises. Each attempt to create a world-picture by starting from a *tabula rasa* and making a series of statements which are recognised as definitively true, is necessarily full of trickeries. The phenomena that we encounter are so much interconnected that they cannot be described by a one-dimensional chain of statements. The correctness of each statement is related to that of all the others. It is absolutely impossible to formulate a single statement about the world without making tacit use at the same time of countless others. Also we cannot express any statement without applying all of our preceding concept formation. On the one hand we must state the connection of each statement dealing with the world with all the other statements that deal with it, and on the other hand we must state the connection of each train of thought with all our earlier trains of thought. We can vary the world of concepts present in us, but we cannot discard it. Each attempt to renew it from the bottom up is by its very nature a child of the concepts at hand.

What is the situation concerning provisional rules in the field of study of the world? In order to make progress one very often finds oneself in the position of having to choose one of several hypotheses of equal probability. The necessity of provisional rules in the field of thinking is usually less clearly understood; this may be related to the fact that one can, so to speak, lead several theoretical lives simultaneously. Serious and bold thought experiments can be risked without hesitation; if they fail, others can be started. However, one cannot, for example, attempt, in the same way, to train for more than one career. Starting from the same initial point one can always develop different theories of light, just as one can undertake different excursions. But one should not overlook the fact that it is certainly of consequence which trains of thought one has once had before a certain investigation. The thinking of a man during his whole life forms a psychological unity, and only in a very limited sense can one speak of trains of thought *per se*. Though Descartes speaks again and again of the process of thinking, he treats it like a system of logical relationships, which as such, of course, has nothing to do with the psychological progression to which it owes its origin. Descartes seems to have in mind the possibility that one can re-start each train of

thought again and again. However, what should one do if, in order to think one hypothesis through to the end, one needs a whole life, and therefore one has to choose one way which one cannot retrace before the completion of the whole investigation? In the field of thinking these cases are certainly not very frequent. If one imagines how a train of ideas would have run on the basis of different premises, then one has thereby already realised this second possibility; however, in the field of action in the narrower sense this is not the case: here the imagination of 'how it might have been' is far removed from making it become real. The most important acts of thinking can be repeated at will; for the most part this is not the case with the most important actions in human life. That an event happens only once is considered characteristic of it. "One cannot step into the same river twice." Thus, Hebbel's Marianne calls out in her prayer:

You did what You have never done: returned
The wheel of time to the position it
Had in the past; please, let him not do as
He did before . . . (Hebbel 1974, Act 3, Scene 6, p. 160).

We saw that there are events that happen only once, and events that happen several times, in both the field of thinking and the field of action in the narrower sense. That any doubt can arise at all results from the fact that there are known and unknown premises from which the conclusion cannot be made unambiguously. Now it can happen that one has to choose a definite course, either in the field of thinking or in the field of action in the narrower sense. Descartes stresses the necessity of being able to make the required resolution quickly and without weakening the will. While he mainly describes the manner in which a resolution, made on the basis of insufficient insight, is to be carried out, here, with reference to Descartes, I want to deal with the question, how such a resolution comes about empirically.

We have seen that in many cases, by considering different possibilities of action, a man cannot reach a result. If he nevertheless singles out one of them to put it into operation, and in so doing makes use of a principle of a more general kind, we want to call the motive thus created, which has nothing to do with the concrete aims in question, the auxiliary motive, because it is an aid to the vacillating, so to speak.

The auxiliary motive appears in its purest form as a drawing of lots. If a man is no longer able to decide on the basis of insight which of several actions to prefer, he can draw lots, or, equally well, declare vaguely that he will just do 'something or other', or that he will wait and see which resolution, after

some hesitation will come out on top, as if leaving the decision to exhaustion, or at any rate to an agent quite outside the motives in question, that belongs to the category of the parrot who draws the 'planets'.[1]

The frame of mind just described is only found so clearly in those men of modern society who are used to making a large part of their actions dependent on individual insight by the exact weighing and examining of means and ends in long drawn-out deliberations. But also the traditional man sometimes becomes conscious of the difficulty of choice, especially when he faces actions that are not adequately determined by tradition. He also finds himself in a painful position if contradictory traditions exert their pressure on him. One can think of all kinds of men in situations in which no further deliberation can help. There is not the slightest reason to doubt that a great military leader like Napoleon is frequently incapable of deciding by means of reflexion exactly what he should do. Nevertheless the method of more or less admitted button counting is an object of abhorrence or ridicule to most contemporaries. However, since these contemporaries are not in possession of complete insight either, the question is which substitute for button-counting do they apply.

In many cases there is action of an instinctive kind, but this can in no way achieve everything. Since it frees one from doubts, it is highly valued by many and its effectiveness is often exaggerated. Yes, many wish for instinctive action even where the problems concern pure expediency. Some are of the opinion that to start with one could reflect, and then when reflexion fails, turn to instinct; this view misuses instinct by consciously introducing it as a mere stop-gap, whereas its significance is evident wherever it rules from the start, though it is perhaps replaceable by reflexion. But an instinct in reserve may well be psychologically doubtful. Precisely if one values the significance of instinctive action so highly should one not misuse it like that. One should clearly realise that instinct must fail with respect to the complex rational relationships created by the consciously shaped institutions of the social order and modern technology. Certainly, part of the significance of instinct is that it did not allow vacillation to occur in periods when cool calculation played a minor role, and in this respect it avoided waste of energy. The world would be in a bad way if we would have had to wait until insight rules, and until it itself systematically eliminates the damage which it causes, for example, by the creation of vacillation.

Thus nature mother's duty takes
and watches that the chain not break
and that the rim not cracketh.

Until the whole of world's domain
is under philosophy's reign
it keeps things on the move
by hunger and by love.[2]

Where instinct recedes we very often discover the unconscious tendency to eliminate any bud of weakening vacillation in some way or other. Here belongs the belief in oracles, omens, prophecies and the like. I do not want this to be understood as a claim that those who follow omens might be of the opinion that this trust in omens may be useful to them and therefore had to be preserved. Rather what actually happens may be this: the view of the value of omens originates from other sources and encounters an emotional disposition for which the elimination of doubt means a release from a feeling of displeasure; therefore, involuntarily, the respective mode of thinking is eagerly absorbed. In the same way I should like to explain why great military leaders, politicians and other men of action so often show a pronounced tendency toward superstition. It should be plainly stressed that such men are often much more superstitious than corresponds to the spirit of their age, and that the forms of their superstition sometimes are strangely primitive or archaic. This is further proof that this superstition is certainly not a product of latter-day reflexion as is occasionally found in spiritualism and other such movements. Given the chance, however, men of the type described above are of course amenable to subsequent systematisation and rationalisation of their original superstitious tendency. If one keeps this in mind, it also becomes understandable why it is precisely in times of political unrest, when further developments are very unclear, that spiritualism and similar currents gain ground more easily. However, there are also other circumstances, which we cannot discuss here in detail, that have an effect. For example, the wish to know the future plays a large part; as can often be observed, this is especially so with individuals whose weak character does not allow them to influence events energetically. From the start this type tends towards the more complicated forms of prophecy and often creates a highly rationalised structure of omens. An extensive occupation with such things must help to fill in the emptiness of will. This product of the weak will can also be used, however, by energetic individuals to strengthen their power of resolution, as shown above.

Other kinds of authority serve as well to eliminate vacillation. In difficult cases, for example, many like to turn to a father confessor or some other adviser because they want to be relieved of troublesome doubt. If they reflect on their behaviour vis-à-vis these authorities, they understandably do not

realise its instinctive basis, and they subsequently try to justify their procedure by the higher insight of the person they asked for advice – an explanation which may even sometimes be correct. In cases of doubt, however, in which a more intelligent person is asked for advice, the problem is only shifted by another step; the question is what this more intelligent person should do if, with all due deliberation, he cannot reach a decision. The tendency and wish to come to a decision is also in the foreground elsewhere; for example, this can be derived from the fact that, in a vote, the president has a casting-vote if no majority has been reached. Perhaps the principle of majority itself serves mainly the purpose of eliminating conflict and bringing about some decision – whether it is the most intelligent one does not matter. For many it may mean the satisfaction of a longing for rest. Somebody may indeed approve the majority principle only because it enhances the ability to act; it is a beloved substitute for the unloved drawing of lots. The umpire too sometimes plays no other role. And when the Italians of the Middle Ages and Renaissance as a matter of principle often got the mayor from another town to end their internal fights, this was also probably the result of their wish for calm, and occasionally it may have been of little concern to the inhabitants of a town whether the man called in from outside was endowed with special insight.

We have seen that instinct nips doubt in the bud, that the belief in omens quickly removes it, and that some institutions of outwardly quite different character also have the partial effect of helping a resolution, some order of things, to break through, should insight fail. Also that simplicity, which does not see more than one possibility for action, has of course the same effect.

In the large centres of civilisation instinct has nowadays lost much ground, and superstition plays a minor role. Most of our contemporaries rely on their insight and want to leave the decision in all things to it once and for all. Their starting-point is the view that given enough thought one could at least determine which manner of action has the greater probability of being successful, should certainty be impossible. That there are cases in which one faces several possibilities of action quite helplessly, is denied or declared so highly improbable that no sensible man need give it any further thought. Men of this type are mostly of the opinion that if difficulties turn up, sharper thinking will have to lead to the goal; they completely fail to see that even the sharpest thinker can end up with several conclusions of equal value if premises are lacking. Whoever adheres to the belief that he can accomplish everything with his insight, anticipates in a way that complete knowledge of the world that Descartes puts forward as a far-off aim of scientific development. This

pseudorationalism leads partly to self-deception, partly to hypocrisy. Education and character support these errors which Descartes, who is usually considered to be the father of rationalism, managed to keep free of in the field of practical action, as we saw above. The pseudorationalists do true rationalism a disservice if they pretend to have adequate insight exactly where strict rationalism excludes it on purely logical grounds.

Rationalism sees its chief triumph in the clear recognition of the limits of actual insight. I tend to derive the widespread tendency towards pseudorationalism from the same unconscious endeavours as the tendency towards superstition. With the progress of the Enlightenment men were more and more deprived of the traditional means which were suited to making unambiguous decisions possible. Therefore one turned to insight in order to squeeze an adequate substitute out of it with all possible force. In this sense pseudorationalism, a belief in powers that regulate existence and foretell the future, as well as reliance on omens, have a common root. The pseudorationalists always want to act from insight and are therefore grateful to anybody who is able to suggest to them that they had acted from insight. This disposition of mind explains sufficiently the striking lack of criticism with which, for example, election speeches of parliamentarians are received. The listeners are glad, so to speak, if they can make up their minds in favour of something with a good conscience; this desire is mostly of a primary nature. If the speaker is aware of this fact, his action becomes a farce; his aim then is only to suggest rationality. People have already begun psychologically to analyse the suggestive effect of the orator, especially of the politician. The arguments with which the orator operates can be put side by side with the shape of the hat he chooses for gaining the sympathies of the members of his his party. The question now is what will happen if psychological knowledge becomes so widespread that most citizens see through the apparatus of suggestion. Through this psychological enlightenment, suggestion may possibly be paralysed, and men are then incapable of receiving the suggestion of insight. If they do not return to superstition, to instinct or to absolute simplicity, nothing remains but seizing an auxiliary motive where insight does not reach far enough; either one is content with arguments like: "Something must happen, let us do this or that, whatever occurs to us, after having eliminated what we have already recognised as wrong," or, when the point is reached where insight fails, one draws lots in style, or leaves the decision to some moment which has nothing to do with the matter in question.

But woe to the statesman who behaved like this publicly. If, in a concrete case, he came to the insight that he could not decide between two alternatives

and therefore wanted to decide by lot, he would expose himself to the reproach of frivolity or cynicism. Popular feeling would be deeply hurt; it demands either the continuation of old traditions or rationally founded changes. In this respect one must keep in mind that the modern statesman is much more conscious of his inadequate insight than the statesman of the past. The statesmen of the past often embraced the total knowledge of their time and were often the leading political economists, while today the statesman must be active in fields which are doubtlessly better known by others than by himself. For example, while Colbert and Turgot are numbered among the most significant political economists of their time, Bismarck as an economist is certainly not on a par with Marx. Political activity demands so much energy nowadays that a great politician can hardly be at the same time a great theoretician. The men who direct the destinies of states usually do not have the greatest insight, and those who do have greater insight mostly have nothing to do with leading. Nevertheless tossing a coin to decide is considered frivolous, and the more frivolous, the more important the matter in question is. Even people who otherwise lack all piety and tradition are usually morally outraged if one suggests to them to decide by lot where insight is at an end. The attitude of Thomas Hobbes in the matter of religion therefore rarely finds approval. His idea that some order is better than none enrages every pseudorationalist who hopes to reach a decision by an adequate measure of thinking. Hobbes' intolerance is purely external, a means to an admitted political end. He simply feels unable to decide which of the positive religions is preferable. It appears to me that this behaviour of Hobbes is the only one possible for an honest rationalist in many affairs of life; however, whether rationalism is at all suited to regulate public life is another question. But once tradition and community feeling are weakened, there is no choice but that between rationalism, which undoubtedly leads to drawing lots, and pseudorationalism which falsifies thinking and feeling.

It is an empirical question how the auxiliary motive meets the test in practice. Its general acceptance could, for example, have the effect that one already uses it at a time when reflexion might still perfectly well make headway. This danger looms in other cases too when there are substitutes for drawing lots, for example, in the form of religious measures. Already the Greek poet warns:

"First set to work yourself, then call the gods for help."[3]

How far the auxiliary motive allows the full intensity of action to evolve depends on the psychological constitution of the individual. Whether the auxiliary motive will one day find general acceptance is still the question.

Today it is already of actual importance for the wise man who is conscious of the incompleteness of his insight, who refuses superstition, and who nevertheless wants to act decisively. Only the auxiliary motive can strengthen his will without demanding the sacrifice of his honesty. He need not artificially constrict his field of vision to be able to be active. The man who hesitates to use the auxiliary motive, who refrains from its use, cannot be helped. So it is also with the man who cannot make up his mind whether to start with 'yes' or 'no' when counting buttons. But this is not an objection to the auxiliary motive; it is not a generally accepted principle that everyone can be helped.

The auxiliary motive is well suited to bring about a kind of rapprochement between tradition and rationalism. While formerly omens and lots had some inner significance, they have now become purely means. But the procedure has remained the same. The adherent of the auxiliary motive will never regard the traditional man, the man who follows his instinct, with that feeling of superiority that characterises many pseudorationalists. He may perhaps even regret that the period of community life, in which tradition and instinct were decisive, has ended and possibly can even treat the auxiliary motive as a substitute that became necessary because rationalism developed. In this sense instinct, tradition and auxiliary motive are in common opposition against pseudorationalism. The application of the auxiliary motive needs a prior high degree of organisation; only if the procedure is more or less common to all will the collapse of human society be prevented. The traditional uniformity of behaviour has to be replaced by conscious cooperation; the readiness of a human group to cooperate consciously, depends essentially on the character of the individuals.

Let us go back to the parable of Descartes. For the wanderers lost in the forest, who have no indication at all as to which direction to follow, it is most important to march on energetically. One of them is driven in some direction by instinct, another by an omen; the third will carefully consider all eventualities, weigh all arguments and counter-arguments and, on the basis of inadequate premises of whose deficiencies he is unaware, he will in the end, his head lifted in pride, take one definite direction which he considers the correct one. The fourth, finally, will think as well as he can, but not refrain from admitting that his insight is too weak, and quietly allow himself to decide by lot. Let us assume that the chances of getting out of the forest are the same for the four wanderers; nevertheless there will be people whose judgment of the behaviour of the four is very different. To the seeker after truth whose esteem of insight is highest, the behaviour of the last wanderer will be most congenial, and that of the pseudorationalist third wanderer most repellent.

In these four kinds of behaviour we can perhaps see four stages of development of mankind without exactly claiming that each of them has come fully into existence. But some things will become clearer when we try to clarify the essential features of the four periods, of instinct, of authority, of pseudo-rationalism, and of the auxiliary motive. Today we live in the period of pseudorationalism; but we can already observe clear indications of decay. Many believe that they can count on a new upsurge of religion, while others expect a return of a more instinctive life. But there are also those who believe that the collapse of our civilisation is unavoidable. If I now try to attribute a future to the auxiliary motive, the culmination of rationalism, I do so on the basis of the following deliberation. We can construct utopias in different ways; we can either think of a further development of the most developed forms; or we can look for germs of future forms. For example, one could elaborate the view that we are approaching a time in which all national events would be systematically precalculated. It would lead us too far to show that it is very improbable that such conditions would begin soon. But we can also discern new movements that have not yet reached full development though they exist, in the way that rationalism already had adherents in the Middle Ages though its future was not predicted. Since it is very difficult to have any idea of some new intellectual trend, it is certainly advantageous to deal more seriously with the possibility that perhaps one day the auxiliary motive will strongly influence private and public life.

Descartes lived in a period of change. At that time one began the all-out fight against instinct and tradition without realising the functions of these forces. In the field of moral action Descartes himself has, as we saw, on the one hand consciously acknowledged tradition, on the other hand, approved of the auxiliary motive. In this a consistent rationalist can follow him. As far as rationalism has a future at all in the moral field, the conscious recognition of its limits and the introduction of the auxiliary motive are unconditional assumptions. But whatever the future may be like, it is well worthwhile to discuss the question, how rationalism and defective insight can be combined with the help of the auxiliary motive.

NOTES

[1] [The expression "parrot who draws the 'planets'" likely means to draw up a horoscope, the planets setting the zodiac so as to prophesy or tell a fortune. The only literary reference of note occurs in Shakespeare, *Comedy of Erorrs* (Act IV, scene 4, line 42f) where the phrase 'to prophesy like the parrot' occurs. Neurath's allusion is probably to

be found in the German and Austrian circuses of the early twentieth century, where parrots were used by fortune tellers to randomly pick out slips of paper with words written on them from a bowl. – Ed.].

[2] [Neurath is quoting the last strophe (minus the first two lines) of Schiller's poem, 'Die Weltweisen' (first called 'Die Taten der Philosophen'):

> Doch weil, was ein Professor spricht,
> Nicht gleich zu allen dringet,
> So übt Natur die Mutterpflicht
> Und sorgt, dass nie die Kette bricht,
> Und dass der Reif nie springet.
> Einstweilen, bis den Bau der Welt
> Philosophie zusammenhält,
> Erhält sie das Getriebe
> Durch Hunger und durch Liebe.]

[3] [Neurath is freely adapting a fragment of Euripides: Αὐτός τι νῦν δρᾶ, χοὔτω δαίμονας κάλει· τῷ γὰρ πονοῦντι χὠ θεὸς συλλαμξάνει

(You accomplish something now yourself, and thus call upon the god;
And truly the god brings safety to those who toil.)

Euripides, *Hippolytus*, Fragment 435 in *Fragmenta Euripidis* ... Edited by Friedrich Wilhelm Wagner. Paris: Didot, 1846. p. 722. – Ed.].

CHAPTER 2

ON THE CLASSIFICATION OF SYSTEMS OF HYPOTHESES

(*With Special Reference to Optics*)

Everyone who takes up theory of science or the history of the sciences feels oppressed by the profusion of facts. Early on, a start was already made with the classification of stones, plants and other objects, though at first only according to rather superficial characteristics; but the systems of human thought that engaged in these things were for a long time simply accepted without any systematic classification. Though one worked at history of botany, history of chemistry, history of mineralogy, history of zoology, one did so as one still works today at history of literature. Once the biographical factor came to the fore, so again did the subject treated. Of course groupings of complexes of ideas were formed, but this was not always preceded by sufficient analysis. There were wild growths of new classifications when the traditional ones were altogether abandoned. There was no continual cooperation of scholars in the field of the history of science.

If, for example, we want to inform ourselves about the views of an age concerning physics – including our own age – it is bad that we are forced to read through ten to fifteen books that always contain much in common. It would be of the greatest value if there was a presentation that made us aware of what there is in common, which often is hidden under all sorts of covers, so that the differences of the individual theories would stand out more clearly. Here and there a physicist undertakes something of this kind for educational purposes; but this undertaking remains isolated, and there is no continuation of the work. The same is true in nearly all fields of knowledge. Especially in psychology, complaints are made that it is difficult to take into account what the different psychologists have in common.

Of course, historical presentation suffers from this. What is the cause of this phenomenon? So far we have not developed a special technique for the analysis of trains of ideas. By a lucky thrust, great historians often gain deep insight, but such achievements can only be insufficiently utilised and above all can hardly be furthered; all too often knowledge is built on a basic view of which the scholar himself is not aware. If the unconscious knowledge could

Translation of Neurath 1916 [ON 82].

become conscious and, possibly, be defined by rules, then also people of lesser genius could cooperate with more success in the great work of historical knowledge. The achievements of a Goethe, a Whewell, a Mach, a Dühring, a Duhem in the field of the history of physics would not be so isolated if one became conscious of the method of historical analysis.

The historians of physics must arrange the views of physicists into groups in the same way as botanists, the plants, or chemists, the compounds. Ordinarily this is done in a rather crude way. Many years ago I became fully aware of this for the first time when I found the remark in Whewell's *History of the Inductive Sciences* that Descartes had created the "first form of the so-called emission theory", whereas the [German] translater Littrow, in an addition at another place, makes the remark that Descartes had, "though in only a vague way", established the wave theory. How can this divergence be explained? In part its origin lies in the fact that Whewell mainly refers to the *Dioptrics* and *Meteora* of Descartes, whereas Littrow probably has the *Principia philosophiae* more in mind. But if one analyses the whole doctrine of Descartes carefully, one must say that it contains elements of emission and wave theory at the same time.

If I sort out a rather consistent group from the often changing and occasionally obscure expressions of Descartes, light appears to be a group of corpuscles that move from the luminous body towards the eye. But the sensation of light does not come about by the moving corpuscles reaching the eye. It is the effect on the eye of a pressure that spreads from the moving corpuscles. A state is spreading. Descartes compares seeing with the groping of a blind man who touches a stone with his stick and receives knowledge of its presence by vibration. I do not want to discuss in detail the fact that at other places Descartes' views deviate from the ones just described. At any rate, it is very well possible that on the one hand one speaks of the motion of particles – an element of emission theory – and on the other of the propagation of a state – an element of wave theory.

The question now is whether such mixed theories are merely exceptions. When we explore the history of the most different sciences, we frequently encounter situations like this. It is the fault of the vagueness of classification: namely, instead of making uniform use of all elements of a theory for its characterisation, only one of them is put forward. Early chemistry also first characterised compounds by individual elements that seemed especially important, whereas modern chemistry gives names to the compounds from which their composition becomes clear. The same would of course be possible in the field of the classification of theories. The theories would have to be

dissected into their elementary components whose combination could then be fixed by a kind of formula.

The most primitive form of classification is that of dichotomies of which emission theory-wave theory is one; there is an abundance of such dichotomies in all fields: realism-idealism, tariff-free trade, etc. The corresponding characteristics mostly come about rather haphazardly and independently of each other. The A-theory is characterised independently of the B-theory. If the B-theory were simply the group of non-A-theories, there would be no logical objection to this classification; however, it would not be of practical use. In order to obtain a scientifically satisfactory systematisation, one must first, willy-nilly, try to give a complete survey of combinations of the elementary notions; by the application of certain principles a selection from the logically possible combinations could already be created. After surveying this totality, one could investigate which of these combinations are realised in 'nature'. As not all chemical compounds are represented in the minerals, so in the world of real combinations of ideas as well, not all possible theories, which can be derived from certain elementary notions, are represented.

Dichotomies, however, are not only crude intellectually, but also mostly the product of scientific pugnacity. One characterises the opponent as pungently as possible for the purpose of beating him down as forcefully as possible. At such occasions transitions are only troublesome. Thus dichotomies are a result of a warlike spirit. I do not want to examine in detail here how far dichotomies, precisely through their deficiencies, have a stimulating effect on scientific life, as pointed out by Vaihinger. Even if that were the case, they would be useful for science perhaps, but themselves unscientific.

If we have certain elementary components of a view as given, we can provide a survey of the possible combinations in the simplest way by admitting each characteristic as well as its negation as an element of a combination, for example, a, b, c and their negations a_1, b_1, c_1. We shall later treat periodicity, polarisability and possibility of interference of light rays in this way. It will be shown that one theory, for example, can have a periodic but unpolarisable light ray, while another does not have periodicity but rather has polarisibility. For example, from three elementary notions and their negations we get eight combinations in all:

(1)	$a\ b_1\ c_1$	(5)	$a_1\ b_1\ c_1$
(2)	$a\ b\ c_1$	(6)	$a_1\ b\ c_1$
(3)	$a_1\ b\ c$	(7)	$a\ b\ c$
(4)	$a_1\ b_1\ c$	(8)	$a\ b_1\ c$.

Let us assume the dispute is grouped around the first four theories. (1) and (2) will act as a-theories, (3) and (4) as *c*-theories. At this stage the subdivision is quite correct. The *a*-theories are at the same time c_1-theories, and the *c*-theories, at the same time, a_1-theories. But if one of the further theories is realised, for example (5) with $a_1\ b_1\ c_1$, one speaks, as a way out, of a mixed theory. At first one may not see anything doubtful in this denomination if one did not know that such denominations mostly have the consequence that they frequently deprive the theories concerned of any exact analysis. Who does not know the ill-famed group of 'eclectics' in the history of the sciences, under which title men are often subsumed to whom only later generations assign the place due to them. Though it is very important to stress significant factors, a uniform treatment of all elementary notions should certainly come first. From a purely logical point of view there are eight groups of theories; they can be grouped in the most different ways as can be readily understood. All these classifications are at first of equal value. A special pleading is needed if one of them is preferred. But most historians of physics operate with dichotomies, as do the historians of other sciences, as if these were the most obvious.

In order not to lose my way in these too general comments, I want to use a concrete example to show how I think the characterisation of the kind just described could be applied in practice; this then will enable me, in conclusion, to give details about possible procedures in the classification of systems of hypotheses that give their due to greater historical currents.

The subject I choose for my comments is the history of optics from the beginning of the seventeenth to the beginning of the nineteenth century. The reasons for the selection of this discipline and this period are mainly the following: the subject of optics was rather clearly fixed at a relatively early time. Advances were made successively by representatives of the most different trends. At one time this trend, at another time that trend achieved important results. The wealth of elementary notions and elementary hypotheses is large enough to make a sufficient number of combinations possible. Acoustics, for example, would have been all too simple; its most important principles were already stated in final form early on, and everything that came later was an improvement. On the other hand, the theory of electricity displays too much confusion. At first there was a rather aimless groping to-and-fro. The subject of investigation changed all the time. Only in our last decade does one get the impression that one day the theory of electricity might quiet down. The electromagnetic theory of light, as stimulated mainly by Maxwell (1831–1879) in his theory of light in 1865 and developed by

Hertz, contributed to bringing some order into the immense complexity. Admittedly recent years again gave rise to much that is new, but there is already a great stock of things that are approximately secure. The history of optics is suited for our comments only up to the middle of the nineteenth century, before the union of electrical and optical theories. After that, one has to take all physical notions into account to do justice to optics; its relative isolation reached its end.

In my comments I do not want to get involved in all elements of the theories of light but to push only some simple characteristic facts to the foreground; I choose a formulation that does justice to the whole period.

(1) Periodicity. Newton made the following experiment: he illuminated a plane-convex lens that was lying on a glass plate (Figure 1) with homogeneous, perhaps blue or red, light.

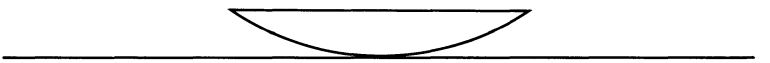

Fig. 1.

If one looks down on it, dark and bright rings appear. The radii of the resultant, apparently dark, rings are in the ratio of $\sqrt{0} : \sqrt{2} : \sqrt{4} : \sqrt{6} : \sqrt{8}$, that is of the square roots of the even numbers. The radii of the bright rings are in the ratio of $\sqrt{1} : \sqrt{3} : \sqrt{5} : \sqrt{7} : \sqrt{9}$, that is, of the square roots of the odd numbers.

(2) Interference. Though in the more modern theories of light, periodicity and interference are most closely linked, I nevertheless introduce them separately because the two phenomena are logically independent of each other. A phenomenon can be periodic without displaying interference. Also there were physicists who introduced periodicity into the theory of light without being acquainted with interference. Interference can be characterised like this: a light ray is split in two, for example, by suitable reflection. If one allows the two light rays to cover distances of different lengths and then unites them again, the resulting brightness can differ, depending on the differences of distances covered; the sum of the two rays can even result in darkness. The conclusion is that there are two states that cancel each other out.

(3) Polarisability. If one takes two layers of tourmaline whose crystal axes have a definite direction (Figure 2) and puts them on top of each other so that the axes run parallel (Figure 3) light sent to them will pass through and the area behind will be illuminated. If, however, the layers are superimposed

so that the axes are at right angles (Figure 4) light does not pass through any longer.

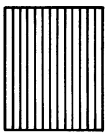

Fig. 2.

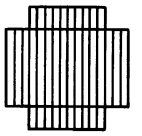

Fig. 3.

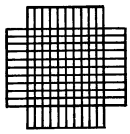

Fig. 4.

From this and other facts the conclusion is drawn that the light ray, by passing through a tourmaline crystal, no longer behaves the same in all directions. Let us imagine the light ray to be cylindrical with all diameters of its circular section being of equal application (Figure 5); after passing the first tourmaline layer, however, one of them is preferred (Figure 6).

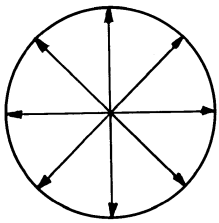

Fig. 5.

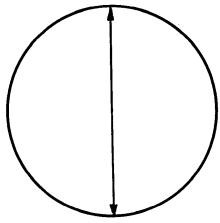

Fig. 6.

Only that part passes through the tourmaline crystal, so to speak, that is parallel to the slits in the crystal. If the second crystal is placed so that the slits are parallel to those of the first, the polarised ray passes through, but not if the slits are at right angles. For the questions with which we are concerned these hints are sufficient.

(4) Diffraction. If we send light through a small hole (Figure 7) the spot of light on the opposite wall $a_1\ b_1$ is larger than the basis of the cone that is determined by the luminous point and the edge of the hole.

Details of this phenomenon, changing brightness, coloured margins and several other factors, do not matter for us here. Anyhow it is easily understandable that this phenomenon could lead to the supposition that there is a component in light that is not in the direction of propagation or that holes and slits effected changes of direction.

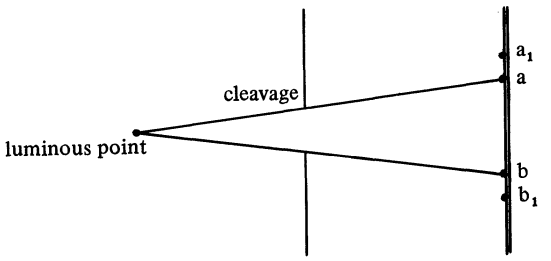

Fig. 7.

Let us now look at how individual authors responded to these phenomena. Periodicity and polarisibility are unknown to Descartes though there are some hints that might be taken for periodicity. However, he speaks of a kind of diffraction when he develops the hypothesis that the larger globules in a part of space press on the smaller surrounding ones. A large globule affects several smaller ones; they rush apart and thus can transfer the light impression onto a wider area. I have already mentioned earlier the extent to which there are elements of emission and wave hypothesis in Descartes. He still belongs to a preparatory period; that is why I do not want to deal with him in more detail.

Francesco Maria Grimaldi (1618–1663) is significant; his main work appeared in 1665. Sound is one of his analogies to light; this is very important. Though Grimaldi's knowledge of the character of sound is inadequate, he still has provided a very fertile stimulus through this juxtaposition which, by the way, was also made independently from another side. Through experiments he is acquainted with diffraction as well as interference. In his lively imagination, light was something very complicated; he believed that many elementary qualities were required to master it. He ascribed current, undulation, motion and rolling, to light.

In his work on truth, which first appeared in 1675, Malebranche (1638–1715) in particular expressed the idea that light was a kind of sound. He imagines space to be filled with lots of small vortices that are pushed and pushing. He believes these vortices to be agitated by periodic vibrations. According to him the different colours are a consequence of the changing speed of vibration. He replaces hard corpuscles by the small vortices in order to explain how it is possible that rays coming from all sides can penetrate through a small opening without disturbing one another. This could only be explained, according to him, if these impacts were transferred to vortices and propagated by vortices.

We find a fertile central idea carried through by Christiaan Huyghens (1629–1695). For this reason his variety of notions of light is small, and the number of facts he takes into account is not all that great either. In his writing on light in 1678, he makes no use of Grimaldi's manifold observations. In his conception, the propagation of light is the ball-shaped expansion of a single impulse in the ether. He does not mention periodicity. Until recently the conception of X-rays was often like this. Only much later was the periodicity of the impulses introduced, so that each trough of a wave was followed by a crest, this again by a trough and so on, at equal distances. However, Huyghens thinks that each point of the surface of the ball becomes itself the centre of another ball that it tends to spread to all sides. The surfaces of these numberless small balls, according to him, form the surface of the next large ball. Light therefore contains a tendency in more than one direction. This supposition is called Huyghens' principle. In principle it is suited to explain diffraction. The points of the large ball-shaped shell that touch the edges of the slit are centres of new ball-shaped shells which spread behind the space of the slit. It is a strange coincidence that Hugyhens knew nothing of Grimaldi's earlier observations of diffraction and did not make any such observations himself. He therefore found himself obliged to explain why the phenomenon, which he had to expect on the basis of his principle, was missing by assuming that the phenomena of diffraction were so weak that they could not make sufficient impression on the eye. Whereas Huyghens did not know of the periodicity of the light ray, its polarisibility was familiar to him.

We now have to deal with Newton (1642–1727); as he does not have as consistent a central idea as Huyghens, he therefore operates with a much greater wealth of elementary notions. It was precisely his inconsistency that was highly stimulating and gave posterity an opportunity to form hypotheses of many kinds, many of which have proved fertile. According to his words he attaches little weight to the character of light, but in fact he is very dependent on the notions that he forms of it. Actually he expresses them several times. In general he advocates the view that light consists of small emitted corpuscles, but remarks can also be found which show that he did not want to exclude the theory of ether completely. He is acquainted with the phenomenon of diffraction, but he explains it, in contrast to wave theory, as an influence of the slit on the light ray. Against wave theory he pleads that the whole wall behind a slit would have to be illuminated if light is viewed as an analogy to sound. In this pleading he overlooks the fact that sound waves are very large, light waves very small in relation to the slit. In order to explain the colours of thin layers that display periodicity as described above, Newton

assigns periodicity to his light ray. He speaks of the ray's "fits of easy reflexion and ... easy transmission" (Newton 1952, Book II, Proposition XII, p. 281). In his view these 'fits' appear periodically. Later authors, for example, Brewster, thought the light particles were small magnets which rotate around an axis at right angles to the direction of the propagation of light, so that periodically now the positive, now the negative pole is in front. The hypothesis of 'fits' introduces periodicity, but not yet interference. From the foregoing it cannot be logically deduced what the effect may be if a positive and a negative pole hit the eye or the photographic plate at the same time. Newton also speaks of the light ray's polarity. He compares the influence of calcite on the ray with magnetic processes. While in the normal light ray the poles of the little magnets could be assumed as being directed to all sides, they are now oriented. According to Newton's theory each particle had two magnetic axes, one in the direction of propagation, the other perpendicular to it. Newton uses precisely the polarisibility of the ray as an argument in favour of the emission theory. Moreover, it should be expressly pointed out that Malus (1775–1812), a main opponent of the wave theory, brought the theory of polarisation to a high level of perfection.

Euler (1707–1783) takes up the wave hypothesis. He stresses the periodicity of the light ray and deduces colours from the speed of the oscillations of light, but he abandons Huyghens' principle. One of Euler's main objections to the emission hypothesis, that small emitted particles were not able to penetrate bodies, is redundant today, since we know a number of corpuscular rays that do penetrate, for example, through aluminum.

At the end of the eighteenth and the beginning of the nineteenth century the controversies become very lively. While the Englishman Young (1773–1829) and the Frenchman Fresnel (1788–1827) advocated the wave theory and perfected it greatly, the Englishman Brewster (1781–1868) as well as the Frenchmen Biot (1774–1862) and Malus (1775–1811) opposed it. The two groups of physicists were a match for each other for a long time. Sometimes the one group, sometimes the other, scored special achievements. If one hypothesis had undergone some completion, the other tried to adapt this to itself. In the end a great many statements on light were common to both groups of physicists and they differed only in certain basic hypotheses. What was periodicity of the waves for some was periodicity of these 'fits' for others. A main difference between the two parties could be seen in the fact that the wave theorists displayed a greater uniformity of hypothesis formation than the emission theorists, who needed a special hypothesis for almost each new phenomenon treated by the wave theorists. Nothing, however, would be more

mistaken than making little of the emission theorists as was usual in many circles, partly as a consequence of school teaching. While it is always reported of the wave theory that it teaches the periodicity of waves, the point is often omitted that Newton had a periodicity in the emission theory at a time when this was still foreign to wave theorists; not to mention other points.

Let us now set to work and analyse the historical development described above. If we go back to the elementary notions we see at once that there can be many ways of grouping the physicists. For example, I shall single out a few of them and characterise them as to their acceptance of periodicity, Huyghens' principle and emission:

Physicist	Periodicity	Huyghens' Principle	Emission
Huyghens	no	yes	no
Newton	yes	no	yes
Euler	yes	no	no
Young	yes	yes	no

If we leave out the basic hypothesis about the character of light we must declare, with reference to the two remaining characteristics, that Huyghens, Newton and Euler differed from the ultimately victorious assumption of Young by one characteristic each. There is no intrinsic reason why the statements about the character of light should be more important for the theory of light than the statements about the occurrence of periodicity, Huyghens' principle, etc.

If, for example, we classify the physicists, according to the characteristic whether they acknowledged Huyghens' principle or not (which probably became of greatest importance since the explanation of diffraction is based on it in the wave theory), then Newton and Euler are in the same group. Why should the fight about emission and non-emission be inherently more important than the fight about periodicity and non-periodicity or about Huyghens' principle and non-Huyghens' principle?

The significance of Newton's views becomes very clear to us if we compare the Newtonian corpuscular ray with the corpuscular rays which frequently occur in modern physics. It is often pointed out quite rightly, that Newton's corpuscular hypothesis was certainly not so nonsensical as had frequently been believed. However, people usually forget to add that Newton's corpuscular

rays were of an incomparably more complicated constitution than, for example, our cathode rays that consist of corpuscles. Newton's rays are more related to X-rays; though they do not consist of emitted corpuscles, they have the quality of periodicity.

If we were to denote the elements of systems of hypotheses with abbreviations, and if we disregard the other elementary notions, Huyghens would present the view Hu, Newton and Euler, Pe; only Young would have the combination PeHu. But I already want to stress here that such characterisations can only represent the first step of analysis; in continuation, one has to attempt to discover the driving ideas. In doing so one will generally have to go beyond the individual sciences and consider the total world view. What views one advocates concerning matter and energy depend only partly on the experiences of physics and chemistry. The views that one has of the structure of the whole world play their part. Some specific hypothesis may be superior to another in terms of pure physics, and still perhaps the latter will be preferred, because it does more justice to certain chemical or biological experiences. The classification of systems of hypotheses of a particular individual science may be based on factors of subdivision that have been acquired outside this science.

I always speak of 'systems of hypotheses' without wanting to delimit exactly what is denoted as hypothesis, what as reality. This question is not directly bound up with our problem. Without prejudging it, one can accept or reject the above-mentioned characterisation and classification. Precisely in the field of optics, hypothesis and experience are thoroughly mixed up. In order to disentangle them one should first define exactly how 'hypothesis' should be understood.

An example will make this clear at once. Newton's first proposition says that differently coloured light has differing degrees of refraction. In his experiment Newton looks through a prism at a strip of cardboard that is partly blue, partly red. If the refracting edge of the prism is on top, the blue strip appears more raised than the red one. Goethe violently criticises Newton's description of this experiment. Goethe imputes a blue margin along the blue as well as along the red strip when looked at through the prism; whereas this [the blue margin] is added to the width of the blue strip, it diminishes the red one because it is there in counteraction and hardly visible. We see how differently the optical phenomena can be grouped. Goethe pushes the coloured margins to the forefront. If something does not suit this view, it is neglected to begin with; Newton *vice versa* puts the course of the rays in the foreground and neglects other things as being, so far, unimportant.

Goethe points out that in Newton's illustration (Figure 8) the red strip has fringes of which Newton makes no mention at all. "Why does he not mention this phenomenon in his text of which he has a careful, though not quite correct, engraving made in copper? A Newtonian will probably answer: this is just a residue of the decomposed light which we never can get rid of entirely, and that still plays its tricks here."

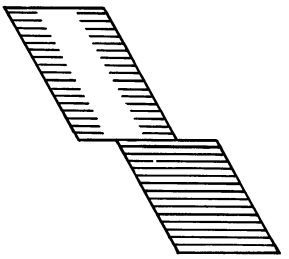

Fig. 8.

Merely by neglecting or stressing some facts, a hypothetical element is already introduced into a theory. The whole fullness of a phenomenon can never be completely reflected [in the theory]. The selection of certain relationships is itself based on more or less distinctly expressed hypotheses. One sifts out certain facts, combines them into a whole and hopes to be able to incorporate the remaining facts. Possibly one assumes that one can do them justice even by modifying the initial view.

We see that the mere enumeration of elementary notions is not yet sufficient to place a system of hypotheses historically. One should also always indicate which facts have been neglected, which favoured. The systems of hypotheses of physics, like all other systems of hypotheses, are *an instruction directing not only the connectedness, but also the selection of facts*.

Each system of hypotheses, even if its formulations are of the utmost precision, has, to use this expression, a blurred margin. This always and necessarily exists. The amount of difficulties can grow through new insight; at best we can approach clarity asymptotically. A complete mastery of the whole multiplicity seems an impossibility to us.

In our attempt to classify the systems of hypotheses, we have observed first how the precisely expressed notions of the systems of hypotheses can be systematically registered and then how their 'combinations' can be classified according to different characteristics. We have further observed that there

remains a blurred margin, and the question now is how one can do justice to this.

If we analyse the systems of hypotheses more closely we find that, consciously or unconsciously, they usually operate with analogies. Sometimes an empirically given phenomenon is directly drawn in as analogy, sometimes a combination of empirical elements; occasionally such elements seem to be incompatible with each other empirically, as happens, for example, with many ether hypotheses. If such systems of hypotheses are mathematically elaborated, they mostly lead to systems of formulas; their interpretation is carried through by some physicists only so far as the results concerned can be empirically checked; possible intermediary correlations can be conceived purely mathematically. To be sure, this system of formulas, without leaning towards interpretation, no longer has that aptitude of development that analogy had before, whether formulated mathematically or non-mathematically. The scarce data that we possess about the world insufficiently determine the wealth of relationships and thus do not enable us to create all imaginable systems of relations by going through all possible combinations, and then to find out about their reality. The systematic investigation of all possible combinations is opportune only where a relatively narrow area has already been delimited. Something like that can be attempted successfully under certain circumstances with chemical compounds of certain materials. Moreover, one should not take the empirically known elements of experience as a basis; in order to exhaust all imaginable possibilities one should establish all possible relations purely logically and deduce their consequences. Of course reality is also contained among these imaginable possibilities; however, there is no means to detect it. For even if one sifts out a system that coincides with reality in certain points and now investigates whether there exist facts in experience that can be related to the remaining qualities of this system, one has to remember that an infinite number of systems can be indicated which are applicable in the part that can be interpreted empirically but exclude each other in the part that so far eludes empirical interpretation. In order to be able to orient oneself in these systems and to make a selection among them, one has to operate again with some assumptions concerning the probability that certain systems may be realised. This probability, however, is mainly based on analogies.

Here an analogy may be found to a field of experience whose system of hypotheses has a blurred margin. This analogy itself must also contain certain elementary views and, if it contains empirical data, will also possess a blurred margin. Both members of the analogy are therefore related with respect to

their structure. The analogy can now be useful to different degrees. It is especially fruitful if it leads at least one step beyond those facts it originally comprised. However, there may be good analogies that lead even several steps ahead.

In my cursory remarks about the history of optics, I have already emphasised a significant analogoy. A number of physicists, such as Malebranche, Huyghens, Euler, etc., expressed the opinion that light is something like sound. In connection with this analogy it is remarkable that the authors mentioned did not know anything about interference of sound or of light. The thinkers who analysed light were influenced by the advances of acoustics, just as those occupied with acoustics were by the advances of optics. Only Young was simultaneously concerned with interference of both light and sound. The theory of interference of sound was later developed by W. E. Weber in the first third of the nineteenth century. Such occurrences in the field of scientific thinking have given rise to the idea that a principal task of genius consists in acts of empathy with the essence of the world. The genius is supposed to be able to recognise whole complexes of facts as related, even if he is not consciously aware of all elements of the two analogised views. It is just the non-formulated part of the analogy that contains, as it were, a driving force. This idea could induce us to subdivide the hypotheses according to their analogies as long as the centres of analogy are more or less fixed. Seen from this standpoint it cannot be denied that for certain periods the distinguishing of optical theories into wave and emission theories was justified. But as soon as science tends to endow such an analogy with more and more new secondary qualities, the power of the central idea loses more and more of its significance. We can observe this very well, especially in the case of the emission theory. If new phenomena are no longer explained in terms of the original analogy, but now require purely superficial additions, then the totality of qualities that are ascribed to a phenomenon, and not the original basic analogy, is the characteristic of the system of hypotheses. A group of analogies as it were has now been taken into use, and in principle there is no longer any justification for the first accidentally chosen analogy to be in further use for the characterisation of the group of analogies.

In some cases, when one makes the dominant analogy the focus of comments, one feels compelled to defend the justification of such an analogy by a corresponding assumption about the structure of the world. The possibility of the analogy can be grounded on the fact that the diversity of the world is less than might be thought. When the physicist Oliver Lodge once declared that "Without pushing and pulling no effect is possible", his statement may

have been trivial, but it may also have originated from a deeper insight into the structure of the world. The assumption that the phenomena of the world on a large and small scale repeat themselves all the time, is being applied continuously. We think of the motion of whole planetary systems as being similar to that of a single planet around its sun. Some people think that the structure of a radium atom is analogous to a planetary system. The smallest particles revolve around a centre. The laws of reflection that apply to solid bodies also apply to light. Maxwell's equations, which are valid for electrical fields, are transferred to the field of the electron, though the correctness of this assumption can never be checked because each test body would have to be much smaller than the electron, which itself is regarded as a minimal quantity. The kinetic gas theory operates with the laws of mechanics, which it applies to the motion and impact of molecules.

But it is not only between the large and the small that analogies are found or presupposed. Also different fields often display analogies. Remember how close light, electricity, magnetism and radiating heat have successively come to each other. At some distance from them there is also sound, which at least still has some relationship with them. Perhaps smell will join them; according to some it is supposed to possess the phenomenon of interference which is characteristic of periodic waves.

Such experiences and hypotheses lead in the end to the assumption that the totality of all phenomena can be mastered by a small number of basic equations that could find application to phenomena of the most varied kind. There is, so to speak, a basic type of relationships that, with certain modifications, is found realised everywhere. If one wants to give rein to bold trains of thought, all phenomena could perhaps be conceived as deviations from certain basic phenomena just as, similarly, the Platonic doctrine of ideas sees in the multiplicity of leaves of a tree different realisations of one uniform idea of leaf. By philosophical assumptions of a very general kind about the structure of the world we can possibly obtain foundations for the classification of systems of hypotheses but, as can be seen at once, this assumes much intellectual preparation. A man who is not equal to these sequences of ideas will hardly be able to achieve anything that furthers the history of physics if he pursues them with inadequate means. He is better off if he takes equal account of the components of the hypotheses; this can lead to very usable and useful results.

In these rough outlines, I have tried to show how the application of analogies and the orientation of the history of physics in this direction can be supported by an assumption about the character of the world. However,

one also can start from another premise: that this reflects the character of our thinking. In crude form this thought may perhaps be formulated like this: the familiar basic notions will always be sufficient only if enough care is taken to complicate and elaborate the hypotheses. As long as the task is not to create new sensual impressions in order to do justice to certain areas of the world, but rather to represent certain relationships, one can always start from any notions whatever, because from them all imaginable relationships can be produced by purely logical operations. If one assumes this standpoint, the use of analogies for classification purposes loses much of its importance. It merely characterises the physicists whereas the earlier interpretation also put the subject of investigation in connection with the formation of analogy. If we consider the present state of thinking, we must say perhaps that both views are valid to a certain degree. There are analogies which really helped to uncover a deeper relationship and there are those that served essentially to help thinking advance, but were of greater complication than reality as far as our view of it can tell. In order to be able to use the formation of analogies for the purpose of classification, with this standpoint as a basis, one needs in fact more general considerations about the psychology of hypothesis formation.

If one sees that the choice of the original analogy is of no decisive significance for the structure of the system of hypotheses, one is involuntarily impelled to accord equal value to different systems of hypotheses to the degree to which they comprise the multiplicity of reality. Thus it easily becomes a task of patience to succeed in modifying a given system of hypotheses until it achieves the same success as another system. Duhem's opinion is that, if a sufficiently high prize is offered, one could get a modified emission theory today that would also do justice to those facts of experience which, one believes, can only be explained with the help of a basic supposition that differs from the emission theory. Some people like to dismiss this point of view as a new fashion that was introduced by Poincaré, Duhem and others. In so doing they overlook entirely the fact that the same way of thinking characterised the period a hundred years ago, one that is akin to our period in many ways. Brewster, for example, discussed periodicity in emission and wave theories and at last arrived at the result that both contained the magnitude d that makes its appearance in the study of Newton's rings. Only it means different things in the two theories, and he says, with all desirable clarity: "These periods and spaces exist in reality, there is nothing hypothetical to be found in them except the names they are given." J. F. W. Herschel (1792–1871) gets even closer to Duhem's comments when he points out that the

interference phenomena could be explained by suitable modifications of Biot's emission theory. Similar remarks can be encountered repeatedly at that time. They only ceased when the wave hypothesis seized absolute power. Very few people study abandoned hypotheses and try to equip them in such a way that they achieve more than they could at the time when they were thrown on the scrap heap as a consequence of often purely accidental circumstances. The refutation of a hypothesis is all too often a refutation *ad hominem*, not *ad rem*. Whoever wants to work at history of physics effectively must above all be able to separate such refutation *ad hominem* from refutation *ad rem*.

Today as well as a hundred years ago there are often several systems of hypotheses for the explanation of the same complex of facts; it follows naturally that one looks for the common parts and regards them as essential. Meanwhile the differences that originate from the basic hypotheses recede into the background more easily than at other times. The system of formulas that is confirmed – whether fitted in to this or that interpretation, or applied to reality only in its final results – easily becomes the characteristic of a doctrine. An outcome of this point of view is also Hertz's well-known pronouncement:

> To the question, "What is Maxwell's theory?" I know of no shorter or more definite answer than the following: – Maxwell's theory is Maxwell's system of equations. Every theory which leads to the same system of equations, and therefore comprises the same possible phenomena, I would consider as being a form or special case of Maxwell's theory (Hertz 1893, p. 21).

How far one and the same scientist is able to think through several systems of hypotheses to the end and deduce their consequences, is another question. It is also imaginable that only epigones are suited for such work and that the great pioneers must always be obsessed by one individual hypothesis. It cannot be excluded that perhaps only the belief in the correctness of a definite analogy or group of analogies can create the energy that is needed to overcome all difficulties. The result of these considerations might possibly be that the great physicist must necessarily be a bad philosopher. In addition, it may be pointed out that in Hertz's quoted conception, the significant blurred margin of analogies, which leads the scientist to further assumptions, does not play any part at all. It is perhaps possible in principle that in two scientists' systems, the precisely formulated statements coincide, but the tendencies toward expansion differ. It can also happen that at first the precisely formulated statements differ, though the advocates of the two

systems use the same basic analogy; only something in this analogy appears to be more important to the one than to the other. In the course of development these scientists can get closer and closer, whereas in the case mentioned earlier the two scientists perhaps get further and further apart. The precisely formulated qualities of systems of optical hypotheses provide food for thought in this respect. The elementary notions of a Brewster are much closer to those of a Fresnel than an Euler's, if one wants to use the term 'close' and 'apart' at all. It matters much in such considerations which part of an analogy is put to the forefront. If Malus speaks of emitted magnet-like corpuscles, he has little kinship with modern light theory, if one stresses 'emitted corpuscles'; but he gets close to it if one stresses 'magnet-like'. Malus would certainly have used the displacement of spectral lines by magnets as support for his theory.

Our comments have shown that a comparative study of systems of hypotheses, which has to be regarded as the basis of all historical research, can proceed in two stages. As a first stage a pure analysis of the system has to be proposed that leads to a grouping of elementary views. We saw that in this way, without special general considerations, some useful insight can be gained already within the specific field of research, within physics, chemistry or any other science. Schemes of this kind allow us to indicate what the contributions of individual scholars have been. By not giving preference at first to any elementary notion, all groupings of hypotheses are taken into account with the same intensity; the usual, and often unjustified, neglect of transitory systems – called like that mostly on the basis of crude dichotomies – is avoided. For a best possible mastery of historical development it is desirable to make a preparatory survey of all groupings of individual views that are possible in principle. Maybe the start can be made from the theory of greatest power, if there is one of this character.

The second stage needs a premise of general considerations. In it one can no longer recognise the significance of certain hypotheses with the help of reflections within the individual sciences. Our reference to a total world-view becomes a duty. Only with special gifts and after comprehensive previous studies can a man satisfy such demands. Historians of physics in this sense will therefore be either philosophically trained physicists or philosophers trained in physics.

It would be desirable to separate these two stages more sharply than has been usual so far. The systematic completion of the first stage could create a routine through which many could be qualified for profitable work. Besides, this separation would lead to a greater insight into the character of systems of hypotheses through preparatory historical work. A classification of systems

of hypotheses in the first stage is of a different kind than one in the second stage. I have only tried to outline in which direction such tendencies of classification might move, without in any way exhausting the possibilities.

The theory of systems of hypotheses has been greatly advanced by men like Mach, Duhem, Poincaré. The right moment may now have come to group the systems of hypotheses of all sciences systematically and to supplement the actual hypotheses by possible ones into a more or less complete whole. It is the task of philosophical reflection to appreciate the significance of this aim; it is not the concern of the individual sciences. As we need theories to classify things, so we need theories to classify theories.

CHAPTER 3

WAYS OF THE SCIENTIFIC WORLD-CONCEPTION

For the most part it is not a good sign if scholars are too eagerly occupied with the foundation and history of their discipline instead of producing new statements concerning the subjects treated by them. Physicists can afford occasional considerations of this kind as they are surely above any suspicion of slowing down their own work by sterile debate about method or by historicizing reflections, or of papering over defects. As representatives of disciplines with very tidy concepts they approach their objects directly; in doing so they also use traditional intellectual means without, however, specially stressing their genealogy. Einstein does not call himself a neo-Cartesian, though in a certain sense he would be justified in doing so; philosophers, on the other hand, like to speak about themselves as neo-Hegelians, neo-Kantians, neo-Thomists. The advocates of a scientific world-conception, which absorbs everything that can be experienced, behave like physicists. They are active and close to the present time even if they move in abstract spheres. They care less for the history of their trains of thought than for new insights, which they try to formulate in clear statements. They do not rest satisfied with the results achieved, but advance and improve formulations year by year.

But occasionally it is advisable to seek the historical conditions of a view and to look around and see how it fits in sociologically, what its connections are with other spheres of life and science, in the interest of the aim of a unified science. For an advocate of a scientific world-conception this is certainly not a task for a work-day, but on a Sunday like this one may be allowed to ponder about such matters, as an introduction to special discussions.

We can state as a historical fact that an idealistic-metaphysical current is on the increase at present. At the same time, however, and with as little doubt, we see that also the scientific world-conception is especially advanced by all physicists, technologists, biologists, medical scientists; even by the scientific research of those who seek personal support from idealistic-theological views.

In its deliberations, the comprehensive scientific conception always starts

Translation of Neurath 1930 [ON 187].

from the individual which it joins with what is similar into greater, and clearly surveyable complexes. It recognises no 'world' as a whole, it does not aim at comprehending a mighty world-picture in its totality, at a world-view. If one speaks of a scientific world-'conception' in contradistinction to a philosophical world-'view', 'world' is not to indicate a definitive whole, but the daily growing sphere of science. This conception is deduced from individual scientific work, which one wants to incorporate into a unified science. It is different with traditional philosophy which arrives at its conclusions about the 'world' from fundamental considerations. It often tries to derive individual judgements from its judgements of the world. Hegel's philosophy allowed only seven planets at a time when the eighth was already discovered by a scientist.

Some people regard the comprehensive scientific conception as a young movement, which would have to sweep out a tradition of millennia and therefore requires a totally new attitude. Following Comte, many people think of the transformation of human thinking like this: it starts with a religious-theological period, followed by a metaphysical-philosophical period until this is replaced by a scientific-positivist one. But there are reasons for a different notion of historical change, and this is relevant from the educational and psychological point of view. If basic elements of the scientific world-conception were already present in the springtime of mankind, then we have a greater chance of being able to revitalise them.

To begin with, it may be pointed out that the changes in the way of thinking, which are closely connected with the concrete technical and social changes of mankind, do not manifest a uniform direction. Certain ingredients of the scientific world-conception continue aspects of the theological world-view, but then human attitudes are now revitalised in part, some of which were familiar in a long-lasting 'springtime', but were to a large extent repressed during the theological interval.

In that springtime religion is accompanied by a powerful magic (see Frazer and others), which was once perhaps the sole ruler. The sorcerers with their practices exerted immense influence for a long time. Equipped with special powers they confronted the masses by frightening them.

In the subsequent religious period, the place of the sorcerer, who is to be compared with a technician, a psychoanalyst or a surgeon in relation to the individual, is taken by the godhead with whom each individual man tries to get in contact, though often with a priest as intermediary.

Magic may seem alien to us at first glance; still, in a certain respect, it is closer to modern physics or biology than theological thinking. In general,

the magician works finite changes, determined by tradition, which can be perceived and therefore checked by everybody.

In pre-animistic periods he operates with touch and analogy-sorcery; in animistic ones he trains spirits as a circus director trains horses and elephants. Yes, the power of sorcery sometimes reaches into the age of the beginnings of theology. The Brahman, for example, forces a godhead by suitable ceremonies to carry out a curse he has decreed.

But the magician does not have to deal with the totality of the world as the theologian has, nor with an all-embracing godhead that lives in everything, does everything. In this respect the modern physicist is close to the magician. In modern physics there remains nothing of infinite absolute space, which was, in a certain sense, a theological residue; one does not speak any longer of the 'initial conditions of the world' that were a requirement, for example, of Laplace's spirit in order to derive future or past events with the help of natural laws. It is characteristic of the modern conception that it can draw conclusions from some parts about other parts, that in many cases all events in a finite region of space-time can be determined by initial and boundary conditions.

Some examples may show how far the magical way of thinking is related to that of the modern scientific world-conception. In the primitive conception, a corpse is often taboo; whoever touches it brings death upon himself. (Modern analogy: infection.) Or: disease is removed by incisions, tattooings, into the body. (Analogy: bleeding, operation.) By incisions into the body the sorcerers make men into women-men. (Analogy: transplantation of glands for changing sex.) One often deliberately presents magical action as being stranger than it is. If we formulate the recorded magical statements more precisely, we certainly encounter strange contents but also familiar relations: a definite observable event is seen as a condition for another.

Considering the method, one can relate the behaviour of Indians in their buffalo dance to the behaviour of modern physicists. If the buffaloes come, the technical measure of the dance is justified; if the buffaloes do not come, auxiliary hypotheses are proposed. For example, the fault could be the wrong place, the wrong moment or a wrong ceremony. Similarly, in modern physics, auxiliary hypotheses are thought out in order to maintain certain hypotheses. If we investigate not the correctness of the hypotheses, but their nature and the nature of their subject, we see that in both cases the hypotheses have to do with facts that are observable by the senses; they can be checked because they do not contain any appeal to infinite totalities, to unknowable and transcendental things. – Certainly it has to be admitted that the special

intellectual means of a magic of similarity and touch are alien to modern science; but for the evaluation of the behaviour and views of that time it would be wrong to establish an antagonism between magical ceremonies and technology, and perhaps to separate early technology from early magic by counting what we accept today, as technology, and what we reject, from the standpoint of modern knowledge, as magic. With respect to behaviour, everything at that time belonged to the same level. When primitive man hunted an animal, a whole system of ceremonies had to be performed. For example, the animal was drawn as precisely as possible inside a cave, and perhaps the picture was then shot at with a miniature bow and arrow ('bushman revolver'). If one avoided the waning moon, which might have a damaging effect, by analogy, on the fleeing animal, if man and woman were chaste, if the children followed certain rules, and if, finally, one added a ceremony to shoot at the animal with a large bow and arrow, then one could kill it; afterwards one had to ask its spirit for forgiveness. The shooting of the arrow, which is acknowledged by us as 'true' technology, is not at all the most essential part of the complex of ceremonies for primitive man. Moreover, let us not forget how quickly our assessment of what 'true' technology is often changes. If an old woman casts spells on warts, we call it superstition; when university professors do the same thing, we speak of suggestion therapy and science.

Magic has to do with the finite, the empirical, just as the modern engineer or physician does. The magician is judged according to his effectiveness. If his sorcery is bad, his prophecies wrong, he is dismissed or even killed. If the sorcerers of a Negro tribe ward off the lions badly, they can take their bag and stick and move on.

It is understandable that the individual magical proposition of belief has a very tough life in spite of its verifiability. In general the testing does not proceed systematically enough, moreover it is not always as easy to monitor the magical measures as it was in the case of the buffalo dance. But in our own time, is it so especially easy to find out whether somebody is a really good physician, for example? If somebody would seriously undertake the defence of a certain magical doctrine with the most modern intellectual means, the falsification would certainly be difficult enough. Remember only how much scholars have already been taken in by spiritualists! But in general people of the magical periods are not inclined to argumentation; therefore magic is so conservative, so hostile toward all novel technical methods. Theology, on the other hand, being more directed towards the divine and towards social life, gives freer scope to technical measures; this has occasionally been emphasised by Max Weber.

Intellectual movements with an elaborate logical equipment are perhaps only rarely of a magical kind. Mostly they are theological, idealistic-metaphysical or materialistic-positivist. However, in the sphere of magic, there are attempts at a 'scientific' astrology, palmistry and the like; spiritualist 'theory' can also be added here. From the fact that we reject hypotheses and general statements of the magical views – assuming they are formulated – it does not follow at all that the conclusions that can be drawn from them are incorrect and that they can always be checked by crude experience. If it were impossible to obtain something correct from wrong premises with the help of correct ways of conclusion, mankind would have perished long ago, since it has ventured into extensive operations, with visions of the future. Frequently it is the case that an individual magical statement, which can be acknowledged by modern science, was founded on a doctrine that we have to reject. From this it does not follow that such a statement must have emerged from manifold experience. Rather a further selection may have taken place under various magical manipulations. It is precisely modern psychology that has put forward all sorts of ideas that were not so unfamiliar to magic.

Whereas the Romans, on the basis of this world-view, regarded it as a bad omen when the military leader fell down in the attempt to mount his horse (sideways), a modern scientific psychologist would perhaps express the opinion that in this a momentary mood was revealed that was not suitable for leading a battle. With the most peculiar reasoning, many herbs and minerals were used in medicine that remain in use today on the basis of chemical analysis. Early on, herbs and minerals were already separated from other substances, which at first were used similarly, but did not prove useful. The astrologers were proved right in their opinion that high and low tides depend on the moon, and the opponents of astrology were wrong.

It must remain undecided whether magical views were always reported correctly. However, it is certain that their thinking is concerned with the connection of individual empirical elements. The report by a missionary about the Iroquois is characteristic enough: "One has to assume that the Iroquois are incapable of reasonable thinking, in contrast to the Chinese and other civilised peoples to whom the belief in the existence of God can be demonstrated. The Iroquois do not respond to reasoning; in general they believe only what they see" (according to Levy-Brühl). In the course of development, magic becomes more and more interlarded with religious notions that assume connections beyond those between individual elements, and also with an all-powerful godhead or with the world-as-a-whole, unless the empirical boundary is crossed in another way as in Buddhism for which the doctrine of rebirth, of entering into Nirvana, cannot be verified.

In fully developed religions, with an apparatus of theological argumentation, the possibility of control by ceremonies and priests ceases. God always reserves decisions for himself. The priest becomes an intermediary between God and man; but he is no longer capable of acting with finality or deciding prophetically. There is, so to speak, only one single great magician left, and that is God. Ceremonies have no effect on him but, to the extent that he can be influenced at all, it is by the moral disposition and behaviour of men. Two stories from the Old Testament can throw light on the transition from a magical to a theological conception.

When Moses and Aaron came to Pharaoh, they competed, at Yahweh's command, in empirically controllable effects with Pharaoh's magicians. " . . . Aaron cast down his rod . . . and it became a serpent. Then Pharaoh also called the wise men and the sorcerers . . . : they also did in like manner with their enchantments. For they cast down every man his rod, and they became serpents: but Aaron's rod swallowed up their rods" (*Exodus* 7: 10–12). In spite of this power of Aaron's serpents, Pharaoh's heart remained hardened. Now Aaron smote the water of the Nile and it turned to blood; all the fish died, and the Egyptians could not drink the water. But the Egyptian magicians achieved the same decisive experiment with their spell. Pharaoh's heart remained hardened. Aaron made frogs arise, and "they covered the land of Egypt. And the magicians did so with their enchantments, and brought up frogs upon the land of Egypt" (*Exodus* 8: 6–7). Now Aaron stretched out his hand with his rod and turned all the dust of the earth into lice throughout Egypt; but the magicians with their spells were too weak for this and "said unto Pharaoh: This is the finger of God" (*Exodus* 8: 19); so experience made the decision!

How much the conception of sorcery resembles the scientific conception could be observed when the Mohammedan scholars responded to physical demonstrations by Napoleonic scholars without special astonishment, because they knew everything from the 1001 Arabian Nights. Sorcery was more advanced than the technology then current which believed it knew how to fly in vehicles that were heavier than air. What distinguishes magic from science is, above all, the missing systematic connections and systematic control of experience; however, these are also often lacking in thinkers of antiquity such as Aristotle and others; Aristotle recounts the strangest things that could have been proved wrong in the simplest way empirically by quick inspection!

The Bible shows the step to the European religious conception in the story of the prophet Jonah. It is told that Yahweh bade Jonah go to the great city of Nineveh and to prophesy its end. But Jonah did not want to do so and make a fool of himself. He boarded a ship to flee from Yahweh, but Yahweh

sent a storm and Jonah was swallowed by a great fish and was finally "vomited out . . . upon the dry land" (*Jonah* 2:10). Now he obediently accepted the command and preached: "Yet forty days, and Nineveh shall be overthrown" (*Jonah* 3:4). Then the people of Nineveh did penitence, man and animal fasted, and were clothed in sackcloth. Then God did not carry out the prophecy, which had been pronounced without condition, to the great annoyance of the disavowed prophet. In his anger he said that he had not wanted to be a prophet from the start because God would in the event be sorry to destroy a people, in spite of the prophecy. To deliver Jonah from his grief, God made a tree grow that cast a shadow, but let it wither the next day. When Jonah was very angry about this, God reproached him that (if) he started such lamentations about a plant, how should God not have mercy on Nineveh? That is, God decides independently of the prophet; there is no possible appeal, no calculation to which his decision could be subjected. The prophet is no longer responsible if his prophecy does not come true; in general, prophesying ceases from then on. The business of prophecy can no longer be empirically verified. God is no longer bound to any words of priests. There is no verification! Whereas Pharaoh was still converted to God sensually by perceivable acts, Christ declares that sensually perceivable acts prove nothing. Not even miracles prove anything in favour of those who perform the miracle. "There will arise false Christs and false prophets who will give great signs and miracles." According to Catholic doctrine, the Antichrist will appear as a miracle worker.

Thus the Christian theologians retire from the sphere of empirical verification. Their notion of God is not deducible from individual experiences – with certain Manichean notions of God this might be possible – but there are also other oversteppings of the empirical boundary. Thus the Mexican priests, for example, declared that in certain time intervals of many years, they had to twirl the sun fire in a special ceremony, otherwise the sun would go out and mankind would perish with all its belongings. Here perhaps is the beginning of a relationship to totality; the possibility of empirical assessment applies actually only for the case of continued twirling, as the verification of the other case is forbidden as too dangerous, and also it is no longer possible when all life perishes, though this can be expressed in a formula today.

Typical transitional forms are also found in Catholicism. A specific picture of Mary can perform miracles; but Mary knows best whether a miracle is good for the person who prays for it; if she does not perform a miracle, nothing is proved against her. An example: a child is dying, the mother raves against Mary, who finally gives in after all; twenty years later the mother

herself wishes the child were dead, for the saved child has become a murderer. Clearly, the habit of Christian-theological statements is always this: in the last resort everything is entrusted to God. If the good man suffers, this is either punishment for secret sins, or it is a test as in the case of Job, or it is God's unfathomable resolve. By way of this hypothesis, everything that happens in the life of men is conformable to the basic theological view. This hypothesis is excluded from empirical verification, therefore it is empty and meaningless for the actively disposed man who finds the meaning of a statement in its confirmation by experience – in contradistinction, by the way, to the pragmatist who is satisfied if a statement helps to enhance a mood of vitality. The pragmatist can, under certain circumstances, also admit theological statements, whereas this is not possible with an activist.

We see how the conscious reasoning of Christian theology destroyed the earthly empirical way of magical thinking though at the same time reducing the power of the priests, and connecting the individual more [directly] with God, a development that came to completion in Protestantism. The medieval church replaced magical ceremonies with the few sacraments that have transcendental effects outside empirical control; concerning the sacrament of extreme unction it is maintained that it might have earthly as well as transcendental effects and contribute to recovery. The ceremonies of the Catholic Church that refer to exorcism and therefore serve the fight against demons are in the background. The great mass of traditional sorcery was declared to be superstition or real deviltry. Thus it is perhaps understandable that at the beginning of modern times, when the decrease of church power and the simultaneous advance of scientific empiricism starts, witchcraft plays a larger role than before: the earthly causality of old magic re-emerges. At the end of the Middle Ages we find only a few witch trials; at the beginning of modern times the Catholic Church (who often see a heretic in the witch, making use of the similar sounding [German] names [(*Ketzer-heretic, Hexe-witch*)] and above all the Protestant church, busied themselves with the persecution and killing of witches in really great style that corresponded to a strong belief in witches. The revitalised sense for earthly things reveals itself to some extent in science, in connection with technology and rationalised action, as well as in sorcery.

The Christian theologians have obviously pushed aside the earthbound attitude of ancient science and primitive sorcery, and promoted the rise of idealist and spiritualist metaphysical notions. But on the other hand Scholastics heaped deductions on deductions in order to deduce statements or demands from transmitted texts; proofs of all kinds were continually

produced that occasionally also referred to physical, chemical, psychological and biological doctrines. The logical side of scientific thinking was greatly advanced, the understanding of empirical fruitfulness rather weakened. Attention was mainly concentrated on a sphere beyond sensual perception.

Modern scientists are characterised by the following: a sense for earthly things, the call for empirical control, and the systematic application of logic and mathematics. However, it cannot be claimed that the most intensive scientific research originated from the anti-theologian precursors. If we disregard magic and focus on the later forms of anti-religious ancient philosophy, we discover that the most successful 'enlighteners', the Epicureans, who were backed by a popular movement, were unscientifically minded, in spite of their opposition to theology and metaphysics. Rarely did scholars spring from their midst. In antiquity mathematics was advanced especially by the theologically minded Pythagoreans and Platonists; God was seen as a mathematical being. The notion of God obviously contains both: completely free play of the will, 'freedom', and on the other hand the highest order and thence its mathematical aspect. This opposition was first overcome in pantheism as against 'freedom': for Spinoza *natura sive deus* has neither understanding nor free will. The mathematical character of the godhead advanced the occupation with mathematics; astrology too was favourable to the development of mathematics. The great astronomical and mathematical theories would hardly have developed as early as they did through everyday needs alone. Kepler discovered his laws of the planets by trying to discover 'God's holy harmony'.

A naive empiricist view of history always looks for series of developments in straight lines, for simple conflicts between two views. A closer inspection of real life, however, reveals far more complicated relations. By elaborating logic as its tool, theology creates its own enemy; by mathematically reconstructing God's holy order of the spheres, it makes God redundant for astronomical calculations. When asked by Napoleon why God was missing in his theory of the spheres, Laplace could already answer that he had no need of this hypothesis. Not the anti-theologians, but the theologians themselves have prepared the sharpest weapons of modern science: logic and mathematics. Such contradictions correspond to Hegel's 'cunning of reason' and his 'dialectic'. Turning from the idealist to the empirical, such sequences of events are investigated from this point of view by Marxism, which comes from Hegel via Feuerbach. Hegel's pantheism can clearly be seen as the boundary line between theism and atheistic science: God reveals himself in the total historical course of the world, therefore not specially by

individual miracles. Thus Hegel can feel himself to be a man of the Enlightenment vis-à-vis the theologians, and in this sense can praise Epicurus as a colleague. Hegel himself obviously shows a double face.

We have now become acquainted with magic and theology, we have seen how logic and mathematics grew up in the land of idealist metaphysics and religion, and then associated themselves with materialist empiricism. As soon as mathematics had become significant, it could quickly develop, especially in connection with astronomy. But also, if detached from any practical applications, it already receives sufficient stimulus from a minimum of empirical matter, for it is indeed a realm of purely tautological transformations. It is psychologically understandable that the real sciences appear in the following succession: astronomy, physics, chemistry, biology, sociology. Thus it can perhaps be explained, for example, that one hundred years ago the Russians already had a Lobachevsky, who worked on non-Euclidean geometry, half a century later a Mendeleyev who created the system of chemical elements, and a Metchnikov whose works on immunity achieved international significance, and today they have their Pavlov. A young people can initially be efficient in the mathematical-logical field, and soon, therefore, in that of physics, where the unfolding of massive experience, as in history, is not necessary; in similar ways a young, not yet matured man can be a great mathematician: Pascal had already found his famous theorem at the age of sixteen. There is no significant biologist or historian at this age. Perhaps it is an analogy to explain why women, who are mostly kept apart from the manifold experience of public life, have distinguished themselves predominantly in mathematics, astronomy and physics if they are active in science – Sonia Kovalevska, Curie, Noether may be remembered here – whereas in history and sociology good female accomplishments are much rarer.

The sciences amenable to mathematical treatment show the greatest and quickest successes. At first theologians try to reconcile themselves with modern times. They construct the doctrine of double truth. What is correct in science can, they say, be false in theology. In vain; the total scientific view is relentless. All dualism is demolished. The old Aristotelian separation into earth and heaven is removed to within the earthly sphere. Galileo, a contemporary of Kepler, amalgamates the earthly and heavenly worlds into one world of the movement and impact of bodies. Also, there is no longer a place that all things seek.

It is understandable that at the beginning of modern times the manner in which the novel branches of life: technology, commerce, bookkeeping, the art of war, were dealt with, was almost exclusively materialistic-empirical and

unphilosophical. In further development, theology loses more and more ground to the exact sciences.

What characterises the modern scientific conception of the world is, as mentioned before, the interconnection of empirical individual facts, with systematic testing by experiment, the joining of the individual into the texture of all sequences of events, and the uniform logical treatment of all trains of thought, in order to create a unified science that can successfully serve all transforming activity. But the road to this way of thinking, even within scientific change, was not straight; the process of detachment from theology and idealist philosophy has taken the most peculiar detours. We see this very clearly in the conceptions of space and time. Newton's concept of absolute movement makes sense only if one thinks of space as an unlimited box with spider's threads as coordinates. This concept can be applied to reality if one assumes the possibility of simultaneous perceptions at all points of unlimited space. The unlimited space thus presents itself to us as the sensorium of a god. In contradistinction Descartes already had a concept of motion that much more resembles the modern one. According to him motion is "the transport of a body from the neighbourhood of bodies in direct contact with it to the neighbourhood of others". This concept of motion easily leads to the notion of biographies of individuals, elements meeting and separating whose summation would result in the scientific description of world-events according to Einstein's conception. The nuclei of basic concepts of relativity theory, 'world-lines' and their section, can be found in Descartes. And yet it was Descartes who provided the means of analytical geometry, the system of coordinates, for Newton's theory of the absolute. In Newton's view inertia is an effect of unlimited space, whereas in the opposing empiricist view, inertia depends on the state of acceleration in relation to the system of fixed stars; this system induces the phenomenon of inertia so to speak. It is often overlooked that these views already opposed each other at an early time. Euler rejected the conception of induction so decidedly and ridiculed it so utterly that subsequently it was difficult for any scholar to advocate it. Only when historical conditions had changed did Mach, on the basis of his positivist attitude, discard the theory of the absolute as a residue of theology. Mach has always stressed that physics always describes only cross-sections taken from real events and never makes judgements concerning the 'world as a whole'. For his outlook, which was searching for functional relationships between observable events, it was quite natural to set forth the connection between inertia and the system of the fixed stars.

Theological residues in science can be suspected wherever empirical

statements are related to a postulated or fictitious 'complete' insight – either with or without an 'as if' expression. The determinism of Laplace's formulation is untenable, for the assumption of knowledge of an unlimited cross-section of the world is totally meaningless. Perhaps there are theological residues also in the search for the ideal language and in certain applications of the concept of infinity in mathematics. The attempts to make mathematics finite, especially in applications to concrete events, are certainly part of tidying up. Frequently we need only to give a finite meaning to statements with infinitesimal or transfinite expressions. Of a different nature are discussions of structures of formulas into which the application to empirical events does not enter. Among these structures are also those of statements about the empirical. As long as this complex of questions is not fully clarified, it remains noteworthy in any case that Cantor personally had a great inclination to religious and metaphysical notions and even gave his set theory a theological interpretation. Obviously this does not say anything at all against the possibility that the set theory can be faultlessly built up in the end.

The roundabout ways of human intellectual work can be seen wherever thinkers with Catholic training have advanced certain modern views, while on the other hand they have at other times hampered the removal of theological residues. Since the very theologically minded Brentano had originally been a Catholic theologian, he avoided the Kantian interlude for himself and his disciples; in connection with Leibniz and the scholastics, he was especially interested in everything constructive and logical. Meinong and Mally showed much understanding of certain aspects of the modern scientific world-conception, but on the whole they obstructed the advance of their own thinking in this direction! Likewise in the phenomenologists, who derive from Brentano, the understanding of the logical is always present though their strong metaphysical propensity always shows forth too; this is even more clearly visible in Scheler, Heidegger and others than it is in Husserl himself. So here the soil of theology and metaphysics produced germinal layers of certain questionings of the scientific world-conception in modern conventionalist currents that were, however, not developed; on the other hand, one finds a religious admixture that can be especially detected in Le Roy, the mystical partner of Poincaré; it should also be said that Duhem praised the scholastics and the way of their thinking. Elsewhere too, even among positivists, empirio-critics and other representatives of modern views, we find remarkable residues of out-of-date ideas, time and again. Only those scientists with a materialist basis have formed an effective counter-weight to all this; they enjoy the support of mathematicians and logicians to a growing extent!

In the field of 'history' and 'political economy' we have to deal with much less clarified material but, in some respects, with less supernatural [trans-earthly] notions, as far as individual research is concerned. Certainly, Ranke, in his *History of the Popes*, still speaks of God's finger in world history, more clearly recognisable in some centuries than in others. But even concepts like 'spirit of the people' and similar ones play a relatively small part compared to that of such empirically meaningful concepts as offer, demand, export and import, warfare, emigration, etc. As far as the historians avoid general laws and try to present facts – and this is done by the majority – they at least keep protocols just as astronomical observers do, and thus they serve the aim of science, at least indirectly. Many data needed for the formulation of laws, however, are certainly not recorded and conversely, much useless stuff is faithfully kept. Most of theology and metaphysics can be found in that 'philosophy of history' that favours more general statements. Many 'mental [or spiritual] sciences' – this separation in itself mainly hints at theological residues – persist in disguised idealistic-metaphysical trains of thought in their scientific work. Their concepts should be critically examined, especially from the viewpoint of their 'fruitfulness' for empirical predictions. It is not sufficient merely to exclude the unempirical. Metaphysical concepts freed from metaphysics are often useless in themselves! After critical examination we would find that such methods as classification according to types, uncontrollable empathy and the like, are preferred for use in the 'mental sciences', notably to dress up essentially metaphysical trains of thought in an apparently scientific garb. It is as if one wanted to translate poetry into strict and precise language. Philosophy of history often is the substitute for general statements of a sociological kind for which certainly there is a need.

'History' and 'political economy' can only enter into the sphere of the sciences if one reshapes them into a sociology on a materialist basis with whose help one makes general statements that are derived from experience and which serve to predict single historical events, as this is undertaken by the Marxist conception of history. All sociology resting on 'empathy', 'interpretation', etc. is full of metaphysical components. It leads away from predictions to more or less classificatory endeavours. The scientific approach is most difficult to introduce wherever there is interest in the future fate of single individuals; there everything presses toward astrology and similar disciplines. Where the subject is masses and groupings of men, stability is larger, and the instability of the individual is less conspicuous. Therefore such questions are more amenable to scientific treatment, and the interest in such questions furthers the scientific attitude. The modern statistical approach,

which has become so significant in physics, has its origin in sociological methods that were advocated about the middle of the nineteenth century and even earlier by Quetelet and others.

The scientific content of such a sociology, which sets itself the task of predicting the concrete future course of events in the field of the social, rests on the use of precise and fruitful concepts. The subject of research is the course of all of human life, the acting together and against each other of certain human groups. The course of human life is treated no differently by a strictly scientific sociology than the life in an ants' nest or in a bee-hive. We investigate the influence of such courses of events on the living standard, on the conditions of pleasure and displeasure of the participants, where pleasure and displeasure are consistently defined by observable behaviour.

Thinking about statistical and organisational matters is of a perfectly empiricist kind and starts from perceived objects. Such a presentation of physical courses of events with their conditions of pleasure and displeasure ('living standard' in political economy) is closed in itself without the need to have recourse to anything 'psychical'. Everything to do with the psyche can be incorporated if it is represented along the lines of 'behaviourism'. Physicalist sociology can cover a great deal with the study of statistics and organisation. The statistical approach overcomes deficient concepts such as 'mutual effect' and others that often occur in prevailing sociology.

It is precisely in connection with the statistical deliberations of modern physics that we learn to overcome the thought of a gapless and all-embracing causality that is often strongly imbued with metaphysics. Occasionally one begins to be satisfied with probability statements, with statements concerning groups of events that replace statements about single events. The ambiguity of approach – whether resulting from lack of knowledge or from the nature of the subject – can be overcome practically, in the last resort, only by the unambiguity of action, that is of decision (for example, experimentally to use a certain statistical law as basis). This unambiguity of decision lies outside the sphere of scientific argumentation. No logical reason can be given if one decides in favour of a certain conception or gives preference to a certain measure of possibilities.

It is of decisive importance for the scientific world-conception to become aware of the narrowness and limitation of knowledge in this way because otherwise there would be the danger that one creates a new idol by the postulate of complete definiteness, one that would take the place of the old *a priori*, or the infinite and the divinity. Where formerly the priest or philosopher stood, the professor would stand. We must refrain from such

hasty postulates. What we state about things must be said with caution: we find chaos and order, and we have to find out empirically which measure of order we can establish in our concept formation in view of the relationships involved.

Our thinking is a tool, it depends on social and historical conditions. One should never forget this. We cannot act as prosecutor and defendant at the same time and in addition sit on the judge's bench. We confront our present thinking with earlier thinking, but we have no possibility of taking a judge's stand on a point outside. Checking statements with the events is itself part of the characteristic method itself.

We owe our means of expression, our rich language and script, to definite historical premises. A people with a conceptual script (as, for example, the Chinese) are handicapped in creating a freely moving symbolism; on the other hand, it is less exposed to the danger of talking philosophical nonsense. What a safeguard that everybody, as in mathematics, can only write and read books that he more or less understands! In a people with alphabetical writing, with unlimited word formation, tongue acrobatics ('glossurgy' according to Stöhr) often initiates philosophical problems. Lichtenberg said: "Always things, no words! The world can move on, and the words remain." The problems of '*Sein*' and '*Sosein*' are so much dependent on language that they cannot even be properly reproduced in the rich Arabic language.

The letters as signs without conceptual meaning are, however, well suited for a strictly scientific symbolism. One combines signs whose meaning one can define at will. The Indians with their language and script that are related to ours are supposed to have already had a kind of symbolic grammar.

The modern scientific world-conception owes its successes partly to the new symbolism that can be used for the purification of language. A condition for the scientific world-conception is that there are general names. If a people has, say, sixty names for cows, according to colour, age, distance from the speaker, etc., it provides few premises for the scientific world-conception. Here again it seems to be true to say that our most fruitful symbolism for mastering the concrete had to have very non-concrete antecedents. Thus we are bound to our historical situation.

Whether institutions like patriarchy or matriarchy, which are connected with the conditions of production, are present, is of significance for ways of thinking. Mixtures, as they are assumed in Europe, seem to have a special power of performance, and the fantastic and unbridled European agility is joined to the strictest exactitude and self-limitation.

Starting from magic, the way leads through religion and philosophy to

materialist empiricism. And then? What have we to expect of the development of the scientific world-conception on a materialist basis? If we could already know this in detail today, the change would already have come about. We can try to make guesses only for short distances ahead. Bound to the cooperation of other thinkers, to the living conditions of the age, each individual is subject to limitations. Intellectual community work that is planned on a great scale is probably only possible as a general phenomenon in a planned, fully organised society, which energetically and consciously shapes the order of life with a view to earthly happiness with the help of earthly means. Social changes put their stamp on intellectual changes.

There are more and more people for whose thinking the scientific world-conception is a guide in their concern for a fruitfulness that can be empirically tested: They study logic as a doctrine of tautological transformations, they busy themselves with the coordination and connection of individual spheres of experience, with the theory of constitution (Carnap) as a basis for the thesis of a unified science that has to be concretely built up and continuously enriched in order to be fruitful.

The scientific world-conception occasionally transcends the boundaries of individual sciences, but not by creating something that is superimposed. In traditional school philosophy, the general is at the beginning; in the scientific world-conception, according to its approach, which starts from the concrete experience, the general is later than the particular. Wherever there is a clear question, there is also a clear answer; it makes no sense to speak of unsolvable riddles. Thus, on the one hand, the scientific world-conception admits the limitation and dependence of human thinking, but on the other hand it transmits the proud and still self-restrictive awareness that we find in the proposition of Protagoras, that man is the measure of all things.

The way has been found by which we proceed. For some time it seemed as if advocates of empiricism could only work in isolated, individual disciplines whose union depended on 'accidental' successes of research, whereas now we again face the possibility of creating a comprehensive structure of unified science by connecting the results of individual research on the basis of conceptual precision and transformation, thus uncovering gaps and serving research as a whole. If we master experience intellectually with resolution and audacity, we can again hope that unification and interconnectedness are possible to the greatest extent. We find ourselves in a mood similar to Hegel's, for with Hegel – even though on a different basis – we can say: "Man cannot have too high an opinion of the greatness and power of the mind."

CHAPTER 4

PHYSICALISM: THE PHILOSOPHY OF THE VIENNESE CIRCLE

Although what is called 'philosophical speculation' is undoubtedly on the decline, many of the practically minded have not yet freed themselves from a method of reasoning, which, in the last analysis, has its roots in theology and metaphysics. No science which pretends to be exact can accept an untested theory or doctrine; yet even in an exact science there is often an admixture of magic, theology, and philosophy. It is one of the tasks of our time to aid scientific reasoning to attain its goal without hindrance. Whoever undertakes this is concerned not so much with 'philosophy,' properly speaking, as with 'anti-philosophy.' For him there is but one science with subdivisions – a unified science of sciences. We have a science that deals with rocks, another that deals with plants, a third that deals with animals, but we need a science that unites them all.

All these disciplines are constructed of the same bricks, as it were. Our knowledge of phenomena is controlled by sight, hearing, tasting – our sense organs. In any such consistent empiricism, psychology must concern itself with human behavior, just as mineralogy (together with chemistry, physics, etc.) is concerned with the 'behavior' of stones.

The followers of this method of reasoning invariably ask: What do I mean by a positive statement, and how can I test it? A statement which cannot be controlled is a *thesis devoid of sense*. Those who thus succeed in formulating a system of laws which they apply in *predicting events* were best regarded as "*representatives of a scientific* conception of the universe" (*wissenschaftliche Weltauffassung*). Mach, Poincaré, Peano and others, as followers of Hume, in a certain sense, have done their best to sweep away the last vestiges of theology and metaphysics. Their work is now being continued by many of the younger intellects, especially in Europe, intellects busily engaged *in analysing the language of science* and the system of signs and building up a *system of symbols* with the aid of logic and mathematics. Bertrand Russell's work has been of decisive value in this effort.

All these adherents of a rigorous empiricism reject anything that smacks of the 'absolute,' whether the subject matter relates to the world of the *a priori,*

Reprint, with minor corrections, of Neurath 1931a [ON 197].

or the world of the categorical imperative. 'School philosophy,' with its definite conception of the fundamental basis of being or thinking, presumes to sit in judgement on science as if it were a court of last resort, and this presumption the representatives of a scientific *Weltauffassung* summarily reject. They know only science and the clarification of scientific methods, and this clarification is all that remains of old-fashioned 'philosophizing.' Philosophy as an independent system of definite doctrines is obsolete. What can not be regarded as unified science must be accepted as poetry or fiction.

This point of view is advanced with especial energy by the 'Viennese Circle', which is strongly influenced by Bertrand Russell and by Wittgenstein, whose *Tractatus* was edited in German and English by Russell. On behalf of this group Moritz Schlick and Philipp Frank are issuing a series of publications which are designed to aid the cause of a scientific conception of the universe in all departments of science.[1] A periodical with the same program, *Erkenntnis*, is edited by Rudolf Carnap (Vienna) and Hans Reichenbach (Berlin).[2]

The system of laws from which single events or processes are deduced, in other words unified science, can be wholly or partially modified whenever the results obtained are contradicted by experience or observation. Every phenomenon is tested by means of sound, light, etc., but sound and light play no part in the final scientific presentation. In the formulas of science, with the aid of which human beings succeed in understanding one another, only logical-mathematical signs are utilized. It is senseless to say: "I see the same red as my friend." How my friend combines the symbol 'red' with other signs clarifies for me the structure of his system of expression. More cannot be done by science. Signs can indicate a 'near,' a 'between' and a 'so much,' but no more. What is at all scientifically expressible is no richer in fundamental relations than the symbols on a Morse tape which the telegrapher reads as they are sounded by his apparatus. In a sense unified science is physics in its largest aspect, a tissue of laws expressing space-time linkages – let us call it: *Physicalism*.

Physics has been successfully purged of metaphysical formulas. For example, the conception of 'absolute motion' has been discarded, a conception which acquired meaning only if one thought of 'absolute space' as a gigantic glass case in which 'coordinates' were woven like spider webs so that it became possible to determine whether a body is at absolute rest or whether it is moving about within the case. The Mach-Einstein conception dispenses with this 'absolute space' which assumes any meaning only when one conceives of God who is present in all places at all times. Absolute space

is a product, in a sense, of a 'sensorium of God' (Newton). In the Mach-Einstein theory we find only bodies and their relationships. A body can move only in relation to other bodies and not in relation to 'space.' It is impossible to draw conclusions which are simultaneously and universally applicable. We can do no more than record the 'biographies' of individual bodies and note how these bodies approach and recede from other bodies. The sum of these biographies constitutes a scientific description which does no more than formulate statements for observational verification.

In the field of psychology, the physicalists are closely allied with Watson and his behaviorists without, however, accepting their formulas. In the field of biology, the physicalists reject 'vitalism' insofar as it maintains that non-space-time entities become 'effective.' In sociology, also, the physicalists find it necessary to oppose transcendental, metaphysical entities, the 'spirit of an age' which 'manifests' itself in various ways, and 'the powers of the spirit' which are in perpetual conflict with one another. It is in this very field that metaphysical tendencies (as in Sombart, for example) constantly crop up, although 'history' and 'economics' now include empirical sociology, deal with such concrete things as human beings, streets, cities, vehicles, factories and the like. In Germany it is the fashion to oppose *Geisteswissenschaften*, the 'intellectual or moral sciences,' to the others, to separate cultural science sharply from natural science and to demand special methods for each of the two fields. In Physicalism no such separation is tenable, which in the last analysis can be traced back to the unwillingness of man to give up entirely his special position as part of a celestial kingdom.

Ethics, which dealt formerly either with the laws of a God or at least with laws *'an sich,'* in other words, laws from which in a certain sense God had been eliminated (Kant's categorical imperative), is now supplanted by inquiries which make it possible for man to attain happiness by definite arrangements or definite methods of conduct (behavior). Instead of the priest we find the physiological physician and the sociological organizer. Definite conditions are tested for their effect upon happiness (*Glückswirkungen*), just as a machine is tested to measure its lifting effect. No science can teach what 'should' be done; it can assert only that because A and B have happened, a very definite C follows. The task demands systematic organization of human effort. This involves engineering, gymnastics (hygiene), and the social technology of today, all of which have an influence on scientific management and commercial organization and thus on human life as a whole.

Everywhere we find a growing sense of technical organization, a sense in harmony with the extension of that new scientific conception of the universe

(*Weltauffassung*) which forges a powerful weapon by the unification of science.

No matter in what country or continent they may be, those who regard themselves as simple laborers in solving the riddle of life unconsciously join forces whenever they devote time and effort to the clarification of science and whenever they systematize and interpret with the aid of logic and mathematics all that we perceive through the senses. To predict what will happen and to guide one's actions accordingly is the greatest triumph of earthly striving, the concrete success of human effort which does not make use of *theses devoid of sense* but is rooted in the soil of Physicalism.

NOTES

[1] Published by Julius Springer, Vienna. [The first six volumes in this series (called *Schriften zur wissenschaftlichen Weltauffassung*) are listed below, with English translations where available. – Eds.]

Vol. 1. Friedrich Waismann: *Logik, Sprache, Philosophie*. [Announced but not published. A very different version was published in 1965 as *The Principles of Linguistic Philosophy*, edited by R. Harré (London: Macmillan), a translation of the German text, which appeared later (Stuttgart, 1976) under the title, *Logik, Sprache, Philosophie*, edited by G. P. Baker and B. McGuinness. – Eds.]

Vol. 2. Rudolf Carnap: *Abriss der Logistik*. 1929. [Revised and translated as *Introduction to Symbolic Logic*. Trans. W. H. Myer and J. Wilkinson. New York: Dover, 1958.]

Vol. 3. Richard von Mises: *Wahrscheinlichkeit, Statistik und Wahrheit*. 1928. [*Probability, Statistics and Truth*. Trans. J. Neyman, D. Sholl, and E. Rabinowitsch. London: W. Hodge; New York: Macmillan, 1939.]

Vol. 4. Moritz Schlick: *Fragen der Ethik*. 1930. [*Problems of Ethics*. Trans. D. Rynin. New York: Prentice-Hall, 1939.]

Vol. 5. Otto Neurath: *Empirische Soziologie*. 1931. ['Empirical Sociology' in *Empiricism and Sociology*. Ed. M. Neurath and R. S. Cohen, with translations by P. Foulkes and M. Neurath. *Vienna Circle Collection*, Vol. 1. Dordrecht, Boston: Reidel, 1973. pp. 319–421.

Vol. 6, Philipp Frank: *Das Kausalgesetz und seine Grenzen*. 1932. [English translation by M. Neurath and R. S. Cohen in preparation *Vienna Circle Collection.*]

[2] Published by Felix Meiner, Leipzig. *Erkenntnis* is the organ of the Verein Ernst Mach in Vienna and of the Gesellschaft für empirische Philosophie in Berlin.

CHAPTER 5

PHYSICALISM

The members of the Vienna Circle (Moritz Schlick, Rudolf Carnap, Philipp Frank, Hans Hahn, Herbert Feigl, Fritz Waismann, Kurt Gödel, Otto Neurath and others) are working out a 'Logical Empiricism'. Following Mach and Poincaré, but above all Russell and Wittgenstein, all the sciences are treated uniformly. Carnap's *Logischer Aufbau der Welt* (1928) shows in which direction future systematic work will move. Wittgenstein's *Tractatus Logico-Philosophicus* (1921) clarified, among other things, the position of logic and mathematics; besides the statements that make additions to what is meaningful, there are the 'tautologies' that show us which transformations are possible within language. By its syntax the language of science excludes anything that is meaningless from the very beginning.

At first the Vienna Circle analysed 'physics' in the narrower sense almost exclusively; now psychology, biology, sociology are more and more drawn into the discussions. The task of this movement is unified science and nothing less. This radical standpoint, a consequence of the direction of development so far, is to be sketched in the following, for those who know the foundations of these endeavours.

All members of the Vienna Circle agree that there is no 'philosophy' with its own special statements. Some people, however, still wish to separate the discussions of the conceptual foundations of the sciences from the body of scientific work and allow this to continue as 'philosophising'. Closer reflexions show that even this separation is not feasible, and that the definition of concepts is part and parcel of the work of unified science.

Wittgenstein and others, who admit only scientific statements as 'legitimate', nevertheless also acknowledge 'non-legitimate' formulations as preparatory 'explanations' which later should no longer be used within pure science. Within the framework of these explanations the attempt is also made to construct the scientific language with the help, so to speak, of pre-linguistic means. Here we also find the attempt to confront language with reality; to use reality to verify whether the language is serviceable. Some of this can be translated into the legitimate language of science, for example, as far as

Translation of Neurath 1931b [ON 198].

reality is replaced by the totality of other statements with which a new statement is confronted: more about this later. But much of what Wittgenstein and others say about elucidations and the confrontation of language and reality cannot be maintained if unified science is built on the basis of scientific language from the beginning; scientific language itself is a physical formation whose structure, as physical arrangement (ornament), can be discussed by means of the very same language without contradictions.

Let us now attempt to carry out the Vienna Circle's demands within the sciences consistently and apply them to everything that we express by language. We start with scientific language as a physical formation.

'Making predictions' is what all of science is about. At the beginning of the process are observation statements, which, of course, contain measurements of time and space, if only in an approximate manner. There are always spatio-temporal formulations behind which we cannot reach at all without saying something meaningless. 'Saying' itself is a spatio-temporal arrangement.

With the help of observation statements we formulate laws; according to Schlick these laws are not to be seen as proper statements but as directives for finding predictions of individual courses of events; these predictions can then be tested by more observation statements.

The study of language can perfectly well be combined with the study of physical processes; for one always stays in the same field. In staying within the closed area of language one can express everything.

Thus *statements are always compared with statements*, certainly not with some 'reality', nor with 'things', as the Vienna Circle also thought up till now. This preliminary stage had some idealistic and some realistic elements; these can be completely eliminated if the transition is made to pure unified science.

'Induction' that leads to laws is a matter of 'decision', it cannot be deduced. The attempts to give 'induction' a logical foundation are therefore bound to fail. If a statement is made, it is to be confronted with the totality of existing statements. If it agrees with them, it is joined to them; if it does not agree, it is called 'untrue' and rejected; or the existing complex of statements of science is modified so that the new statement can be incorporated; the latter decision is mostly taken with hesitation. *There can be no other concept of 'truth' for science.*

Under certain circumstances it must be possible to link the laws of all sciences with each other to make *one* definite prediction. One can only know whether a certain house will burn down if one can take into account how the building components behave, how the human groups behave who push on to fight the fire. The various scientific disciplines together make up the 'unified

science'. It is the task of scientific work to create unified science with all its laws.

A prediction can be checked [controlled] by observation statements only if we indicate where and when a predicted change is to take place. It does not make any fundamental difference how this is defined in detail by statements. What matters is that all statements contain references to the spatio-temporal order, the order that we know from physics. Therefore this view is to be called 'physicalism' (see Neurath 1973b). Unified science contains only physicalist formulations. The fate of physics in the narrower sense thus becomes the fate of all the sciences, as far as statements about the smallest particles are concerned. For 'physicalism' it is essential that *one* kind of *order* is the foundation of all laws, whichever science is concerned, geology, chemistry or sociology.

The Vienna Circle is making particularly vigorous efforts to give unified science a solid framework through 'syntax', along the lines of the logicians, of Wittgenstein and others, and to eliminate everything that is 'meaningless', i.e. all metaphysics, by a proper use of language. It is a defect of language that it admits something like a 'neighbour without a neighbour', a 'command without a commander' ('categorical imperative') within its rules. The defects of syntax reveal the standard of scientific study. An unblemished syntax is the foundation of an unblemished unified science. Language is essential for science; within language all transformations of science take place, not by confrontation of language with a 'world', a totality of 'things' whose variety language is supposed to reflect. An attempt like that would be metaphysics. *The one scientific language can speak about itself, one part of language can speak about the other*; it is impossible to turn back behind or before language.

This also corresponds perfectly to the 'behaviouristic' approach of 'unified science'. Thinking in terms of language as physical process is the starting point of all science. It is quite possible to speak about the behaviour of someone who does not speak; it is however, impossible to discuss some pre-linguistic circumstance with pre-linguistic means – this at once seems to us to be meaningless.

Carnap, who has so far probably advanced the work of the Vienna Circle the most towards empiricism, made an attempt to create a constitutive constructive system; in this he distinguished two languages: a 'monologising' (phenomenalist) one and an 'intersubjective' (physicalist) one. He tries to deduce the physicalist one from the phenomenalist. However, in my opinion it can be shown that this division cannot be carried out, that on the contrary only one language comes into question from the start, and that is the language

of physics. One can learn the language of physics from earliest childhood. If someone makes predictions and wants to check them himself, he must count on changes in the system of his senses, he must use clocks and rulers; in short, the man who supposedly is in isolation already makes use of the intersensual and 'intersubjective' language. The forecaster of yesterday and the controller of today are, so to speak, two persons.

The words 'blue' or 'hard' or 'shrieking' are then used in a physical sense alone. They either indicate that a man shows a certain behaviour under certain conditions, that he speaks words or exhibits nervous changes ('field statements'), just as, for example, a test body in the neighbourhood of a charged ball in some experiment; or they indicate that there is a certain oscillation somewhere. If someone says: "I see blue", this is coordinated as a 'statement about reality', if one accepts the statement as being about spatio-temporal changes that have taken place outside the man, or as an 'hallucinatory' statement if certain changes are assumed to be only within the human body, that is in certain areas of perception in the brain, whatever their delimitations are. Finally one can also speak of a 'lie', namely if only the speech centre and word formation participate in making the statement. In all cases, however, the statements are physicalist.

The statements themselves also form part of other statements as physicalist elements. A comparison of 'statements' with other entities is meaningless, as mentioned before. If someone says, "I see blue", we base on this the construction of statements about changes in his eye nerves, in his brain, but we do the same if he says, "I feel anger". The statements about 'organ sensations', which play an important part along with statements about other behaviour, are then joined to the structure of the system of physicalist statements. 'Behaviourism' in its widest sense makes use of statements about organ sensations as well as of statements about sense perceptions (in order to combine all physicalist statements about human behaviour). This widens the scope of our considerations, compared, for example, with those of Carnap, who so far has made behaviouristic use of 'statements about feelings (anger, etc.)' only as *statements*. The statement, 'I feel anger', is only vaguer than the statement, 'I see blue', but just as serviceable as a statement about reality.

Unified science based on physicalism recognizes only statements with spatio-temporal data. 'Equivalent statements' are constructed physicalistically; for statements are physicalist structures, written or spoken words. If, to a command, "Do this, if the table is red", the statement is added, "The table is red", something definite happens. The same happens if it were said, for

example, "Mensa est rubra". The two formulations would be equivalent physicalistically. On the other hand 'tautologies' do not add anything meaningful. "Two times two equals four" is always true. The addition of this condition to a command or statement does not make any difference, it is always fulfilled.

In the framework of physicalism, 'psychology' becomes a system of behaviourism in its widest sense. Also sociology has to be formulated in physicalist language as 'social behaviourism'. We can speak of men, things and their correlations, but not of 'norms in themselves', 'values' or 'essences'. There is only one kind of science; the separation into 'natural sciences' and 'mental (moral) sciences', which in any case plays a minor role outside Germany, is not elicited by any practical or theoretical considerations within the framework of unified science. The demand for this separation comes mostly from metaphysical groups.

Since there is no philosophy with meaningful statements, there is even less a 'philosophy of nature' or a 'philosophy of culture'; these are dichotomies that have their sources in theology and idealism. 'Physicalism' is perfectly monistic; idealistic philosophy, including phenomenology, is alien to it.

We find related endeavours in Berlin (Reichenbach, Dubislav, Grelling and others), Paris and Warsaw. In Berlin attention is given less to unified science, more to certain questions of the foundations of physics and mathematics in the narrower sense, while in Warsaw mainly logic and metalogic, as well as the foundations of mathematics, are treated. In Paris 'rationalism' is furthered in a similar sense by a circle of resolute anti-metaphysicians.

The standpoint of the Vienna Circle is set forth in essays in the journal *Erkenntnis*, in the publications of the 'Verein Ernst Mach – Wien', as well as in the *Schriften zur wissenschaftlichen Weltauffassung* [Monographs on the Scientific World-Conception], edited by Schlick and Frank, with works by Waismann, Carnap, Mises, Schlick, Frank. An 'Empirical Sociology', a behaviouristic sociology in the framework of physicalism, was published by the author of this article.

If one wants to speak of the further development of physicalism, it can probably be expected that the attempt that Carnap undertook in his *Logischer Aufbau der Welt* (1928) will be repeated in order to create the syntax for unified science in the sense of physicalism as represented here. The work on unified science replaces all former philosophy. At this point 'science without a world-view' confronts 'world-views' of all kinds, 'philosophies' of all kinds. Physicalism is the form work in unified science takes in our time. Whatever else is said in pronouncements is either 'meaningless' or merely a means to

emotion, 'poetry'. For physicalism as it is represented here quite strictly, everything that was put forward as philosophy by scholastics, Kantians, phenomenologists, is *meaningless* except that part of their formulations that can be translated into scientific, that is physicalist, statements.

CHAPTER 6

SOCIOLOGY IN THE FRAMEWORK OF PHYSICALISM

1. PHYSICALISM WITHOUT METAPHYSICS

The so-called 'Vienna Circle of the scientific world-conception' attempts to create an atmosphere free of metaphysics, along the lines of Mach, Poincaré, Frege, Russell, Wittgenstein and others in order to further scientific work in all fields by means of logical analysis.[1] It would be less misleading to speak of a 'Vienna Circle of *Physicalism*' because 'world' is not a term of scientific language, and because world-*conception* [Welt*auffassung*] is often taken to be interchangeable with world-*view* [Welt*anschauung*]. All the representatives of this Circle agree that there is no 'philosophy' existing side by side with the sciences as a discipline with its own special statements; all meaningful statements are contained in the sciences.

When the sciences are joined together into *unified science*, the work in them is the same as it previously was in their separation. Their uniform logical character has not always been sufficiently stressed. Unified science is the result of comprehensive *collective work* in the same way as the structures of chemistry, geology, biology or even mathematics and logic.

Unified science will be pursued as the separate sciences in it were formerly, and therefore, the 'thinker without a school' will not be more significant than he was in the former separate sciences. The individual can by sudden flashes of insight achieve here as much or as little as hitherto in any one science. Each proposed innovation must be so formulated that one can expect its general acknowledgment. Only through the cooperation of many others will its full impact become apparent. If it is wrong or meaningless – i.e. metaphysical – then of course it falls outside the sphere of unified science. Unified science, beside which there is no 'philosophy', no 'metaphysics', is not the work of individuals, but of a generation.

Some representatives of the 'Vienna Circle', who, like all other representatives of this group, declare explicitly that one cannot speak of special 'philosophical truths', nevertheless still occasionally use the term 'philosophy'. They want this term to signify 'philosophising', the 'activity of clarifying

[1] Translation of Neurath 1931d [ON 202].

concepts'. This concession to the traditional linguistic usage, though understandable for various reasons, easily gives rise to misunderstandings. In this paper the term will not be used. No new world-view is contrasted with an old one, nor is some old world-view replaced by clarification of concepts, but rather now 'science *without a world-view*' *confronts* all world-views. For the Vienna Circle the traditional structure of metaphysics and other related formations consist of meaningless statements except for scientific statements that are found among them 'by chance'. But the objection to the term 'philosophising' is not only terminological; it is impossible to separate the 'clarification of concepts' from the 'pursuit of science' to which it belongs. Both are inseparably bound up together.

The activities of unified science are closely interlinked, if for example, one thinks about the consequences, of new astronomical observation statements, or if one investigates which chemical laws can be applied to certain digestive processes, or if one examines to what extent the concepts of different branches of science can already be connected with each other as unified science demands. Namely in unified science it must be possible to connect each law with every other law under certain circumstances, in order to obtain new formulations.

Certainly different kinds of laws can be distinguished from each other: for example, chemical, biological or sociological laws; however, it can *not be said of* a *prediction of a concrete individual process that it depend on one definite kind of law only*. For example, whether a forest will burn down at a certain location on earth depends as much on the weather as on whether human intervention takes place or not. This intervention, however, can only be predicted if one knows the laws of human behaviour. *That is, under certain circumstances, it must be possible to connect all kinds of laws with each other*. Therefore all laws, whether chemical, climatological or sociological, must be conceived as *parts of a system*, namely of *unified science*.

To establish unified science (Kurt Lewin has pointed out that the expression was used by Franz Oppenheimer, though in a somewhat different manner) a *unified language* with a *unified syntax* is needed. The deficiencies of syntax at the time of preparation indicate the attitude then prevailing in individual movements and eras.

Wittgenstein and other advocates of a scientific world-conception, to whom we owe a great deal for their rejection of metaphysics, their exclusion of meaningless statements, are of the opinion that each individual, in order to reach science, first needs meaningless word sequences for 'elucidation' (Wittgenstein 1974, 6.54):

My propositions serve as elucidations in the following way: anyone who understands me eventually recognizes them as nonsensical, when he has used them – as steps – to climb up beyond them. (He must, so to speak, throw away the ladder after he has climbed up it.)

This statement seems to suggest that one had repeatedly to undergo some sort of purification from meaningless, i.e. metaphysical statements, that one had repeatedly to use that ladder as it were and throw it away. Only with the help of elucidations consisting of word sequences that are later recognized as meaningless could one reach a unified language. These elucidations, which may well be characterised as metaphysical, are not isolated occurrences in Wittgenstein's writings; there we find further expressions that resemble less the rungs of a ladder than a quietly formulated metaphysical side-doctrine. The end of the *Tractatus* – "What we cannot speak about we must pass over in silence" – is at least misleading in its wording; it sounds as if there were 'a something' of which one cannot speak. We should say: if one really wants to abstain fully from a metaphysical mood, "we must pass over in silence" but not 'about something'.

We do not need a metaphysical ladder of elucidation. On this point we cannot follow Wittgenstein, whose great significance for logic does not thereby diminish in merit. We owe him, among other things, the contrast between 'tautologies' and 'statements of events'. Logic and mathematics show us which linguistic transformations are possible *without adding to meaning*, however we may formulate this fact.

Logic and mathematics do not need any further observation statements for their elaboration. Logical and mathematical errors can be removed within their own sphere. It is no contradiction to this that experiential statements can bring corrections. Let us suppose a captain strikes a reef with his ship. All the rules of calculation have been correctly applied, the reef can be found in the geographical maps. In this way we could discover a mistake in the logarithmic tables, which was the cause of the accident; however, it can also be found without such experiences.

In Wittgenstein's 'elucidations', which have occasionally been characterised as 'mythological prolegomena', an attempt seems to be made in a pre-linguistic stage, so to speak, to make investigations of pre-linguistic conditions. These attempts have to be rejected not only because they are meaningless, but also because they are not necessary as a preparation for unified science. It is certainly possible to speak about one part of language with the help of another part; it is, however, not possible to make pronouncements about language as a whole from a 'not yet linguistic' standpoint, as Wittgenstein and

some individual representatives of the Vienna Circle seek to do. A part of these attempts may perhaps, after suitable transformation, find a place within the sphere of science; while another part would have to be dropped.

It is also impossible to confront language as a whole with 'experiences' or with the 'world' or with something 'given'. Every statement of the kind: "The possibility of science rests on an orderly constitution of the world", is therefore meaningless. Such statements cannot be saved by counting them among the 'elucidations' for which a somehow less strict standpoint is assumed. Such an attempt is hardly different from metaphysics in the accepted sense. The possibility of science becomes apparent in science itself. We enlarge its domain by augmenting the *mass of statements*, by comparing new statements with statements taken over from the past, thus creating a consistent system of unified science that can be used for successful *predictions*. As makers of statements, we cannot, so to speak, take up a position outside the making of statements and then be prosecutor, defendant and judge at the same time.

This standpoint, that science remains in the domain of statements, that statements are the beginning and end of science, is sometimes admitted if by the metaphysical side itself, with the addition, however, that besides science there is a domain that contains something like quasi-statements. In contradistinction to the frequent interlocking of science and metaphysics, this separation of science and metaphysics – though without eliminating metaphysics – is carried out by Reininger (1931); his standpoint is akin to that of the Vienna Circle also with respect to behaviorism, as far as science is concerned.

Unified science formulates statements, changes them, makes predictions; however, it cannot itself anticipate its future condition. Alongside the present system of statements there is no further *'true' system of statements*. To speak of such, even as a conceptual boundary, does not make any sense. *We can only state that we operate today with the spatio-temporal system suitable for physics*, and that we obtain successful predictions in that way. This system of statements is that of unified science – that is the standpoint that we can call *physicalism* (see Otto Neurath 1931c, p. 2). If this term should be adopted, it might be advisable to speak of 'physicalist' when we give a spatio-temporal description in the sense of contemporary physics, for example, a behaviorist description. The term 'physical' would then be reserved for the 'statements of physics in the narrower sense', those of mechanics, electrodynamics, etc.

The unified science as physicalism, which is characteristic of a definite historical period, avoids all meaningless sentences and proceeds from statement

to statement; these are combined in a consistent system as tools for successful prediction, that is for life.

2. UNIFIED LANGUAGE OF PHYSICALISM

Unified science contains all scientific *laws*; these can be connected without exception. Laws are not statements; they are directions for obtaining *predictions* from observation statements (Schlick).

Unified science expresses everything in the unified language that is common to the blind and the sighted, the deaf and those who hear, it is 'intersensual' and 'intersubjective'. It connects the statements of a man talking to himself today with his statements of yesterday; the statements he makes with his ears closed, with those he makes with his ear open. In language nothing but order is essential, and that is already represented by a sequence of signs of Morse code. 'Intersubjective' and 'intersensual' language in general depends on *order* ('next to', 'between', etc.) that is, on what can be expressed by sign sequences in logic and mathematics. All predictions are formulated in this language.

This unified language of unified science, which by and large can be derived from everyday language by certain alterations, is the language of physics. Here it does not matter for the *uniformity* of the physicalist language which language the physics of a certain period uses, whether it explicitly uses a four-dimensional continuum in its more precise expressions, whether it utilizes a spatio-temporal order in which the position of all occurrences is always *exactly* defined, or whether the basic elements are coupled positions and velocities for which *precision* is *limited in principle*; what matters is that the concepts of unified science always share the fate of the fundamental concepts of physics, wherever use is made of utmost precision as well as where nothing but a rough description is attempted. With this common fate the standpoint of physicalism becomes apparent. But all predictions by whose confirmation we measure science, can always, in the last resort, be based on observation statements, on statements in which perceiving persons and stimulus-producing things occur.

The claim that the more or less complicated relationships which thus result are less clear if they must lack the greatest precision in accord with modern physics than if they introduce hypothetical electron paths, probably originates in certain old habits (see Philipp Frank 1931).

We meet the unified language of physicalism wherever we make a scientific prediction on the basis of laws. If someone says that he will hear a certain

sound at the same time that he sees a certain colour, or the other way round, or when he speaks of the 'red spot', which under certain circumstances, will appear at the side of the 'blue spot', he is already moving within the sphere of physicalism. He himself as perceiving subject is a physical entity, he must localise the perception, for example, in the central nervous system, and he must formulate everything he says about spots as statements about these processes in the central nervous system or at some other place. Only in that way can he make predictions and come to an understanding with others, and with himself at another point in time. Each definition of time is already a physical formulation.

Science tries to transform everyday statements. They are given to us as 'bundles' consisting of physicalist and pre-physicalist components. We replace them by the 'unifications' of physicalist language. If one says, for example, "The screeching saw is cutting the blue wooden cube", then 'cube' is obviously an 'intersensual' and 'intersubjective' concept that can be used equally for the blind and the deaf. If a man soliloquizes and makes predictions that he can check himself, then he can compare what he, as a seeing person, said about the cube with what he reports in the dark using his sense of touch.

How to incorporate the word 'blue' into the unified language is at first doubtful, however. One can use it in the sense of the number of oscillations of electromagnetic waves. But one can also use it in the sense of a 'field statement' as follows: If a sighted man (defined in a certain way) enters the area of this cube as an experimental test body, then he behaves in a certain physicalistically describable way; he says, for example; I see 'blue'. While for the case of 'blue' it may be doubtful what people mean by it in everyday language, 'screeching' will be meant predominantly as a 'field statement', i.e. the hearing subject will always be taken into consideration together with it; however, closer examination shows that 'cube', 'blue' and 'screeching' are of one sort.

Let us try to explain the above statement according to our analysis along the lines of physicalism, and to reproduce it in another way so that it may serve for predictions:

"Here is a blue cube." (This wording, like the following, can be replaced by a physical formula in which place is defined by coordinates.)

"Here is a screeching saw." (The screeching would at first enter the formulation only as vibrations of saw and air; this can be expressed in physical formulas.)

"Here is a perceiving man." (Perhaps a 'field statement' could be added

to indicate that under certain conditions the perceiving man enters into relation with the physical blue and the physical screeching.)

This perceiving can be subdivided, for example, into:

"Nervous changes take place here."

"Brain changes take place in the sphere of perception, perhaps also in the sphere of speech." (It does not matter here for our consideration whether these spheres can be localized or must be described structurally. Whether changes in the area of speech – the 'speech-thought' of the behaviourists – is connected with changes in the larynx or in its innervation, may also be left aside.)

Perhaps some additions are needed in order fully to extract the physicalist sense of this simple statement for example, clock data, place coordinates; but the main thing at any rate is that we have to consider only statements with physical concepts.

It would be a mistake to believe that the physicalist rendering of everyday affairs must be complicated because very complicated physical formulas are needed – some of which are not yet at hand – for the *calculation* of certain correlations. The physicalist everyday language comes from prevailing everyday language: only certain parts are dropped, others correlated in a different manner, and certain additions are made. From the start the perceiving subject will be more closely linked with the perception statement and the object determination than was done before. The distinguishing of certain groups of statements, for example, the perception statements, will be made differently than hitherto.

Children can learn the physicalist everyday language. They can advance to the strict symbolic language of science, can learn to make predictions of all kinds with success, without even having to resort to 'elucidations' that supposedly function as a meaningless introduction. The set purpose is a clean manner of speech in which, for instance, the highly confusing term 'sense deception' does not occur. Though at some later time the physicalist language may be learned as the general language of communication, at present our task is still to free the 'bundles' of our language from metaphysical trappings and to define everything that occurs in them physicalistically. When the metaphysical tie has gone, many things may lie before us as an unconnected heap. The further use of such a residue will not be precisely fruitful, and a new construction will be unavoidable.

Frequently we can go on using existing '*bundles*' after re-interpretation. But this should be done with caution, because people who are prepared to adapt themselves but are lazy, often take comfort from the fact that so much

can be re-interpreted 'in principle'. It is more than questionable whether, for example, it is expedient to go on using words like 'instinct', 'motive', 'memory', 'world' etc., and to adopt a quite unusual interpretation for them that is easily forgotten if we go on using these words to keep the peace. In many cases a new shaping of the language is certainly superfluous, indeed dangerous; and as long as we express ourselves only 'approximately', we have to beware of wanting to be all too subtle at the same time.

As the views of this paper are above all close to those that Carnap has expressed, it might be stressed that the special *'phenomenal language'* from which Carnap tries to derive the physical one, is discarded here. The exclusion of 'phenomenal language' in its present form, which does not seem to be even suitable for 'predictions', that is, for what is essential for science, will probably necessitate a number of alterations in the *constitutional* [*constitutive*] *system*. Together with this the 'methodological solipsism' (Carnap, Driesch) will probably also disappear; it can probably be understood as a weakened residue of idealist metaphysics from which Carnap in particular always tries to keep clear. The thesis of 'methodological solipsism' – as even Carnap would probably admit – cannot be formulated scientifically; it cannot even be used any longer to give an idea of a certain attitude in contrast to another attitude, because there is only *one* physicalism. It contains everything that can be formulated scientifically.

The 'ego', the 'thinking subject' can be separated as little as anything else from the 'experienced', or from 'experiencing', or the 'thinking'. The statements of physicalism rest on statements, and they are connected with seeing, hearing, touching and other 'sensations' (as physical occurrences) but also with 'organ sensations' that are mostly only roughly discerned. We can, of course, close our eyes, but we cannot switch off processes of muscle enervation, digestion, blood circulation. What we try to detach as 'ego' includes, in the language of physicalism, these processes about which we are not informed through the usual 'external' senses. All 'personality coefficients' that distinguish one individual from another are of a physicalist nature!

Though we cannot confront the 'ego' with the 'world' nor with 'thinking', we still can differentiate within physicalism between statements concerning the 'physicalistically described cube' and statements concerning the 'physicalistically described person', and then can, under certain circumstances, extract 'observation statements' and thereby create a substitute for 'phenomenal language'; but careful investigation will probably show that the bulk of *observation statements are part of the bulk of physical statements*.

A distinction will certainly be made between the protocol statements (that

turn up as physical formations) made by an astronomer, or a chronicler, and the statements that have a precisely defined place within a physical system, though obviously there are some overlapping transitions. But no special 'phenomenal language' confronts the 'physicalist' one. *From the very beginning each of our statements can be physicalist – in this respect what is said here differs from everything that has been said by the 'Vienna Circle'* which otherwise stresses again and again the significance of *predictions* and their confirmation. Unified language is the language of predictions which are the centre of physicalism.

In a certain sense the view advocated here starts from a given state of everyday language, which in the beginning is essentially physicalist and only gradually becomes intermixed with metaphysics. Here is a point of contact with the 'natural concept of the world' of Avenarius. The language of physicalism is nothing new as it were; it is the language familiar to certain 'naive' children and peoples.

Science is at times discussed as a system of statements. *Statements are compared with statements*, not with 'experiences', not with a 'world' nor with anything else. All these meaningless *duplications* belong to a more or less refined metaphysics and are therefore to be rejected. Each new statement is confronted with the totality of existing statements that have already been harmonised with each other. *A statement is called correct if it can be incorporated* in this totality. What cannot be incorporated is rejected as incorrect. Instead of rejecting the new statement, one can alter the whole existing system of statements until the new statement can be incorporated; in general, however, this decision is taken with hesitation. *Within* unified science there are important tasks of transformation. The definition of 'correct' and 'incorrect' as proposed here abandons the definition that is usually accepted in the Vienna Circle and recurs to 'meaning' and 'verification'. In the present presentation one always remains in the sphere of speech-thinking. Systems of statements are transformed. However, generalising statements, as well as statements by which certain relations are elaborated, can be confronted with the totality of protocol statements.

In the framework of unified science then, there are all sorts of classifications of statements. For example, we decide whether certain statements are 'statements of reality', 'hallucination-statements', or 'lies' according to the degree to which the statements can be used to draw conclusions concerning physical events beyond the movements of the mouth. We have a 'lie' if a conclusion can be drawn that there was a certain stimulation in the speech centre but no corresponding occurrences in the perception centres; however,

these are essential in the case of hallucinations. If we can conclude that there were not only stimulations in the perception centres but also events outside the body of a clearly definable kind, then we have a 'reality statement'; in this case, for example, we can use the statement, 'a cat is sitting in this room' spoken by someone, as a physicalist statement. A statement is always compared with another statement or with the system of statements, and not with a 'reality'. Such an endeavour would be metaphysics, would be meaningless. However, 'the' reality is not replaced by 'the' system of physicalism, but by groups of such systems, of which actual practice uses one.

From all this it becomes clear that within a consistent physicalism there can be no 'theory of knowledge', at least not in the traditional form. It could only consist of defence actions against metaphysics, i.e. unmasking meaningless terms. Some problems of the theory of knowledge will perhaps be transformable into empirical questions so that they can find a place within unified science.

This cannot be analysed any further here, nor can the question, how all 'statements' can be incorporated into physicalism as physicalist formations. 'Two statements are equivalent', could perhaps be expressed physicalistically in the following way. We expose a man to a system of commands that are connected with all sorts of statements. For example: "If A behaves in such and such a way, do this." We can now fix certain conditions and then observe that the *addition of a certain statement* causes the same change of response as that of another statement. We will then say the first statement is *equivalent* to the second. By the addition of tautologies the stimulus of the system of commands remains *unchanged*.

All this could be developed experimentally with the help of a 'thinking machine' as suggested by Jevons. Syntax would be expressed by means of the construction of the machine, and through its use, logical mistakes would be avoided automatically. The machine would not be able to write the sentence: "two times red is hard."

The arguments of this paper link up best with a *behaviourist* basic attitude. We speak not of 'thinking' but straight away of 'speech-thinking', that is, of *statements as physical events*. Whether a perception statement concerning something past (e.g. "I heard a melody a while ago") can be traced back to a past speech-thought, or whether past stimuli bring about a response in 'speech-thinking' only now, is fundamentally without significance here. All too often the discussion proceeds as if, through the refutation of any individual claims made by behaviourists, the principle were somehow affected that only *physicalist statements* have a meaning, that is, can become part of unified science.

With statements we begin, with statements we end everything. There are no 'elucidations' that would not be physicalist statements. If 'elucidations' are understood as calls, then like whistles or caresses, they cannot be logically analysed. It is the physicalist language, *unified language*, that all science is about: no 'phenomenal language' beside the 'physical language'; no 'methodical solipsism' beside another possible standpoint; no 'philosophy'; no 'theory of knowledge'; no 'new world view' beside other world views; *only unified science* with its laws and predictions.

3. SOCIOLOGY NOT A MENTAL SCIENCE

Unified science makes predictions about the behaviour of machines as well as that of animals; about the behaviour of stones as well as of plants. Sometimes it makes complex statements that we can already dissect today, sometimes it makes statements in whose dissection we are not yet successful. There are 'laws' of the behaviour of animals, of machines. The 'laws' of machines can be reduced to physical laws. But even in this field a law concerning mass and rods is often sufficient and a reduction to a law concerning atoms or other elements unnecessary; also the laws of the animal body can often be formulated without resorting to the laws of the microstructure. Often, however, where one expected much from the study of the macro-laws, they did not suffice: certain irregularities defied calculation.

We are always searching for *correlations* between magnitudes that occur in the physicalist description of events. It makes no difference in principle whether the descriptions are *statistical* or *non-statistical*. Whether the statistical behaviour of atoms or of plants or of animals is being investigated, the methods of stating a correlation are always the same. As we saw above, all laws of unified science must be capable of being linked with each other if they are to fulfil the task of predicting as often as possible individual events or groups of events.

Thereby each basic decomposition of unified science is eliminated from the start, for example, that into 'natural sciences' and 'mental sciences' which are also called 'cultural sciences' or some other name. The tenets with which we want to justify the division are different, but always of a metaphysical kind, that is, meaningless. It does not make sense to speak of different 'essences' that rest 'behind' the events. What cannot be expressed by the relations between the elements cannot be expressed at all. Therefore it is meaningless *to speak of the 'essence of things' beyond their correlations*. We will no longer talk of 'different causalities' once we realise what the real

significance of the unified language of science means. We can only compare the order of one sphere with its laws with the order of another and perhaps state that the laws of one sphere are more complicated than the laws of another, that certain arrangements that are missing in one sphere occur in another, and that, for example, certain mathematical formulas are required in one case but not in another.

We cannot make a division between 'natural sciences' and 'mental sciences'; even less can we make a division between 'natural philosophy' and a 'philosophy of mental sciences'. Even if we overlook the fact that the term 'natural philosophy' is inexpedient for the reasons mentioned above because it contains 'philosophy', we still can interpret natural philosophy as a kind of introduction to the whole work of unified science; for how should one make a division between 'nature' and 'non-nature'?

Not even the practical needs of everyday or of the operation of science can be cited to justify this division. Should the theory of human behaviour be seriously opposed to the behaviour of all other things? Should we really want to place the theory of human societies in one discipline, the theory of animal societies in another? Should the 'breeding', 'slavery', 'war' of ants be treated in natural science, and the 'breeding', 'slavery', 'war' of men in the mental sciences? Or the division is not stronger than that between the 'spheres of natural science' in the old sense.

Or is there anything to be said perhaps in favour of speaking just of 'mental sciences' each time that one means 'social sciences'? To be consistent the theory of animal societies, together with the theory of human societies, would then have to be numbered among social sciences, that is, among 'mental sciences', a consequence from which most people will probably shrink back. Understandably so; for what would become of the cleavage that lurks behind all this, the cleavage that rests on centuries-old theological habits: to place everything there is in several, or at least two spheres, one of them, for example, being 'noble', the other 'not noble'. The dualism 'natural sciences'-'mental sciences', the dualism 'natural philosophy'-'cultural philosophy' is, in the last resort, a *residue of theology*.

The ancient languages are, on the whole, more physicalist than the modern. Though they are full of magical elements, they above all treat 'body' and 'soul' as two kinds of body; the soul is a shadowy little body that escapes from the mouth of the dying. Only theology changes this; no longer does it confront 'mental body' and 'corporeal body', but 'non-corporeal soul' and 'corporeal body', as well as 'non-corporeal God' and 'corporeal world' . . . with all sorts of sub- and supergroups of things earthly and unearthly. The contrast

between 'earthly' and 'unearthly' can only be determined by meaningless expressions. These expressions, being meaningless, do not come into conflict with the statements of unified science, nor do they conform to them; however, they create much confusion. The claim that these expressions are as meaningful as those of science, pronounces open conflict (see Hahn, *Ueberflüssige Wesenheiten*, 1930b).

How great a part the habit of theological dualism plays in the creation of such separations can perhaps be concluded from the fact that as soon as one dualistic separation has been abandoned, another finds easy acceptance. The contrast of 'to be' and 'ought to be', which we find especially in the writings of philosophers of law, belongs here. It may have its origin in part in the old theological contradistinction of 'ideal' and 'reality'. The possibility of forming nouns in our language eases all these meaningless undertakings. Without coming into conflict with syntax [of German] we are allowed to say *'das Sollen'* (the ought), as we can say *'das Schwert'* (the sword). And now we make statements about this 'ought' as elsewhere about a 'sword' or at least about 'being'.

The 'mental sciences', the world of the 'soul', the world of the 'categorical imperative', the sphere of 'empathy', the sphere of 'understanding' – these are spheres of terms that more or less merge into each other, that frequently can replace each other. Some authors prefer the one, some the other group of meaningless terms, some mix and accumulate the meaningless terms. Whereas with many they appear only as marginal decoration of science, with others they influence the whole sphere of statements. Also if we do not overestimate the practical effect of the tenets on which the conception of 'mental sciences' rests and do not put too high a value on the confusion caused by it in empirical research – *the slate must be wiped clean of them when a systematic foundation of physicalism and sociology is laid, if clarity is the aim.* To take a resolute stand on such separations is one task of the advocates of unified science; it cannot be left to the arbitrary actions of scientists in the various disciplines.

That even thinkers of an essentially antimetaphysical bent are uncertain regarding these questions may be partly connected with the fact that there is no sufficient clarity about the subject and problem of 'psychology'. The separation of the disciplines of 'mental science' from others agrees in much with the separation of matters of the 'soul' from other subjects. This separation is in principle overcome only by *behaviourism*; we advocate this word here, always in its widest sense. It admits only physicalist statements about human behaviour into its system. When the sociologist makes predictions

about human groups, as the behaviourist does about human individuals or animal individuals, then, to use a commensurate term, he is practising *social behaviourism*.

That is: *sociology is not a 'mental science'* [*Geisteswissenschaft*] nor a 'science of mind' [*Geistwissenschaft*] (Sombart), which is in some fundamental contrast to some other sciences, the natural sciences, but *as social behaviourism sociology is part of unified science.*

4. SOCIOLOGY AS SOCIAL BEHAVIOURISM

We can speak about men's painting, building of houses, cults, agriculture, poetry in the same way. And yet, it is claimed again and again that it is something fundamentally different whether we 'comprehend' the other person or 'only' observe him from outside and state regularities. The sphere of 'comprehension', of 'empathy' with other personalities is close to the traditional sphere of mental sciences. We find here a revival of a separation that had already been overcome at an earlier stage: the separation into 'internal' and 'external' perception (experience, meaning, etc.) that are of the same empirical character.

The literature of philosophy, especially of philosophy of history, often stresses that it is impossible to deal with history without 'empathy', 'comprehension', to group and describe human actions with some conciseness.

How can we roughly try to remove these difficulties from the standpoint of physicalism? From the start we have to assume that the persistent assertions of many sociologists and philosophers of history that the appeal to comprehension is unavoidable, also try to protect very remarkable scientific experiences. There may be, as so often, a complex, not easily disentangled, of dualistic habits of theological origin and of scientific practice. Everyone who is familiar with the monism of unified science will see in all this that certain statements are put forward unphysicalistically by mistake and can perfectly well be formulated physicalistically.

Statements like: "I see a blue table in this room" and "I feel anger" are not far apart. It is expedient to replace the 'I' by a personal name; as all statements are equivalent, it must be possible that an 'I'-statement can be made by someone else. Then there are side by side: "A blue table is in this room" and "Anger is in this man." The discussions about 'primary' and 'secondary' qualities come to an end, because in the last resort all quality statements are of one kind from which only the tautologies can be separated. All quality statements become physicalist statements. Apart from these are

the tautologies as specially formed links between statements. The statements of geometry can be interpreted as tautologies, but also as physical statements. Thus many difficulties disappear.

What else is characteristic of the sentence: "In this man is anger"? It can hardly be analysed. It is as if someone could tell us: "Here is a heavy thunderstorm", without being able to indicate how it is composed of lightning, thunder, rain, etc., and whether he has acquired his observations with the help of his eyes, ears, or nose.

If we speak about anger, we use the '*organ sensations*'. The changes of the intestine, of internal secretion, of the blood pressure, of muscle contraction are fundamentally equal to changes in the eye, ear, or nose. If we elaborate behaviourism systematically, a man's statement: "I am angry", enters into physicalism not only as a reaction of the speaker but also as a formulation of the 'organ sensations'. In the same way that we can make physicalist statements about changes in the retina and 'other' events from the formulations of 'colour sensations', we can derive physicalist statements about 'changes in the intestine', 'changes in blood pressure', etc. from the formulations about anger, that is, organ sensations, which are often the only way that such statements become known to another man. This may be added to Carnap's statements about this subject in which no reference was made to this evaluation of statements about 'organ sensations' (using the other terminology).

No objection is to be made against someone who says that he needs these experiences of 'organ sensations' in order to feel empathy with another man. That is, we use physicalist statements about our own body in order to make physicalist statements about another's body; this is perfectly in line with our scientific work, which 'extrapolates' like that all the time. The decision regarding induction leads us again and again to such expansions. The situation is not different if we make statements about the back of the moon based on experiences of its front. That is: we can speak of 'empathy' in the physicalist language if thereby we mean nothing but drawing conclusions of physical events in other persons with the help of constructions that we have made about our own organic changes. What takes place here is a physicalist induction as in so many other cases, but as elsewhere a determination of certain correlations; however, the clarity of language with respect to many events still leaves very much to be desired here. If someone would say that the mental sciences are predominantly those sciences in which correlations are made between events which are very inadequately described for which one has only complex names, he would get rather close to the actual state of affairs.

If we analyse the concepts of 'comprehension', of 'empathy', carefully, we find that everything in them, which can be used physicalistically, is a statement about order, exactly as in all sciences. The alleged difference between 'natural sciences' and 'mental sciences' – that the first are 'only' about order, the second *also* about comprehending – does not exist.

If we systematically formulate everything that we find in non-metaphysical formulations, we get nothing but physicalist statements. There is no longer a special sphere of the 'soul'. For the standpoint advocated here it does not matter whether certain individual tenets of Watson, Pavlov or others are maintained or not. What matters is that only physicalistically formulated correlations are used in the description of living beings, whatever is observed in these beings.

It would be misleading to phrase this: there is no longer any difference between matters 'of the soul' and 'of the body', something 'neutral' has taken their place. What is stated is not at all about a 'something' but about correlations of a physicalist kind. Only inadequate analysis can lead to saying, for example, that today we cannot yet see far enough whether really the whole sphere of the 'soul' can be expressed physicalistically; it might well be possible that here or there some other kind of formulation might be contemplated, that is, concepts that cannot be denied physicalistically. This is the last residue of a belief in a 'soul' as a separate entity. When people have observed the moving pendulum of the clock and now see how it stops, they can, in the noun-forming language and without difficulty, raise the problem where 'the movement' now has gone to. And if we explain to them that by analysing the connections between the components of the clock and the environment everything could be found out that there is to know, an unbeliever will perhaps still say that though he could understand that the notion of 'the movement' is metaphysics, he still had doubts about whether physicalism is adequate to deal with certain more complicated problems of clock movements.

Without wanting to say that each sociologist must be trained in behaviourism, we can still demand that each sociologist who wants to avoid mistakes must be careful always to describe human behaviour quite plainly in physicalist terms. He should therefore not speak of the 'spirit of an age' unless it is quite clear that he refers to certain word connections, cults, architectural forms, sounds, styles of pictures, etc. It is perfectly legitimate that he undertakes to predict the behaviour of people of other ages by variation of his own well-known behaviour, though it can sometimes be misleading. 'Empathy', however, should not be expected to have some peculiar magical power beyond ordinary induction.

Inductions in this or in another field are always a matter of *decision*. This may be characteristic of certain human groups or whole ages, but cannot be derived logically. Within the physicalist sphere, induction always leads to meaningful statements. It must not therefore be confused with the insertion of metaphysical constructions. Some people admit that they perform metaphysical constructions, that is, insert meaningless word combinations, but refuse to see how damaging this is. We have to advocate the exclusion of such constructions from the field of sociology and psychology as well as other fields, not only to get rid of superfluous matter and to avoid meaningless word combinations though they may give pleasure to some people. The exclusion of the metaphysical becomes scientifically fertile by the avoidance of inducing certain false correlations in the empirical sphere. The example of sociology will show that for the most part the significance of certain physicalistically formulated elements is overestimated if we believe them to be linked with certain metaphysical entities. From the priest of the transcendental God too, certain empirically controllable super-achievements are expected which cannot be derived from empirical experiences.

Some people say in favour of metaphysical constructions that, with their help, better predictions can be made. We start from physicalistically formulated observation statements, then move to the field of metaphysical word sequences, and on the basis of certain rules that are applied to meaningless word sequences in the metaphysical field we arrive at predictions which are in harmony with a system of protocol statements. *If* we really get results in this way, metaphysics is in this case not essential for prediction but is perhaps a stimulus like some narcotics. For if predictions can be made in this roundabout way, then they can be derived "*directly from the data mentioned*." This is a clear point of logic: if Y follows from X, and Z from Y, then Z follows directly from X" (Otto Neurath 1973b, p. 357). If Kepler made use of the world of theological notions in deducing the planetary orbits, this world of theological notions nevertheless does not enter into the scientific statements. It may be similar with certain metaphysical components of the highly productive fields of psychoanalysis and individual psychology whose behaviourist transformation will certainly be no easy task.

After the characterisation of metaphysical deviations from the general line of behaviourism, the way has been opened for a *sociology free from metaphysics*. Just as we can treat the behaviour of animals side by side with the behaviour of machines, of stars, of stones, so we can also treat the behaviour of groups of animals. In principle we can take into account changes of individuals through 'external' stimuli as well as changes that can be traced

back to 'autonomous internal changes' of the living beings (for example, rhythmic sequences in a process), just as we can, for example, investigate the disintegration of radium, which cannot be influenced in any way, side by side with the decomposition of a chemical compound when oxygen is added. It may remain undecided whether analogies to radium decay play a part within the human body.

Sociology does not investigate purely statistical changes of animal, or chiefly human, groups; it takes an interest in *connections among stimuli* that take place between individuals. Without analysing these connections in detail it can sometimes under certain circumstances make statements about the overall behaviour of groups linked by stimuli, find laws and with their help make predictions. How can we develop 'social behaviourism' free from metaphysics? *In the same way as any other objective science.* Naturally certain correlations result that cannot be found with individuals, with stars or machines. Social behaviourism produces laws of a definite kind peculiar to it.

The development of physicalist sociology does not mean the transfer of laws of physics to living things and their groups, as some have thought possible. Comprehensive sociological laws can be found, as well as laws for definite narrower social areas, without the need to be able to go back to the microstructure, and thereby to build up these sociological laws from physical ones. The sociological laws found without the help of physical laws in the narrower sense must not necessarily be changed by the addition of a physical substructure discovered later. The sociologist is completely unimpeded in his search for laws; only in his predictions must he always speak of entities in space and time.

5. SOCIOLOGICAL CORRELATIONS

In sociology, as well as in other sciences, it is impossible to indicate from the start on the basis of theoretical considerations which correlations can be used with some prospect of success. It can be shown, however, that certain traditional endeavours are regularly unsuccessful, whereas other successful methods to discover correlations are not sufficiently cultivated.

Of which kind are sociological correlations? How do we arrive at sociological predictions with some certainty? For the prediction of the behaviour of a group in some respect it is often necessary to know the whole life of the group. The individual ways of behaviour that can be lifted out of the totality of events, the construction of machines, the building of temples, the rules of

marriage are, in their changes, not 'autonomously' computable; they have to be regarded as parts of the complex that is being investigated at the time. In order to know how the building of temples will change, we have to be acquainted with the manner of production, the social order, the kinds of religious behaviour at the initial moment in time, we have to know to which modifications *all this together* will be subjected.

In such predictions not all events appear equally difficult to approach. If we know certain conditions we can often roughly deduce from the mode of production of one age, the mode of production and social order of the succeeding periods; with the help of such predictions we can then try to make further predictions about religious behaviour and similar phenomena with some success. The opposite procedure, however – namely first the prediction of religious behaviour alone, and from that the deduction of the prediction of the mode of production – does not succeed, as experience shows.

But, whether we direct our attention to the mode of production, religious behaviour, architecture, music, we are always dealing with events that can be described within physicalism.

Many social institutions of an age can be deduced only if we know their distant past, whereas others, considered remote, could, so to speak, be invented at any time. Guns, as stimulus of a certain kind, trigger the response of armoured turrets by their presence, whereas the swallow-tailed coat of our time is not a response to dancing and would hardly be invented anew. However, we can understand that formerly a man on horseback with a long-tailed coat turned the tails up and thus invented the original swallow-tailed coat. The coherence of institutions in these two cases is quite different.

One must not only be familiar with the kind of coherence in order to be able to make predictions, one must also know whether it is easy or difficult to extract a certain institution from a social structure and whether, in case of loss, it will be replaced. The state, for example, is a rather stable structure whose functioning is highly independent of the change of personnel; if many judges or soldiers die, new ones take their place. However, if we take wheels off a machine, it will, in general not replace them.

It is a perfectly physicalist question to what extent the existence of individuals of a special kind, different from the average, assure the continuance of a state structure. Here another question, of the degree to which such significant individuals can always be replaced, is to be considered separately. The queen bee holds a special position in the hive; but when a queen is lost there is the possibility for a new queen to develop. There are always latent queens so to speak. How do matters stand with human beings?

It is a wholly concrete sociological question to ask to what extent predictions about social structures can be made without heeding the fate of certain especially prominent individuals. For example, a claim can probably be made with good reason that the creation of bourgeois Europe could be predicted by the end of the eighteenth century as soon as the modern capitalist transformation had received its special complexion through the machine system, whereas, for example, Napoleon's march into Russia and the burning of Moscow were hardly predictable. But it may perhaps make good sense to say that if Napoleon had been victorious against Russia, the transformation of the social order would have been very similar to the one that actually took place. Had he been victorious, Napoleon, who had reinstated the Catholic Church, would probably have been obliged to favour the old feudalism in Central Europe to a certain extent for some time.

The question to what extent predictions can be made, whether they are combined with predictions about individuals or not, does not affect the character of special behaviourism in any way. The course of a leaf in the wind cannot be predicted either, though kinematics, climatology, meteorology are quite well-developed sciences. It is not an intrinsic property of a developed science that it should be able to predict any individual event. It is of little concern to scientific research that occasionally the fate of an individual leaf interests us greatly, for example, if it were a thousand-mark note that is carried away by the wind. It may remain undecided whether a theory of the pathways of leaves could gradually be created by a chronicle of 'random' leaf pathways in the wind. A great part of the trains of thought, which are linked to Rickert and related thinkers, produce no scientific laws, even where they can be physicalistically interpreted.

Sociology, like any genuine science, casts about for correlations that can be used for *predictions*. It tries to establish its basic structures as unambiguously and clearly as possible. For example, we could make the attempt to define groups as '*commercium*' and '*conubium*': one finds out who deals with whom, who marries whom. The result will be clearly distinguishable places of accumulation with thinly occupied fringes. We then could investigate under which circumstances such accumulations change or even disappear. The discovery of the correlation between the places of accumulation and the production process of the time is obviously a legitimate sociological task that could be significant for the theory of 'classes'.

We can investigate, for example, under which conditions there is matriarchal right, ancestor worship, agriculture and other phenomena, when the founding of cities begins, which correlations exist between systematic

theology and the rest of human life. We can also ask how the dispensation of justice is qualified by social conditions, though it is doubtful whether such delimitation will produce enough material for finding laws. It could be, for example, that certain events happening outside justice have to be added to those of the dispensation of justice if lawful connections are to be discovered.

What is acknowledged by one group as legal right, may be considered by another as being outside the legal order. We can therefore only establish correlations between statements of men about 'law' or between their behaviour and their statements. But without special preparation it is not possible to correlate 'legal events' as such with other events.

We could ask whether simple sociological correlations could be found between the legal charging of interest on the one hand and the living standard of the time on the other, or whether simpler relations would not turn up if the 'legal charging of interest' and the 'illegal usury' were joined together. Equally, ways of behaviour that are judged in 'jurisprudence' or in 'ethics' could be sociologically incorporated, together with the judgements. These are proper sociological sub-disciplines, but not the commonly pursued 'ethics' and 'jurisprudence'. These disciplines probably produce no sociological correlations or few of them. They are predominantly composed of metaphysics, or if they are free from metaphysics, their approach and grouping of statements can only be explained as theological residues. In part they present purely logical deductions, the derivation of certain commands from other commands or of certain consequences from certain legal premises. But that is outside the sphere of well-ordered correlations.

6. ETHICS AND JURISPRUDENCE AS METAPHYSICAL RESIDUES

Originally 'ethics' was the discipline that tries to establish the totality of divine commands in order to find out, by means of logical combinations of commandments and prohibitions of a more general kind, whether a certain individual action is demanded, allowed or forbidden. The 'casuistry' of Catholic moral theologians has greatly developed this kind of deduction. It is obvious that the indefiniteness of divine commands, the indefiniteness of their interpretation, did not allow a proper scientific approach to develop. The great profusion of logical conclusions was, so to speak, wasted on an inappropriate object though historically it prepared the way for the coming logicising period of science. If we define the commanding God physicalistically as well as the consequences in heaven and hell, which were placed by

some theologians in the centre of the earth, we have to deal with a discipline that is, though unmetaphysical, still highly *uncritical*.

But how should we demarcate a discipline as 'ethics' if God is eliminated? Can we make a meaningful transition to a 'command in itself', to the 'categorical imperative'? We could just as well introduce a 'neighbour-in-himself without a neighbour', a 'son-in-himself who has never had father or mother'.

How should certain commands or ways of behaviour be defined to make a 'new ethics within the framework of physicalism' possible? It seems to be impossible. Men can take common resolutions to behave somehow, and the consequences of such action can be investigated. But which ways of behaviour, which directions, should be distinguished as 'ethical' in order to establish correlations?

The continued use of an old name is based on the opinion that we can find something permanent that is common to the old theological or metaphysical and the new empiricist discipline. If all metaphysical elements are removed from ethics, as well as all theological physicalisms, then what remains is only either statements about certain ways of behaviour of men, or their commands to other men.

However, we could also think of a discipline, which, within the framework of unified science investigates in a perfectly behaviourist manner which responses are stimulated by a certain order of life, whether men become happier or less happy by certain orders of life. A perfectly empirical 'felicitology' could be devised on a behaviourist basis; it could take the place of traditional ethics.

But in an ethics free of metaphysics one tries for the most part to analyse the 'motivations' of people as if these were a suitable basis for laws of interrelations. What people say about the 'reasons' for their actions, however, is much more accidental than their average behaviour is otherwise. If we know what the general social conditions at one time are, we can much more easily predict the behaviour of whole groups than by the arguments that the individuals will produce for their actions. The same kinds of action will be supported in very different ways, and moreover, few will notice the correlation between the social situation and average actions.

These mostly metaphysically formulated 'debates on motivation' are avoided by an empirical sociology, whose concern is to do fruitful work, as well as by the most successful sociology today: *Marxism*. Marxism is engaged in tracing correlations between the social condition and the behaviour of whole classes, and then deducing the frequently changing word sequences that are used to 'motivate' behaviour which is thereby conditioned and

deducible with the help of laws. Since Marxism, in its description of inter-relationships expressible as laws, makes as little use as possible of what people state about themselves, their 'processes of consciousness', their 'ideology', it is related to the types of 'psychology' that attribute an important role to the 'unconscious' in some form. Thus it comes about that *psychoanalysis* and *individual psychology*, by their relaxation and dissolution of the rather obsolete motivation-psychology of consciousness, prepare the way for modern empirical sociology whose aim is to discover correlations between behaviour and conditions of behaviour along the lines of unified science.

Though psychoanalysis and individual psychology in their present form contain many metaphysical expressions, they are still forerunners of the behaviourist way of thinking and the sociological approach because they lay stress on the connection between behaviour and unconscious pre-condition.

It makes good sense to ask whether a certain order of life spreads more or less happiness than another, as 'happiness' can be perfectly described behaviouristically; it makes good sense to ask on what the demands depend, which masses of men make of each other, and which new demands are put forward, which ways of behaviour will make their appearance at the same time – demands and ways of behaviour often differing greatly. Such sociological questions can be asked quite legitimately. Whether it is advisable to call them 'ethical' may remain undecided.

Matters are similar with 'jurisprudence' if it wants to be something different from a sociology of certain social phenomena. If it sets itself the task of finding out whether a system of demands is logically consistent, whether certain consequences of the statute books can be harmonised with certain observation statements of legal practice, we are dealing with purely logical investigations. If we investigate whether the directions of a chemist are in logical agreement, we are not yet doing work in chemistry. In order to be able to do work in chemistry, we must state certain correlations between certain chemical processes and certain temperatures and the like. In spite of mainly metaphysical introductory formulations, the representatives of some schools of the philosophy of law can produce results of logical and scientific significance; this, however, cannot prevent us from opposing metaphysical formulations, as for example:

Though the thinking of mathematical or logical laws is a psychical act, the subject of mathematics or logic – the thought – is not something psychical, not a mathematical or logical 'soul', but a specific mental factual content, because mathematics and logic abstract from the psychological fact of thinking such content: Thus the state, as subject of a specific study, different from psychology, is a specific mental content, but not the

fact of thinking and wanting such content; it is an ideal order, a specific system of norms, but not the thinking and wanting of these norms. The state is not in the kingdom of *nature* – of physical-psychical relations – but in the kingdom of the *spirit*. The state as binding authority is a value, or – insofar as the appropriate expression is inserted – a norm, or a system of norms, and as such essentially different from the specifically real fact of imagining or wanting the norm, that is indifferent to value (Kelsen 1925, p. 14ff).

This kind of formulation is connected with similar ones on 'ethics' and related disciplines, but no attempt is made to investigate how the term 'objective aims' is to find a place in unified science, and no suggestion is made as to through which observation statements 'objective aims' might be defined as such:

If the 'general theory of state' asks what the state is and how it is, that is, what its possible basic forms and main contents are, then politics puts the question whether the state should exist at all, and if so, which is the best of its possibilities. With this approach it proves to be part of ethics, as knowledge of morals, that puts objective aims to human behaviour, that is, some contents as what ought to be done. Insofar as politics looks for suitable means for putting into operation the somehow set and thus presumed objective aims, that is, finds those contents which, according to experience, cause those effects which in content correspond to the set aims, it is not ethics, it is not directed towards normative lawfulness, rather it is technology, if one wants to call it that: social technology, and as such directed towards the law of causality of the connection of means and end (Kelsen 1925, p. 27).

Within the framework of an *empirical sociology*, that is, within the framework of a *social behaviourism*, most of these comments cannot be used even after a thorough transformation. Which correlations are to be stated? And if the intention is to show that certain directions, legal regulations, combined with each other are logically equivalent to other regulations, a fact that may not be noticeable at first glance, no special metaphysical discussions are needed for such observations, that are, indeed certainly important for practical life.

It is obvious that these tautologies of the legal order will be less prominent when the basic mood of unified science is the rule. There will then be greater interest in what the effects of certain measures are, and less interest in whether the wording of the regulations in books of law is such that their interrelationship is logically consistent. Nobody would think that a special discipline is needed to examine the logical compatibility of the instructions of a hospital administration. One wants to know what effect certain measures together have on the state of health in order to arrange actions accordingly.

7. THE EMPIRICAL SOCIOLOGY OF MARXISM

From the start the unified language of physicalism safeguards scientific procedure. Statement ranges itself with statement, law with law. It was shown how sociology finds a place within such unified science, as do biology, chemistry, technology, astronomy. The fundamental separation of special 'mental sciences' from the 'natural sciences' proved to be meaningless theoretically; but also as a merely practical distinction, which would be stronger than many others, it proved inexpedient and in no way required.

In continuation, the concept of the sociological correlation was roughly sketched, as it could find application in the framework of an elaborate social behaviourism. We saw that within this conception, disciplines like 'ethics', 'jurisprudence' lose their traditional ground. Without metaphysics, without demarcations that can only be explained from metaphysical usages, these disciplines cannot preserve their independence. *What is scientific in them enters the structure of sociology.*

All that is useful in protocol statements and laws produced by political economy, ethnology, history and other disciplines will gradually come together in this sphere. At times the fact that people alter their manner of response plays a great part in sociological studies, while at other times the starting point is that people do not change in their response behaviour but get into changed relationships to each other. Political economy, for example, in general takes people to be constant and then investigates what the consequences of the existing economic order are, for example, the market mechanism. It attempts to find out how crises, unemployment arise, how net profits come about, etc.

If attention is paid to the fact that the given economic order is being changed by men, then sociological laws are needed to describe these changes. It is not sufficient, then, to investigate the economic order and its behaviour, the laws that rule the change of the economic order itself must also be investigated. Sociologists of the most varied leanings study how certain changes in the mode of production change the stimuli so that people alter their traditional habits, often through revolutions. Of the existing sociological doctrines, *Marxism* contains the sociology of the most empirical sort. The most important tenets of this direction that are used for *predictions* are either already phrased rather physicalistically as far as this was possible with the traditional language, or they can be so phrased without essential loss.

The example of Marxism shows us how sociological correlations can be investigated and how laws of relations can be established. If one tries to find

out which correlations exist between the mode of production at consecutive times and contemporary cults, books, speeches etc., then one is studying the *correlations between physicalist entities*. Marxism adds other further tenets to the tenet of physicalism (materialism). If it confronts the one group of entities as 'substructure' with another group of entities as 'superstructure' ('historical materialism' as a special physicalist doctrine), it continually moves within the framework of social behaviourism. It has nothing to do with a juxtaposition of things 'material' and 'spiritual', that is, with 'essences' and their 'different causalities'.

The coming decades will pay ever increasing attention to the discovery of such correlations. How much concrete research is hampered by metaphysical formulations becomes obvious in Max Weber's powerful attempt to deduce the rise of capitalism from Calvinism. To an advocate of social behaviourism, it appears plausible from the start that certain word sequences, such as the formulation of certain divine commands, are recognised as being dependent on certain modes of production and power situations. However, it does not sound very plausible that word sequences uttered by individual theologians, the always rather vaguely phrased commands of the divinity transmitted by theologians, should condition the living standard of broad masses occupied in commerce, trade and other work. Nevertheless Max Weber advocated this view. He tried to show that the 'spirit of Calvinism' gave birth to the 'spirit of capitalism' and thus to the capitalist order.

A Catholic theologian, Kraus, pointed out that Weber's overestimation of the influence of theological formulations could probably only be explained by the assumption that he endowed the spirit with a kind of 'magical' power. Weber and others think 'spirit' is very closely connected with words and formulations; thus we can understand why Weber searched hard for sharp theological formulations of individual Calvinists to deduce pointed formulations of a capitalist kind from them. The 'rationalism' of the one sphere is supposed to give birth to that of the other. It might be formally possible, also within the framework of physicalism, to suppose that theological speeches and writings have such enormous power; *but experience proves otherwise*. The Catholic theologian mentioned above points out, as the Marxists do, that human behaviour is little influenced by theological subtleties, which in any case are hardly known to the average merchant or trader. It should be much more plausible to assume, for example, that in England merchants, who fought against royal monopolies, that usurers who wanted to charge interest, despite the orders of the Anglican Church, readily supported a doctrine and its advocates who turned against the church and its ally, the

Crown. *First these men were to a high degree capitalist in their behaviour, then they became Calvinists.* According to all experience with theological doctrines elsewhere, one must expect that the doctrines were subsequently altered and adapted to the mode of production and business. In contrast to Weber, Kraus incidentally shows that the theological formulations that are 'coherent' with capitalism, make their appearance only later, whereas the original Calvinism was more in harmony with the doctrines of the anti-capitalist Middle Ages. *Weber's metaphysical starting point impeded his scientific work* and had an unfavourable influence on his selection of observation statements. Without an adequate selection at the start, fertile scientific work is impossible.

Let us analyse one concrete example in somewhat greater detail. *What bore on the decline of slavery in antiquity?*

Many are inclined to believe that Christian teaching, the Christian way of life, had brought the end of slavery after Stoic philosophers had already undermined the notion of slavery as an eternal institution.

If such a claim is expressed as a correlation, the first thing that comes to mind is to examine whether Christianity and slavery have existed together or not. Then one notices that the most oppressive forms of slavery make their appearance at the beginning of modern times, at a time when Christian states spread their power everywhere and Christian churches were strong, especially in the colonies. Out of a feeling of humanity Catholic theologians were able to intervene to protect the declining Indian slaves, with the result that shiploads of the more sturdy Negro slaves were transported to America.

First it should in fact be defined more precisely how 'Christian' and how 'slavery' should be understood. If we try to express the correlation between them more acutely, we have to attest that the statements of people of a certain kind, their cult behaviour, etc. never occur at the same time as the keeping of masses of slaves. In addition a certain kind of use would have to be defined, because someone can 'legally' be a 'slave' and 'sociologically' a 'master'. However, sociological concepts have to be connected with other sociological concepts.

'Christian teaching' is an uncommonly vague concept. Many theologians believed that one could prove from the Bible that God had declared the Negroes to be slaves. To wit, when Ham treated his drunken father without respect, Noah cursed him and declared that he and his descendants should be subject to the brothers Sem and Japheth and their descendants. On the other hand other theologians tried to discover arguments against slavery in Christian teaching.

The sociologist clearly has a much better chance of success if he defines a certain system of men, cult behaviour, doctrines, etc., and then looks to see whether it occurs together with certain ways of behaviour of the society, or not. This is certainly a rather crude procedure. We must aim to discover not only such simple, but also more complicated correlations. Laws must be combined with one another so that certain predictions can be deduced.

Many sociological 'laws' are valid for certain periods only; similarly there are laws of ants, of lions, besides more general biological laws. That is, we cannot yet indicate precisely on what certain correlations depend: 'historical period' = non-analysed complex of conditions. Much confusion was caused by some analytical sociologists who thought that the sociological laws discovered by them must be like chemical laws, that is, be valid under all imaginable conditions on earth. In fact, however, correlations in sociology are mostly valid for *definite time intervals*. Marx was justified when he pointed out that it is meaningless to speak of a general law of population as Malthus did. However, one can, he said, indicate the law of population in each sociological era.

If, in order to clarify the question: how did the decline of slavery come about?, one analyses the struggle of the Northern and Southern states of North America about the emancipation of the slaves, one realises that the fight is between the industrial states and the plantation states. The plantation states suffered serious damage by the emancipation. Might there not be a connection between the freeing of slaves and the process of production? How can such a matter be made plausible?

We investigate under what conditions slavery is advantageous to slave owners, under which not. If one asks the masters who free slaves why they do so, only few will say that they oppose slavery because it does not bring enough advantages. Many will report without hypocrisy that the reading of a philosopher in sympathy with the slaves had made a deep impression, others will describe in detail the struggle of their motives, they will perhaps comment on how slavery would in fact be more advantageous, but how the wish to make a sacrifice, to renounce possession, had led them, after a long struggle of motives, to the difficult step of freeing their slaves. If we are used to proceeding along the lines of social behaviourism, we will focus attention above all on the very complex 'stimulus' of the way of life with slavery and then investigate the 'response' – keeping or freeing slaves – and then ponder how far the theological teachings concerning the emancipation of slaves can be taken into account as 'stimulus' and how far as 'response'.

If it can be shown that relatively simple correlations can be established between the effects of slavery on the living standard of the masters and the

masters' behaviour in view of the emancipation of slaves, and on the other hand no *simple correlations* between the teachings of the time and the behaviour of the slave owners can be shown, then the first manner of investigation will be preferred.

Therefore we shall investigate the coherence between hunting and slavery, agriculture and slavery, manufacture and slavery under various conditions. We will find, for example, that the possession of slaves is in general of no advantage when enough free workers are available who try to find a position with all their energy in order to escape death from starvation. For example, Columella, a Roman author on agriculture of the Late Empire says straightforwardly that it is disadvantageous to use slaves to drain the malarial swamps of the Campagna: a disease of the slave means loss of interest, his death, loss of capital; however, it was always possible to get free workers on the market – their disease, their death is not the employer's loss.

When there are strong fluctuations in business it is desirable for the employer if he can dismiss free workers; slaves, like horses, have to be kept alive. Thus we read in Strabo that in antiquity papyrus plants were already being cut down in Egypt to keep the monopoly price up, and hence we will understand that the general use of free workers can no longer be far away.

What the conditions were that resulted in fluctuations in business activity (economic institutions of early capitalism) can also be investigated. Correlation is joined to correlation. We see that 'free workers' and 'destruction of produced goods' seem to be coherent under certain conditions, as are 'plantation slavery' and 'constant sales'. One can regard the Civil War as a struggle between the industrial North, which had no interest in slavery, and the cotton-producing agrarian South; with this in mind quite a number of predictions can be made.

Therefore the religious and ethical opponents of slavery did not lie when they said that they felt unalloyed joy when the slaves were emancipated but not when the industrial profits increased in the Northern states. The empirical sociologist will be able roughly to deduce from the general situation that such joy about the emancipation of the slaves could unfold at this time and find such ample satisfaction.

As soon as a theory of agriculture was developed, some people, among them theologians, developed a perfectly empirical theory of the 'use of indigenous people' that leads to all sorts of correlations (see Neurath 1913b, p. 474 and bibliography). And through combination with other law-like connections, we can make all sorts of predictions about the fate of slavery in individual countries and areas.

When in the Rome of the Late Empire grain was distributed to the free people but not to the slaves, there was a further stimulus for the slave owners to free their slaves, to re-employ them as freedmen at less cost and to use them as clients in elections. It is also easy to understand how declining Rome turns to the tenant farmer and serf system by dismantling the early capitalist institutions. To develop a production process with slave labour, one must have great amounts of money at one's disposal, because labour as well as tools would have to be bought. If labour is free, the purchase of tools would be sufficient. The tenant farmer required no investment at all by the owner who secured all kinds of levies for himself. The 'free' workers are forced to work by the total social order – idleness is punished by death – whereas the slaves have to be trained to order by each individual master. He had to take care of their health and their life, as he looks after a horse or an ox, even if it is unruly.

We see how such analyses help to establish correlations between general social conditions and certain ways of behaviour of human subgroups. The 'statements' of the groups are not significant for these correlations; they can be added, often with the help of further correlations. This working method of empirical sociology can be found above all in Marxism (see, for example, Ettore Ciccotti 1910).

A system of empirical sociology along the lines of social behaviourism as it develops mainly in the United States and the Soviet Union would first of all have to investigate the typical 'responses' of whole groups to certain 'stimuli'. But often calculations or estimates of significant historical movements are also made without such analyses. And now it can be shown that by perfecting certain institutions, by growth of a certain magnitude, a reversal is underway that makes the further development go on in an entirely different direction. The primitive 'doctrine of progress', the idea that some quantity continues to grow, cannot be maintained. We have to look at the whole system of interwoven sociological phenomena and then see what changes can be predicted. If large cities have so far been growing, we cannot conclude that this will go on in more or less the same way. A growth by leaps and bounds can trigger off stimuli that lead to a sudden growth stoppage and perhaps to the new creation of many small centres. The increase of large capitalist establishments, the growth of proletarian masses dependent on the establishment, can have the result that the whole capitalist mechanism approaches its end in connection with economic crises.

8. POSSIBILITY OF PREDICTIONS

We can give an account of the extent to which predictions can be made successfully within the framework of social behaviourism. It becomes clear that *the different 'predictions', that is, the scientific theories, are sociological events and depend essentially on the social and economic order.* Certain predictions can, under certain conditions, either not be made at all or not be elaborated, for example; this is realised only as an after-thought. Even if an individual believes he has an inkling of the direction of further successful research, by lack of sympathy, even by resistance of other people, he may be prevented from finding partners who are especially necessary in sociological research.

Rudiments of social changes are difficult to notice. Predictions of novel events can mostly be made only when novel experiences already exist to some extent. Only the changes of the historical course of events will often give the scholar the necessary data for further investigations. But since sociological investigations play a certain role as stimuli and as aids for the shaping of life, an advance in sociology is very closely connected with social struggles. Only established sociological schools, which need social support, can, by working together, master the masses of material that have to be utilised for the stricter formulation of correlations. For this again it is necessary that the powers that finance such work are favourably disposed to social behaviourism.

In general this is not the case today. Indeed, in the ruling circles there is an aversion toward social behaviourism (as well as toward individual behaviourism) that is far in excess of the scientific misgivings that could be explained in terms of the imperfections of this theory. The sociological explanation of this resistance on the part of the ruling circles, which mostly find support in the universities of capitalist countries, may be for the most part that empirical sociology, through its avoidance of metaphysics, unmasks the meaninglessness of such ways of speaking as 'categorical imperative', 'divine command', 'moral idea', 'transpersonal state', etc., and thus weakens important doctrines that are used to support the ruling order. The advocates of 'unified science' do not stand for *one* worldview side by side with other world-views, so that the question of tolerance might be mooted. They declare transcendental theology and metaphysics not wrong, but meaningless. Without denying that strong emotions, happiness and depression, can be attached to meaningless doctrines, the advocates of 'unified science' can in practice 'allow seven to be a holy number' by not molesting the advocates of this

doctrine, but they cannot declare that this claim could have any, even a 'secret' meaning, that is, it could confirm or refute scientific statements. Though such argumentation of the pure scientist leaves metaphysics and theology undisturbed, it undoubtedly unsettles the respect for them that is demanded by many.

In the framework of purely scientific formulations, all metaphysical entities whose commands one tried to follow, whose 'holy' power one venerated, are replaced by – an empirical substitute – the actual behaviour of groups *whose commands, as empirical formulations*, have an effect on the individual man. Within the framework of social behaviourism it is a perfectly meaningful statement to say that human groups encourage individual human beings to adopt certain ways of action and inhibit them in respect to others.

The social behaviourist also gives commands, begs, reprimands, *but he does not think that these utterances, connected with statements, could produce a system*. We can make use of words as we can use whistling, stroking or whipping; with such use, they can neither become inconsistent with statements nor consistent with them. *A command can never be deduced* from a system of statements. This does not mean a 'limitation' upon our scientific activity; rather it is nothing but the result of logical analysis. That commands and predictions are so often mixed up together is perhaps due to the fact that both have to do with the future. A command is an event which it is assumed will bring about certain changes in the future; prediction is a statement which it is assumed will be consistent with a future statement.

The advocates of 'unified science' endeavour to make predictions in the 'unified language of physicalism' with the help of laws. In the field of empirical sociology this is done through the development of 'social behaviourism'. In order to reach useful predictions one can first remove the *meaningless word sequences* with the help of logic. But this is not enough. The removal of all wrong formulations must follow. After removing metaphysical formulations, the representatives of modern science have to dispute about wrong doctrines, for example, astrological, magical and similar ones. In order to liberate someone from such doctrines it is not sufficient, as it is for the elimination of the meaningless, to agree on logic; if we want our own teaching to be adopted, we have to create a basis by educational means, so that the inadequacy of these 'also-physicalist', *but uncritical doctrines* can be recognised.

The fertility of social behaviourism is proved by the establishment of new correlations, by the good predictions made with their help. A youth brought up along the lines of physicalism and its unified language will avoid many

inhibitions in research to which we are still exposed at present. The successful language cannot be created and applied by an individual, for it is the work of a generation. Thus also sociology as social behaviourism will be able to make correct predictions in great numbers only when a generation [trained in] physicalism will be active in all fields. Though today we can observe a growth of metaphysics, there are also many indications that doctrines free from metaphysics are also spreading and are gaining more and more ground as a new 'superstructure' of the changing economic 'substructure' of our time. [See Otto Neurath, 'Physicalism. The Philosophy of the Viennese Circle', pp. 48–51.]

NOTES

[1] [At this point the author refers in a footnote to six volumes of the *Schriften zur wissenschaftlichen Weltauffassung* series, listed on p. 51, note 1 above; the journal *Erkenntnis*; and two pamphlets published by the Verein Ernst Mach: *Wissenschaftliche Weltauffassung: Der Wiener Kreis* (Vienna: A. Wolf, 1929) by Rudolf Carnap, Hans Hahn, Otto Neurath (translated as 'The Scientific Conception of the World: The Vienna Circle' in (Neurath 1973b), pp. 299–318); and Hans Hahn, 'Ueberflüssige Wesenheiten (*Occams Rasiermesser*)' (Vienna: A. Wolf, 1930).

CHAPTER 7

PROTOCOL STATEMENTS

(Remarks about Rudolf Carnap's essay 'Die physikalische Sprache als Universalsprache der Wissenschaft' (1932a). As we thoroughly agree with Carnap, our terminology is linked to his. To avoid repetition, reference may be made to two of our papers 'Physicalism', [pp. 52–57] and 'Sociology in the Framework of Physicalism' [pp. 58–90].)

In the interest of scientific work, more and more formulations in the unified language of unified science are becoming increasingly precise. No term of unified science, however, is free from imprecision, since all terms are based on terms that are essential for *protocol statements* whose imprecision must be immediately obvious to everyone.

The fiction of an ideal language composed of neat atomic statements is as metaphysical as the fiction of Laplace's spirit. Scientific language, with its ever growing equipment of systematic symbol formations, can by no means be regarded as an approximation to such an ideal language. The statement, 'Otto observes an angry man' is less precise than the statement, 'Otto observes a thermometer registering 24 degrees' inasmuch as 'angry man' must be less precisely defined than a 'thermometer reading of 24 degrees'; but 'Otto' itself is in many respects an imprecise term; the statement, 'Otto observes', can be replaced by the statement, 'The man whose carefully taken photo is No. 16 in the file, observes'; but the term 'photo No. 16 in the file' has not yet been replaced by a system of mathematical formulas that is unambiguously coordinated to another system of mathematical formulas that takes the place of 'Otto', of 'angry Otto', 'kind Otto', etc.

What is first given us is our historical ordinary language with a multitude of imprecise, unanalysed terms ['*Ballungen*'].

We start by purifying this ordinary language of metaphysical components and thus arrive at the physicalist ordinary language. A list of forbidden words can serve us well in doing this.

In addition, there is the physicalist language of advanced science that we can design to be free of metaphysics from the very start. It is at our disposal only for special sciences, indeed only parts of sciences.

Translation of Neurath 1932/33 [ON 210].

If we want to embrace the entire unified science of our age, we must combine terms of ordinary and advanced scientific languages, since in practice, the terms of both languages overlap. There are certain terms that are used only in ordinary language, others that occur only in scientific language, and finally terms that appear in both. In a scientific treatise that touches upon the whole range of unified science, therefore, only a 'jargon' that contains terms of both languages will do.

We expect that it will be possible to replace each word of the physicalist ordinary language by terms of scientific language – just as it is also possible to formulate the terms of scientific language with the help of terms of ordinary language. We are not very used to the latter and sometimes do not find it easy. Einstein can somehow be expressed by means of Bantu language, but not Heidegger, unless one first introduces misuses into the Bantu language which have been adapted to those of the German. A physicist should, in principle, be able to satisfy the witty thinker's demand: "It must be possible to make the main features of any strictly scientific theory plain to a hackney-coach-driver in his own language."

Highly scientific and ordinary languages are in harmony today especially in the field of calculation with figures. But even the expression 'two times two is four' – a tautology – is linked to protocol statements in the system of radical physicalism. Tautologies are defined by statements that record what effect tautologies have if they are inserted as additional stimuli to certain commands under certain conditions: "Otto says to Karl: go outside when the flag waves *and* when two times two is four." The effect of the command is not affected by the addition of the tautology.

Even on the basis of the strictest scientific principle, in unified science, we can only use a 'universal jargon'. Since there is as yet no agreement as to what it should be like, each scholar who turns to these problems must use a universal jargon for which he for the most part has to create some new terms.

There is no way to establish fully secured, neat protocol statements as starting points of the sciences. There is no *tabula rasa*. We are like sailors who have to rebuild their ship on the open sea, without ever being able to dismantle it in dry-dock and reconstruct it from the best components. Only metaphysics can disappear without trace. Imprecise 'verbal clusters' ['*Ballungen*'] are somehow always part of the ship. If imprecision is diminished at one place, it may well re-appear at another place to a stronger degree.

From the beginning we shall teach children the universal jargon – purged of metaphysics – as the language of unified science which has been historically

provided. Each child can thus be 'trained' to start with a simplified universal jargon and gradually advance to the universal jargon of adults. It makes no sense in our discussion to segregate this children's language as a special language. Otherwise one would have to distinguish all sorts of universal jargons. The child does not learn a 'primitive' universal jargon from which the grown-ups' universal jargon derives; the child learns a 'poorer' universal jargon, which is gradually enriched. The term 'ball of iron' is also used in adults' language; while here it is defined by a sentence in which words like 'radius' and 'pi' occur, in the definition for children words like 'nine-pins', 'gift from uncle Rudi', etc., occur.

But 'uncle Rudi' is not absent from the language of strict science either, if the physical ball is defined by protocol statements in which 'uncle Rudi' appears as an 'observer' who 'perceives a ball'.

Carnap, however, speaks of a *'primitive'* protocol language (Carnap 1934c, p. 42ff and p. 76ff). His remarks on the *'primitive'* protocol language, on the protocol statements that 'require no verification', are only marginal to his significant anti-metaphysical expositions where the basic idea is not touched by the misgivings brought forward here. Carnap speaks of a "first language" also called "language of experience" or "phenomenalist language". Here he stresses that "the question of a more precise characterisation of this language cannot yet be answered at the present state of inquiry".

These remarks might induce younger people to search for this protocol language, and this easily leads to metaphysical digressions. Although metaphysics essentially cannot be defeated by arguments, it is important for the sake of the vacillators to press for physicalism in its most radical version.

Leaving tautologies aside, unified science consists of factual statements. These are either protocol statements or non-protocol statements.

Protocol statements are factual statements of the same linguistic form as other factual statements, but in them a personal name always occurs several times, in a definite connection with other terms. A complete protocol statement might for example be worded like this: "Otto's protocol at 3:17 o'clock: [Otto's speech-thinking at 3:16 o'clock was: (at 3:15 o'clock there was a table in the room perceived by Otto)]". This factual statement is so constructed that, after 'deletion of the brackets', further factual statements appear, which, however, are not protocol statements: "Otto's speech-thinking was at 3:16 o'clock: (at 3:15 o'clock there was a table in the room perceived by Otto)" and further: "At 3:15 o'clock there was a table in the room perceived by Otto".

From the start, each of the terms occurring in these statements can to a

certain degree be replaced by a group of terms of the highly scientific language. Instead of 'Otto' a system of physicalist definitions can be introduced; this system of physicalist definitions can be further defined by the 'place' of the name 'Otto' in a group formed of the names 'Karl', 'Heinrich', etc. All words used in the protocol statement above either are words of the universal jargon or can easily be replaced by words of the universal jargon from the outset.

It is *essential* for a complete protocol statement that the name of a person occur in it. 'Now joy' or 'now red circle' or 'a red cube is lying on the table' (see Carnap 1934c, pp. 46–47) are not complete protocol statements. Even as expressions within the innermost brackets they are not acceptable. According to our version it should at least be said – in rough correspondence with children's language – "Otto now joy", "Otto sees a red circle now", "Otto sees a red cube lying on the table now". That is, for the protocol statement to be complete, the expression within the innermost bracket is a statement that again features a personal name and a term that belongs to the sphere of perception terms. The extent to which ordinary terms or highly scientific terms are used is of no essential importance, since linguistic usages within the universal jargon are highly flexible.

The expression after the first bracket, 'speech-thinking', recommends itself; this becomes apparent if one wants to form different groups of sentences, for example, sentences with 'reality terms', with 'hallucination terms', with 'dream terms', and especially if, moreover, one wants to segregate 'untruth'. One could say for example: "Though Otto's 'speech-thinking' was: In the room there was only a bird perceived by Otto, he wrote down, as a joke: In the room there was only a table perceived by Otto." This is important, especially for the discussions in the next section, in which we reject Carnap's claim that protocol statements are statements that "need no verification".

The process of change in the sciences is like this: statements that were used at a certain age drop out at a later age and are often replaced by other statements. Sometimes the wording remains, but the definitions are changed. *Each law and each physicalist statement of unified science or of one of its factual sciences is subject to such change. The same is true for each protocol statement.*

In unified science we try (see Carnap 1934c, p. 43ff) to create a *consistent system* of protocol statements and non-protocol statements (including laws). When a new statement is presented to us we compare it with the system at our disposal and check whether the new statement is in contradiction with the system or not. If the new statement is in contradiction with

the system, we can discard this statement as unusable ('false'), for example, the statement: 'In Africa lions sing only in major chords'; however, one can also 'accept' the statement and change the system accordingly so that it remains consistent if this statement is added. The statement may then be called 'true'.

The fate of being discarded may befall even a protocol statement. There is no '*noli me tangere*' for any statement though Carnap claims it for protocol statements. An especially drastic example: Let us assume that we know a scholar called Kalon who can write with both hands simultaneously, and that he writes with his left hand: "Kalon's protocol at 3 hours, 17 minutes: [Kalon's speech-thinking was at 3 hours, 16 minutes 30 seconds: (At 3 hours, 16 minutes there was *only* a table in the room perceived by Kalon)]", and with his right hand: "Kalon's protocol at 3 hours, 17 minutes: [Kalon's speech-thinking was at 3 hours, 16 minutes, 30 seconds: (At 3 hours, 16 minutes there was *only* a bird in the room perceived by Kalon)]". What can he and what can we do with these two protocol statements? We can of course make statements of the following kind: Certain marks are on paper, sometimes shaped like this, sometimes shaped like that. With reference to these marks on paper the word 'verification' used by Carnap can, however, find no application. 'Verification' can only be used with reference to 'statements', that is, with reference to rows of signs that can be used in the context of a reaction-test and can be systematically replaced by other signs (see 'Physicalism' pp. 55). 'Synonomous statements' are to be defined as stimuli, which, under definite reaction-tests, evoke equal reactions. Strings of 'ink blobs on paper' and strings of 'air perturbations', which can be considered equal under certain circumstances, are called statements.

Two conflicting protocol statements can*not* be used in the system of unified science. Though we cannot say which of the two statements is to be excluded, or whether both are to be excluded, we can be sure that not both can be 'verified', that is, it is not the case that both statements can be incorporated into the system.

If, in *such* a case, a protocol statement has to be given up, why not also sometimes when, only after long chains of logical argumentation, contradictions appear between protocol statements on the one hand and a system of protocol statements and non-protocol statements (laws, etc.) on the other hand? According to Carnap we could only be forced to change non-protocol statements and laws. *But in our view the cancelling of protocol statements is a possibility as well.* It is part of the definition of a statement that it requires verification and therefore can be cancelled.

Carnap's claim that protocol statements 'need no verification', however it may be understood, can easily be related to traditional philosophy's belief in 'immediate experience'. For these there were of course certain 'ultimate elements' out of which the 'world picture' was composed. According to this traditional philosophy these 'atomic experiences' were obviously above any criticism and required no verification.

Carnap tries to introduce a kind of 'atomic protocol' by demanding that "a strict distinction be made between the making of a protocol and the processing of the statement in the scientific procedure"; according to him this will be achieved by "not adopting any statements gained indirectly into the protocol" (Carnap 1932a, p. 437).[1] The formulation of a complete protocol statement given above shows that insofar as personal names occur in protocol statements, 'processing' must *always* have taken place. In scientific protocols it may be useful to phrase the expression within the innermost brackets as simply as possible, for example: "At 3 o'clock Otto was seeing red", and a further protocol: "At 3 o'clock Otto was hearing C sharp", etc.; but such a protocol is not 'primitive' in Carnap's sense, because one cannot get around the 'Otto' and the 'perceiving'. Within the universal jargon there are no statements that could be characterised as 'more primitive', all are factual statements of equal primitiveness; in all factual statements words occur like 'men', 'acts of perception' and other words of little primitiveness, at least under the presuppositions from which they are derived. *That is to say: there are neither 'primitive protocol statements' nor any statements that 'do not require verification'.*

The universal jargon, in the sense explained above, is the same for the child and for the adult. It is the same for a Robinson Crusoe as for a human society.

If Robinson wants to join what is in his protocol of yesterday with what is in his protocol today, that is, if he wants to make use of a language at all, he must make use of the *'inter-subjective'* language. The Robinson of yesterday and the Robinson of today stand in precisely the same relation in which Robinson stands to Friday. Let us assume a man who 'has lost his memory' and 'his eyesight', and at the same time learns afresh to read and write. His own notes of earlier times, which he can read with the help of special apparatus, will for him be those of 'another' person as much as the notes of any contemporary. This remains true even if he afterwards becomes aware of the continuity of fate and writes his own biography.

In other words, *every* language *as such* is 'inter-subjective'; it must be possible to incorporate the protocols of one moment into the protocols of the next moment, just as the protocols of A can be incorporated into the

protocols of *B. Therefore it does not make sense to speak of monologising [private] languages*, as Carnap does, nor of different protocol languages that are later related to each other. The protocol languages of the Robinson of yesterday and of today are as close or distant as those of Robinson and Friday. If, under certain circumstances, one calls Robinson's protocol language of yesterday and today *the same* language then, *under the same conditions*, one can call Robinson's and Friday's the same language.

Also in Carnap's writings we encounter here the emphasis on the 'I' familiar to us from idealist philosophy. In the universal jargon, one cannot speak meaningfully of one's 'own' [*'eigenen'*] protocol, nor of 'now' and 'here'. In the physicalist language, personal names are replaced by coordinates and coefficients of physical states. One can only distinguish an 'Otto-protocol' from a 'Karl-protocol' but, in the universal jargon, not one's 'own protocol' from 'another's protocol'. The whole problematic connected with one's 'own mind' and 'other minds' does not arise.

The 'methodological' solipsism and 'methodological' positivism (see Carnap 1932a, p. 461) do not become more usable by the addition of the word 'methodological' (see 'Sociology in Physicalism' p. 65).

If, for example, I had said earlier: "Today, 27 July, I am busy with protocols of my own and of others", it would be more correct to say: "Otto Neurath's protocol at 10 a.m., July 27 1932: [Otto Neurath's speech-thinking at 9 hours, 55 minutes was: (Otto Neurath occupied himself between 9 hours, 40 minutes and 9 hours, 54 minutes with a protocol by Neurath and with a protocol by Kalon both of which contained the following two sentences . . .)]". Although Otto Neurath words the protocol concerning the application of the protocols, he incorporates his own protocol into the system of unified science in the same way as that of Kalon. It may very well happen that Neurath deletes one of Neurath's protocol statements and adopts one of Kalon's in its stead. That a man clings more obstinately to his own protocol statements than to those of another, in general, is a historical fact – without any fundamental significance for our discussion. Carnap's contention: "Each individual can only use his own protocol as a basis" cannot be accepted, for his argument is *not* conclusive: "S_1 can certainly also utilise S_2's protocol, and this utilisation becomes especially simple through the incorporation of both protocol languages into the physical language. Still, it is made indirectly: S_1 has to describe in his protocol that he sees a piece of writing of such and such a shape" (Carnap 1932a, p. 461.)[2] But Neurath has to give the same description of Neurath's protocol as of Kalon's protocol. He describes how he sees the Neurath-protocol as well as how he sees the Kalon-protocol.

In further developments the protocol statements of all men will be treated alike. Fundamentally it makes no difference at all whether Kalon works with Kalon's or with Neurath's protocols, or whether Neurath occupies himself with Neurath's or with Kalon's protocols. In order to make this quite clear, one could think of a scientific cleaning machine into which protocol statements are thrown. The 'laws' and other 'factual statements', including 'protocol statements', which have their effect through the arrangement of the wheels of the machine, clean the stock of protocol statements thrown in and make a bell ring when a 'contradiction' appears. Now either the protocol statement has to be replaced by another or the machine has to be reconstructed. *Who* reconstructs the machine, *whose* protocol statements are thrown in, is of no consequence at all; everybody can test his 'own' as well as 'others'' protocol statements.

To sum up:

Unified science uses a universal jargon in which terms of the physicalist ordinary language also must occur.

Children can be trained in the use of the universal jargon. Apart from it we do not use any specially separable 'primitive' protocol statements, nor 'protocol languages of different persons'.

Within unified science we have no use for the terms 'methodological solipsism' or 'methodological positivism'.

It is impossible to start from finally secured, pure protocol statements. Protocol statements are factual statements like other factual statements, in which personal names or names of groups of persons appear in a certain connection with other terms that are also otherwise used by the universal jargon.

The work of the Vienna Circle is concentrating more and more on the task of presenting unified science (sociology as well as chemistry, biology as well as mechanics, psychology – preferably called behaviouristics – as well as optics) in a unified language, and of creating the often neglected 'cross-connections' between the individual sciences so that it is possible to relate the terms of each science to the terms of every other science without effort. The word 'man', which is connected with 'making statements' is to be defined in the same way as the word 'man' that occurs in statements containing words like 'economic order', 'production', etc.

The Vienna Circle has received powerful stimuli from different sides. The achievements of Mach, Poincaré, Duhem were utilised as well as the contributions of Frege, Schröder, Russell and others. Wittgenstein had an

uncommonly enlivening effect both through what was accepted from him, and what was rejected. His first attempt to use philosophy as a necessary *ladder of elucidation*, however, can be regarded as having failed. What matters in all scientific work is to establish harmony between the statements of unified science: protocol statements and non-protocol statements. For this purpose a 'logical syntax' is needed, which is the main issue of Carnap's work; Carnap has created the first preparations for this in his book, *Der logische Aufbau der Welt* (1928).

The discussion begun here – Carnap will certainly find all sorts of things to correct and supplement in these corrections – serves, as do so many other efforts of ours to reinforce the broad common basis of work done by us, the physicalists. Such marginal debates will play an ever decreasing role; the rapid progress of the work of the Vienna Circle shows that the *planned collective work* devoted to the construction of unified science is in continuous development. We physicalists will succeed in this construction the more quickly and thoroughly, the less time we have to devote to the elimination of old errors and the more we can occupy ourselves with the formulation of scientific correlations. For this purpose we have to learn above all to make use of the *physicalist language*; this is what Carnap advocates in his article.

NOTES

[1] [This passage is given in M. Black's translation as follows: "A 'primitive' protocol will be understood to exclude all statements obtained indirectly by induction or otherwise and postulates therefore a sharp (theoretical) distinction between the raw material of scientific investigation and its organization" (Carnap 1934c, p. 43). – Ed.]

[2] [The relevant passage was omitted in Black's translation (Carnap 1934c). – Ed.]

CHAPTER 8

RADICAL PHYSICALISM AND THE 'REAL WORLD'

[Reply to Moritz Schlick's 'On the Foundation of Knowledge' (1979b) and Thilo Vogel's 'Bemerkungen zur Aussagentheorie des radikalen Physikalismus' (1934). Reference will also be made to Fred Bon's 'Der Gegenstand der Psychologie' (1934).

The point of view represented below is more fully developed in three of Neurath's papers in this volume: 'Physicalism', pp. 52–57, 'Sociology in the Framework of Physicalism', pp. 58–90 (particularly sections 1 and 2), 'Protocol Statements', pp. 91–99 as well as in *Einheitswissenschaft und Psychologie* (1933).

In addition cf. Philipp Frank's *Das Kausalgesetz und seine Grenzen* (1932) as well as Rudolf Carnap's most recent publications [i.e., up to 1934], a short summary of which appears in *Die Aufgabe der Wissenschaftslogik* (1934a).]

INTRODUCTION: SCHLICK'S OBJECTIONS

Logical analysis of the scientific language helps us to overcome difficulties; above all it reveals many problems of philosophy as metaphysical pseudo-problems. A representative of the Vienna Circle once expressed the opinion that each of us was better at noticing metaphysical residues in his neighbour than in himself. If he is right, we are expanding the common sphere of logical empiricism if we help one another overcome such residues.

Moritz Schlick has attacked fundamental formulations of radical physicalism with some sharpness in his essay 'On the Foundation of Knowledge'. At other times he has liked to stress, along the lines of certain fundamental ideas of the Vienna Circle, that there are only two classes of philosophical problems: the questions of the one class are basically answered by science, the questions of the other are pure pseudo-questions – meaningless word connections – and thus there are no special philosophical questions at all, especially not those in which scientific knowledge itself is raised as a problem. In his latest essay, however, he has written precisely about this problem.

A systematic criticism of Schlick's comments should first make the attempt

Translation of Neurath 1934 [ON 218].

to define the scientific language used by him. However, here for the purpose of orientation, I want to limit myself to showing which of Schlick's tenets we have to reject necessarily and for what reasons, in order, then, to make progress by positive investigations on common ground.

In his objections against radical physicalism, Schlick has used examples from my comments on protocol statements and their position in science, and from my rejection of a confrontation between 'knowledge' and the 'real world'. However, he does not give the reader a clear picture of my formulations; he does not stress their *physicalist* and *empiricist* character but characterises them as the 'well-known' theory of 'coherence' (p. 374); moreover he classes my view with the 'general' coherence theory and makes 'short shrift' of both together, while mine could at least be classed *with* the variety which represented the 'economy standpoint' (p. 377) to whose rejection Schlick grants extenuating circumstances.

Years ago Schlick himself (1910) showed how the view that truth consists of the "conformity of thinking with itself" has its place within idealistic metaphysics. Whether such an opinion is put forward by certain followers of Kant or by the English representatives of idealistic metaphysics, it is always interlocked with reflexions on the soul, on the absolute, or on similar 'metaphysical objects'; at best, the metaphysics is treated separately.

Precisely for the purpose of evading such idealistic metaphysics, physicalism tries to replace pseudo-content statements (Carnap's 'content language') by statements about language conventions (Carnap's 'formal language') and, as to the rest, to make additions to the content statements of science. It tries to express geology as well as sociology, mechanics as well as biology, and likewise 'statements on statements' in the *physicalist unified language*.

In harmony with consistent empiricism, one tries again and again to refer back to 'experience'; however, this easily leads to a doctrine of 'personal experiences' which then declines into idealistic metaphysics. In order to escape from this I suggested avoiding the term 'personal experience' and using the term 'experience statement' instead. I showed that one can formulate the experience statements ('observation statements' called 'protocol statements' if carefully worded) – in physicalist language and avoid a special 'phenomenological language' – for example like this: "Charles' protocol in the time interval around 9 hours 14 minutes at a certain place: Charles' formulation ('thinking', 'statement thinking' – better than 'speech thinking' because this term reminds one too much of the specific doctrines of the American behaviourists) in the time interval around 9 hours 13 minutes was: there was a table in the room during the time interval around 9 hours 12 minutes 59

seconds perceived by Charles." The suggestion here is to replace the term 'I' – to avoid traditional pseudo-problems – by mentioning the observer's name twice in careful formulations; this is, moreover, perfectly on the lines of children's language. By declaring that the terms 'here' and 'now' can in principle be replaced by definitions of place and time, we avoid a great many pseudo-problems. With the help of hypotheses one can proceed from protocol statements to predictions which are verified by protocol statements in the last resort. In opposition to Schlick, I advocate the following tenets:

(1) All content statements of science, and also those protocol statements that are used for verification, are selected on the basis of decisions and can be altered in principle.

(2) We call a content statement 'false' if we cannot establish conformity between it and the whole structure of science; we can also reject a protocol statement unless we prefer to alter the structure of science and thus make it into a 'true' statement.

(3) The verification of certain content statements consists in examining whether they conform to certain protocol statements; therefore we reject the expression that a statement is compared with 'reality', and the more so, since for us 'reality' is replaced by several totalities of statements that are consistent in themselves but not with each other.

(4) Within radical physicalism statements dealing with 'unsayable', 'unwritable' things and events, prove to be typical pseudo-statements.

These concern our four main points against which the following tenets of Schlick have to be put:

(1) Radical physicalism lacks the 'firm ground of absolute certainty' (p. 370, 383).

(2) Radical physicalism lacks an 'unambiguous criterion of truth' (p. 376).

(3) Radical physicalism does not speak of 'agreement conformity between knowledge and reality' (p. 376).

(4) Radical physicalism does not acknowledge that "an authentic affirmation [*Konstatierung* i.e., pre-statement awareness] cannot be written down" (p. 386).

We want to deal with these points one by one.

1. *'Absolute Certainty'*

Schlick praises the endeavour of all who search for the "bedrock which is there *before* building commences, and does not itself sway" (p. 370). With this he perhaps continues a view that is today obsolete in science, which

presupposed the structure of the world as being in horizontal layers of different evaluation. 'Above', was supposed to be heaven, 'underneath', the earth, itself assumed to be 'carried'. If theologians put forward the argument that the world must have a carrier – namely God – the counter-question was not far, which was the carrier that God needed. Schlick, too, will probably admit that all these are pseudo-formulations.

According to modern physical thinking, whose significance for our total conception was recognised very early, and precisely by Schlick, there is no metaphor that operates with something like 'above' and 'below'. Everything is in connection with everything else, relatively simple connections are, if possible, replaced by even simpler ones. In spite of this Schlick looks for statements that are "absolutely assured" (p. 373), "immune from all doubt" (p. 379), in short for the "absolutely certain foundation of knowledge" (p. 383). Schlick thinks that a "strange relativism" results if protocol statements are regarded as empirical facts (p. 373); obviously an argumentation is to be declared unusable by denotating it as 'relativistic' and, similarly, as 'contradictory'.

Schlick sees in affirmations, i.e., in the "pure observation statements", the "absolutely fixed", "unshakeable points of contact between knowledge and reality" (p. 387). If this is supposed to be a statement of logic, then the fact that it appears to be a content statement through its wording should be avoided. If it is supposed to be a scientific statement, then the terms 'unshakeable' or 'indubitable' (p. 384) have to be defined adequately. At one place (p. 370) Schlick stresses that the term 'uncertain' can only be connected with 'statements', not with 'facts'. In more precise language we shall replace the term 'facts' by the term 'statements' so that the distinction proposed by Schlick becomes redundant. It leads to that confrontation of 'knowledge' and 'reality' which we reject in principle.

Schlick refers to 'analytic statements' for the explanation of these certainty terms. We shall admit without reservation that analytic statements (cf. Carnap's proposal concerning the definition of 'analytic', 'contradictory', 'synthetic') are treated differently from content statements. While to make progress at all, of course, we have to choose between several equally possible groups of content statements and do this on the basis of a decision, such a decision is unnecessary in each logical statement (that, for example, a system of statements in a given language is consistent). *This distinction is essential.*

But though such a decision is discarded in the sphere of combinatorial analysis, this does not mean that the statement 'this system of statements is consistent' is more 'certain' under all circumstances than, for example, any

statement in optics, if we understand 'certainty' to mean the greater or smaller probability that we shall alter a statement. In logic and mathematics we are dealing each time with *one* possibility *in principle*, in the sciences with *several* possibilities *in principle* that are in competition with each other.

The statements of logic and mathematics are also not 'certain'; and if Schlick thinks that I have only understood a statement when I know whether it is analytic or synthetic (p. 384), what about the case when I declare a statement to be analytic today and reach another opinion tomorrow, declaring that I had been wrong and not understood the statement yesterday. I have no means available at all to reach a final verdict about whether a statement was understood by me or not – this is a typical pseudo-formulation. When one asks someone why he corrects a mathematical or logical proposition, he will refer in the last resort to protocol statements in which it is said that it could be noticed that if certain signs are combined, a result is reached that is different from the one assumed before.

Following the view advocated here, Karl Menger has turned against Poincaré who declared certain logico-mathematical measures to be defective because they corresponded to the behaviour of a shepherd who wanted to protect his flock from wolves by surrounding it with a fence, without being quite sure, however, whether he had perhaps also fenced in a wolf. Menger rightly stresses in 'The New Logic' (1979, p. 41) that

> This activity of the mathematician is not in general contradiction-proof. He is not sure that he has not enclosed Poincaré's wolf within his fence. But that Poincaré lays this situation to the mathematicians' charge is due to the fact that he demands from mathematics a certainty surpassing that of all other human activities not only in degree but in essence.

Similar views are advocated by Brouwer (1928, p. 157), who otherwise does not always agree with Menger at all.

'Certain' is to be defined as a term within the doctrine of human 'behaviour'. When discussions about these problems become pointed, one should say, for example, as a measure of precaution: "Charles makes the following mathematical statement: ... Francis replies: this statement is not very *certain*, for Charles' calculation is somewhat superficial." In this way mathematics is not made at all 'psychological'; but care is taken that extra-logical terms are not occasionally treated like logical ones.

It is a common quality of logical statements and content statements that they can be incorporated into groups of statements without contradiction, or that they are in contradiction with them. Schlick claims at first that all

analytic statements *eo ipso* are "indubitable" (p. 384), and that the same can be said of observation statements: "both have absolute validity" (p. 385). And thus Schlick arrives at the version that does not admit a stricter formulation:

> But whereas in all other synthetic statements, establishing the meaning and establishing the truth are separate, clearly distinguishable processes, in observation statements they coincide, just as they do in analytic judgements ... The analytical or tautological proposition, however, is at the same time devoid of content, whereas the observation statement gives us the satisfaction of a genuine acquaintance with reality (p. 385).

Here obviously human behaviour is discussed within the framework of science – situations full of pleasure or displeasure ('satisfaction').

2. "*Unambiguous Criterion of Truth*"

Schlick sees the main deficiency of our view to be in our fundamental acknowledgment that science is *ambiguous – and is so on each level.* When we have removed the contradictory groups of statements, there still remain several groups of statements with differing protocol statements that are equally applicable; that are without contradictions in themselves but exclude each other. Poincaré, Duhem and others have adequately shown that even if we have agreed on the protocol statements, there is a not limited number of equally applicable, possible systems of hypotheses. We have extended this tenet of the uncertainty of systems of hypotheses to all statements, including protocol statements that are alterable in principle. (How selection is conditioned by the 'simplicity' of possibilities of connection and other considerations cannot be discussed here in detail.)

We select one of the systems of statements that are in competition with each other. The system of statements thus selected is not, however, logically distinguished. With resignation we now could assume that at least the equally applicable totalities of statements for choice remain constant and that we change only our choice. But even that is not true, since the totalities themselves are changed by us.

The practice of living reduces the multiplicity quickly. The unambiguity of the plans to be put into operation enforce the unambiguity of predictions. Furthermore we are restrained by the views of our environment. An individual hardly has also the power to work out *one* system properly, let alone several systems, in which, moreover many unprecise terms ('clusters' [*Ballungen*]) occur that I best characterise by the nature of their application. In general

one can only very inadequately try out the usefulness of different systems of scientific hypotheses; this is similar to the case where the effect of different railway systems on our total life situation cannot be tested.

If one considers that in protocol statements the name of the observer and terms of perception occur that are highly imprecise, that furthermore the content of protocol statements depends on the definition of these terms in the competent sciences, the ambiguity will not surprise us from the start. Nothing is more misleading than the supposition also suggested by Schlick that protocol statements could be taken to be "those propositions which in absolute simplicity . . . set forth the *facts* . . . " (p. 370).

This doctrine that while logic is unambiguous, real science is ambiguous (neither is 'absolutely certain'), is objected to by Schlick and, in connection with Zilsel, in a similar way by Thilo Vogel. That radical physicalism arrives "at as many internally non-contradictory proposition-systems" as it likes and therefore knows of "no unambiguous criterion of truth" is called by Schlick a "logical impossibility" (p. 376). Something is logically impossible – that is, put more carefully, a certain statement is self-contradictory. Why is the following proposition self-contradictory: we can formulate several groups of content statements that are free from contradictions; among these groups, verified by protocol statements accepted by us, we make a selection on the basis of *extra-logical* factors. Where is the 'nonsense' claimed by Schlick?

Schlick's accusation obviously rests on the fact that he speaks of "the" one reality that can be described only by one of several irreconcilable systems of statements", whereas we stress that this formulation does not occur within scientific language, but that the task is to select one among several possibilities.

In reply to Thilo Vogel it should be observed that, according to our standpoint, we admit not any systems whatsoever, but certainly more than one. I do not believe that neo-Platonism can be represented as a consistent system whose predictions can be verified by protocol statements accepted by us. Nevertheless 'our standpoint' is defined only historically: "physicalism is the form work in unified science takes in our time" ('Physicalism', p. 56).

In spite of Thilo Vogel's questioning I continue to have no misgivings in having the 'truth' of a protocol statement determined by the totality of statements. In the last resort Vogel seems to desire something like the 'atomic statements' or 'elementary statements' of Wittgenstein's metaphysics to which probably also Schlick's metaphysics and the 'absolutely certain' but 'not fixable' affirmations are related. 'Atomic statements' are to be rejected even as 'approximations'. Though we can coordinate precise mathematical

formulations with our imprecise observation statements, the assumption that one would have to arrive at precise elementary statements if one only had sufficient intelligence at one's disposal, leads to a fiction that resembles that of Laplace's spirit – a perfectly metaphysical notion. (See Neurath 1973b, p. 404.) Here too, we have a metaphysical endeavour to put the unambiguity of 'atomic statements' or affirmations as the eternal unambiguous reality against the fluctuations of humanly paltry science.

3. *'Conformity with Reality'*

Schlick finds 'absolute certainty' and an 'unambiguous criterion of truth' in his views about the conformity of knowledge with reality. One cannot use the expression 'agreement with reality' (p. 376) even as metaphor, since consistent totalities of statements are under discussion which together must fill the gap in our thinking so to speak, created by our renouncement of 'reality', of 'the true world' and other terms of this kind.

Certainly we too have a court to appeal to, one that is formed by the protocol statements accepted by us; but it is not finally fixed. We do not renounce the judge, but he is replaceable. "All content statements can be traced back to a certain section of the mass of content statements, namely the protocol statements. Within the system of statements the protocol statements are the last to which one refers back" (Neurath 1933, p. 6).

If we incorporate part of the above mentioned protocol, the statement "in the room was a table perceived by Charles" along with the whole protocol into the body of science, then we can speak of a 'reality formulation', whereas we would speak of a 'dream or hallucination formulation', if we accept the whole protocol but not the part "in the room was a table perceived by Charles". With Schlick, on the other hand, a protocol would be a reality formulation if it gives a "correct report of the fact observed" (p. 373). How Schlick defines the single concepts and can build up a syntax that makes this statement possible, cannot be detected.

Where we say that one group of statements contradicts another group, Schlick would like to say it contradicts reality; this is either a content statement or a disguised syntactic statement. In the former case it is not defined as to what it means if a statement logically conforms with a thing or not. We find the consistent application of the principle to always compare one statement with another logically also where, instead of protocol statements, other accepted statements are used as 'control statements' (Carl G. Hempel 1934, p. 52, in conformity with Popper). The renunciation of confronting

all predictions in the last resort with protocol statements, can probably not be maintained without endangering the fundamental *empirical* standpoint. (Carnap is probably of the same opinion in *The Logical Syntax of Language*, (1937, pp. 316–317; 319–320) where the term 'protocol statements' is used, on the lines of our proposal, as meaning the same as "more precisely formulated observation statements" and not as "control statements" in general.)

In the framework of our discussions, statements are compared with statements, not with a 'reality', not with 'things' ('Physicalism', p. 53). This does not mean that one cannot form statements of comparison of another kind, in which designations of statements occur side by side with designations of other things. For example, one can say, of course: the statement "this chair has four legs" contains more words than "the chair has legs." this statement of comparison can, for example, be deduced from two statements: "this statement contains 5 words" and "this chair has four legs". I assume that herewith Thilo Vogel's misgivings (p. 163) are removed. If one formulates it cautiously, one could, with Thilo Vogel, form the apparent content statement ('content language'): "a statement is a thing just as a chair is", a conception, by the way, to which Schlick would not generally agree: "According to this view, protocol propositions would be real occurrences in the world, and would have to be prior in time to the other real processes constituting the 'construction of science', or even the production of knowledge in an individual" (p. 372). Does Schlick think that the protocol statements or his affirmations could ever be anything else but real events, or does the stress lie on 'prior'?

The terms 'statement', 'language', etc., must be defined historio-sociologically. This in no way prevents our construction of a language's logical syntax. Vogel's suggestion that we should explain more precisely when we speak of a statement and of a language coincides perfectly with our own tendencies. The term 'protocol statement' is of the same kind as the term 'chair' or 'star'. Logically one can compare a protocol statement with another statement; as we have seen, however, one can form a statement of comparison in which the word 'protocol statement' and the word 'chair' occur; however, we have no possibility of forming a statement of comparison that in a similar way compares the protocol statement (knowledge) with reality. Schlick, however, must cling precisely to 'the reality' because he operates with the metaphors 'prosecutor, defendant, eternal judge', so to speak, without noticing that these metaphors would only be admissible if not all three persons were represented by himself, but if some transcendental superbeing guaranteed

the "true statements about the real world". (See Frank 1932, p. 258). For Schlick striving after knowledge of the 'true world' is not only 'laudable' and 'sound', but in his opinion also at work among 'relativists' and 'sceptics' who are inclined to be ashamed of it (p. 370). However, if someone is of the opinion that "the 'true world' is a meaningless arrangement of script signs or sounds, he does not see anything sceptical in the refusal to speak of a 'true' world" (Frank 1932, pp. 270, 273).

Thus for us striving after knowledge of reality is reduced to striving to establish agreement between the statements of science and as many protocol statements as possible. But this is very much; in this rests *empiricism*. For if, by our 'resolution', we grant so much weight to the protocol statements that in the last resort they decide the validity of a theory, our 'new scientism', in spite of the stress it lays on logic, does not deviate from the old program of empiricism, which is even reinforced by pointing out that protocol statements too can be physicalist statements. Of course one tries to axiomatise science wherever possible.

However, if Vogel (p. 163) is of the opinion that one has to have the system axiomatised in order to have contradiction revealed, he overlooks the fact that in practice one proceeds much more clumsily and is mostly glad to have some contradiction pointed out or a greater number of conformities. It is precisely the history of physics that shows that our procedures are often quite consciously defective. It happens that occasionally two contradictory hypotheses about the same subject are used at two places with some degree of success. And still, one knows that in a more complete system only *one* hypothesis should be used throughout. We just resign ourselves to a moderate clarification in order to delete or accept statements later. Our approach, free from metaphysics, has nothing to do with Wittgenstein's concept of meaning to which Vogel refers. I do not see how, from this incompleteness, the justification of Wittgenstein's 'statements of elucidation' can be deduced, which are best characterised as 'pre-linguistic discussions on pre-linguistic subjects' to reveal their metaphysical character.

For Schlick 'reality' has its roots in one's 'own' protocol statements. "That a man clings more obstinately to his own protocol statements than to those of another, in general, is a historical fact – without any fundamental significance for our discussion" (Neurath, 'Protocol Statements', p. 97). It can very well happen that, on the basis of longer experience, a man makes little use of certain protocol statements of his own and prefers certain protocols of others. But someone might decide never to give up any of his own protocol statements. As a matter of course one has to use protocol

statements 'about another's protocol statements', but these too can be changed. For Schlick only observation statements are sacrosanct. He finds fault with us that we have no sacrosanct statements at all.

Schlick is of the opinion that if the other's picture of the world does not fit together with my observation statements, I shall just reject it and instead regard the others as "dreaming fools" (p. 380) rather than change my own statements. Schlick speaks here of statements "about the world" (p. 379) and overlooks the fact that these statements can be brought into harmony with any observation statements by introducing enough auxiliary hypotheses, without the need to assume that one was dealing with dreaming fools. The matter becomes difficult only when there is a Charles protocol, "Charles formulates: Charles feels pain", and now this Charles has to deal with the claim: "there is no Charles and no Charles protocol". Now hypotheses would have to be made to explain how the others can arrive at this formulation. However, it can hardly be assumed that Charles comes to the formulation, 'there is no Charles'. One defends oneself against being transformed into the undreamt dream of the others, and indeed one does not even want to be the dreamt dream of the others. But these metaphors show us also how little all this affects our attempt to construct science as a system of statements that is as consistent as possible, always ready to make changes if necessary. Never is even only *one* sphere of science completed, as Schlick thinks (p. 378); all are connected with all others and somehow participate in the general incompleteness and uncertainty.

In stressing one's 'own' protocols, it is mostly overlooked that the comparison of two of one's own protocols at two points of time does not differ from the comparison of one of one's own protocols with that of another and that therefore even before Friday came, Robinson could make all the comments that we have made above. Schlick hopes at least to be able to give preferential treatment to the *Konstatierungen* [affirmations] as reality substitutes by ascribing to them only 'momentary' duration. The tendency to master a difficulty with the help of an extensionless point can be found, with respect to space, in Descartes when he suggests that soul and body touch each other at a single place, the pineal gland, or, with respect to time, when Büchner declares the spirit to be a product of the brain that consumes itself at the moment of its birth. In conscious opposition to this, I have introduced the concept of duration into my proposal concerning the formulations of protocol statements from the start, in full correspondence to physicalist notions. But these are problems of behaviouristics (psychology) and not of the logic of science. (See Neurath 1933, p. 17).

4. "*A Genuine Affirmation* [Konstatierung] *Cannot Be Written Down*"

Schlick's attempt to secure absolutely certain knowledge of reality induces him again and again to leave the sphere of logic and to slide into another one. If we do not want to count this as metaphysics, we must try to understand it as a section from Schlick's 'behaviouristics of knowledge'. Let us start from the following formulation of Schlick:

> . . . the mental acts of judgement appear fitted to serve as a basis for intersubjectively valid knowledge only after having been translated into oral or written expression (i.e., into a physical sign-system) . . . (p. 372).

It is not quite clear whether Schlick regards the acts of judgment as rows of signs that can be translated into other rows of signs. For only in this case does it probably make sense to say that the written expression expresses the same as the act of judgment or something else. Schlick's strongly emphasised antithesis, 'psychical' – 'physical', cannot be said to serve as clarification. At least we see that Schlick distinguishes between the act of judgment and the act of making a protocol. How he arrives at the statements about the act of judgment cannot be recognised clearly. – Schlick's behaviouristics of knowing can be sketched somewhat like this:

Statement a:
At the moment t_1 *MS* formulates, in the state of anticipation: *MS* will perceive blue at the moment t_3 and is prepared for this event.

Statement b:
At the moment t_2 *MS* is observing.

Statement c:
At the moment t_3 the affirmation (an observation statement) inserts itself between observation and protocol: "Here now blue" that has no duration and cannot be written down (p. 386); simultaneously a state of satisfaction arises in *MS*, if the observation statement (that generally is not a pure one – p. 381) conforms with the prediction.

Statement d:
At the moment t_4 *MS* notes the protocol statement: *MS* made at the moment t_3 the affirmation: 'Here now blue' (equivalent to: 'perceived blue').

Statement e:
At the moment t_5 *MS*, filled with satisfaction of authentic knowledge of reality, makes inductions.

Statement f:
At the moment t_6 *MS* formulates a new prediction in the state of anticipation, etc.

We feel urged to formulate at least statement c in a basically different way, namely: at the moment t_3 *MS* makes the affirmation 'here now blue'. This however would be in contradiction to Schlick's strict direction for this moment:

"When I make the affirmation 'Here now blue', that is *not* the same as the protocol proposition 'On such-and-such a date in April 1934, at such-and-such a time and place, Schlick perceived blue'" (p. 386).

But the latter statement is a hypothesis and as such is qualified by uncertainty. The last statement is equivalent to the statement: "*MS* made the affirmation 'here now blue' (here indication of place and time have to be given). And that this statement is not identical with the affirmation mentioned in it, is obvious."

But it becomes increasingly difficult to follow Schlick's directions, for example, when he declares: "A genuine affirmation cannot be written down, for as soon as I put down the demonstrative terms 'here' and 'now' they lose their meaning. Nor can they be replaced by an indication of time and place . . ." (p. 386). Does this not represent a typical pseudo-formulation of the kind "there are things that cannot be formulated", that is "there are statements that are not statements"? (See (Carnap 1937) for his detailed criticism of such formulations.) This is similar to the type of formulations that we find in Fred Bon. "The content of consciousness as such can only be experienced as the originally and immediately given but not defined as such" (p. 364). And "a judgment like: 'I experience such and such a content of consciousness' cannot be reinterpreted into a statement about a physical state of affairs without losing its proper meaning" (p. 374).

However, if we make an effort to interpret Schlick's pronouncement as possibly being not metaphysical, we would suggest writing as follows, disobeying Schlick's direction:

Statement c:
At the moment t_3 between *MS* observing and *MS* protocolling, an *MS* in the state of affirmation is inserted, and *MS* then affirms "now here blue".

Now one could add the certainly unusual, but still physicalistically possible, statement that *MS* cannot make any further use of the observation

statement, but replaces it by another statement that takes the place of the statement he can never write down. The question remains unanswered as to how one arrives at this strange hypothesis and what the source of our knowledge is that *MS* has thought something that he could not write down. We would try to verify this strange hypothesis by protocol statements. At first we do not see which predictions it would be used for.

Schlick claims that each time that the affirmation, which obviously must therefore be a statement, agrees with the prediction – who observes this is not even hinted at – a feeling of satisfaction sets in; this may have a certain relationship to the feeling of evidence in traditional philosophy. A consistent empiricist would suggest experiments to test this hypothesis. Perhaps there are people in whom the feeling of satisfaction arises just when a prediction is not confirmed by an affirmation. Who is to decide when we want to apply the term 'satisfaction of true knowledge of reality'? Schlick stresses that the assumption that the satisfaction and the making of the affirmation are simultaneous, is "of utmost importance" (p. 382). With the help of which stop watches is such simultaneity to be assured? The crucial question is obviously how the term 'affirmation' [*Konstatierung*] is used, and what the special phrasing of '*Konstatierungen*' should be. Schlick stresses (p. 385) that the "*demonstrative* terms [have] the meaning of a present gesture", and then again he expresses the opinion, on the contrary, 'this there' has meaning only *in connection with* a gesture. "In order, therefore, to understand the significance of such an observation statement, one must simultaneously make the gesture, one must in some way point to reality" (p. 385). If one does not assume that this is a typical case of the metaphysics of duplication, these sentences would say that there are cases in which word-language has to be linked with gesture-language, but that the translation of sentences of this combined language into pure word-language was impossible. But does this reproduce the meaning of Schlick's comments? And when Schlick points out that the later protocol statement deviates more or less considerably from the affirmation, this seems to contradict the claim that the latter is of only momentary character and therefore probably remains unknown. Perhaps these endeavours of Schlick's can better be appreciated by comparison with certain formulations of the phenomenologists. It is certain that they do *not* affect scientific formulations, they are therefore alien to science; but we join together precisely for the purpose of furthering science; even such 'resistance' leads us a little away from our main road.

Should it be shown that these comments of Schlick's cannot be granted strictly scientific significance, Schlick's scientific comments would not

be affected by this. It is probably hardly a coincidence that just at the most doubtful places in his article discussed here, Schlick makes use of teleological phrases and speaks of the 'mission' that the short-lived affirmations have to fulfil (p. 381). It looks as if here we come across the last residues of a network of metaphysics; when this has been swept away we probably shall have to deal only with diffuse metaphysical elements in our scientific establishment, beside the multitude of errors we try to remove.

Almost in the sense of the 'East-West Divan',[1] Schlick praises 'burning' and 'knowing' like a 'dying and becoming' with the words:

> These moments of fulfillment and combustion are of the essence. From them comes all the light of knowledge. And it is this light for whose source the philosopher is actually asking, when he seeks the foundation of all knowledge (p. 387).

I, however, would like to declare quite simply: one may well like such poetry; but in conformity with many other of Schlick's comments – the advocate of a radical physicalism in the service of science *will not claim to be a philosopher in this sense.*

NOTE

[1] [Goethe's *Der west-östliche Divan*, a collection of poems written in 1814–1815. The 'East-West' of the title reflects Goethe's interest in the enrichment of Western civilization by Arab culture; '*Divan*', meaning collection, was taken from the fourteenth century Persian poet Hafiz (the name, in Persian, meaning one who knows the Koran by heart) one of whose poems is the source of the *Divan* poem that ON probably has in mind, namely 'Seelige Sehnsucht', which ends:

> Und so lang du das nicht hast,
> Dieses: Stirb und werde!
> Bist du nur ein trüber Gast
> Auf der dunklen Erde.

(And until you have grasped this – 'Die and be transformed!' – you will be nothing but a sorry guest on the sombre earth.)

Translation taken from *The Penguin Book of German Verse*. Edited by Leonard Forster. Baltimore: Penguin, 1961. p. 227. – Ed.]

CHAPTER 9

THE UNITY OF SCIENCE AS A TASK

The best discussions are between people who, by and large, are of the same opinion. That is why we advocates of 'scientism' – I gladly accept this name – try, through planned debates, to overcome old obscurities in our 'logical empiricism', to detect new ones, in order to start new clarification. As *scientific* people, we are prepared to check all our tenets by observation statements, but also – far removed from every absolutism – to alter the principles on which the checking is based, when this seems necessary. But for our attempt at a common procedure *uniformity* is needed. Is this uniformity the logical consequence of our program? It is not; I stress this again and again; I see it as a *historical fact* in a sociological sense.

I am inclined to think that even if my preferred formulation of our program had been generally adopted – I can hardly assume more – the multiplicity of science would be possible; even then the uniformity needed for collective work and communication could only be reached historically, by special decisions or by life on a common social and technical basis. After the removal of traditional metaphysics, in constant struggle against metaphysical leanings, positive work could be our occupation, namely the creation of an *encyclopedic* synthesis of the sciences on uniform logical foundations. We would establish the 'cross connections' from science to science and thus create a structure that knows no 'philosophy', no 'epistemology' with special propositions – whichever one of these two is applicable has found its place either in the 'logic of science' or in 'behaviouristics': *the program of unified science*. When we are asked how we arrive at a statement we can reply: "look at the 'ornaments' that we create from symbols" or "look at the experiment that we are making": *the program of empiricism*. We always formulate observation statements (when carefully constructed, called 'protocol statements') and compare their logical content with that of other statements. In this way we can use a uniform language that corresponds to the language of physics, particularly because the protocol statements also can be formulated in this uniform language. We can replace the term 'I' by a personal name, the term 'here' and 'now' by data of place and time.

Translation of Neurath 1935a [ON 219].

'Men', 'yardsticks', 'clocks', 'water', 'animals', 'stars', these are terms that we link with terms such as: "are in the state of motion", "state of observation", etc. Statements such as "the photo-camera records" or "Charles observes" are of the same kind: *the program of physicalism.*

One may try to accomplish all these programs as systematically as possible and to carry out the logical construction as carefully as possible – we do *not* arrive at '*one*' system of science that could take the place of the 'real world' so to speak; everything remains ambiguous and in many ways uncertain. *'The' system is the great scientific lie.* Not even as an anticipated goal is it a useful guiding thought as it takes us close to Laplace's spirit which, it is thought, with knowledge of all the equations of the sciences, constantly makes correct predictions: an assumption that serves no prediction, an assumption that cannot be verified in any way; metaphysical formulations, useless for the purposes of science ('isolated' trains of thought in Reach's tolerant terminology). *Multiplicity* and *uncertainty* are essential. From the data at our disposal we can, in more than one way, deduce predictions that are in harmony with science; the multiplicity of predicting cannot be excluded by any method; no degree of systematic procedure can alter this. One can, so to speak, not agree on a 'machine' that unambiguously produces 'inductions' in the wider sense. The progress of science consists, as it were, in constantly changing the machine and in advancing on the basis of new decisions. Still, the result in fact is far-reaching unity that can *not* be deduced logically.

A few points may be mentioned. Our predictions proceed like this: if the data A and the data B are given, C can be predicted, but if also in another case the data A are given and the data B_1 in place of the data B, C can be predicted in the same way. For example, one says the sun has a certain location, therefore after some time it will have a certain other location, as was the case previously, though formerly the observation statement was added: the air has a bluish shimmer, and this time: the air has a grey shimmer. That, from partly equal premises, partly equal predictions can be made, makes our science possible. Everything is simpler than one could imagine, though also more complicated. But even the initial statements of successful science are not fixed, since one could begin at the beginning with different unified languages that cannot be translated into each other straight away. And even if the unified language were more or less fixed – in fact the statements of yesterday and today, appearing at the beginning and at the end of a book, belong to often slightly differing languages – nevertheless, to make good predictions, we could set out from different observation statements

that we select from the large number at our disposal that can be steadily increased. What one person neglects as unimportant – and then he shapes his concepts accordingly – may seem essential to another for the predictions. For example, Goethe strongly criticised Newton for omitting certain blurred margins of the spectrums as unimportant, whereas he himself started from this very point.

This is how matters stand in *every 'layer'* of scientific work, not only in the narrower sphere of systems of hypotheses, as Poincaré and Duhem have pointed out with such intensity. But these initiatives in multiplicity are constricted by life. A whole human lifetime is hardly long enough to immerse oneself in even a single view and to give full thought to its consequences. And how soon one senses the weakening effect of isolation. Thus one deserts the lonely, though perhaps auspicious, notions of an outsider to join in the work in a way of thought that enjoys more support and has therefore better chances of greater scientific achievement. In such ways it happens that not even too many possibilities are treated by several groups at the same time: through adaptation and selection a kind of assimilation of whole generations takes place – not to speak of the cases in which certain trains of thought are anathema, persecuted and suppressed.

This insight that a logically tenable multiplicity is reduced by life has little hope of response because it contradicts the usual view of a connection between achievement and 'success'. The representatives of a victorious doctrine are too much inclined to believe that their victory could be justified as it were by closer logical investigation. Many see the course of the history of science like that. There Ormuzd fights with Ahriman, the wave theory with corpuscular theory, and whoever does not fit into this dichotomy is perhaps even relegated to the shadowy life of a weakly eclectic.

Looking at the development historically, we should from the start push certain theoretical aids into the foreground and give importance to the correlations between certain predictions, but not to the more or less fanciful 'images' of individual systems of hypotheses which increase the multiplicity unnecessarily. How enormous is the achievement of the advocates of the corpuscular theory, of a Malus, a Brewster, for the most complicated parts of optical theory, for the theory of polarisation. And only now, when a combined theory has gained victory so to speak, will one be able to do more justice to a man like Biot in whose work we find statistical notions within the framework of corpuscular theory.

Various possibilities offer themselves, and even more are only vaguely conjectured, and only little of this takes shape in science. This restriction

by life corresponds to the behaviour of the active man who chooses one of several possibilities – the act called planning. But such unambiguity of decision and action is *not* the logical result of some premises that lead to *one* single prediction about the success of the action, but rather the result of life taken as a whole or, under certain circumstances, of drawing lots. This conception of the multiplicity of scientific theorising and predicting, based on the multiplicity of possible protocol statements, must be advocated, and especially against the view that I wish to characterise as *'pseudorationalism'*. The pseudorationalist discredits logical empiricism when he wishes to bring the unambiguity of action into connection with an unambiguity of a deduction from the data of experience, and when he refers to 'the' real world, this unambiguous something that many suggest using, at least as a notion of a limit. It is the pseudorationalist who likes to speak of the 'simplicity' of certain initial elements – statements or concepts – of the 'exactness', of the 'certainty' due to certain statements. And though we advocates of 'scientism' are striving with care, and as systematically as possible, to formulate statements that are as exact as possible, as permanently useful as possible, as simple as possible, we still know that basically 'everything is fluid', that multiplicity and uncertainty exist in all science, that there is no *tabula rasa* for us that we could use as a safe foundation on which to heap layers upon layers. *The whole of science is basically* always under discussion. And if we as empiricists refer to observation statements in which it is said, "Charles sees the mercury at scale point 30", the terms 'Charles', 'sees', etc., are already representatives of the whole of science. But we empiricists start from such 'rich' statements and with effort derive simpler statements.

And if we maintain all this against the pseudorationalists, we have also to stress that even a term of chemistry such as 'H_2O' cannot just be treated as equal with the term 'water' which appears, for example, in a statement that says "we have distilled water in the laboratory." The *uncertainty* of all terms, sometimes greater, sometimes smaller, belongs to the nature of language. On it rests part of the efficiency of language. We must not forget either that we are constantly working with terms which we know are good at providing certain predictions, but on the other hand lead to yet unresolved contradictions at other places. Also we must get used to working with terms which we do not exactly know are 'usable' or 'not usable' – perhaps metaphysical. By sharp criticism one can hope to remove only some coarse errors and certain coarse nonsense; much remains uncertain at first, *though one cannot do without it*. One's back is *never* completely free, and working with 'dubious' statements has to be learned.

If, in spite of these comments on multiplicity and uncertainty, one sets unswervingly to the work that is seen as a common one, one can do so only because one knows how much the historical situation reduces the manifoldedness *via facti*. How great is the centuries-old tradition present in language, in other behaviour, how greatly developed is the terminology that – rightly or wrongly – separates the spheres of the senses. How many generations have ever again given 'training' to their children, that these be handed on. These are behaviouristic problems that deserve to be investigated from closely related quarters (cf. Arne Naess) in connection with the rich literature that approaches these problems from another side. Unification of argumentation and unification of science is related to the unification of our technology in production, transport, war, with which other connections probably exist. Mechanics, chemistry, optics are connected with machine engineering which today is far more international than social engineering. Therefore the social sciences are much less uniformly developed; moreover, for certain reasons, in social measurements theological and metaphysical formulations play a considerable role as well as uncritically applied (natural-) scientific formulations.

Therefore whoever discusses *the unity of science* as a possible task starts from the assumption that cooperative work is increased, that within mankind scientific thinking will win out more and more in all spheres. If he cannot give specific reasons for this but only has mere hopes, he must understand that this is *a historical matter*. Though the store of predictions that are of further use may grow with the ability to produce new ones, the development of hypotheses and of the scientific systems need not proceed continuously; there can even be sharp setbacks, but nevertheless, creation of the *Encyclopedia of the Logical Foundations of Unified Science*, creation of a *New Encyclopedia of Scientism*.

Here is a great task of anti-metaphysical empiricism, that above all makes it sharpen the logical instrument in such a way that it can serve science immediately. The task is to repulse traditional metaphysics, especially traditional teleology, traditional anthropomorphism in a new shape, in order to create a unity of science that comprises geology as well as ethnology, astronomy as well as sociology, mechanics as well as biology and behaviouristics. And if we are soon to gather at the "First International Congress for the Unity of Science", we do so not only to advocate scientism, but also to stress our resolution to work together for the logical development of science. Thus the Parisian friends, who have suggested that we come to Paris, can welcome us in their old tradition with the call for: *unité et fraternité*.

REFERENCE

For further references see 'The Lost Wanderers of Descartes and the Auxiliary Motive', pp. 1–12, and 'On the Classification of Systems of Hypotheses', pp. 13–31 as well as (Neurath 1973a (in particular Section 4, pp. 197–213), 1973e, 1932, and 1933). See also (Ichheiser 1930). In addition to Philipp Frank and Rudolf Carnap, see also certain publications of the Lemberg-Warsaw school and the Dutch group around Brouwer and Mannoury.

CHAPTER 10

PSEUDORATIONALISM OF FALSIFICATION

Popper's *Logik der Forschung* (1935)* (see Reichenbach 1935 and Carnap 1935b) contains many remarkable passages whose significance for the logic of science has already been acknowledged by Carnap. But by a certain kind of *pseudorationalism*, Popper blocks his own way to a full appreciation of the practice of research and the history of research to which his book is basically devoted. Namely, he does not use the *ambiguity* of all factual sciences as the basis of his comments, but, following Laplace's spirit, as it were, aims at one unique distinguished system of statements as the pattern or paradigm of all the factual sciences.

One can enter the debate without much preparation because Popper fortunately pursues certain basic ideas that were developed within the Vienna Circle, especially in connection with physicalism, in order to overcome the metaphysics of 'finality'. The basic ideas to which Popper's attitude is, on the whole, close, are approximately these: In logically analysing the factual sciences as masses of statements, our starting point is that we can change all factual statements that are constructed similarly to those of physics, as well as 'protocol statements' under certain circumstances. In the effort to get consistent masses of statements, we discard certain statements, alter others, without, however, being able to start from absolute 'atomic statements' or other conclusive elements.

1. POPPER'S MODELS

Although Popper on the whole advocates similar views and thus avoids certain errors, he on the other hand still uses well-defined theories built up of clean statements as models, so to speak, of the factual sciences. Through the form of his 'basic statements' is defined what is to be regarded as an empirical, that is a 'falsifiable', statement (p. 87). Theories, according to him, are tested by basic statements that were acknowledged beforehand for the time being (p. 109). They are rejected if these basic statements "confirm a falsifying hypothesis" (p. 87, 88n). *Falsification* is the basis of all of Popper's further

Translation of Neurath 1935b [ON 220]

comments. His thoughts are constantly circling around a certain ideal; though he does not call it attainable, he uses it somehow as a model when he wants to come to an understanding of what it means that an empirical scientific system runs aground on '*the*' experience (p. 41). To this end, according to him,

> [an easily falsifiable] theory would describe 'our particular world' as precisely as a theory can; for it would single out the world of 'our experience' from the class of all logically possible worlds of experience with the greatest precision attainable by theoretical science. All the events or classes of occurrences which we actually encounter and observe, and only these, would be characterized as 'permitted' (p. 113).

Again and again the approximation to this general system plays a part in Popper's comments, as we shall see.

2. ENCYCLOPEDIAS AS MODELS

We, on the other hand, try to use models that give no scope at all to thoughts of an ideal of this kind. We start from masses of statements whose connection is only partly systematic, which we also discern only in part. Theories and single communications are placed side by side. While the scholar is working with the help of part of these masses of statements, supplementary additions are made by others, which he is prepared to accept in principle without being quite certain what the logical consequences of this decision might be. The statements from the stock with which one really works use many vague terms, so that 'systems' can always be separated only as abstractions. The statements are linked to each other sometimes more closely, sometimes more loosely. The interlocked whole is not transparent, while systematic deductions are attempted at certain places. This situation is not open to the idea of an 'infinite regress', whereas Popper has to reject it especially in a certain connection (p. 90). If one wants to say that Popper starts from *model-systems*, one could say that we, on the other hand, start from *model-encyclopedias*; this would express from the outset that systems of clean statements are not put forward as the basis of our comments.

3. NO GENERAL METHOD OF 'INDUCTION' AND 'TESTING'

We believe we are doing the most justice to scientific work if, in our model construction, we set out from the assumption that *always the whole* mass of statements and *all methods* can come under discussion.

Certainly we demand of an empiricist that he accept only encyclopedias within which predictions must conform with protocol statements; by our work we can also be led to alter slightly the form of the protocol statements. For while the form of the protocol statements may be more or less established beforehand, the individual protocol statements that are characteristic for a certain encyclopedia and function as test-statements, are not previously distinguished. For the purpose of the model discussion one should realise that for some scientific work, use is made of one of the various encyclopedias that are considered consistent. By thus accepting one definite encyclopedia, definite theories, hypotheses, predictions and their test-statements have been accepted as well.

Various factors determine the methodical scientist in his choice of a model. We deny that the encyclopedia preferred by the scientist can be logically selected by using a method that can be only generally outlined. Together with this we not only deny that there could be general methods of 'induction' for the factual sciences, but also that there could be general methods of 'testing' – however, Popper advocates just such general methods of 'testing'. In our way of viewing things 'induction' and 'testing' are linked much more closely than in Popper's. Though we reject a model of science as being a closed system with such general methods, we are still of the opinion that every presentation of scientific research must attempt to present the methods applied in as explicit detail as possible and above all to give adequate credit to each formation of theoretical systems within an encyclopedia. It may happen that certain of Popper's trains of thought that claim the greatest generality, have some special value for special problems of research, within a narrower framework of the kind we have hinted at. In his attack on Reichenbach's works, Popper himself seems to overlook entirely the fact that in spite of their tendency to establish a general theory of induction, they obviously are valuable for scientific research within a more limited space.

4. SHAKING, AND CONFIRMATION

Whereas Popper does not want to treat 'induction', this "unfounded anticipation" logico-systematically, not even in its special forms, he tries to characterise falsification logically, as a general method, as strictly as possible – though he must admit that this cannot be done precisely – and to base the whole logic of scientific research uniformly on it.

When Popper replaces 'verification' by 'confirmation' of a theory, we replace 'falsification' by 'shaking' of a theory. When a scientist has chosen a

certain encyclopedia (mostly characterised by certain rather general theories that are missing in other encyclopedias available) he will not be induced at once by any negative results to sacrifice a theory, but he will first give careful thought to what the encyclopedia, which he would give up together with the theory, might have been able to achieve for him in the future. Negative results can *shake his confidence* in an encyclopedia, but not reduce it automatically to zero so to speak through the application of certain rules.

We can very well imagine that a falsifying hypothesis that Popper would call 'confirmed' is pushed aside by a successful scientist because, on the basis of very serious general considerations, he deems it an impediment to the development of science that itself would show how this objection is to be refuted. Such a decision may be difficult to make; it is certainly not supported by Popper's tendency always to envisage sections as falsifying entities, and not the total encyclopedia.

When a traditional total view is threatened, Popper's stand is, in principle as it were, on the side of the aggressor. It would be very interesting to show what the defensive motions of the practitioners are in such cases. The practitioners of research are at first especially seriously disturbed by such a change. Popper, however, sees the main resistance not in such practitioners and their general attitude, but among the conventionalists (pp. 41, 79–82, etc.). He sketches a type of conventionalism that is perhaps discussed among school philosophers and may occasionally occur among philosophising theorists, but is hardly characteristic of men engaged in the practice of research. This would have to be discussed in conjunction with the history of the sciences.

Moreover the reason why a cautious scientist accepts an encyclopedia with certain theories cannot be generally determined by Popper's 'simplicity' (p. 136), whatever value his comments on this subject may otherwise have (p. 136ff.). The unconditional preference for falsification cannot be successfully maintained in the framework of a theory of research. We put 'shaking' side by side with confirmation and try to present each in its way as explicitly as possible, from case to case.

5. INDEFINITE EXISTENTIAL STATEMENTS – LEGITIMATE

Since Popper starts from the '*modus tollens*' of classical logic as his paradigm (p. 41), he calls 'universal singular statements' (that are the 'indefinite existential statements') 'metaphysical', that is non-empirical statements, because according to him they are not falsifiable (p. 69). However, we see what a

blessing they were in the history of the sciences, and we can design a theory of research in which they play a legitimate part.

In order to be able to apply his paradigm with the least possible restriction Popper suggests regarding 'natural laws' as statements of not merely 'numerical', but always of 'specific' generality. We should think that a theory of research should phrase its methods so tolerantly that it can satisfy both scientists who from special caution set up all laws only for a limited sphere, viz. treat the world as finite (this is even mentioned by Popper himself), as well as scientists who for some reason just prefer formulations of specific generality of the kind that Popper envisages. In astronomy, geology, sociology and many other disciplines in which experiments – which are overstressed by Popper – play only a small role, such indefinite existential statements, as one-sidedly decidable predictions, are components of normal research, though less, of course, in optics or acoustics. If, for example, we say that on a future day at a certain place a comet can be observed, we have "an only *one-sidedly decidable* statement before us. If namely the statement is true, soon the day will come when we can decide that it is true; if, however, it is not true, there will never be a day when we can decide that it is untrue" (Reichenbach 1930, p. 168). How significant can it be that a scientist, for example, searches continuously through a certain area of the sky because, by a confirmation of his prediction of a return of a comet at that place, a perhaps very bold theory would be confirmed anew, whereas no falsification of it in Popper's sense seems to be possible in a foreseeable future. Just as Popper counts these 'universal singular statements' among metaphysics, he is inclined to count models that give no access to immediate falsification among 'metaphysical regions' (p. 277). Popper, for example, subsumes the older corpuscular theory of light under 'metaphysical ideas', whereas we would certainly subsume it under the series of scientific models, because in a vague way it shows that certain correlations of optical phenomena, for example, with which we are acquainted from our encyclopedia without special theoretical connections could, according to their type, be deduced from certain more general premises, for example, a corpuscular theory. In our view there are many intermediary stages between these somewhat vague models and the more definite ones of our science. For us there is no dividing line that is supposed to exist between 'falsifiable' and 'non-falsifiable' theories. We only try to discuss the cases of 'confirmation' and 'shaking' as explicitly as possible.

6. FACTUAL SCIENCES WITHOUT EXPERIMENTS

It is not enough for Popper that the statements of the factual sciences are *potentially* testable according to their form (whether this form can be described precisely may remain undecided), that is 'unmetaphysical' in our view (see especially Carnap), but he stresses excessively that they should be *actually* testable. This is a proposal for restriction that we cannot find recommendable for the theory of scientific research.

> Any empirical scientific statement can be presented (by describing experimental arrangements, etc.) in such a way that anyone who has learned the relevant technique can test it (p. 99).

The over-emphasis on 'falsification' makes Popper see the practice of research all too much from the angle that

> the [theorist] puts certain definite questions to the experimenter, and the latter, by his experiments, tries to elicit a decisive answer to these questions, and to no others (p. 107).

Collections of data (sky photographs, etc.), travel journals (for example, Darwin's journal of his voyage around the world is most instructive about these problems) must of course set out from certain theoretical attitudes to make a selection among possible statements feasible, but these theoretical attitudes are not identical with Popper's acute approaches to the theory that are somehow to enforce his 'falsification'. He speaks rather contemptuously of that "myth of a scientific method that starts from observation and experiment and then proceeds to theories. (This legendary method, by the way, still inspires some of the newer sciences which try to practice it because of the prevalent belief that it is the method of experimental physics.)" (p. 279) How much ethnographic material has often to be accumulated before a theory is reached, and how often a group of events is systematically described in physics before it can be arranged. I remember the voluminous literature on 'magnetism of rotation' in the twenties of the nineteenth century. Precise data were available on the basis of which predictions could be made, how, for example, a magnetic needle would move if a copper disc is rotated; but there was no mention of the incorporation of these formulations into a more general theory. How much of the voluminous observation material that was assembled in the fight against the elementary electrical quantum – mentioned by Popper – can perhaps fit in later theoretically; for the time being a great many observation statements that seem to contradict the

theory of the elementary quantum are not regarded as essential 'shakings' because one just deems the 'confirmations' of the theory of the elementary quantum to be very significant. Popper, however, wishes to see forceful decisions forcefully founded. That is probably a basic tendency of many pseudorationalist endeavours that should be explained perhaps with the help of the 'psychology of decision'. People who carry out one definite action on the basis of one definite decision are often not content to have carried out such a decision after weighing many individual factors; if they are not able to receive 'transcendental' approval, they at least would like to be able to refer to an *unambiguous* logical deduction as justification. Whereas people of our attitude sometimes waver between two decisions – should we regard something as a serious shaking or should we simply disregard it for the time being – Popper's formulations obviously point to a more absolutist posture: "But if the decision is negative, or in other words, if the conclusions have been *falsified*, then their falsification also falsifies the theory from which they were logically deduced" (p. 33). – as if there were a system that could be so cleanly unveiled that such a procedure was possible. Understandably Popper, with such an attitude, must overestimate the usability of the concept 'degree of falsifiability' (pp. 118–119) for the analysis of the work of research. Out of this whole attitude one can probably explain why Popper – in spite of all the warnings by Duhem – likes so much to speak of the '*experimentum crucis*' (pp. 246, 277, also p. 237ff.):

> In general we regard an inter-subjectively testable falsification as final (provided it is well tested): this is the way in which the asymmetry between verification and falsification of theories makes itself felt. Each of these methodological points contributes in its own peculiar way to the historical development of science as a process of step by step approximations (p. 268).

We have already expressed doubts about these "step-by-step approximations" and shall have to say more about them. Popper's opinion is, for example, that 'occult effects' have not to be taken seriously because they cannot be reproduced at any time (p. 45). In reply it should be pointed out that there are many non-reproducible but well-attested effects that are theoretically safeguarded and are taken very seriously. However, there is no recognisable advance in 'occult' research (a fact to which Philipp Frank occasionally referred); it has often come about through deception, etc. These are arguments, however, that are not derived from the overemphasis on experiments for which Popper has a liking. We could sketch the model of a development of science that does not recognise any experiments, for example, along

the lines of Plato's parable of the cave; he tells of prisoners who were chained to the wall and knew perfectly well how to predict shadows and voices although they were deprived of any possibility of making experiments. It is in no way my intention to dismiss the experimental method as of minor significance; but only to reject the idea that the experimental method were as decisive for science as one had to assume from Popper's individual remarks and his total theory of falsification.

It is the aim of this essay to fence off certain of Popper's trains of thought that introduce the old philosophical absolutism in a new shape, but not to enter into detailed discussions; otherwise it would be stimulating, in connection with this over-emphasis on reproducible effects, to deal with Popper's remarks on quantum mechanics that distinguish between 'measurement' and 'separation' (p. 238). Nor do we want to deal with Popper's discussion of probability problems (Carnap, Hempel, Reichenbach have already said something about this) though they play a considerable role in his book, for the fundamental conception is not affected by this. It does seem, however, that here too Popper creates difficulties for himself in dealing with certain problems of research, because of his way of putting the questions (p. 195ff.).

7. PROTOCOL STATEMENTS AND PHYSICALISM

In this book, we see Popper's attitude, which is not adapted to empirical research, as a consequence of his decision to choose for his paradigm a system that is composed of clean statements and therefore suggests the '*modus tollens*'. This sympathy for 'cleanliness' seems to play a part when Popper decidedly rejects our proposal to use 'protocol statements' as test-statements in our encyclopedia model. The protocol statements – in their simiplified form: "Karl's protocol: (in the room is a table perceived by Karl)" – came about as a result of our attempt to avoid a special 'experiential language' ('phenomenal language') and to use nothing but the unified language of physicalism. It is also important to see in this way at once that complex (messy) statements of little cleanliness – '*Ballungen*' – are the basic material of the sciences. Popper is wrong when he thinks that these protocol statements were intended as elementary statements (p. 35). In this form they are even a protest against elementary statements. (Carnap, who on this point, is closer to Popper's proposals, uses the term 'protocol statements' in a sense that differs slightly from the sense in which I use it.)

If protocol statements are, in the last resort, the test-statements of the encyclopedia model (that does not mean that one has always to refer to them),

then there is no reason to speak of more or less complex test-statements (p. 126–128). Curiously enough Popper thinks:

> Most people would see that any attempt to base logical statements on protocol sentences is a case of psychologism. But curiously enough, when it comes to empirical statements, the same kind of thing goes today by the name of 'physicalism' (pp. 98–99).

Here he overlooks the fact that he himself regards protocol statements as possible, though hardly suitable, basic statements (p. 105). The protocol statements are of a different nature than logical statements; they are indeed statements of the factual sciences; their confrontation with other factual statements at once secures their significance, a confrontation which they do not have with statements of logic.

The protocol statements in the form suggested by us have the advantage that they can be maintained if one accepts or rejects the expression within the brackets – taken as an independent statement. If the protocol is accepted – rejection of a protocol is not a frequent occurrence – and in addition the expression within the brackets is taken in isolation, then the protocol can be characterised as a 'reality statement'; if, however, the expression within the brackets taken in isolation is rejected, then the protocol can perhaps be called an 'hallucination statement'. Popper is of the opinion that it is "a widely spread prejudice that the statement, 'I see that the table here is white' has epistemologically greater merits than the statement, 'The table here is white'" (p. 99). For us such protocol statements have the merit of *greater stability*. The statement: 'In the sixteenth century people saw fiery swords in the sky' can be retained whereas the statement 'There were fiery swords in the sky' would have to be deleted. Just the continuity of formulations, however, plays a great part in the selection of model encyclopedias. Such continuity rests in part on constant use of *quaternio terminorum*; though in contrast with purity this makes possible a connection from people to people, from age to age, from scientist to scientist (problems of this kind are discussed by Ajdukiewicz). When a primitive man says: 'The river runs through the valley,' he certainly defines the terms in a way that is different from that of the European who goes on using the statement. Compared with such impurity, the impurity of protocol statements plays a minor role, though it has to be admitted that the statements of theoretical physics – as long as they are not used to formulate predictions tested by protocol statements – give the impression of greater purity.

We do not believe that Popper with his attempt to introduce 'observable' "as an undefined [basic] term which becomes sufficiently precise in use"

(p. 103) and to operate with terms like 'macroscopic', etc., can master the difficulties that result if one wants to turn, for example from the research work of experimental physicists to that of sociologists and psychologists.

8. OLDER SUCCESSFUL THEORIES ARE NOT ALWAYS APPROXIMATIONS TO LATER ONES

In order to be able to design a model for the history of research that reflects its characteristic changes, it is not necessary to take the change of protocol statements into account. It is, however, essential that the stock of successful predictions change. If theory I produces group A of good predictions and theory II, group $A + B$ of good predictions, we should say that theory II is the more successful of the two and say that the stock of predictions A is an approximation to the stock of predictions $A + B$. However, this in no way implies that the principles of theory I have to be an approximation to the principles of the more successful theory II. This is logically obvious, but this approximation is not even historically always given. We believe it points to Popper's basic pseudorationalistic attitude when he comments:

> For a theory which has been well corroborated can only be superseded by one of a higher level of universality; that is, by a theory which is better testable and which, in addition, *contains* the old, well-corroborated theory – or at least a good approximation to it (p. 276; see also p. 268).

Duhem, whom Popper mentions more than once, shows very beautifully with the various stages of gravitation theory how little they can be seen as 'approximations' to successive stages.

Though Popper may declare that science is not "a system which steadily advances towards a state of finality" (p. 278), the above-mentioned passage still indicates that he thinks of this succession of theories when he speaks of "the belief that there are regularities which we can unveil, discover" (see p. 252ff.). These phrases all fit with the basic tendency that we have characterised and that is developed expressly at more than one place. If we want to make a choice among several encyclopedias we permanently use the unified language of physicalism without being forced to use such a terminology, gliding into the metaphysical, which in roundabout ways re-introduces, in the last resort, the term 'real world'.

9. PSEUDORATIONALISM AND PHILOSOPHY

Popper's pseudorationalistic tendency can be seen historically as a kind of

metaphysical residue from the development of 'philosophy', for this view cannot emerge from the analysis of the factual sciences that are operated without metaphysics. It would be in agreement with this historical supposition that Popper advocates a special 'theory of knowledge' side by side with the logic of science and the factual sciences. Perhaps this closeness to certain metaphysical tendencies explains why Popper behaves to Kant and other metaphysicians much more kindly than to the group of thinkers he calls 'the' positivists – without, however, characterising it sufficiently by an indication of a system of doctrines or an enumeration of names.

> The positivist dislikes the idea that there should be meaningful problems outside the field of 'positive' empirical science – problems to be dealt with by a genuine philosophical theory. He dislikes the idea that there should be a genuine theory of knowledge, an epistemology or a methodology. He wishes to see in the alleged philosophical problems mere 'pseudo-problems' or 'puzzles'. . . . Time and again an entirely new philosophical movement arises which finally unmasks the old philosophical problems as pseudo-problems, and which confronts the wicked nonsense of philosophy with the good sense of meaningful, positive, empirical science. And time and again do the despised defenders of 'traditional philosophy' try to explain to the leaders of the latest positivistic assault that the main problem of philosophy is the critical analysis of the appeal to the authority of 'experience' – precisely that 'experience' which every latest discoverer of positivism is, as ever, artlessly taking for granted (pp. 51–52).

This pleading in favour of traditional [systematic] philosophy makes us foresee that later it will be shown which important role it is called to play as teacher of scientific empiricism, which sees its special fundamental task in the elimination of 'apparent problems'. The pseudorationalism in Popper's view would best make us understand why he could feel attracted by traditional philosophy and its absolutism while his book contains so much of that analytical technique advocated precisely by the Vienna Circle. The aim here was not to give a general presentation of Popper's view, but rather to criticise the *absolutism of falsification* that is in many ways a counterpart to the *absolutism of verification* which Popper attacks. It is precisely this book, which is close to the scientific empiricism of the Vienna Circle, that shows once again very clearly that the road to science is far from free of certain residues of compact metaphysics which can only be overcome by common work.

NOTE

* All references in this paper are to the 1968 edition of the English translation.

CHAPTER 11

INDIVIDUAL SCIENCES, UNIFIED SCIENCE, PSEUDORATIONALISM

Misgivings are frequently expressed that logical empiricism could decay into empty scholasticism and dogmatism. We escape such dangers – which Enriques, among others, has pointed out – the more easily, the more we devote ourselves to the continuation of scientific work to the greatest extent. The stages characterised by Reichenbach and Carnap: from metaphysics to theory of knowledge, from theory of knowledge to logic of science, can be followed, as the next stage, by the step to unified science. Built up with the help of the logic of science, unified science replaces a comprehensive view as it was attempted by metaphysics, by a planned synthesis of everything that we have produced in the way of scientific statements.

One could think of building up unified science (total science, *scientia universalis* or whatever one wants to call it) in such a way that one starts from as detailed an analysis as possible of individual sciences, their structure and formulations. Though such an analysis may lead to success of all kinds, it does not much recommend itself for our purposes because the individual sciences were delimited in a rather accidental way as a result of historical circumstances. We have modern analogies to the earlier grouping of mathematics, mechanics and the architecture of fortresses. How many contingencies are involved in the peculiarities of disciplines dealing with man.

One might think of designing new boundaries between individual disciplines, but such an attempt is questionable. Longer experience teaches us that we avoid pseudo-problems of all kinds if, in the analysis of sciences, we set out from predictions, their formulation and their control. But it is precisely this starting point that is little suited for the delimitation of special disciplines. One does not arrive at individual disciplines of stars, stones, plants, animals during the deduction of certain predictions, because time and again the conjunction of statements of different origin becomes necessary. In order to formulate the individual prediction: "This forest fire will soon be extinguished", we combine biological statements (concerning trees, etc.), chemical statements (concerning fire, etc.), sociological statements (concerning fire service, etc.) and statements of other disciplines. The situation is different for

Translation of Neurath 1936a [ON 225].

theories, they can be restricted to definite terms. On the other hand they cannot be controlled in isolation, but only in connection with the concrete predictions that we have just characterised. The theory speaks of electric currents that originate when closed conductors and magnetic fields move relative to each other in a certain way whereas a prediction has to speak of a dynamo in a certain laboratory and of the behaviour of an experimenter. Moreover the fact is that the whole mass of statements that we accept is not systematically split up into sections by theories, but rather that we succeed in only partial systematisations (even in axiomatisations that admit a simplicity sufficient for the practice of science); but in addition there are loosely correlated statements, which cannot be coordinated with theories straight away.

Much may be said for the proposal to start from the whole mass of statements that we acknowledge for the time being, which is in constant change and is also changed by third persons while we are using it. Let us investigate it for its logical peculiarities, in order also to find out what can be achieved by way of *logically* significant results in the framework of 'induction' and 'control' of scientific statements. In trying to do so we are much disturbed above all by the variety of scientific formulations in the individual disciplines; this obviously rests more on historical origins than on demands of scientific technique. This leads us to a closer inspection of the question of scientific language. The program of physicalism shows us the possibility of building up a uniform scientific language with a uniform terminology; this is in perfect harmony with the circumstance that we have to connect statements and terms of different disciplines for the deduction of individual predictions, and that we have to connect the statements of the theories with the individual predictions. It can be shown that on the basis of certain proposals the observation statements (protocol statements) can also be formulated in the language of physicalism on which we are to agree, whose 'layers' and 'sections' we shall not discuss here. In the language of physicalism the precise terms of the theories can be found as well as the not so precise but sufficiently exact terms that appear in individual predictions, the clusters (for example 'dynamo in a certain laboratory').

The mass of statements acknowledged by us for the time being can further be systematised; on the one hand partial systems and axiomatizations can be further developed, on the other hand much attention is given to the creation of cross-connections that make the coherence of as many statements as possible more apparent and combine so far isolated partial systems into larger systems. Thus a generation ago 'physical chemistry' began to fill the gap

between 'physics' and 'chemistry', partly through logical unification, partly through the establishment of new factual statements.

If we, as advocates of logical empiricism, systematically pursue such a synthesis with the means of logic of science, we are driven to develop the theory of uniform logical tools which can be used in the most varied disciplines. Precisely this occupation with these uniform logical tools is stimulated by the realisation that the work in unified science is a promising task for representatives of the logic of science who in this way come into closest contact with the totality of our treasury of statements.

This comprehensive work can be started without previous over-elaborate discussions of the individual sciences, their relationships and differences. In analogy to 'experimental geology' that was so actively advanced in France, in a certain sense one might also speak of experimental astronomy if, for example, one is prepared to regard the parabolic thrust on earth as a model process of certain star movements. The results of experiments with model aircraft can be transferred to transport aircraft only after the application of certain corrections; strictly speaking some such corrections must be applied in all experiments, for example, in each chemical experiment, as in each case the circumstances differ from those to which the results of the experiment are to be transferred. It is a *quaestio facti* in which cases such corrections are to be applied – but a sharp line cannot be drawn here. All too often it is overstressed that one discipline essentially provides very insecure, another very secure results; but closer inspection shows that under certain circumstances predictions on animals can be rather good and on meteorites rather bad. It is correct that astronomy not only shows deductions in its theory for long spans of time, but concrete astronomical predictions are also possible for millennia ahead. An analogy to this is found, however, in chemistry which makes use of the disintegration of radium for purposes of measuring time; an indication of the degree to which one believed the disintegration to be independent of the environment. On the other hand a concrete laboratory experiment can be predicted only for short spans of time ahead if the fate of the experimenter is taken into account. These hints may explain why we do not start with an analysis of individual sciences; they suggest instead combining their analysis with the analysis of the total mass of statements.

If we regard the mass of statements as the result of experiments, travels, certain other behaviour, then we move in the fields of the significant *behaviouristics of scholars, history of science, sociology of science*. Mach himself has shown how productive such work is also for the logic of science with which we are specially concerned. If we state: "On the basis of experiments

a scholar has replaced an earlier published statement by a new one", this is no statement of the logic of science; but the logic of science can compare this statement with other statements as to its logical content; in the same way it can compare the scholar's earlier statement with his later statement.

One can also say: "Scholars wrote statements contrary to their own views" or "scholars have consciously recorded events and thus advanced science". If we need material for an analysis along the lines of the logic of science, we could, roughly, propose replacing these statements by statements of the following kind: "The scholars made certain formulations for themselves, but wrote down formulations in contradiction to them" or "The scholars made and then wrote down certain formulations". 'Formulations' are to be treated as 'statements' for our purposes along the lines of our proposal; they can therefore be compared with other statements as to their logical impact. Such 'formulations' can be presented in the form of 'protocol statements' that are discussed elsewhere (Neurath 1936). We suggest using the term 'comparing' in the sense of 'formulating a statement of comparison' and always indicating with reference to what the comparison is made. Many of these proposals aim at evading well-known pseudo-problems, for example, formulations like: "In statements, thought expresses itself in a sensually perceivable way. The statement is an image of reality. Reality is compared with the statement", as if a statement were not part of 'reality'. There is also the obvious question with referenece to what are statement and reality to be 'compared'.

Let us say that there is an account of a sixteenth century traveller with a passage stating that a certain animal lived in a certain country: however, from the mass of statements accepted by us (among which are many observation statements concerning the sixteenth century) there is the following statement which we decide not to change because of this account: "This animal did not live in this country in the sixteenth century"; we can then say that the account is in contradiction with the accepted mass of statements. Since the term 'true' together with the term 'the one true world' ('the one real world') – which was used by many philosophers as a court of last resort – had become displaced through aphoristic criticism and systematic analyses, I suggested, in the interest of terminological continuity, to reserve this term for the mass of statements accepted by us, and to call 'false' every statement in contradiction with it, and 'true' every statement that is in agreement with its consequences or can be accepted into it. At each moment there are statements about which a decision has not yet been made. This schematically reflects the situation in the practice of science. However, the term 'true' can also be discarded entirely (the Poznanski-Wundheiler proposal). As to some other

proposals (Tarski, etc.) objections could be made which would not affect the proposal advocated here.

Whatever conscious behaviour is under discussion, this proposal, like the others, aims at coordinating 'statements' with this kind of behaviour, which can be compared with each other logically. The reason for a 'rejection of a theory by new observations' is then replaced by 'rejection because of contradictions between a theory and observation statements'. These are endeavours that have been furthered by the investigations of many scholars, for example by the work of Ajdukiewicz, Carnap, Frank, Hempel, Menger, Mannoury, Popper, Reichenbach, Schlick, Tarski, etc. If, however, attention is concentrated on individual questions and loses sight of the aim of advancing analysis through the logic of science by these formulations, sometimes the danger may lurk that thoughts are sterile, the approach to problems is mixed up and may even occasionally decay into the metaphysical.

A special danger, however, is *pseudorationalism*. It appears wherever one wants to establish all too general rules for 'induction' and 'control', wherever one treats statements in which the above-mentioned, less precise terms, the clusters, appear in the same way as precise statements, also and above all wherever the ambiguity of predictions is replaced by unambiguity, indefiniteness by definiteness. With a view to the indefiniteness in our mass of statements, we can best start from the 'shaking' and 'confirmation' of theories that are dispensed to them by certain protocol statements, and leave it undecided whether the concepts of 'falsification' and 'verification', which are defined for pure statements, can be used even merely as 'approximations'. The danger of pseudorationalism also appears where the replacement of the decision of the practice of science for a certain mass of statements, so to speak, is believed possible through the calculus of the logic of science; this is possible, for example, in the sphere of connections of purely logical and mathematical deductions where we can regard scholars as a sort of automata that detect contradictions and deduce consequences. Let us assume that two masses of statements are given – they may differ mainly by one definite theory, a difference that can be treated by the logic of science. Assume there are a number of statements that bring about partly a shaking, partly a confirmation of a theory, and there is no way, with the help of the logic of science, to show which of the two masses of statements is 'less contradictory'. Now the two scholars have to act as persons in an experiment as it were. They can, for example, develop opinions about the chances that each of the two masses of statements will allow in the future history of the sciences. Assuming that the sociological predictions coincide in all points (including

all uncertainties, etc.), it is still possible that one scholar prefers the one mass of statements, which, for example, seems especially favourable to physics in the opinion of them both, whereas the other person in the experiment chooses the second mass of statements which to both of them appeared to be especially favourable to the development of the empiricist total view but less favourable to the development of physics. Such are the comments from the sphere of *the behaviouristics of scholars* which a pseudorationalist would try to present in terms of the logic of science without having the necessary data as a foundation.

Pseudorationalism will time and again try to reach, in roundabout ways, 'the *one* real world' ('the *one* mass of statements distinguished by certain characteristics'), for example, by putting forward the doctrine of a perfection, perhaps 'infinitely far away' to which science gets closer and closer. It is also easily assumed that if a new theory simply increases the number of existing confirmed predictions, the older theory is to be regarded as an 'approximation' to the new theory. What is overlooked here is that, besides the already tested predictions of a theory, there are also not yet tested predictions, as well as all sorts of consequences and it is not quite clear whether they can be interpreted in terms of a factual science, etc. (Quite apart from this, all statements altogether containing 'cluster-concepts' cannot endure precise confrontation so that even masses of statements with the same confirmed predictions can otherwise differ essentially. Therefore a new mass of theories, which augments the old stock of predictions, can differ essentially from the earlier mass of theories).

In short we see: there are always whole masses of statements under discussion with all their uncertainties, with all their subtle mathematical and logical parts. And everything makes us beware of pesudorationalism and pretension of all kinds. For our work there is no magical sieve at our command that would help to eliminate, automatically as it were, the terms and statements that easily lead to pseudo-problems. Sometimes we let formulations pass that we later reject as scientifically useless and therefore 'isolated' (metaphysical); equally we will often destroy valuable buds because we do not like to handle concepts and statements when we do not know exactly whether we can vouch for them. Wherever we can, we want to systematise and create clarity and establish connections; but let us beware of speaking of '*THE*' system and '*THE*' science in anticipation, seduced by the impressive successes that have been achieved in detail. Let us speak rather of the encyclopedia that has so far been acknowledged.

We choose, as it were, among various encyclopedias, in history, encyclope-

dias succeed one another, compete with one another, encyclopedias are in continuous change. Thus the unified science for which we struggle is something in the making, promoted and opposed from the most different sides. In a much stricter sense than our predecessors, we could call ourselves 'encyclopedists' of unified science; for us the encyclopedia is not an eclectic expediency but, for the time being, the most perfect form of scientific synthesis. And if in Prague I expressed the hope that we would be welcomed in Paris with the exclamation, "*unité et fraternité*", I now should like to express the further hope that on the broad basis of scientific empiricism there may develop '*unité de la science et fraternité entre les nouveaux encyclopédistes*'.

REFERENCES

See 'Physicalism', pp. 52–57 and 'Pseudorationalism of Falsification', pp. 121–131, as well as (Neurath 1935d).

CHAPTER 12

AN INTERNATIONAL ENCYCLOPEDIA OF UNIFIED SCIENCE [1]

One can say that, from the point of view of scientific empiricism, it is not the notion of 'system', but that of 'encyclopedia' that offers us the true model of science taken as a whole. In the spirit of scientific empiricism, the Mundaneum Institute in The Hague is preparing an *International Encyclopedia of Unified Science* destined to serve as a complement to existing encyclopedias.

The best encyclopedias of our time present each of the different branches of knowledge as a vast survey; experts show what has been achieved and what idea has to be formed of these results. Depending on the degree to which the sciences have developed, the different disciplines have elaborated particular scientific languages which makes it difficult today to establish contact between disciplines. Certain eminent thinkers have even accentuated and underlined these differentiations. However, it has to be remarked that from the point of view of scientific empiricism it is possible to remedy this plurality of language, that one can throw bridges between the sciences; not enough thought is given to these bridges today, or they may not even exist yet. It is precisely this question that is the special concern of the International Congresses for the Unity of Science, the first of which was held in Paris in 1935 with 'scientific philosophy' as its particular program.

It is therefore in response to an actual need that we are creating an encyclopedia, destined to complement the existing ones, and to show to what extent actual science can be unified, and to make the internal connections apparent. To start with, one can maintain from now on, with concrete examples for support, that it is possible to unify scientific language to a large degree and at the same time avoid metaphysical formulations.

Within the framework of this project, the aim is not to expound certain particular, matured disciplines; rather, one will present as far as possible, the whole of science with its numerous ramifications. It will have to be made especially clear up to which point one can already put the *logico-scientific* analysis at the service of the unification of science. One has to note the *essential unity of the auxiliary processes* of science; but one has also to underline the fact that, precisely in the areas in which a certain axiomatisation

[1] Translation of Neurath 1936c [ON 227].

and other forms of systematic deductions exist, one has, on the whole, pushed forward only some points, that there remain *gaps* that are now evident, and that a mass of serious scientific works that are successfully undertaken in different parts of science still let *contradictions* between them appear. Whereas the other encyclopedias give a retrospective synthesis, so to speak, this new work will have to show above all in which direction new ways open themselves, where the problems lead, and where, from the point of view of a unified science, unsuspected possibilities can be discovered. Whereas up to now the general encyclopedias that want to present the totality of our knowledge still adapted themselves to the needs of a country or a definite group of countries by subjecting many of their claims to this point of view, the vast international encyclopedia that we are in the process of preparing must above all be applied to show the whole large and profound unity of the general idea of science and to indicate its differentiations only afterwards.[2]

The program that has just been drawn up implies broad cooperation between specialists of different disciplines. But this is one task that precisely the advocates of scientific empiricism can contemplate with good hope of success, for scientific empiricism is very suited to coordinating the efforts of scholars. It seems therefore legitimate and opportune to undertake this Encyclopedia, provided only that one succeeds in unifying the terminology and the scientific symbolisms of all kinds. While striving to utilise the results of contemporary logic for the whole of science, one will have to refrain, however, from concealing the ambiguity of certain pronouncements and from attempting to design a unitary system, as one possesses only some bits of it, of a high degree of perfection perhaps, but unsuited to be coordinated with each other straight off. The previous encyclopedias have often been considered as more or less successful examples of eclecticism whose imperfection one resignedly accepted in advance while admitting that the true ideal would have been precisely a 'system'. For us, on the contrary, we would declare right away that the form of the encyclopedia is the most perfect we could ever attain to present the whole of science; thus we put our concrete scientific work, which carefully avoids anticipating the general systematisation of science, expressly against the pseudorationalism of all 'centralist' philosophies.

It is not only necessary to emphasise the unity of auxiliary procedures of science in the case of each discipline; this has also to be made the subject of a separate systematic exposition. The new Encyclopedia will show in detail, for example, how the probability calculus, or how certain methods of

coordination, can be applied in all possible domains, so that one could install a kind of instrumentarium, a stock of instruments for science in general whose effective uses will be shown at the same time.

Besides this general survey of the instruments of science we can also seek the unification of scientific language in general or of special parts of it, and we have to see which of the numerous possible modes of unification has to be chosen for the concrete aims of the Encyclopedia. It is not enough to establish only the principle of this unity on the lines of the program, we have also to demonstrate it by what we do. We will commit ourselves to a mutual agreement to avoid certain terms and certain special formulas in the Encyclopedia. There are such terms of suitable use in special sciences that have to be renounced if we can adapt ourselves to a general terminology convenient for all the sciences.

These are the fundamental directives that will hardly be contested. But the actual execution requires serious efforts of organisation. Whereas up to now the encyclopedias limited themselves to recommending their collaborators to treat each subject with care and discernment, this new Encyclopedia has to induce its collaborators to agree among themselves in order to push the unity of the form of their contributions as far as possible. This uniformity has its limits, that is obvious. But one will already have done much if one succeeds in determining, by common discussions, the fundamental terms or 'word types' to be adopted in the different divisions. As the Encyclopedia will not be arranged alphabetically but by subject, its general index will be the expression of a strictly scientific attitude concerning this ideal of unity.

This Encyclopedia, which will not adopt an alphabetic order, will publish several volumes of three to six monographs each year so that the whole work will not be finished for some years. But as each volume will form a whole in itself, and as one can always present the latest achievements of science in the supplements, the readers will always have at their disposal a partial encyclopedia, each part of which, however, will be complete in itself, and incorporated in an ordered whole. The plan of the Encyclopedia calls for a first series of volumes or basic 'layer' that will provide the general perspective; to this one can add further 'layers', and nothing will prevent one, if one so desires, from going on within this framework up to the publication of rather specialised articles. The latter, however, will find a well-determined place within the general plan, though not all disciplines have to involve such particular studies. In many branches one can simply rely on already existing works. In others, works must be composed along the special lines of this Encyclopedia, for tracing the internal links, the unity, the vertical and

horizontal connections. Such special work must find a place within the framework of the Encyclopedia.

Each of these volumes will appear in the three languages of the International Congresses for the Unity of Science: German, English and French. This will be the opportunity to create a small *trilingual vocabulary* of the most important terms. This vocabulary will not give the already known terms but only those that will have been chosen by common agreement for use in the Encyclopedia. If this agreement should continue to be realised, this would be a precious contribution to international understanding in the sphere of logical empiricism.

It is evident that this Encyclopedia will tend towards the unification of not only the scientific language, but also *graphic representation*. Curves and other figures are also instruments of scientific expression. All pictures that the Encyclopedia will present will be made of standardised elements, if technical, biological, sociological or other subjects are to be represented. These standardised elements can be catalogued in a sort of glossary of symbols. One can combine them with each other according to a grammar of symbols. The Mundaneum Institute in The Hague will adapt the picture language ISOTYPE (International System Of TYpographic Picture Education) to the particular needs of the Encyclopedia; so far it has used this language elsewhere. The Encyclopedia, which addresses a very large public, will thereby gain in intelligibility (cf. Neurath 1936f).

As this Encyclopedia does not want to present each discipline as a completed picture but wants to show precisely the gaps and the inadequacies of actual knowledge, it will also stress what there is of the 'contingent' in all research, and that all science depends on historical conditions; but equally the close connection between practical life and science has to be pointed out. However, these efforts to point out that scientific thought is close to life will in no case take the form of an 'imperative'; the Encyclopedia will even accept the principle of avoiding any 'emotionally' tinged pronouncement, be it blame or praise. It is clear that such a work is conditioned by emotional elements, as all human behaviour in history is; even the simple choice of questions to be treated is in itself not scientifically justifiable. But that does not change anything in the essential difference that exists between one mode of exposition using emotional elements and another taking care to avoid them. Naturally emotional elements will play a part in the historical articles of the Encyclopedia in which one will treat the evolution of all the modes of expression that do not enter into scientific empiricism. Here a historical enquiry might be made to find out whether primitive languages already give

rise to metaphysical pronouncements or whether these appear only later, and under which conditions.

Through its fundamental logical attitude this Encyclopedia is linked to a certain degree with Leibniz who, in his projects, had also thought of visual representations. But the general tendency preferably to cultivate intuitive teaching methods can, in the last analysis, be traced back above all to the *Orbis pictus* of Comenius, and to the enterprise of Paul Otlet who wanted to display the whole knowledge of our time by modern intuitive methods (*La cité mondiale*, Brussels, 1929). In a certain sense this Encyclopedia of unified science also continues the work of Auguste Comte and Herbert Spencer, who wanted to give a picture summarising the sciences, in purely empiricist inspiration. But that is a task that can be executed methodically only today because we have at our disposal the resources of the new logic and the modern means of visual representation (*visualisation*).

The Committee of the Encyclopedia at the Mundaneum Institute in The Hague, consisting of Carnap, Frank, Joergensen, Morris, Neurath and Rougier, will not have the task of finding representatives of each discipline and of charging them with 'convincing' people of the new doctrine; it seeks collaborators who endeavour to show, by their common work, all that can be achieved in the present day with the help of logical empiricism, and how the results of science can be incorporated into this new framework. Here is a new road that opens itself to the young. In order to give the greatest number of people, especially the young, easier access to the Encyclopedia, we will take account of certain educational demands while always observing scientific rigour. The aim is less to refine the already highly developed disciplines, than to occupy oneself more with the branches that have so far been kept a bit apart, such as psychology, biology, sociology. It will be one of the important tasks of this Encyclopedia to show how far these disciplines can share a unique common language with physics, and how the laws of the different sciences each time present distinct particularities. In the framework of the search for unity, we will bring out all the problems posed to those intellects who feel attracted by the new things brought up by contemporary logical empiricism. It is precisely these people who will be able to appreciate this manner of presenting the unity of knowledge:

Those who have ceased to grow, find nothing right;
Those who are growing still, will not spare thanks.[3]

Well, this Encyclopedia of unified science is conceived precisely as an encyclopedia 'growing still'.

NOTES

[1] At the proposal of Prof. Charles W. Morris of Chicago, the Congress gave its approval to the project, *The International Encyclopedia of Unified Science*, sponsored by the Mundaneum Institute, The Hague.

[2] This new encyclopedia does not aim to give a summary of the totality of knowledge, as have other works with this name, but only to show the framework of our science. It will therefore not be as extended as ordinary encyclopedias and will possess an essentially distinct character.

[3] [Goethe, *Faust*, 'Prelude in the Theatre', lines 182–183:

> Wer fertig ist, dem ist nichts recht zu machen,
> Ein Werdender wird immer dankbar sein.

The translation is by W. Kaufmann (*Goethe's Faust*. Garden City, New York: Anchor Books, 1961). – Ed.].

CHAPTER 13

ENCYCLOPEDIA AS 'MODEL'

ENCYCLOPEDISM

The tendency to want to constitute a system with an absolute value is a danger that also threatens Logical Empiricism. From the fact that in some particular discipline one can give a theory the form of a system of statements, it does not follow that there is any reason to consider the totality of statements with which one can deal, as being in any way a beginning of a definite and complete system. I propose that one no longer use the term 'the system of science' or any other similar terms, and that one equally avoid all expressions that sound as if they supported the 'absolutism of the system'.[1] We should never say that certain formulas are 'unshakeable', 'definitely free from contradiction', 'absolutely true', nor that they 'approximate' such a state more and more, as if this were something determined or determinable. The object of the following pages is to show that we can always start from our ordinary speech, that is, from ordinary statements of an intermediate generality, and refer our scientific work to this point of departure. All the efforts that have been made in order to reach greater certainty lead to well-known difficulties that always appear when one wants to use expressions like 'the real world', 'the immediately given', as the basis of discussion.

Since our program is to 'develop a general empiricist attitude' we shall remain faithful to it while always referring, in all our considerations of science, to statements used by a definite group of men at a certain period, for example, the scholars of the period in question. To start with we find ourselves in the presence of a multitude of rather imprecise statements with little coherence among them; we see that only in isolated cases has one succeeded in forming systematisations to some extent, notably in physics. (It is these deductive constructions, closed in themselves, that constitute what one commonly calls a *system*.) If we wanted to make a cross-section of the totality of usual statements that well express the character of this aggregate, we would arrive at the *encyclopedia as 'model'*. This unavoidably

Translation of Neurath 1936d [ON 228].

contains precise statements, other less precise ones, and diverse groups of statements, more or less coherent.

For an advocate of the empiricist attitude it is absurd to speak of a unique and total system of science. He must conceive his work as tending towards precision and systematisation within an always variable framework which is that of an encyclopedia. What we call 'encyclopedia', it seems to us, is nothing but a preliminary assemblage of knowledge, not something still incomplete, but the totality of scientific matter now at our disposal. The future will produce new encyclopedias that will perhaps oppose ours; but for us it does not make any sense to speak of the 'complete encyclopedia' that could serve as a 'standard measure' for estimating the degree of perfection of the historically given encyclopedias. The encyclopedia that is the model of science is in no way unique and select; but we are dealing with encyclopedias each of which is a model of science, and one of which is applied at a definite period. The march of science progresses from encyclopedias to encyclopedias. It is this conception that we call *encyclopedism*.

CERTAINTY

When we discuss chemical, biological or sociological theories, we ordinarily treat their logico-mathematical auxiliary tools as given and do not question them. This is only one form of working technique and does not mean that we consider these mathematical tools as definitely secured. We cannot even pretend in a general way that we regard logico-mathematical proofs as more certain than statements of chemistry, biology or sociology. When we say that one statement is more certain than another, we maintain something concerning our 'conduct' in this respect; for example that we do not intend to spend more time and effort in order to test its truth; moreover, that we do not foresee that the development of science must soon change it; in other words, what would be necessary to do in this case, we do not feel obliged to do.

When we use logico-mathematical deductions as auxiliary tools in the positive sciences we can never know whether we are not going to modify these tools considerably – perhaps on the very basis of the principles that we apply today. Poincaré once declared certain logico-mathematical propositions of Russell inadequate because they resembled the behaviour of a shepherd who wanted to protect his flock against wolves by surrounding it with a solid enclosure, but without being absolutely certain that he had not shut the wolf in with the flock in the pen itself. To this Karl Menger has replied that without doubt a mathematician is never absolutely safe from

the risk of contradiction and that he can never know whether he has not enclosed the wolf with the flock of Poincaré's parable; but he has added that if Poincaré blames the mathematicians for that, he can do so only because he demands from mathematics a certainty that is not only of a higher degree but also of a superior nature to that which can be attained by other human activities. To be complete one could also add that a closer examination would perhaps reveal holes in the fence of the sheep-fold. Groups of statements that, on the basis of certain considerations, we have declared free from contradictions today can very well be ranged among the groups of contradictory statements tomorrow and, vice versa, certain contradictions that we denounce today may be rehabilitated after more intensive study. The 'imprecision' of terms of which we also have to beware in the positive sciences is not the question here. It constitutes a second source of indetermination. Our terminology frankly is hardly appropriate to such considerations. Though in principle we acknowledge the demand of relativism, we often still drag with us the traditional absolutistic terminology that allows reference to the 'real world', the 'ideal totality of statements' and other similar things.

Since we are developing scientific theories in the framework of our encyclopedia, we can indicate by which observation statements certain predictions which they authorise are 'confirmed' and, on the other hand, by which observation statements they are 'shaken'. By choosing these terms of 'confirming' and 'shaking' we avoid from the start the logical absolutism hidden behind phrases such as 'verification' or 'falsification' (declaration of error). See Karl Popper on this subject. For alongside the 'absolutism of certainty' these formulations also imply an 'absolutism of precision'.

There are formulations in the positive sciences that not only show clearly the uncertainty of the logico-mathematical developments of which we have spoken, but are also infected by the *specific inexactness that characterises our current language*. Certain positive new statements change the meaning and usage of the traditional statements with which they are linked. Obviously expressions like 'Niagara Falls', 'yesterday's exchange rate of a certain loan', 'the dynamo in our laboratory', are precise enough to be used in propositions like: 'on such a date, under these conditions Niagara Falls were frozen; the exchange rate of the loan has fallen; the dynamo has increased its speed', expressions with whose help we check predictions deriving from certain scientific theories.

The theories with their scientific symbols must lead us in the end to statements that can be checked by means of statements of ordinary language,

for example the following formulations: 'at a temperature of so many degrees even large waterfalls freeze', 'when the interest rate rises, the exchange rate of loans falls', etc. We shall call these expressions and formulations of the common language 'clusters' to distinguish them from scientific formulations. Perhaps one should envisage the possibility of a series of intermediary steps that lead from cluster to formula. What we demand of a cluster-concept is that we can somehow make a 'formula' correspond to it, in connection with a theory. Often the cluster and the 'formula' have the same name. Yesterday's formula is often today's cluster. Thus hydrogen peroxide has also passed from the chemistry laboratory to the hairdressers.

Without doubt it is something of this kind that Duhem has in mind when he speaks of concrete facts (instead of formulas in common language, or cluster as we say) to set them against the abstract symbols of science – a manner of speaking that, in our opinion, is hardly adequate.

It is a characteristic feature of the encyclopedia that it is incessantly aware of the difference between a cluster and a 'scientific formula' whose mutual connections are complex, without yielding to the temptation to use, or at least to conceive, a closed system of formulas as a model for the aggregate of statements.

UNIFIED SCIENCE

Thus, even within the framework of encyclopedism, one does not vainly attempt to conceive 'THE system of science'. In each particular discipline we aspire to precision and systematisation: let us assume that we could deduce a group of assured predictions from a certain theory and another group of assured predictions from another theory; certainly we shall consider it scientific progress if we succeed in creating a third theory from which one could deduce both these groups of predictions. The history of science shows us that frequently such attempts were strongly stimulated by speculative intuition. There is no scarcity of metaphysicians of the first order who have tried grandiose syntheses whose bold impulse has been unifying efficacy. Experience shows that attempts made to reduce the variance and disjunction of the diverse disciplines have no chance of success unless the representatives of these particular sciences cooperate on such a synthesis. In this way one avoids both the depressing compilation that strings together science after science in the accidental form that history has given them and those vague generalisations that always lack a solid empirical basis. The aim is to arrive at "coordinating the particular sciences directly by demonstrating their

concrete relations, and not indirectly by referring all of them to a common abstract system of little clarity" (Philipp Frank 1936).

The program of unified science thus conceived does not aim at assembling a unique edifice of the traditional sciences by applying certain modifications, nor at searching for the most general propositions from which one could deduce the individual sciences. We shall consider rather the statements and groups of statements that we have defined, as raw material, and our efforts are directed towards linking them to each other as closely as possible. Often it happens that one and the same question is treated in two different manners in two different disciplines, because they have developed separately, instead of trying to bring about unity of presentation. But this effort presupposes that one is perfectly clear in one's mind about the use of various verbal elements, not only the meaning of scientific formulas, but also of everyday language from which we cannot detach ourselves entirely. This means that one must concern oneself with the 'logical syntax of language' (the title of a work by R. Carnap) and at the same time with a behaviouristic study of the actions of men of science. The relationship linking scientific formulas with the formulas of everyday language, which has been studied from different sides, can be discussed much better in the framework of the 'encyclopedia' model than in that of the 'system' model for in the latter everyday statements cannot really find a place. The encyclopedia of unified science will therefore not try to give systematisation, important though it may be, a privileged place superior to that which it already occupies in the various spheres of knowledge.

STABILITY

In the framework of the encyclopedia we encounter statements that prove to be of great stability in the course of history, a stability on which we also count for the future. Now these statements are just those not very precise ones of everyday language (partly inspired by science). Take for example a passage from a chronicle: "In a certain year B.C. a ship moved up the waters of the Tiber in the direction of Rome." The terms of this statement can be used today in about the same way as some centuries ago, although what corresponds in science to the common term 'water' has today a definition that is different from that of some centuries ago and even of a very short time ago when one did not know the difference between 'heavy' and 'light' water. The terms of science must adapt themselves much more to the new theories than a cluster. Ostwald was not wrong when he said that

the sciences have much to suffer from the fact that the same words are used for very indeterminate concepts of everyday life and for the well-defined concepts of science. The terms of common language such as 'water', 'tree', 'cave', etc., are eminently more stable than the terms of magical, theological, metaphysical, yes, even scientific theories, e.g.: 'taboo', 'nirvana', 'thing in itself', 'heat'.

This stability explains how it is possible that people of different epochs and different ethnic groups can understand each other so well on a great variety of things concerning ordinary life. The stories of rubbed amber and small dancing balls of elder pith are more stable than considerations of the structure of the ether. And it is precisely the least systematised statements such as the phrases of a chronicle that can be transferred from one edition of the encyclopedia to the next with particular facility, though in the meantime the deductive apparatus of science and the list of available scientific terms would have changed enormously.

However, *all these very stable statements are little suited to provide new predictions*. Undoubtedly the formulations of current language are always somewhat modified by ruling doctrines: magical, metaphysical, scientific, and certain groups of clusters are eliminated in time. But what survives is always very considerable as the reading of old epics, inscriptions, etc., makes us see at first glance.

Our whole life consists in two opposite movements: in the one we tend to acquire always new concepts and to modify those that tradition has left us; but in the other we are obliged to take the traditional statements as the basis for our departure. We can never make a *tabula rasa* from which to begin – if I dare to say it – a new life.

This historically given stability of which I have spoken incited many people to offer metaphysical speculations about 'supra-empirical' axioms' that would be given 'prior to all experience', and other things of this order. Helmholtz, who rejected such speculations, still inclined towards the point of view in question when he said – without doubt a little thoughtlessly:

> . . . one is well advised to examine all the more rigorously the grounds of proof against propositions of old authority, the longer these propositions have so far proved to be factually correct in the experience of many generations (Helmholtz 1977, p. 151).

The common language, or more exactly *the* common languages, somehow contain within themselves all humanity known to us. The idea of starting from ordinary statements in the observable field is, in a certain sense, the fulfilment of the program formulated by Avenarius: to choose as one's point

of departure the "natural (initial) notion of the world", that is "the general conception formed by men who are capable of expressing it" (Avenarius 1891, pp. 4 and 5). According to him, this concept of the world is altered on the one hand by 'psychoses' and on the other by 'philosophies'. Here we have an empiricist and even quite a behaviouristic point of view. The problem actually is to know how one can link it with logical analysis (cf. Arne Naess 1936). The study of this problem, which was also discussed at the first international congress for the unity of science (Congrès International de Philosophie Scientifique, Paris 1935), seems all the more urgent as the analysis of 'formalised' languages makes very rapid progress (cf. the works of Carnap, Tarski and others).

The mass of relatively stable ordinary statements *of which the observation statements of the laboratories also form part* (and one is used to calling these the 'data of experimentation') forms the point of departure of scientific theories, which, with their help, arrive at predictions that are checked in turn by common language statements. The very fact that the relatively great stability of common language statements is not linked with a precision equivalent to that of scientific formulas, will doubtlessly continue to provoke research like that originated by Mach, Poincaré, Duhem, Abel Rey, Russell, and before one had the present resources of logico-scientific analysis at one's disposal.

PROTOCOL STATEMENTS

We formulate the positive statements with whose help we check predictions, in ordinary language. We increase the stability of our control statements when we refer in the last instance to 'observation statements.' Suppose we say: "the thermometer stood at some degree below zero"; to the question: "how do you know?" we can reply: "we have seen it." We can formulate these observation statements in such a way that they conform absolutely to the grammar of other positive statements. We have no need of a special 'phenomenalist' language but, as has been shown elsewhere, it is in the 'physicalist' language that we find everything we need.[2] This is what the tenor of one of these observation statements, or 'protocol statements', might be: "Charles' protocol at the point in time 9 hours 14 minutes, at a determined place (for example, his study): Charles' declaration at the point of time 9 hours 13 minutes was: in the room, at the point of time 9 hours 12 minutes, 59 seconds there was a table seen by Charles". This complicated expression can admit of abbreviations depending on the case.

What is essential is that only physicalist terms appear in it, and especially Charles' 'behaviour'.

If one accepts this protocol statement in the encyclopedia, and in addition the statement: "The declaration of Charles was: in the room was a table seen by Charles", as well as the statements: "In the room was a table seen by Charles", implying "that there was a table in the room", then we shall call the protocol statement a 'reality statement'. But if, on the contrary, the protocol was valid, as well as the statement containing the declaration, but not the statement "there was a table in the room", one could speak of a 'hallucination statement' or a 'dream statement', etc. If one limits oneself to formulating protocol statements in the form of those above, one obtains statements of a particularly high stability: for example, one includes statements emanating from a state of civilisation that does not yet separate dream statements from reality statements.

Thus today one can still maintain the following statement: "The people of the sixteenth century saw swords of fire in the sky". Whereas one would reject the statement: "In the sixteenth century there were swords of fire in the sky". But if one had to limit oneself to these statements of particularly high stability, the encyclopedia would be rather poor, as it also would be if one left aside the theories and scientific hypotheses whose degree of stability is ordinarily not very great. To discern which elements of the encyclopedia are particularly stable is very important for scientific reflection in general. It does not follow that one has to discredit those elements of science that are less stable. Auguste Comte, in his *Philosophie positive*, has gone much too far in his extreme aversion toward hypotheses. Moreover he has not been perfectly consistent since he did admit the theory of atoms. We find a similar attitude in Mach who viewed with scepticism all contemporary atomic theories, even Einstein's theory of relativity, though he contributed to its creation. Perhaps the excessive caution of Comte and Mach is connected with the fact that in their time one did not yet have the logical instruments at one's disposal that allow one to begin to find one's way in the awful confusion of more or less stable statements, of more or less indeterminate formulas. We, however, can follow new scientific ways with more confidence if we are aware of the manner in which one has to proceed with manifestly imprecise, equivocal, indeterminate formulas. Now we shall also be able to apply in theories scientific terms that do not occur in everyday statements, but with the help of which one can formulate statements that can be coordinated with confirmable predictions.

The protocol statements are statements of medium complexity and

uncertainty like those familiar to us in current language. In no way do they correspond to the ideal that so many people desire, of possessing 'atomic statements' with which one could compose 'molecular statements', the statements that in fact one uses in ordinary life and in science. We follow quite a different course since we accommodate the empirically given protocol statements a little, without, however, going so far as 'formalising' the common language.

It is precisely the protocol statements and their function of checking scientific theories within the framework of an encyclopedia with empiricist tendencies that separates us completely from formulations of systems that are so often represented as paradigms of science in general. We see how complex is the dependance of the formulation of each protocol statement on the formulation of other protocol statements and on scientific doctrines, without always being able to demonstrate this dependance in detail in each case. We are handicapped from the start by the fact that we must undertake this analysis with the means of the common language itself without being able to benefit from the technical clarity of formalised languages.

SYSTEMATISATION

We have already indicated earlier that we do not agree that the 'model' of our knowledge taken as a whole is the system, that is, this effort to reach an absolute point from which all particular things should somehow radiate. We have shown that we cannot give all formulations the same rigour and that within the great, rather badly coordinated mass of statements scientific systems develop like little islands, which we must try to enlarge. But our criticism of system as model is nevertheless accompanied by very intense work – in the sense of 'scientism' that has more and more consciously developed since Saint-Simon, Comte, Cournot and others – for installing a new order and development in science that, without pretending a universal clarity prematurely, takes as its point of departure the mass of given statements. Our program is the following: No system from above, but systematisation from below.

For this effect we have to turn to certain instruments that have been created in the course of scientific activity. To these belongs the unification of terminology, that is the effort always to use each term that occurs in several different sciences in the same way. The word 'man' that is connected with the property of 'pronouncing judgments' is to be defined in the same way as the word 'man' occurring in phrases containing words such as 'economic

organisation' or 'weighing', etc. With the same term 'man' we can form the following phrases: 'the doctor treats this man'; 'this man has been weighed by his trainer'; 'this man stands in admiration before the Parthenon'. When a man *A* has said, speaking of himself: "*A* is angry", and a person *B* replies: "*A* is not angry", we are dealing directly with the examination of this contradiction without needing to declare that *A* – called the psychical subject of *A*'s statement – differs from *A* – called the physical object in *B*'s statement – and without taking pains to identify them expressly afterwards (cf. Carnap 1935a and Hempel 1935). Equally we do not speak of a certain ball *A* (touched) and of a ball *A* (seen) to declare afterwards that the two are one and the same. In this and similar cases we prefer to operate with unique, well-tried terminology and let the distinction become clear through the situation itself. If one wanted to start disjoining the 'unifications' of our current language, the result would be general confusion, for our everyday language rests precisely on these 'unifications'.

Consider the three following cases: *A* says that "*A* is angry", because he has looked at himself in the mirror, or perhaps has felt his pulse – *A* says that "*A* is angry", without having looked in the mirror or felt his pulse – *B* says that "*A* is angry", because he has observed the conduct of *A*. If we treat these three cases grammatically in the same way, we forego making a fundamental difference between the perception called 'internal' and the perception called 'external'. We have no reason to envisage 'internal' perception separately and give it preferential treatment; but we have no reason either to dismiss it as a number of behaviourists have done, who were more consistent on this point than Comte, who, in fact, had scruples about believing in his own thought and its presentations, but not in his emotional activity. In the latter case man could divide himself into observer and observed, but he could not in the first case. As Comte basically admitted only the observation that looks 'outside', he reduced the field of behaviourism more than seems necessary to us. In addition he insisted on the fact that biology, just like physics, tries to find laws and forecasts, and therefore also that part of biology that we can consider as the substitute of psychology. If one takes this standpoint of '*foreknowledge*', one could say that one could make no valid prediction of the behaviour of a man if one only knows his declarations of his 'internal perception', of the 'world of his representations and sentiments'. Data to be determined in another fashion, on his behaviour, are certainly important if one wants to predict reliably. Whether one can make as well-founded predictions with or without considering 'introspection', is a pure question of fact that is not of major importance for our effort of of systematisation.

Our view is only that one should go as far as possible with the simple resources of a primitive ordinary language – close to children's language – and not apply subtle considerations on this hardly subtle subject earlier than is reasonable. With this remark, however, one does not intend at all to raise objections against special experimental research concerned with what is called 'problems of perception' in which the disjunction of certain current formulas concerning the 'senses' certainly proves useful and can probably also have an influence on the general considerations that we make here. (Cf. Rubin, Tranekjaer-Rasmussen and others). Precisely investigations of this kind, as well as those on the lines of behaviourism (Tolman, Brunswik, etc.) can be of great usefulness for the delicate studies that extend to the behaviourism of science.

Of greatest importance also is the linking of disciplines among themselves by the establishment of 'cross-connections'; these can either be obtained by a logico-scientific analysis of formulas already existing in the particular sciences, or they have to be created by special research. Think of the progressive approximation that takes place between chemistry and physics. A particularly topical problem is this: to what degree can biology and physics be presented from a unified point of view; to what degree, besides the unification of vocabulary (reduction of biological terms to physical terms, cf. Carnap), can the statements of biology be reduced to statements of physics.

The establishment of cross-connections is in close relationship with the question of unity of terminology, with the creation of a 'universal jargon' containing at the same time everyday terms and scientific formulas, the different languages that one can either join together or reduce one from the other. This concrete problem concerning our actual science leads us to more general considerations of the number of possible different languages (cf. Ajdukiewicz) – a question that, from a certain point of view, is of real interest and can perfectly well be discussed.

The unity of logical instruments is revealed as being much greater today than ever before. Thus the notion of probability appears to be of the same nature in all sections of the positive sciences, and this is the case also with the application of the notion of 'type' (cf. Hempel and Oppenheim 1936) which is not only a conceptual, ordering and classificatory operation, but also provides elements for formulating laws.

Without departing from traditional ordinary language one can often already formulate uniformities or rough laws. The lack of precision characteristic of ordinary language contributes, as we have seen, to the generalisation of its use. It formulates the age-old results of the so often renewed experiences of everyday life; that is why it is universally applicable. Our daily

manipulations, our tools, represent the state of science on which our ordinary language rests. Here one could recall the words of J. F. Brown (1934): "The instrument represents the law in action."

When we underline the relative unity of ordinary language it does not escape us that in certain countries and at certain times there are in the ordinary language itself many still unreduced contrarieties that are intimately linked with magical, theological, metaphysical beliefs (cf. Rougier and other papers in the *Actes du Congrès international de philosophie scientifique*, Paris 1935; and the works of L. Lévy-Bruhl). But one can eliminate these contrarieties to a large extent, and there always remains a quantity of common matter.

It is on this common basis of ordinary language that the whole array of sciences is formed that only history can make us understand. What a variety of 'dissections', what richness in differentiation! It is only step by step that one begins to establish unity among the particular sciences, a beginning that we can consider as the "necessary prologue to the unification of science" (Marcel Boll 1921, p. 83). That this process of unification must continue, so to speak, on all levels of scientific formulation, and that, in addition, only collective work makes it possible to achieve this work of synthesis, similar to that which Henri Berr, Abel Rey and others advocate, is exactly what we are trying to show here.

ENCYCLOPEDIA

Let us visualise an encyclopedia that could be the 'model' of our knowledge taken as a whole. If we have given up the traditional wish to see our ideal in a system that is free from internal contradictions and based on the most secure foundations, we can, as shown above, take the mass of statements in use as a point of departure for our considerations. An encyclopedia that does not want to omit essential features of our knowledge would also to have to present statements of which we say that they are in contradiction with each other. One knows how frequent it is in the history of the sciences that two incompatible theories are in use at the same time. The one provides good predictions in a certain field, the other in another. Our effort will therefore be bent towards replacing these theories by others that are in harmony among themselves.

An encyclopedia-model that would not present any contradiction would make us perceive certain particularities of the totality of our knowledge. There is evidently more than one encyclopedia that is free from internal

contradictions and can satisfy our scientific demands. We have no means at our disposal – for logical reasons – for designing and putting one encyclopedia above the others that would be 'THE encyclopedia'. It is the practice of life that imposes on us a certain encyclopedia. Since it takes a great number of people to carry the totality of knowledge of an epoch, it is understandable that through a series of successive assimilations and rejections uniform manners of thinking, in short, are formed. The deviations are only of a restricted number. The powers of an entire generation of scholars are hardly sufficient to perceive all the consequences of a single theory. Most often a theory dies before fully being exploited. In practice it hardly happens that very different competing conceptions join battle in grand style by mutually rejecting their respective results.

Let us imagine that we have to choose between several completed encyclopedias that are in contradiction with each other: very different factors could determine our choice. One can assume, for example, that one of the encyclopedias would strongly develop a certain limited domain while through its structure it would be less favourable to the development of other domains. The other encyclopedia would be characterised, on the contrary, in our hypothesis, by its equal and uniform elaboration of all disciplines but would not give rise to the hope for the certain perfection of work that the first seemed to promise. Since we do not possess a special theory of the importance of these possibilities for the progress of science or life in general or since we cannot assign a rank to these two encyclopedias from the point of view of this theory, we must function as a 'touchstone' ourselves and decide for the one or the other. It is a simple question of fact whether one will ever have a theory that allows one to predict with some degree of certainty the behaviour of people who have to make such a choice.

The encyclopedia that we advocate, the encyclopedia that we use, is an historically given formation to which no 'extra-historical' ideal can be opposed. According to our conception we make efforts to endow this encyclopedia with the greatest logical coherence that we can achieve, to build it up in the empiricist spirit of radical physicalism, as far as one can succeed here, and to make it contain the greatest possible number of disciplines while at the same time incorporating the statements that have so far remained isolated but have been in constant use. We have here a program that links itself to the panlogism of a Leibniz, the empiricism of a Hume, the total science of a Comte. But we are trying to abstain from the metaphysical speculations that were always associated with these three attitudes.

For those who, in a language alien to us, speak of the idea of the true

system of the world, this basic encyclopedia must seem a miserable resignation, a scepticism; whereas we see in it the expression of an activist that we equally meet elsewhere. Starting from the situation in which we live and act we march on as well as we can. And we do not think that we can replace acts by dreams. Many things can insert themselves into a well-knit whole, others are rejected, but perhaps will be carried on further unless they are dropped. This attitude corresponds to that of Jules Romains' original series: *Les hommes de bonne volonté*. Here we encounter several destinies of which some join, others detach themselves and break up: an intolerable spectacle for a novelist, who loves rigorous deduction and consistent development of destinies. Novelists can choose between one mode of exposition and another, but we encyclopedists cannot do that, because the total knowledge of our time is a given thing. Our choice is only between what seems to us to be an alignment of uncontrollable terms and absolute formulations – metaphysical poetry and dreaming – and what we consider to be part of active life. Without doubt, our attempt, after some time, may very well be seen to be rejected by ourselves – this is still an eventuality that we must envisage. But nevertheless, the coming generation will perhaps, better than the one that remained attached to the idol of an absolute system, have the power to work with zest and success in the field of unified science.

Thus the encyclopedia is for us the very territory in which science lives. The representatives of logical empiricism in some way continue the work that d'Alembert, with his aversion to systems, originated. But they are 'encyclopedists' much more consciously, and in a sense much more rigorous than their great forerunners. The encyclopedia can thus become the symbol of a developed scientific cooperation, of the unity of the sciences, and of the fraternity between the new encyclopedists.

[From a French translation by R. Bouvier.]

NOTES

[1] [It will be useful to compare Otto Neurath's ideas on 'system' with those developed by Hugo Dingler (1934), p. 5. – Editor of *Revue de Synthèse*.]

[2] For *physicalism* see 'Physicalism', pp. 52–57; 'Protocol Statements', pp. 91–99; 'Radical Physicalism and the 'Real World', pp. 100–114; 'Individual Sciences, Unified Science, Pseudorationalism', pp. 132–138; and 'An International Encyclopedia of Univied Science', pp. 139–144, as well as Neurath 1973b, 1933, 1935d, 1936b. See also (Schlick 1979c, Hempel 1935 and Carnap 1935a).

CHAPTER 14

PHYSICALISM AND THE INVESTIGATION OF KNOWLEDGE

Discussion of Ake Petzäll: *Zum Methodenproblem der Erkenntnisforschung* [Problems of Method in the Investigation of Knowledge] Göteborg 1935.

As *Theoria* is conceived as a forum for discussions between adherents of different views, I should like to open the debate on the investigation of knowledge that was expressly suggested by Petzäll in his book, since in his investigation he deals with a view that I advocate myself.

In his book Petzäll refers to an intricate debate that is presently taking place concerning physicalism, predictions, protocol statements, etc.[1]

The rapid development of modern logical empiricism had the effect that there are still all sorts of points in dispute, even within the Vienna Circle. Petzäll takes pains to interpret certain careless phrases with some suppleness in order to do justice to the general tendency of the Vienna Circle. Certain formulations that speak of '*Konstatierung*', that can sometimes be treated as statements, sometimes as non-statements, formulations that speak of unchangeable statements, and of the comparison of statements with reality, and other tenets have certainly made an occasional appearance, so that with further careless application they can give rise to problems of knowledge in the traditional sense from which logical empiricism distances itself in principle. Here I do not want to deal with Petzäll's presentation and criticism of these conceptions but only to elucidate the problem of knowledge, as formulated by him, from the standpoint of logical empiricism. A radical physicalism – this is to denote the total conception, not merely a special tenet – does not lead to a theory of knowledge of its own as Petzäll demands (p. 82). If, for example, within physicalism we use the term 'validity', we deprive it of any 'absolute' meaning and avoid what we call 'pseudo-problems' of the theory of knowledge. We best start from the operation of science and look at its procedure.

When we formulate concrete predictions and want to verify whether they come about or not, we often have to combine statements of the most different disciplines, statements of biology and mechanics, of sociology and geology. Whenever we use a term in several sciences, we want to define it equally in all. This does not reduce its applicability and avoids confusion. The term 'man' can equally be used by biology, mechanics, sociology, etc.,

Translation of Neurath 1936e [ON 230].

whether in a sentence like: 'Man digests in a certain way', or in sentences like: 'Man is weighed in a definite manner', 'Man forms a warring group together with other men', 'In certain layers of soil we find residues of men'. The planned unification of terminology which facilitates 'cross-connections' from science to science becomes the foundation of a uniform language of the sciences. Within these considerations we can treat the coordination, for example, of psychology, biology, mineralogy, etc.

Problems hinted at by Petzäll appear, for example, where we use statements in which the term 'statement' occurs, such as: "The scholars of a certain epoch made experiments, undertook voyages of exploration, formulated statements of a certain kind' or 'Scholars who are under the influence of great amounts of alcohol formulate different statements than scholars who have consumed no alcohol'. Such statements belong to the sphere of a 'behaviouristics of scholars'.[2] But in our total science we shall also find statements like: 'This group of statements is of equal content with a second group of statements of the same language' or: 'This statement is in contradiction with other statements within a certain system' or: 'From the statements "Homer is a Negro", "all Negroes are poets" follows: "Homer is a poet" '. These statements are not treated in the behaviouristics of scholars, but in logic.

Taking up the thread of the last example, instead of putting the question as to which statement follows from which other statements, we can put the question: which of these statements is in harmony with other statements used by us, which not. For example, the first statement 'Homer is a Negro' is in contradiction with a group of statements in the chapter under the heading 'Homer' in the history of literature. Or, one states for example that although many Negroes have a poetic disposition, the statement 'All Negroes are poets' is in contradiction with the stock of scientifically guaranteed statements. However, the statement 'Homer is a poet' is in harmony with the scientific statements accepted by us. Here it has to be stressed that the term 'accept' belongs to behaviouristics. We can think of the mass of statements that we accept as being unified in an encyclopedia. By using the term 'encyclopedia', we avoid speaking of 'THE' anticipated system from the start.

In the actual encyclopedias that we use, there are often theories in contradiction with each other, which, if restricted to certain areas, however, produce good predictions. But we could have the notion of a model encyclopedia in which there are no such contradictions. In it we could, for example, study how the statements above are linked with observation statements and more things like that, without being forced to enter into the problem of freedom

from contradiction. The reduction of testing with observation statements – protocol statements[3] – would determine the empiricist character of the encyclopedia. If now we found out that a given statement occurs in the encyclopedia used by us, or can be deduced from the statements of this encyclopedia, then we could say that this statement is 'valid' for us.

However, as has already been shown by Duhem, Poincaré and others, we cannot say of isolated positive statements that they are 'valid'; this can be said only in connection with masses of statements to which these positive statements belong.

It has become evident that the use of the terms 'true' and 'false' easily leads to all kinds of difficulties. One can completely renounce the use of these terms, but one can also try to redefine them appropriately. It would, for example, be perfectly expedient to use the term 'true' for all statements that are 'valid' for us in the sense given above, that is, are either part of our encyclopedia or can be deduced from it. 'False' would then be the qualification of the statements that are in contradiction with the encyclopedia. In addition, there would be statements of which we cannot decide whether they are 'true' or 'false', as well as statements that through their structure or special grammar could not be placed within the language of the encyclopedia – in general 'isolated' statements, which are statements 'without meaning within a certain language'. For these statements the Vienna Circle has often used the term 'metaphysical statements'; and this is felt by some people to be emotional or offensive.

If we avoid such 'isolated' statements, we can build up an encyclopedia that contains only statements that can be interconnected. In such an encyclopedia, we find, for example, hypotheses that are in harmony with many observation statements and contradict only a few; the observation statements they contradict may themselves have little connection with others so that doubt about these observation statements would have no major consequences. On the other hand there are other hypotheses that are supported by perhaps few observation statements and contradict many that are moreover rooted in the total mass of statements. In this sense one can speak of 'confirmation' or respectively a 'shaking' of [confidence in] hypotheses, theories, etc. But within the encyclopedia as envisaged by us one could not speak of 'verification' or 'falsification', not even of a 'limit' to which confirmation or shaking would approach. Verification and falsification need as a premise the use only of precise terms, the discussion only of precise formulations as is actually the case if we move in the sphere of 'pure' theory. But the total encyclopedia with all its observation statements necessarily also contains

terms that are just precise enough to be used within certain boundaries. For example, we do not use completely precise terms when we say: "Man A formulates: in the room was a table perceived by A." But this kind of formulation as known in everyday language is always needed where predictions are empirically checked by confronting predictions with protocol statements.

I want to call the sphere in which formulations are not rendered completely precise, the sphere of the 'cluster concept' [*Ballung*]. It is characteristic of the encyclopedia of unified science that not only are the statements more or less closely interconnected, but that there also occurs among them those of the cluster type. The term 'contradiction', 'equal as to content', etc., are at first defined for the sphere of the precision of logic and are only meaningfully transferred to the sphere of the cluster. In the sphere of clusters we often use equal terms in different cases although we can suppose that some time later this use cannot be maintained. This last statement itself, for example, is a historical, and not a logical statement. These all are important considerations of the analysis of science, but they do not take place within a special theory of knowledge: rather they are *either* logical *or* scientific considerations. Let us now approach the problems with which Petzäll is concerned, from another angle.

In the encyclopedia we shall find certain hypotheses supported by statements of the following kind: "Many scholars have made such and such observations on their exploratory travels." We have already hinted that such references are a mark of the empiricist character of the encyclopedia concerned. But one may now wish to stress that the scholars have made these observations 'consciously'. One can use this term 'conscious' always if, for example, one makes the hypothesis that the scholar, if asked, would have *said*: "I have made the following observation ... " We then say: the scholar has *formulated*: "I have made the following observation." That is, this statement is 'coordinated' to him to characterise a state of consciousness. Whether one considers this state of consciousness, this state of formulation, as a state of 'speech-thinking' that is connected with changes in the speaking apparatus, depends on the special hypotheses that we advocate. This is not essential for our considerations here. Therefore when we say, "The scholar was in the state of a certain observation", this must not mean that we also say: "The scholar made certain movements to innervate the larynx." This would be a special hypothesis of 'behaviourism', of an empiristically arranged theory that is not under discussion here. Besides the statements that we pronounce we thus get in addition the statements that we think are coordinated to

certain 'states of consciousness'. Checking a prediction consciously would then be a confrontation of one statement with another statement, whether this statement is pronounced by the tester or coordinated in the way described above. We can logically compare all statements with each other, whether they are pronounced or only coordinated.

If one restricts the term 'compare logically' to statements – this is a safeguard against many confusions – we are not prevented from saying: "The statement 'this chair has four legs' has one word more than the chair has legs" or in short: "The statement *A* has more words than the chair *B* has legs". '*A*' and '*B*' are just names of things. It depends on our agreement whether we want a certain statement to be understood as being only the one with which we are confronted or each statement that is logically equal with it. Along the lines of our terminological proposal we would not say that the man 'compares' a statement with a chair, or a chair with a table, if we could not add: the man compares the statements: "The statement has 5 words" and "The chair has 4 legs". Without these additions the word 'compare' would, according to our proposal, be here as inappropriate as the case of a magnet; we do not say either that the magnet compares wood and iron and chooses iron. When I say I 'compare' what is printed in a guidebook about a church with the church itself, this would be expressed according to our proposals: "The guidebook contains the statement 'this church has two steeples'" and "I formulate the statement 'this church has two steeples'". The two 'inserted' statements coincide; to put it less carefully: the guidebook is confirmed by experience. Such careless phrases are tenable as long as one does not slide over from the 'comparison of the statement with the chair' to a 'comparison of the statement with reality'. For if one wanted to speak of the totality of things as of 'the' reality, then one might at most, though uncautiously, say that one compares 'parts of reality', for example, written statements, with other 'parts of reality', for example, chairs. But in this way one would never reach the formulation 'language is compared with reality', not even if one suggested the formulation for formalised languages (with Tarski, semantics): a statement 'fulfils' a state of fact. Formulations like these are always meant in relation to a definite language, and one would never get to the confrontation of statement and reality; language and reality; thinking and being; knowledge and reality; subject and object; logical form and experience etc. – all these are formulations that Petzäll quotes as the starting point for considerations of the theory of knowledge (cf. his p. 7, 56, 60, 66).

All the problems that we have discussed are linked with 'statements about

statements and other things', to keep to the usual manner of speech; but if we apply some caution, we do not arrive at all at the questions in the theory of knowledge that are put by Petzäll.

Our endeavour has the aim, among others, of evading the pseudo-problems that come up if one continues a careless traditional way of expression that starts from the opposition between 'I' and 'world' and easily glides into speculative philosophy. Therefore we take special care that the testing of predictions is formulated so that we can always replace the term 'I' by the twice quoted personal name in the manner of children's language.[4] All terms that occur in protocol statements can also occur in biological, sociological, chemical, etc. statements. Protocol statements are not absolutely distinguished either by terms or by validity. In principle it can happen that one also changes protocol statements. But in general there is little cause for this (about which no more can be said here). The protocol statements are of especially great stability – historically speaking (see Otto Neurath, 'Pseudorationalism of Falsification', pp. 121–131).

The protocol statements do not contain special terms of the 'theory of knowledge', etc. that do not occur in the sciences themselves; this is essential for physicalism which undertakes to show that the terms of all sciences (biology, psychology etc.) can be reduced to the terms of physics. However, this tenet that the terms of all the positive sciences, that is, also the terms of observation statements, can be reduced to terms of physics, is *not* identical with the tenet treated by Petzäll, that all statements and laws of the individual sciences can be reduced to statements and laws of physics (see p. 16). How far the statements of one positive science can be reduced to the statements of another, requires a special investigation that takes place within physicalism on the lines mentioned.

Physicalism allows us a formulation like this: "By burning sodium in the laboratory, scholar *A* was caused to write down the statement: 'sodium can be burned'." It would, however, be a matter for misgiving if one would then somewhat vaguely declare that the statement of the scholar was physically 'reduced' to an event.

Physicalism allows us to formulate more than one behaviouristics and can be in agreement with more than a single type of scientific psychology, as it is represented on the whole, for example, by American 'behaviourism' (cf. Petzäll p. 20). It is precisely the Vienna Circle that stresses that behaviourism as formulated by Watson seems too narrow from the standpoint of a pure special science and also contains all sorts of things that cannot withstand physicalist criticism.

It has rightly been pointed out that it would be better to speak of the *languages* of physicalism, since more than one language could satisfy the conditions demanded by physicalism in principle. Here again it is shown that logical empiricism takes pains to replace absolute formulations with their 'either-or' by several possible 'suggestions'. In practice, however, we advocates of this 'tolerance' will make some pronouncements with greater definiteness than our own principles allow: then we just have to 'relativise' each other.

If a language is given, we can discuss 'logical' and 'physical' deductions; within a language we can 'base' a statement on other statements; but we have not defined what it should mean to 'base' the whole of languages (in short 'the' language) on something. From the standpoint of logical empiricism, therefore, no reproach can be seen in the fact that "physicalism with its own method cannot find anything on which to base physicalism" (Petzäll p. 27). We can only show how one can go on with the help of only a physicalist language, without a 'phenomenal' language, how a phenomenal language can be reduced to a physicalist one, how certain positive sciences are connected with each other. One can even analyse the scientific activity of the physicalists historically, but all this does not lead to Petzäll's problem of the theory of knowledge. The inquiry into the theory of knowledge seems to us occasionally like a formulation of the demand that one should 'speak about language without a language'. To be consistent, logical empiricism can only arrive at discussing the question how one formulates meta-languages and further meta-meta-languages. But these problems, which are discussed especially by the Polish logicians, do not lead to a special sphere of the theory of knowledge as they in no way produce an 'absolute' position. Physicalism uses the concept of 'validity' in a historical sense and with reference to a certain mass of statements, it does not arrive at formulations of 'dignity' (Petzäll p. 58) that should somehow lead us to 'the real world', to the 'one true world in itself'.

Historical considerations were perhaps not amiss at raising some doubt about some kinds of apparently quite obvious problems in the theory of knowledge by revealing their origin. Petzäll rightly points out that the formulations of the modern theory of knowledge are strongly conditioned by scholasticism (p. 52). Since the whole of modern philosophy has its origins in the Scholastics, it is not astonishing that also logical empiricism is the continuation of certain scholastic forerunners, the nominalists.

Our comments can only be seen as suggestions for further discussion. Representatives of logical empiricism especially have to be aware of the risk that more general debates of this kind descend into vagueness; the endeavour to get rid of pseudo-problems is no guarantee of success. This has to be won

by hard work in the whole of science, by elaboration of precision, but also by more precise rejection of excessive precision. According to our view, analysis of science by logic has to prove itself by successes in the sphere of the sciences. However, it could be shown in which sense the theory of science of logical empiricism can concern itself with 'research into knowledge', that is, above all with problems of the logic of science. Though much may still need clarification, the present state of research gives no cause for the assumption that we need specific terms and specific statements of a separate 'theory of knowledge' besides the statements of science as a whole (including the logical disciplines) for the building up of our science. Taken as a whole, the work of modern logical empiricism, especially the work of the Vienna Circle, moves in the direction of the logic of science.

[PETZÄLL'S REPLY:

It is of special value that Otto Neurath in his comments on my book *Zum Methodenproblem der Erkenntnisforschung* tries to throw light on its treatment of problems from the standpoint of logical empiricism instead of referring again to earlier discussions of some phrases that I had criticised, among which were some that Neurath himself characterised as 'careless'. For my part I shall not refer to what I have written in my book but shall more closely discuss Neurath's points of view as explained in *Theoria* with regard to a continuing debate.

Neurath states that radical physicalism does not lead to its own field of the theory of knowledge (p. 159). According to Neurath the task of the so-called theory of knowledge is taken over by two sciences, by the logic of language and the so-called behaviouristics of scholars. The difference between these two spheres of research is characterised by examples that Neurath gives of statements belonging to these spheres (p. 160). It is especially stressed "that the term 'accept' belongs to behaviouristics".

With reference to Duhem, Poincaré and others it is stressed that a statement is only 'valid', can only be 'accepted', if it can be linked with other statements. All accepted statements can be thought to be assembled in an encyclopedia. If I understand Neurath correctly, the term 'encyclopedia' is used to avoid misunderstandings that might arise if one spoke of the totality of all true statements in science or of something like that. But as it is necessary to understand the task of logic and the task of behaviouristics correctly, it must be said more precisely how the term 'encyclopedia' has to be understood. It is mentioned that we use 'concrete encyclopedias' in which contradictions occur. These concrete encyclopedias are confronted with a 'model-encyclopedia',

a construction in thought, in which no contradictions occur. No indications are given how this encyclopedia is to be constructed. Neurath only hints that the reduction of testing to observation statements establishes the empiricist character of this encyclopedia (p. 161). The structure of this encyclopedia might therefore be a matter of behaviouristics. It should contain only accepted statements and acceptance is a matter of behaviouristics. Since, however, no contradictions should occur in this encyclopedia, it must also satisfy a demand of logic. To understand Neurath's starting point – the difference between logic and behviouristics – it might therefore be necessary to get to know more about the way in which these two spheres of research participate in the construction of the encyclopedia.

That this is of greatest importance becomes immediately clear in the following comments. It is stated that a model-encyclopedia can only be built up if we avoid 'isolated' statements, so that it "contains only statements that can be interconnected" (p. 161). In the following sentence, however, it is said that even in such an encyclopedia, hypotheses occur that contradict certain observation statements! That is, the encyclopedia does contain statements that cannot be interconnected. That this obviously contradictory condition for the encyclopedia has crept into Neurath's comments can probably be explained by a confusion of the logical with the behaviouristic point of view.

It also becomes clear that for the right understanding of Neurath's comments it is necessary to know precisely the difference between logic and behaviouristics, when the question is discussed as to what it means to 'compare logically'. If I say that I compare what is printed in my guidebook with, for example, a church, this means, according to Neurath, the following: I compare 'the guidebook contains the statement "this church has two steeples"' and: 'I formulate the statement: "this church has two steeples"'. It is then said that the two 'inserted' statements coincide. The first of the two 'inserted' statements is "this church has two steeples" and the second: "this church has two steeples". It is not correct to say that these two statements 'coincide'. These are not two statements at all, but one and the same statement in two different linguistic contexts. The question now is what made Neurath speak here of *two statements*. May one perhaps say: logically the two statements are identical – but behaviouristically different? If we simply write the two statements down, they tell us nothing. It is only when we take into account the linguistic 'environment' of the two statements that they make sense, only then can they be 'compared' with each other, but what does 'comparing' mean here? If it is the procedure that Neurath calls

'behaviouristic', comparing could perhaps be formulated in a statement like this: "I state that there is a statement in the guidebook which is identical with a statement formulated by me." According to Neurath this does not signify that the statement about the steeples of the church should be 'true' or 'in harmony with reality'. It only signifies that it can be used in a system of statements or in an encyclopedia. But there is still the question whether this usability is stated logically or behaviouristically. If it is printed in the guidebook that the church has two steeples but I formulate, "the church has one steeple", only one of these two statements can be 'used'. Which of them can be used can, of course, only be determined, according to physicalism, by comparison with new statements. *How* I should find these new statements is, according to Neurath, a matter of behaviouristics. Whether the statement that I formulate coincides with the statement in the guidebook or not, is a matter of logic. But how are logic and behaviouristics *combined* here? If – as Neurath seems to suggest – we want to build up an encyclopedia that contains only statements that do not contradict each other, it seems to be the task of 'behaviouristics' to find out what it means for the usability of statements, if, for example, two identical statements appear in different 'linguistic environments'. But how can the result be examined? Obviously only by new statements. If this should not go on endlessly under tautological transformations, it has to be indicated *how* logic and behaviouristics *cooperate*.]

NEURATH'S CONCLUSION:

I gladly take the opportunity to discuss Petzäll's remarks and to give further clarification of some points. As I had already stressed at the end of my comments, we can examine the usability of proposals of scientific formulations only within the work of the sciences. That is why I pointed out (p. 160) that everything that can be used by us from the traditional theory of knowledge must be part of the whole of science. The two disciplines, logic of language and behaviouristics of scholars, which Petzäll mentions in connection with my article are therefore to be seen only as examples; there could be other examples from other disciplines.

I have suggested the term 'encyclopedia' primarily in opposition to the term 'system' by means of which a kind of total science based on axioms is postulated that has to be discovered, as it were. Such a notion is especially dubious if one starts to give the outlines of such a system – a circumstance that has already been pointed out by the leader of the French encyclopedists, D'Alembert.

If we take the empirical as our basis, then we can think of the scientific statements that we use together with the scholars of our scientific community as being assembled, so to speak, in an encyclopedia. Experience shows us that it is already difficult to decide which statements we want to eliminate, that often it cannot clearly be recognised whether certain formulations are admissible within the scientific language or not. But even if we eliminate the statements which we are convinced are 'isolated' statements, for example, "Das Nichts nichtet" [the nothing nothings] then there can still be non-isolated statements whose consequences contradict each other, although we know from experience that the one group of these contradictory statements can be successfully used in one area and the other group in another area. A 'concrete' encyclopedia will therefore well be able to eliminate the 'isolated' statements, but it will not straightaway be able to avoid certain statements that contradict each other, though one may hope that the elimination of these contradictions within the sphere of non-isolated statements will succeed.

For certain exemplifying discussions we can sketch a 'model-encyclopedia' that we think free from all discovered contradictions, of course from all statements that we qualify as isolated. Where I speak of 'concrete' encyclopedias, I also speak of possible contradictions between hypotheses and observation statements. A concrete encyclopedia that no longer contains any isolated statements, but only statements that can be 'interconnected', may still display contradictions, that is, statements that can be interconnected.

The question which contradictions can just be tolerated, which not, how one behaves altogether in the development of the whole of science, is a question of behaviouristics, of history of science, of behaviouristics of scholars. But the discussion of contradictions, the discussion of the question, which groups of statements are logically of equal content, belongs to the sphere of logic. If I am occupied with the behaviour of people who produce encyclopedias, I am concerned with behaviouristics; if I am occupied with the logical interconnecting of statements themselves, I am not concerned with behaviouristics.

The other remarks of Petzäll relate mainly to the discussions of 'truth', to the 'comparison of statements with reality' and to similar matters. One can renounce the term 'true' altogether. But one can also try to preserve a frequently occurring peculiarity of the discussion of truth within a changed environment. Very often a statement is confronted with the 'higher dignity' of 'reality', and it seems that the concluding remarks of Petzäll indicate that he also expects to be given a 'final' criterion, an 'absolute' basis of all discussions, as he has special misgivings about being led from each verification

to a new verification, from these observation statements to those, without being told where this work of testing would find its end.

Logical empiricism leads to an occupation with predictions and their testing, but it has no cause to agree to the proposal to take certain statements as 'final', never to be altered. Time and again the statements of science are altered, and though the protocol statements may be of special stability, they are not excluded from alteration in principle. The statements of the encyclopedia that we have accepted can, however, at times be used as an 'authority' to characterise certain individual statements as false or true, depending on their acceptance or rejection on the basis of the decisions of this authority. The mass of statements that is used as court of law can be changed, however, at a later time so that we do not reach an absolute basis on this way. The peculiarities of statements that lead to contradictions are treated by logic; 'contradiction', 'deduction', etc., are logical terms. 'Acceptance', however, is not a logical term.

Whether one says about two statements printed on paper that they are logically the same statement or that they are two statements with the same logical content, does not touch on the question of behaviouristics, but at most on the question of whether one wants to operate a 'physics of statements'. It depends on my speaking of statements, or of the people who formulate the statements, whether I move in the sphere of logic or behaviouristics. When I discuss the problems of unified science and encyclopedia, I can, if I wish, separate the behaviouristic considerations strictly from those about the logic of science, but I can also use both ways of consideration alternately within the same book. But thereby I still do not arrive at special statements of a theory of knowledge.

These discussions strongly carry the character of 'formulations in defence'. It would certainly be advantageous for our further discussion if Petzäll would quote examples from the operations of science that show that we cannot properly work with the means of logical empiricism, which does not accept statements of a special theory of knowledge, examples that show we need special statements of a theory of knowledge in addition to the statements of the various individual sciences.

NOTES

[1] Petzäll has singled out Carnap, Neurath, Schlick as the main representatives of the debate. In his *Allgemeine Erkenntnislehre* Schlick has in many ways prepared physicalism as we advocate it now and that was also in principle the aim of Mach and others.

Now, as ever, I think that the differences within the Vienna Circle, in spite of their pointed formulation at places, concern only a marginal area. Precisely in his most recent contributions Carnap stresses physicalism and all that we have in common. Besides the articles in *Erkenntnis*, especially those in *Analysis* should be inspected, and now the papers and discussions of the First International Congress for the Unity of Science in Paris, 1935; see the remarks of Ajdukiewicz, Ayer, Braithwaite, Fred Bon, Frank, Hempel, Juhos, Lutman-Kokoszynska, Mannoury, Popper, Poznanski and Wundheiler, Rougier, Russell, Tarski, Vogel, Zilsel.

[2] For the term 'behaviouristics' see (Otto Neurath 1933).

[3] For example: "Charles' protocol at a certain time was: Charles' formulation was: In the room was a table perceived by Charles."

[4] See note 3. The terms 'now', 'here' etc., will always be used so that they can be replaced by coordinates of time and space. If it is said in this essay, for example: "I am of such-and-such an opinion now", this could be expressed in detail: "Neurath says Neurath is of such-and-such an opinion in 1936."

REFERENCE

See (Neurath 1933).

CHAPTER 15

UNIFIED SCIENCE AND ITS ENCYCLOPEDIA

I

The movement for the Unity of Science has been making headway for a number of years, and each year more workers in the various scientific domains have expressed their interest in it by participating in the discussion of its aims and its specific problems. A characteristic feature of this movement, which explains this growing interest, is that it does not propose a 'super-science' which is to legislate to the special sciences. Its proposal of a Unity of Science is of a different kind, not based upon some grand metaphysical view of any sort. Those who are active within this movement are emphatic in their insistence that instead of aiming at a synthesis of the different sciences on the basis of a prior and independent philosophy, the special sciences will themselves supply their own synthesizing glue. *For appreciating* the significance of this movement it is therefore necessary to keep in mind that its tendency is toward a *unified science*, departmentalized into special sciences, and not toward an artificial and speculative juxtaposition of an autonomous philosophy *and* an autonomous group of sciences.

The key to this movement is to be found in the fact that the special sciences themselves exhibit in various ways the need for such a unification. For example, the different psychological theories employ so many different terms and phrases that it becomes difficult to know whether they are dealing with the same subject or not. Thus, individual psychologists analyze the case of a man who becomes ill when he finds an examination more difficult than he had expected; reflexologists discuss the case of a dog which becomes ill when it is required to discriminate between sound signals that are too complicated for it, as a condition to obtain food. And yet these different psychological schools employ completely different terms and rules for combining them when they explain these essentially similar situations. Gestalt psychologists use a distinctive formulation quite different from other schools of psychology, when they explain, for example, the deliberative activity of monkeys seeking food; and behaviourists, in accounting for the trial and error

Reprint of Neurath 1937b [ON 239].

activity of rats seeking food use a still different language. The difficulty of communication is further increased when we examine the formulations of psychoanalysts. It is natural to seek a common platform for all these different investigations and theories. Similar illustrations could be given from biology, sociology and other sciences.

Many difficulties occur in connection with problems which are on the borderline between the different sciences, for example, at the points of contact between biology and physics. This is especially the case when the larger issues between vitalism and its opponents are being discussed. In a great many cases it is not clear whether the difficulties arise because of a defective formulation of the problems or because of defective and insufficient investigations.

One of the most important aims of the Unity of Science movement is therefore concerned with the unification of scientific language. Distinct terms occur in different disciplines which nevertheless may have the same function; and much fruitless controversy may arise in trying to find a distinction between them. It is, for instance, an open question whether in biology the special terms 'stimulus' and 'reaction' are needed, or whether the usual physical terms 'cause' and 'effect' are sufficient. A large collection of terms have been gathered by the various sciences during the centuries, and it is necessary to examine this collection from time to time, for terms should not be multiplied beyond necessity.

Such a movement toward a unified scientific language is fundamental for eliminating a great many so-called metaphysical questions. For example, within the last few years heated discussions have occurred over the problem of the 'freedom of the will' in connection with the theory that an atom is 'free' to choose different paths for its motion. Whether there is an intimate connection between the behaviour of atoms and of living organisms is of course a genuine problem, but it has no relevance to the one mentioned above.

It is by no means an easy task to unify scientific language. A great many terms of the successful special sciences are not univocal and clear. This is especially the case when new theories are in the process of growth. It is not certain in such cases that the new terms and formulations will continue to be employed in the future. Our attempt to systematize science must leave those questions which can be decided only by future investigations. And we must keep in mind that future investigations will themselves evolve new open questions. It is therefore misleading as well as confusing to speak in anticipation about THE SYSTEM of science, as if the form and content of

such a system were determined once for all. The unification and systematization of science are permanent activities.

It will be clear that the attitude here indicated is incompatible with one which assumes certain fixed elements, which are forever beyond dispute. Those philosophers who operate with such fixed elements, *a priori* principles, absolute truths or similar apparatus are simply continuing the Cartesian tradition. Descartes supposed all our daily knowledge and experience eliminated in order that he might start anew with a handful of indubitable truths upon which to erect all the sciences. In a certain sense that is also the tradition of Kantianism and similar schools, even though further qualifications would have to be added to differentiate these schools from one another.

The Unity of Science movement is part and parcel of the broad stream of scientific empiricism which is an extension and development of our daily experience, employing initially the language of every-day use. In the main this is an attitude similar to the Commonsensism of Peirce, the 'natural world view' of Avenarius and the theory of experience of Dewey. According to this outlook, all the predictions of the sciences start with and are controlled by observation-statements formulated in our every-day language. We begin with terms at first very vague, but which can immediately be seen to be capable of greater precision. The more one reflects upon this matter the more one is led to abandon the notion of *absolutely precise terms* even as limits of a refining process. The supposition that there are such limits seems to me to be another illustration of what may be called 'Pseudo-Rationalism.'

II

The program of the unification of scientific language requires a logical analysis of the sciences. The history of science during the last decades shows the importance of such an analysis for the progress of concrete scientific work. Within the framework of the general critique of the traditional concepts of space and time the analysis of Ernst Mach was of fundamental importance in preparing the way for Einstein's theory of relativity. Perhaps a very remarkable feature of Mach's work is the fact that his far-reaching conclusions did not depend upon new experimental data, but simply upon a rigorous logical analysis of the traditional formulations. He of course employed all the knowledge common to physicists of his day. Mach's work is a milestone in the evolution of the modern analysis of the sciences, which was further developed by Poincaré, Duhem, Boltzmann, Bertrand Russell and others.

The technique of such logical analysis was gradually perfected and employed consciously. As a consequence, feats of logical analysis which were heretofore possible only to men of genius may now be taught systematically to scientists of ordinary attainments. This method of analysis is the subject matter of the new discipline called the *Logic of Science*.

What is called modern symbolic logic or logistic has been cultivated not only as an autonomous discipline but always in more or less close reference to its use as an instrument of the logical analysis of the sciences. The evolution of modern logic makes possible the organization and utilization of all research as upon the foundations of mathematics; and at the same time it clarifies the application of mathematical and other calculi to concrete subject matter. We have thus become clearer concerning such matters as the relation between what are called pure and physical geometry, or the logical structure of the probability calculus and its relevance to all the concrete sciences.

The contacts between modern logistic and the modern analysis of science have already begun to alter the general scientific attitude of our period. Even those workers who have not even an inkling of the logistic calculus or who make no use of it as an instrument, nevertheless can carry on scientific research in the spirit of modern logical empiricism.

In the history of human thought logic was frequently cultivated by metaphysically oriented philosophers, especially by different types of rationalists. As a consequence, scientific empiricism received the support of workers using different methods of empirical research and of applied mathematicians – but not of logicians. But as an outcome of the fact that modern logic has as one of its roles the task of supporting the empirical sciences, the empiricism of our time has acquired an altered physiognomy. The logical calculus in its widest sense becomes an essential apparatus of a unified science, and all logistic research acquires very great direct and indirect importance for the evolution of the logic of science.

The task of modern logical analysis of science is to build up a more consistent framework for the special science and for a unified science. The process of the logical organization of a single science cannot be divorced from the process of building up bridges or connections between the different sciences. A logical empiricist in carrying through an analysis within one science must always keep his eye on the interrelations of all the different disciplines. A concern of this sort for the logical organization of the sciences, which aims at a comprehensive and suggestive survey of them, is an alternative to and a possible substitute for the traditional philosophical attitude.

As has already been indicated, the unification of scientific language is a

special and technical task. The fundamental thesis of our movement is that terms similar to those employed in physics and in our everyday language are sufficient for constructing all sciences. This thesis, known as Physicalism, has been progressively confirmed by special investigations in recent years.

Within the broad framework of Physicalism a great many different proposals may be discussed. Various attempts to formulate the program and technique of Physicalism have been made within the Viennese Circle, and within other groups having a similar orientation. Of greatest importance are those techniques which show us how to use consistently the language of daily life as the basis for all our scientific work, and to correlate the more or less complicated formulas of science with the statements in our everyday language. Carnap has proposed certain special devices, which make possible the introduction of terms so that they become dependent upon physical terms without, however, being 'defined' by the latter in the strict sense of the word. A very important discussion is now in progress concerning the manner in which all scientific terms depend upon physical statements and the manner in which all scientific laws depend upon physical laws. For instance, one problem is whether biological statements can be *formulated* by means of physical terms, exclusively; a problem distinct from this one is whether biological laws can be *derived* from physical laws. It is possible that at a given time, for example, the present, the former could be affirmed while the latter is denied. This nest of problems is of particular importance in the fields of biology, the theory of behaviour and sociology.

A further problem of great importance is the manner in which we can incorporate into one language statements occurring in educational discussions, discussions about art, ethics, law and other subjects like these. It is possible that the result of such attempts to incorporate statements of different kinds within the framework of the language of scientific empiricism will lead to a change of the traditional demarcations between the different separate disciplines. Some interesting possibilities are already apparent. The fruitfulness of the unity of science movement is indicated by the fact that more and more students of special fields participate in the discussion of the relation of their discipline to other domains. In this way bridges between different fields are constructed at points where only isolated domains were found before.

III

If we reject the rationalistic anticipation of *the* system of the sciences, if we reject the notion of a philosophical system which is to legislate for the

sciences, what is the maximum coordination of the sciences which remains possible? The only answer that can be given for the time being is: *An Encyclopedia of the Sciences*.

It is toward the realization of such an encyclopedia that the present efforts of the International Unity of Science Movement are directed. The initial plans for such an encyclopedia were proposed many years ago within the Viennese Circle. The first public formulation of this plan was presented to the First International Congress for the Unity of Science in Paris, 1935 [1] which agreed to collaborate on the project and to promote it. The Second Congress, held in Copenhagen, in 1936, resolved that the theme of the Third Congress, which is to be held in Paris, July 29 to 31, 1937, is to be the Encyclopedia.

A committee consisting of Rudolf Carnap (Chicago), Philipp Frank (Prague), Joergen Joergensen (Copenhagen), Charles W. Morris (Chicago), Otto Neurath (The Hague), Louis Rougier (Besançon and Cairo), is at work upon a detailed prospectus of the Encyclopedia. The University of Chicago Press has already arranged to publish the first two volumes, which are to appear in 1939, in time for the Fifth International Congress for the Unity of Science, to be held at Harvard University. This arrangement is contingent upon there being a sufficiently large number of subscribers. The estimated subscription price of one volume is about $7.50, while the price for non-subscribers will be about $10.00.

The new Encyclopedia is so planned that it will provide an opportunity for a meeting of minds of scientific workers of different kinds. For the project is based on the idea of scientific tolerance within the framework of scientific empiricism. This will be clear from some details concerning the early volumes of the work.

The plan is to publish two volumes at first, containing sixteen to twenty pamphlets on the Foundations of the Sciences. These will therefore contain articles on the Logical Analysis of Science, General Linguistics, Mathematics and Logic, Scientific Procedures of Empirical Sciences, Probability, Physics, Cosmology, Biology, Theory of Behaviour, Social Sciences, Empirical Axiology, History of Science, History of Logic, History of Empiricism, Logical Empiricism, as well as a bibliographical survey and an index to the volumes. The following have already agreed to collaborate in the writing of these volumes: Andrade, Brunswik, Carnap, Dewey, Frank, Joergensen, Lenzen, Mainx, Morris, Nagel, Neurath; and a number of other scholars have also been approached for contributions to these as well as subsequent volumes.

In later volumes of the Encyclopedia the attempt will be made to show the differences in conclusions reached and points of view maintained in the

special sciences, and it is planned to give people with differing opinions an opportunity to explain their standpoints. It will be a special aim of this Encyclopedia to show the gaps in our present knowledge and the difficulties which are found at present in various fields in which new ideas are growing. Such an encyclopedia which makes provisions for showing gaps and conflicting points of view, and which emphasizes the incompleteness of our knowledge, is designed especially for people in the process of growth and development.

It is also planned that the collaborators are to remain in permanent contact with one another, so that the Encyclopedia may serve the building of bridges from one science to another, without, however, limiting personal expressions of opinion. Without pursuing utopian ideals, the effort will be made to have the scientific language of the Encyclopedia as homogeneous as it is possible to make it at present. It is planned, consequently, to develop a unified vocabulary of the main concepts of a scientific empiricism. For the selection of a suitable terminology the collaborators will consider the translations of different terms used in English, French and German. Such a nucleus of a trilingual dictionary of scientific empiricism will undoubtedly possess much educational value.

The Encyclopedia will appear in English at first, to be followed, if it is found feasible, by editions in other languages.

Reports will be published from time to time concerning the progress made in realizing the plans for the Encyclopedia, as well as any important discussions concerning the 'bridges' constructed between the various sciences, the unification of scientific language, and the elimination of superflous or dangerous terms. The editors of *Philosophy of Science* are prepared to provide a special page for the 'new encyclopedists.'

IV

It may be of interest to the reader if I say something about the origin of the idea of this Encyclopedia. My own intellectual development in the direction of a comprehensive scientific view was influenced by Mach, Poincaré, and other modern thinkers, and especially by Gregorius Itelson. My central conviction became that the elaboration of the differences between the various sciences is an unessential task, but that, on the contrary, it was especially important to develop an account of all the sciences using only one kind of a scientific 'style.' That is to say, I became convinced of the possibility of speaking about the stars and about men with the same logical techniques and

with the same scientific dispassionateness. I also realized that such a comprehensive scientific view could serve as an important basis for a sound general education. In this way there occurred to some friends and myself the idea of working up a series of about 100 pamphlets which would deal with all sorts of things, – stars, stones, plants, animals and men. A comprehensive alphabetical index was to be included, so that this collection could be used as a dictionary as well. This educational project was heartily welcomed by many, including Philipp Frank and Einstein. The latter wrote me that such a well-planned collection would serve the same function for the masses today as did the French Encyclopedia for the intellectual groups in France during the eighteenth century. However, it was essential for this project that all the texts be accompanied by illustrations. This plan could not be worked out, and our various discussions and attempts to begin the series were without success.

Two stimuli influenced me in the following years. As director of a museum in Vienna founded on Visual Education, I, together with my collaborators Gerd Arntz, Friedrich Bauermeister, Erwin Bernath, Marie Reidemeister and others, became profoundly interested in developing a consistent method of visual education. Contacts with a great many countries stimulated this activity, and we began an International Visual Language with a Visual Dictionary and a Visual Grammar. Our object was to build up a visual thesaurus, a store of pictographs and other pictorial representations, with an accompanying explanatory text. We found it possible to publish only one large atlas containing 100 maps and pictographs with statistics and text: '*Gesellschaft und Wirtschaft*," published by the Bibliographisches Institut, Leipzig; but this was intended simply as the first item in a whole series of publications. This work was similar in certain respects to the attempts of Paul Otlet, Director of the Palais Mondial (Brussels), who promoted the 'Mundaneum' idea of preparing exhibitions and publishing books with illustrations all within a comprehensive framework. Our group consequently planned to cooperate with Otlet. But it was not possible to accomplish any concrete work, and the idea of a new Orbis Pictus, of a Visual Thesaurus remained, and still remains an idea, a plan for the future.

During the same period there developed in Vienna what is known as the Viennese Circle. Moritz Schlick, Professor at the University of Vienna, was a stimulating influence upon a group of men interested in the logical problems of science, and he was one of the first thinkers who recognized and explained the importance of Einstein's Theory of Relativity for modern Empiricism. In the interest of advancing scientific empiricism he, together with Hans Hahn,

was instrumental in having Rudolf Carnap come to Vienna, and it was Carnap's special contribution to develop modern logic as an essential tool for scientific analysis. Schlick, together with Philipp Frank (Prague), one of the leading figures in the Viennese Circle, became the editor of the series *Schriften zur Wissenschaftlichen Weltauffassung*. Bertrand Russell also became an important influence upon members of this group, especially upon all those interested in mathematics and logistic. Wittgenstein's philosophy was carefully read and discussed, and the Viennese Circle concurred in his view, that we do not have special philosophical statements. Philosophy was regarded by a great many members of the Viennese Circle as the analysis of science and the discussion of its logical problems, not as a special superscience.

However, within this discussion group, I objected to Wittgenstein's metaphysical tendencies and proposed to take the consequences of the thesis, that we have only scientific statements. In other words, I maintained that we must build up the sciences as a body of empirical statements, and discuss all problems concerning them within the scientific framework. I proposed to call this complex of statements: Unified Science. I thought it would be useful to show that we could assemble the different discussions of scientific problems in a unified collection, and together with Carnap, Hahn, Joergensen, and now Morris, I began to edit the series *Einheitswissenschaft*, with the aim of breaking ground for a more systematic unification of the sciences.

The trend of my arguments led me to emphasize the importance of a purified everyday language ('universal slang') as the language of physicalism. By rejecting the various attempts to construct a special 'world of things' and a special 'world of thinking' juxtaposed in opposition to one another, we are in the position to unify our scientific language, eliminate the use of two languages, 'physical language' and 'phenomenal language,' and to advance the work of instituting the common language of physicalism.

If we use the universal slang as the basis for all our predictions and for their control, we discover that the statements we formulate can always be corrected when we concentrate our attention upon special points. This suggests that it is misleading to use such terms as 'verification' and 'refutation,' because of their absolutistic flavor; and I therefore proposed that we employ the terms 'confirmation' and 'disconfirmation.' This is not the place to discuss the logical consequences of this proposal. In brief, however, its adoption destroys the last vestiges of a philosophical absolutism, whether it be the absolutism of exactness, of 'the' truth, or similar dogmas. We possess no fixed point which may be made the fulcrum for moving the earth; and in like manner we have no absolutely firm ground upon which to establish the

sciences. Our actual situation is as if we were on board ship on an open sea and were required to change various parts of the ship during the voyage. We cannot find an absolute immutable basis for science; and our various discussions can only determine whether scientific statements are accepted by a more or less determinate number of scientists and other men. New ideas may be compared with those *historically* accepted by the sciences, but not with an unalterable standard of truth. I will add that views similar to these were developed by Carnap, Frank, Morris and others.

This approach is fundamentally opposed to every conception which employs the notion of *the system* as the 'limit' of scientific research. And it seems natural and an immediate consequence of these views that the idea of a 'unified science' be realized in an encyclopedia as a 'model' of our knowledge. For since we cannot compare the historically given sciences with 'the real science', the most we can achieve in our scientific work seems to be an encyclopedia, constructed cooperatively by scientists who are interested in scientific empiricism. This program may be called "Encyclopedism."

That is the general background for my proposal of an international encyclopedia to embody the idea of a unified science. Such an encyclopedia will show that scientists, though working in different fields and different countries, may nevertheless cooperate as successfully within this wide field as when they normally cooperate within such special fields as physics or mathematics. Such an encyclopedia will exhibit the logical framework of logical empiricism, and will be a mainstay of scientific empiricism in general as well as of the unity of science movement in the widest sense.

Considerable interest in the Encyclopedia project has already been shown by men of science and by the educated public. It is hoped that a larger public will be won for the Encyclopedia in all parts of the world, and that suggestions from different sources will stimulate the activity of the collaborators and organizers of the Encyclopedia. In this way the Encyclopedia will become a living intellectual force growing out of a living need, and not a mausoleum or a herbarium.

NOTES

[1] See Otto Neurath, 'An International Encyclopedia of Unified Science', pp. 139–144 this volume; and the following three papers from the *Actes du Congrès International de Philosophie Scientifique*, Part II: *Unité de la science* (Paris: Hermann, 1936): Charles W. Morris, 'Remarks on the Proposed Encyclopedia', pp. 71–74; Philipp Frank, 'Diskussionsbemerkung zur Encyclopaedie', pp. 75–76; and Rudolf Carnap, 'Ueber die

Einheitssprache der Wissenschaft. Logische Bermerkungen zum Projekt einer Encyclopaedie', pp. 60–70. A brief appreciation by Bertrand Russell of the aims of the Encyclopedia will be found in Part I of the *Actes* (*Philosophie scientifique et empirisme logique*) in the paper entitled 'The Congress of Scientific Philosophy', pp. 10–11.

CHAPTER 16

THE CONCEPT OF 'TYPE' IN THE LIGHT OF MODERN LOGIC

The book by C. G. Hempel and P. Oppenheim, *Der Typusbegriff im Lichte der neuen Logik* (1936)[1] renders an important service to scientific practice in all disciplines. It prepares a general 'theory of order' (but not in the sense in which Driesch understands it), a theory that is a constituent part of the whole future 'grammar of science' and will orient us with respect to the subject of the logical apparatus necessary for characterising and giving a systematic order to minerals, systems of hypotheses, situations of life, fingerprints, social orders or characters.

It is not expedient for their goal if scholars working in some special field or other are forced to create a theory of classification *ad hoc*, instead of being able to learn in a book like this that the application of a 'metric' is not a necessary element of the scientific character, but that on the contrary, on the sole basis of a purely 'topological' order, divisions and subdivisions are already possible that are irreproachable from the point of view of the logic of science and can be used for practical purposes. In a very suggestive manner and with numerous examples taken from a discipline that is very topical today, it is shown how awkwardly one often proceeds; sometimes on one point, sometimes on another, one is constrained to reflect about hierarchies of concepts, instead of simply realising from the start, for example, that one has not to arrange the objects of a certain field under two headings at all costs or that one has not to think it indispensable, if one needs intermediate links, to place them between two poles, which often leads to the neglect of important details. As the authors show with extraordinary insight, the case may be *of much richer orders in which there are perhaps no 'components' that have been measured*: when we are dealing with colours we can distinguish 'tone', 'clarity', 'saturation', all things that can be graded; but it can appear opportune to envisage, for the qualities, a 'space of more than 100 dimensions', for example, if one wants to analyse and classify in a certain manner the given diversity of temperaments and given human characters. All this, with many other things, is explained in a way that everyone can understand but that also shows how certain problems can be clearly presented by means

Translation of Neurath 1937d [ON 242].

of a *symbolic logic*; there is even more than one question that one learns to formulate with certainty and neatness only with its help.

As the authors try to give their logical considerations an orientation founded on practice, they underline how important for the development of a theory is the choice of qualities that one takes as a point of departure, as well as the fashion in which one forms and orders one's 'series'. To declare that there is an empirical link between qualities that are logically independent of one another, in order to be able to predict still other common features of the same kind, is exactly, they say, only a "particular way – though not a very transparent one – of expressing such laws". They show how the "purely topological concepts of order allow one to establish laws even in the cases in which measurement fails and mathematical functional relationships do not find any application". And nowhere are the authors content with similar statements, though they are already very useful in their general form, but in indicating problems in detail, for example, they examine how one can speak of a coefficient of correlation that can be considered as a "measure of the 'rigour' of interlocking between two orders of series".

Without doubt, quite a number of the problems discussed here have been successfully treated by scholars in various fields, as the authors often remind us; but what matters is revealing the logical structure of all these previous attempts, underlining what they have in common, independent of the subject treated, filling the gaps, and in short, executing the work of scientific logic advocated by all thinkers who take part in what the Americans call the 'Unity of Science Movement'. The authors expressly declare that their objective is to show that there is no difference in principle between psychology and the so-called exact natural sciences, and that through the work in question they want to contribute to "providing an intensified and detailed demonstration of the *logical unity of science*".

But the two authors do not present the conceptions just set forth, as if they had to complete, for example, the theory of *classification* with its many obvious gaps; on the contrary, they want to oppose to this the theory of a "formation of concepts that is susceptible of degrees", a theory, whose "elastic typological formation", treated by them in detail, is, according to them, a good example.

An idea that comes up repeatedly throughout the book is that, in opposition to the classification which, according to the authors' definition, either ascribes or denies a quality to an object, there has to be put clearly and neatly the formation of concepts that starts from the fact that a property belongs to an object to a "greater or lesser degree". This point would be easier to

analyse if one always saw plainly how the term 'concept' has to be used: it presents itself sometimes as if it had manifestly to do with the comparison of groups of qualities, and sometimes one speaks of a possibility of comparison that evidently refers to qualities taken in isolation, and not to these groups. At times one has the impression that the authors find themselves prevented from a reflection, which we shall indicate below and which, as it seems to us, must above all occur to those who, starting directly from empirical data, are interested as empiricists in the further development of science. At the same time more than one remark sounds as if the two authors reject this view or judge it to be of a nature that leads to evil ways. The reflection of which we speak is this: When we distinguish different kinds of plants, different minerals, etc., we are dealing with properties that can already be measured ("distance a whistle is carried" is replaced by a "length in metres"), then with others that can only be put in topological order ("to be as hard as quartz" is replaced by "hardness 7"), and finally with others that have so far been neither measured nor arranged topologically.

One could now start from cases in which objects are characterised by names, given to themselves and to their properties; for example, one could say: This special piece of iron ore that comes from a certain mine has a certain specific weight, a certain hardness, and a smell that one knows from iron ore of the island of Elba. In all cases of this kind one either ascribes *or denies* a property to an object. And it seems to depend on the richness of our 'catalogue of qualities' – if one can use this expression – whether we have enough names at our disposal, with the understanding that in the case of classification through measurement one will use cardinal numbers, in the case of a topological order, ordinal numbers, and in the case of non-ordered qualities, perhaps numbers that, by their value of position, indicate neither quantitative nor topological order.

Therefore we can use qualities of all kinds as 'predicates of one term', that is treat all characteristics of this kind syntactically in the same way. I would have thought that in the history of the sciences one has always made efforts not to rest satisfied with a 'catalogue of qualities' but to transform, if possible, at least part of this catalogue into an 'atlas' that arranges these qualities, in order to pass subsequently to a 'tables of qualities' that can be replaced in part by formulas. If we had prepared, in our laboratories, a substance that, when heated, passes through decreasing degrees of hardness, we could draw from this a complete scale of hardness for empirical aims. On the basis of these standards of hardness we would then characterise any mineral whatsoever with the help of names 'of a single term', *though*

knowing at the same time that only with the help of 'two-term predicates' can one express the fact that one is dealing with qualities of a topological order. Hardness that carries the name 7–56–35 could, for example, be conceived as lying between 7–55 and 7–57, that is between 7 and 8.

Now, if the physicist or the sociologist in his concrete work is not able to use 'one-term' predicates, he cannot – to his great regret – say of such a quality that it finds itself between two other qualities. For him, getting a predicate of a single term *does not depend on the possibility of applying a metric*: it is something given by the 'catalogue of qualities', or better still by the 'atlas of qualities' that is topologically arranged, should this consist of protocols of 'tests' only.

Hempel and Oppenheim, on the contrary, insist on this point that when one characterises, one must express the qualities of topological order by 'predicates of two terms', and consequently, when one begins to discuss this question, one cannot speak strictly 'of the situation of an individual in the space of qualities'. With Hempel and Oppenheim there is, so to speak, a maneuvre against an imaginary enemy when they turn against those who want to use 'one-term' predicates such as 'soft' and 'hard': "Yes, this manner of posing the question is absolutely meaningless, because the 'one-term' predicates 'hard' and 'soft' in this connection are not at all defined". The task, however, is to discuss with those who use the one-term predicate such as 'hardness 7' and who, if the question of hardness had enough importance for the continuation of scientific work, would make efforts to fix the 'hardness 7–56–35'.

A fundamental objection against a preference accorded to the use of 'two-term' predicates where one could very well manage with 'one-term' predicates, is the following: If we have an object R, a property g of it can be found in the catalogue of qualities together with h, i, k, l, m, etc. We might succeed in placing g in a series m, l, h, i, k, topologically arranged, and it would find itself between i and k, and moreover, in another series of the same domain of qualities k, m, h, i, l, in which it would find itself between m and h. With the help of two terms, it would therefore once be called '(i, k)', and the second time '(m, h)', instead of 'g', and one would then have to say of this quality g that, under certain pre-conditions, it finds itself in a series 'between' i and k and, under other conditions and in another series, 'between' m and h.

Hempel and Oppenheim think that a "formation of concepts that can be graded" is necessary because the "division into classes, at each level of its refinement" allows one to "distinguish only a finite number of possibilities".

But as these possibilities in 'finite number' can be provided practically speaking in an unlimited quantity, the empiricist can only be satisfied with this sort of finite. The authors have in view primarily qualities that are 'capable of being graded' and discuss above all the question how one can order groups of characteristics with the help of such qualities. But one could ask oneself whether there is not a wider problem besides this to which the expression 'theory of concepts that can be graded' would be better applied.

The representatives of logical empiricism owe a great deal to modern logic. But perhaps it is not always good to do as a great many do – among them also Hempel and Oppenheim – that is, underestimate the way in which logicians of the past put the questions, instead of recognising that they have prepared our modern logical analysis and often pushed this preparation very far. Aristotle, for example, who on numerous points found himself entangled by the whole of his philosophy, made efforts as a thinker with strong empirical interests to do justice to qualities that can be graded, as to everything relating to this, and more than one of his developments can be understood in a way that is in perfect harmony with scientific logic. He discusses, for example, the question whether one can speak of a 'middle term' between black and white, of colours that are neither black nor white, without introducing perturbations in his theory of 'opposite terms'. He mentions in particular that one gets gradually from the lowest tones to the highest, but that it is nonsense to seek the middle term if one speaks of shoe and hand.

The theories of types that are analysed with so much penetration and displayed with such logical neatness by Hempel and Oppenheim in a certain sense do not stop speaking of "constitutional alloys" that are mixtures, so to speak, of certain extremes. It is not quite certain that this way of looking at things, suited to this very successful research, the study of constitutions, does not itself also contain certain disturbing elements, and one could ask oneself whether it would not perhaps be more fruitful to use the analogy of chemical compounds, for the discussion of which it does not matter whether or not extreme cases exist, of which they represent a combination. The point on which the interest is then directed, if one wants to establish new predictions, is not the one- or multi-dimensional order based on certain directly given qualities.

It is precisely in Aristotle that one can find a parallel to this concept of a mixture of extremes that is presented to us by the modern theory of types, a parallel that will perhaps one day reveal itself as an historical precedent. In fact it results from Aristotle's philosophical doctrine that everything that

constitutes the middle must be made up of polar opposites 'mixed together'. It would be worthwhile to analyse especially to which point the advocates of the modern theory of types, influenced by certain philosophical conceptions, have developed this, in some respects Aristotelian, view. But as our modern historians of logic show, in order to study Aristotle and other philosophers successfully with reference to scientific logic, as well as to analyse any contemporary theory whatsoever, whether it deals with studies of constitution or with any other work of scientific research, it is necessary to know the whole logical arsenal.

Whether the approach, such as Hempel and Oppenheim bring to the foreground, partly under the influence of the 'subject' they have chosen, is of such decisive importance for the further development of this branch of science and for other disciplines, as is claimed in the work discussed, is a problem we shall not decide. But whether one studies this or another way of putting the question from the point of view of their logical structure, one can always take the discussions put forward by Hempel and Oppenheim as a point of departure, and often even use their results directly. Moreover they will prove to be fruitful precisely by the rigour and neatness of thought that they develop under such diversity of references, and by the importance that they will preserve, independent of the sphere to which they are applied.

NOTE

[1] Carl G. Hempel and P. Oppenheim, *Der Typusbegriff im Lichte der neuen Logik. Wissenschaftstheoretische Untersuchungen zur Konstitutionsforschung und Psychologie* (The Concept of Type in the Light of Modern Logic. Scientific-Theoretical Investigations in Constitution Research and Psychology). Leiden: Sijthoff, 1936.

CHAPTER 17

THE NEW ENCYCLOPEDIA OF SCIENTIFIC EMPIRICISM

For a long time there has been a growing movement that is called 'logical empiricism' because of its theoretical basis, and 'Unity of Science Movement' because of its most important practical aim; thinkers and representatives of individual sciences, who share an empiricist, anti-apriorist standpoint, are found united in it. The name 'logical empiricism' characterises the combination of two interests that had so far always been hostile to each other in the history of human thinking, namely interests in empiricism and logic. The application of these basic ideas leads to the attempt to investigate the individual sciences – which at present find themselves quite isolated while standing side by side – for their logical structure, to show up their 'cross-connections' and their common foundation, and to present them as parts of one single comprehensive unified science, with the help of a unified language. For the advancement of this program there have been annual international congresses for the unity of science since 1935. At the first of these congresses (Paris, 1935) a resolution was adopted to support the International Encyclopedia of Unified Science whose publication had been suggested by the Mundaneum Institute. The first two parts, which will be of a general introductory character and will especially stress what the standpoints have in common, are to appear in 1938 and 1939. In the succeeding volumes, above all single questions of the logical foundation of the special disciplines will be treated; divergences of opinion are to be brought out and presented by advocates of opposing views in order to ease and further the solution of these contradictions. Since the encyclopedia addresses itself to people with a general scientific interest, special attention will be given to the manner of presentation.

For a long time there has been a movement afoot to urge that what all sciences have in common be stressed and to show that physicists, geologists, sociologists, historians make use of the same logical tools, support and test their theoretically based predictions with the help of everyday observation statements.

This 'Unity of Science Movement' – as it is called above all by Americans – emphasises the planned use of observation statements on the one hand, and on the other the significance of constructive logical work; it comprises representatives of a view that speaks of 'logical empiricism', as well as representatives of the view that stands for an 'empirical rationalism' (*'rationalisme expérimental'*) or 'scientific rationalism', in contradistinction to a rationalism *a priori* that uses absolute statements as the foundation of its argumentation;

Translation of Neurath 1937e [ON 243].

thinkers, who come from pragmatism, instrumentalism, conventionalism, positivism in the widest sense, and from other schools, are united here. Insofar as all these currents emphasise the relativism of theory, and the fundamental role of statements of experience, one speaks more and more of late of 'scientific empiricism'. The close connection of logical analysis and empiricism is novel, and 'logical empiricism' is a child of our age. How much it was advanced by the analysis of historically given research is shown by the work of Mach, Duhem, Enriques and others. The discussion of theories of the past suggests formulating 'statements about statements'; in this way the ground was laid for the discipline that Rudolf Carnap has proposed calling the 'logic of science'.

People use the results of scientific work in everyday life much more than formerly, whether they travel by rail, undergo an operation in a hospital, eat fruit from planned cultivation or use a fountain pen. But among those who not only use all these results of research, but also as specialists made all these achievements possible, there are a great many who in no way care for a scientific total view, but adhere to either speculative metaphysics or the uncritical application of rather vaguely formulated experiences.

The most modern form of scientific empiricism, with its principles, has been thoroughly developed in central Europe, perhaps simply because here more than elsewhere one had to grapple with a comprehensive speculative metaphysics and with formulations of different schools that are, as it were vague in principle. In the United States of America, where 'common sense' enjoys wide respect, a very empiricist atmosphere has developed; many of their metaphysicians, for example, deal with the 'world as comprehensive reality', that is, their general philosophical thoughts are linked to empiricist discussions. But there advocates of a humanist synthesis also generally show more understanding for the variety of human thinking including logical empiricism, than do advocates of related views in central Europe, where many influential metaphysicians cut themselves off from everything dealing with scientific empiricism. A somewhat milder form of this American attitude can also be found in western and northern Europe. The central European criticism of metaphysics seems to be somewhat exaggerated and alien to people from these areas, though they fully appreciate the constructive work of 'logical empiricism'. It is understandable that in a country in which Peirce, James, Dewey and others have created a general atmosphere that is empiricist in many respects, the attempts of the 'Vienna Circle' and related groups are given a friendly welcome. The very fertile American manner of thinking successfully combines with the European in this field, and important results may probably be expected from such cooperation.

Ever more clearly it is realised that what is at stake is not to develop a 'superscience' as a 'philosophy substitute', but to unite the totality of the sciences including the logical analysis of their structure into one whole. All endeavours that lead to demonstrating what the sciences have in common, to demonstrating or even creating 'cross-connections' from science to science, drive towards one *unified language* for all positive sciences.

Representatives of the most varied sciences in the most varied countries are to be counted among the members of this ever growing movement without always being conscious of this membership, yes, even without agreeing in every respect with the explicitly formulated aims of scientific empiricism. What also matters greatly is a certain practical attitude, for example, the wish to overcome the segregation of the sciences for pedagogical reasons. Within the movement of scientific empiricism, therefore, the resolution was formed to take this synthesis seriously and to establish the program of a 'unified science'.

When we look further back into the history of human thinking, we frequently encounter interest in logic and mathematics on the part of strongly metaphysically minded thinkers, especially rationalists, whereas many heralds of a coarse empiricism valued mathematics as a good instrument at most but thought little of what they contemptuously called 'panlogism'. Like Galileo and those close to him, who were ill-disposed towards scholastics and scholastic logics, Kant and his adherents have treated logic with disdain, without an understanding of the logical attempts of a Leibniz or of the endeavours of a Lambert, who even was Kant's personal friend. What Gregorius Itelson called 'empirical rationalism' and what many of us call 'logical empiricism' means, *as a combination of empiricism and logic, the overcoming of a rather old antithesis*. This is not the place to go into the details of this development (cf. Neurath 1935d and Joergensen 1937). I only want to point out how comprehensive the nascent cooperation is in this field, concerning the problems as well as the persons.

Since scientifically active people are joining the movement all the time, it is not easy to give even an approximate survey of the groups and persons who contribute to advancing the preceding synthesis on the basis of logical empiricism, either by adding to the empiricist total view, or by performing significant special tasks as logicians or scientists. The old empiricist endeavours in French and Anglo-Saxon thinking still have their effect and secure permanent sympathy for scientific empiricism in those countries, while in Austria, for example, there are stronger vacillations; in Germany, however, the question can only be generally whether metaphysics is developing more or less strongly. In Poland, Hungary, Italy, Scandinavia, South and Central

America there were always strongly empiricist currents that never entirely ceased; in Poland, for example, we find positivism before Comte.[1] Also in the Far East scientific empiricism finds friends.

These comprehensive endeavours are being advocated every year by meetings of various sizes with changing program points. At these meetings, invited speakers present papers on individual subjects and carefully prepared discussions make a productive exchange of thoughts possible. The first of these international congresses for the unity of science took place in Paris in 1935, the second in Copenhagen in 1936; the third meeting, which took place in Paris in 1937, had the character of a conference that gave its close attention to the new Encyclopedia and to the unification of symbolics in logic. The organising committee of the congresses (Carnap, Frank, Joergensen, Morris, Neurath, Reichenbach, Rougier, Stebbing) is already busy preparing the next two meetings. On 18 July 1938 the fourth meeting will open in London. [Later changed to Cambridge, England. – MN] Its main subject will be scientific language. On 5 September 1939 the fifth meeting will open at Cambridge, Mass., U.S.A. (Harvard University) – main subject, logic of science. So far experience has shown that each year a greater number of people will gather in order to discuss the questions of scientific empiricism.

The Paris conference of 1937 brought together summarising papers on the progress of work on the Encyclopedia as well as of work done by the symbolics committee set up by the 1935 Paris Congress. Neurath reported on the Encyclopedia in general, Brunswik conducted the discussion of the incorporation of psychology into the exact sciences and associated himself with the proposal to use the term 'behaviouristics' in the future. Enriques introduced the discussion of the position of the history of science within the Encyclopedia. Among the participants of the conference were Ayer, Woodger who is concerned with the formalisation of biology, Clark L. Hull (Yale University) who reported on work of his institute in the field of 'human relations' and on his attempts to build up and formalise sociology. Arne Naess, Hempel, Oppenheim, Helmer, Dürr, Gonseth, Kraft etc., Scholz from the school of Münster, Behmann, Bernays and others discussed the question of symbolics intensely. Carnap and Neurath introduced a debate on the semantic concept of truth, Carnap and Reichenbach another on truth and probability in which chiefly Tarski and Kokoszyńska took part as representatives of the Polish school of logicians, as well as R. v. Mises. Rougier opened the conference; Philipp Frank gave, in his conclusion, a summary of the work of the conference with prospects for the future.

The life of the movement is characterised by all sorts of gatherings and

publications. The first two 'meetings for the theory of knowledge of the exact sciences' in Prague, 1929 and in Königsberg, 1930 had already discussed significant problems, further discussed in *Erkenntnis*, which was founded by Rudolf Carnap and Hans Reichenbach, commissioned by the Society for Scientific Philosophy in Berlin and the Verein Ernst Mach in Vienna. The interest in the subjects treated by these two congresses and the pre-conference of the international congresses for the unity of science (Prague, 1934) has increased rapidly, and the sphere of problems of this new movement for the unity of science is being treated, beside other questions, in a number of journals, such as *Scientia*, that from the beginning offered space to representatives of the Vienna Circle. In *Analysis* (London) of which L. Susan Stebbing is co-editor, Hempel, Juhos and others have discussed questions of logical empiricism. Also *Theoria* (Göteborg) is very obliging to the advocates of logical empiricism and suggests special discussions. One of the editors, Ake Petzäll, has for some time participated in the meetings of the Schlick circle in Vienna and has treated different stages of the development of the Vienna Circle in two presentations. W. M. Malisoff, the chief editor of *Philosophy of Science* (New York City) offers more and more space to the representatives of the Unity of Science Movement and advances the movement. *Revue de Synthèse* is paying more and more attention to this movement and advocates an idea of scientific synthesis that is fully along the lines of what scientific empiricism aims at. Other journals as well have a friendly or interested attitude towards the movement, and books also refer to it more and more frequently.

Such an expansion of interest in logical empiricism and the unity of science allowed the maturation of the plan to put the comprehensive survey of the total structure of science actually in the place of speculative metaphysics. The Paris Congress of 1935 devoted one item of its agenda to the project of the 'international encyclopedia of unified science' (see Neurath, Morris, Frank, Carnap in *Actes 1936*; 'Unified Science and Its Encyclopedia', pp. 172–182), and decided to advance this plan of the Mundaneum Institute in The Hague and to collaborate in it. The preparatory steps of the Encyclopedia committee (composed of Carnap, Frank, Joergensen, Morris, Neurath, Rougier) proved so successful that the work on the Encyclopedia was handed over to a special department of the Mundaneum Institute in The Hague, the 'International Institute for the Unity of Science' (Executive committee: Frank, Morris, Neurath).

The fundamental idea of the new Encyclopedia is to display the whole logical framework of our modern science, and to do this in such a way

that attention is drawn to gaps, difficulties and points of discussion, thereby avoiding the false impression that one wanted to replace a speculative system by 'the system of science'. The program of such a comprehensive system would essentially show metaphysical and above all aprioristical anticipations. 'Encyclopedism' presents a programmatic front as it were against the absolutism of system (Otto Neurath, 'Encyclopedia as 'Model'', pp. 145–158). The comprehensive plan of the encyclopedia is to be executed by publishing separate monographs of about 70 pages, each of which treats a definite subject. But a series of independent articles should not be created, rather everything should be done to establish 'cross-connections' from science to science and to make a start with the unification of scientific language. About ten monographs are to form a volume. The whole plan is so laid out that the Encyclopedia may consist of many layers like an onion. The first published monographs may already be available in several improved editions before the last layer of monographs has begun to be published. But the procedure should be such that the volumes available at any one time form a rounded-off whole. Should further publication be slowed down for some reason, there would never be just a torso.

The University of Chicago Press will publish the first two volumes of the Encyclopedia, for the time being only in English, in 1938 and 1939.[2]

They will consist of twenty monographs under the main title: *Foundations of the Unity of Science*. These two volumes are to contain nothing but introductions to the later volumes. Together they form a complete whole in themselves. The task of the first two volumes is to give a general survey of the problems taken up by modern scientific empiricism. These first monographs will therefore put special emphasis on what the branches of the movement have in common. Their task is not only to present the logical framework of individual disciplines, but also to show especially from which other disciplines individual branches of knowledge are nourished, and which they themselves again provide with formulations. It is important that the reader grasp from the very beginning how significant the exposition of 'cross-connections' is, those 'bridges' that link individual sciences with each other in so many ways. Of course these twenty monographs shall also contribute to demonstrating the great empiricist significance of 'systematisations' and 'axiomatisations' within individual sciences and groups of sciences without, however, giving rise to the feeling that 'The System' was to be introduced as an anticipation.

There will be opportunity to hint how certain misunderstandings can be avoided, which formulations, which terms are usually especially doubtful and misleading. In these first two volumes it will already be mentioned that

within the movement different views are advocated on certain points; but the intention is to treat such disputes in the following volumes in detail; the main presentation of a certain question is to be supplemented by shorter treatments written by advocates of opposing views.

While this new encyclopedia gives prominence on the one hand to the interconnection and union of science in any imaginable way, above all by as much unification of used terms and symbols as possible, on the other hand it is to reveal gaps and uncertainties, contradictions and difficulties explicitly. It is in no way a mausoleum of achievements of the past, but an instrument of most lively activity, dedicated especially to those who are still growing up, who are less oppressed than usual by the fullness of mature perfection but see what new problems await them. A hundred gateways are open. Thus the young will grow up to become collaborators in this 'eternal' encyclopedia that can publish each monograph in new editions according to need and deal with more and more details of the logic of science in later 'layers'. There are many people who like to co-experience the living discussion of problems of the day and who are not disturbed if traditional views are called in question: "Those who have ceased to grow, find nothing right; / Those who are growing still, will not spare thanks."[3]

The subjects and authors of the first two volumes are: The unity of science, introductory essays; Theory of Signs, Morris (Chicago); Mathematics and Logic, Carnap (Chicago); Procedure of Empirical Science, Lenzen (Berkeley); Physics, Frank (Prague); Cosmology, Freundlich (Istanbul); Probability and Empiricism, Nagel (New York City); Biology, Mainx, (Prague); Formal Biology, Woodger (London); Behaviouristics, Brunswik (Vienna and Berkeley) and Naess (Oslo); Social Sciences, Neurath (The Hague); Empirical Axiology, John Dewey (New York City); General Linguistics, Andrade (Chicago); Sociology of Science, Wirth (Chicago); History of Science, Enriques (Rome); History of Logic, Lukasiewicz (Warsaw); From Rationalism *a priori* to Empiricism, Rougier (Besançon and Cairo); Problems of Empiricism and Rationalism, Dubislav (Prague) and Santillana (New York City); Logical Empiricism, Joergensen (Copenhagen); Bibliography, Hempel (Brussels); Joergensen (Copenhagen); Neurath (The Hague).

These introductions give a rough outline of the range of subjects that will be treated at length in the succeeding volumes of the Encyclopedia. Special attention is to be given to the compilation of an international bibliography of scientific empiricism. The monograph 'Bibliography' at the end of the second volume is nothing but a first introductory survey. The central office of the Encyclopedia, with the help of consultants and correspondents who

represent different countries and different spheres of science, will try to collect international information on the works that either are directly concerned with logical empiricism and the unity of science or serve them indirectly. The contact with certain scientific institutes, with scientific associations and other bodies will contribute to this.

The new *International Encyclopedia of Unified Science* will at first be published only in English, but publication in other languages is under consideration. In any case such a publication in several languages is to be prepared by first developing suggestions for the translations of the most important terms and summarising them in a special monograph. This could be linked with the significant preparatory work of Lalande and his friends and similar attempts in special fields.

Since the new Encyclopedia does not address itself to specialists but wants to orient people of general scientific interest, special attention has to be given to the manner of presentation. Wherever possible, unified graphical representation will also be applied, above all for explanations with the help of examples (1936f).

Since this Encyclopedia has not the task of conveying the multitude of individual facts but rather to show the logical structure of the sciences and to provide certain historical insights that are suited to further logical empiricism, it does not enter into competition with the existing encyclopedias *but has to be seen as a supplement*. Since the Encyclopedia will deal with all sorts of disciplines, it will do so not only with the so-called 'applied sciences' in principle, with the question, what is the logical connection of education, medicine and similar disciplines with others, but also with the question, to what extent ethics, aesthetics and similar spheres of problems are to be treated strictly scientifically. Scientific research originating in the most varied camps will prove fertile and therefore be presented in their main results. It is certainly not the intention of this Encyclopedia to present the results of research of only its own movement. The history of human thought provides very clear evidence that an anti-empiricist total view of a scholar does not always prevent him from achieving important single results that have to be acknowledged by scientific empiricism; vice versa, it is also true that scientific rules are no magic tools that protect the man who advocates them from committing errors and mistakes that damage the development of scientific empiricism. For example, a man who does not give prominence to pseudo-statements and consistently uses an empiricist terminology, can still continuously neglect the principles of strict and above all uniform scientific testing.

The idea that it is possible to pursue successfully a synthesis of the sciences, to overcome the split into 'mental sciences' and 'non-mental sciences' and similar splits, is today much more widely accepted than the program of logical empiricism; it is even advocated by some who at the same time attack logical empiricism and the movement for the unity of science. A scholar's high esteem of logical means and logical analysis is far from being a guarantee for a kind disposition towards empiricism. There will be men who will advocate unified science and panlogism in the name of speculative metaphysics (cf. Neurath 1935d).

Within the encyclopedia there will be an opportunity to broach the question how far certain forms of metaphysics, especially *a priori* rationalism have had a stimulating effect on the development of the empirical sciences, and in continuation, the question whether still today they are not indispensable as a stimulant, as some perfectly scientifically minded people think. This is an important problem of all behaviouristics of research; especially people who adhere to a more rationalist metaphysics will easily be inclined to advocate this view, that will be much more positively judged than the attempts at speculative metaphysics by those interested in scientific work.

People who are involved in active life are often faced with the question what position could be accorded to such an empiricist general view within our personal and social existence, whether such a comprehensive scientific vision could replace what was otherwise reached by a more metaphysical route. Perhaps some understanding could be found by investigating the schools of ancient philosophers who addressed themselves to wide circles, as the Stoics and Epicureans; with them (especially the latter) metaphysical speculation played a relatively small role. It conforms to a scientific attitude to investigate all these problems carefully without wishing to anticipate an answer. As acting people we are forced to make our decisions on the basis of insufficient insight – this becomes evident especially to the empiricist who does not overestimate the significance of directed thinking, in contradistinction to the pseudo-rationalists who somehow want to 'jump over their own shadow'.

This new Encyclopedia is not organised by people who are in search of absolute truth. It is to be an expression of a conscious scientific attitude, to the extent that is possible today. Such a conscious endeavour may attain some educational significance, especially because in a new field cooperation of a kind is suggested that so far existed only in special fields. Many people are astonished that this Encyclopedia has no program that is somehow to be advocated by all collaborators – this Encyclopedia will demonstrate

by action how much of scientific empiricism, especially of logical empiricism and empirical rationalism is alive today. It is one thing to advocate a standpoint programmatically, and another thing, to realise it in scientific work. Through the encyclopedia itself, cooperative effort will put before every one's eyes what it may mean programmatically.

NOTES

[1] In *France* where the formation of [philosophical] schools does not play a large role we find many individual thinkers such as Boll, Lalande, Lecomte de Noüy, Lévy-Bruhl, Rougier and others who are close to this movement; among them are also those connected with the Centre international de Synthèse to which Abel Rey belongs, whose work in the logic of science has influenced the development of logical empiricism in Central Europe; beside and with him are Paul Langevin, Henri Berr, Paul Masson-Oursel and others like Robert Bouvier who was strongly influenced by Ernst Mach. In *England* the strongest impact probably comes from Bertrand Russell who in a certain sense combined logical and empiricist traditions of the most varied kinds in himself and then became a centre from which different endeavours started, although his 'realism' causes some criticism. Among those in England especially close to the whole movement Ayer, Stebbing, Woodger may be named, but men like J. B. S. Haldane and others have directly served the movement without entering into its views as a whole. Many supporters and friends of the movement in Central Europe further the internationality of scientific empiricism by their activities in various countries; a number of younger scholars continue the traditions of the older generation and cooperate in the building of unified science. Of this wing of the movement may be mentioned: Brunswik (Vienna and Berkeley), Dubislav (Prague), Philipp Frank (Prague), Freundlich (Istanbul), Gödel (Vienna), Hempel (Brussels and Chicago), Alexander Herzberg (London), Hollitscher (Vienna), Mainx (Prague), Mises (Istanbul), Neurath (The Hague), Oppenheim (Brussels), Reichenbach (Istanbul), Waismann (Vienna and Cambridge), Zilsel (Vienna). Also the late Hans Hahn and Moritz Schlick should be remembered. In *Poland* there is a great school that had its origin in Brentano through Twardowski (Lvov); its main work is in the field of logic with an empiricist flavour; many others besides may be mentioned: Ajdukiewicz, Chwistek, Kokoszyńska, Kotarbiński, Lesniewski, Lindenbaum, Lukasiewicz, Tarski, Zawirski. In *Scandinavia* above all Niels Bohr (Copenhagen), Joergensen (Copenhagen) and Arne Naess (Oslo) work along the lines of a scientific empiricism, but many others are close to the movement, as Petzäll (Göteborg and Paris), Kaila (Helsingfors), Uuno Saarnio (Turku), Alf Ross (Copenhagen), Rubin (Copenhagen), Tranekjaer -Rasmussen (Copenhagen); also elsewhere in the world many who raise objections against the fundamental principles of logical empiricism allow themselves to be drawn into detailed discussions with its advocates, as for example Julius Kraft (Utrecht), the phenomenologists Felix Kaufmann (Vienna) and Ingarden (Lwow). The connection with *German* scholars exists above all in the sphere of logic; one may think of Behmann, Bernays, Burkamp and others, but especially of Scholz and his group in Münster. Although Scholz makes valuable contributions to the analysis of the sciences with his collaborators,

he nevertheless stands for a full-fledged metaphysics. Grelling shows a strong inclination towards empiricism, Grete Hermann shows an interest in the problems of logical empiricism, as do others that also come from Fries and Nelson. One finds interest in logical empiricism especially among young physicists like Martin Strauss; Swiss like Dürr, Gonseth, Walther and others are connected with the movement although sometimes with critical reservations.

In the *United States of America* where empiricist behaviour is widespread, the friends of scientific empiricism can hardly be enumerated. Only a few may be mentioned who either have played a leading role or are in more or less close connection with the movement: Andrade (Chicago), Benjamin (Chicago), Bloomfield (Chicago), Bridgman (Cambridge, Mass.), Rudolf Carnap (Chicago), Morris Cohen (New York City), John Dewey (New York City), Herbert Feigl (Iowa City), Olaf Helmer (Chicago), Hempel (Chicago), Sidney Hook (New York City), Clark L. Hull (New Haven, Conn.), Victor F. Lenzen (Berkeley), Kurt Lewin (Iowa City), C. I. Lewis (Cambridge, Mass.), Charles W. Morris (Chicago), Ernest Nagel (New York City), Meyer Schapiro (New York City), Senior (Chicago), Tolman and Brunswik (both Berkeley), Louis Wirth (Chicago). In America there is the especially lively wish to advance the individual sciences by logical work: stricter formulations and logical analysis of all kinds. More and more universities have 'their logician', either in a mathematics or in a philosophy department.

In *Italy* Peano with his school has prepared the logical approach to the sciences. His attempts at replacing the verbal text of mathematical presentations by symbolics were uncommonly stimulating. Also the works of Vailati, which are too little known, have not remained without effect on the whole development. Peano's tradition is continued mainly by Padoa, in a way in which Frege's fundamental work has found continuation in Germany. The circle around *Scientia*, especially Enriques' school, prefers to characterise their own endeavours as 'scientific rationalism' (*'rationalisme scientifique'* or *'rationalisme expérimental'*), but in many ways they are related to those of scientific empiricism. For though these and similar groups very strongly emphasise the creative significance of the formation of theories, their tendency is nevertheless wholly against every rationalist absolutism, and they stress historic relativism, that is, they can be considered as part of the whole movement that has been characterised.

[2] Edited by Otto Neurath in association with Rudolf Carnap and Charles W. Morris. The abovementioned organising committee of the Encyclopedia is assisted by an advisory committee to which belong so far: K. Ajdukiewicz (Lwow), E. Brunswik (Vienna and Berkeley), J. Clay (Amsterdam), J. F. Dewey (New York City), F. Enriques (Rome), H. Feigl (Iowa City), W. Kaempffert (New York City), V. F. Lensen (Berkeley), J. Lukasiewicz (Warsaw), W. M. Malisoff (New York City), G. Mannoury (Amsterdam), E. Nagel (New York City), A. Naess (Oslo), H. Reichenbach (Istanbul), L. S. Stebbing (London), A. Tarski (Warsaw), E. C. Tolman (Berkeley), J. H. Woodger (London).

[3] Goethe, *Faust*, 'Prologue in the Theatre', ll. 182–183 (Translation by Walter Kaufmann (Garden City, New York: Anchor Books, 1961)).

CHAPTER 18

THE DEPARTMENTALIZATION OF UNIFIED SCIENCE

I. MANY SMALL SCIENTIFIC UNITS AS A LOGICAL START

We have no classification of the sciences, forming a consistent system, which has been generally adopted; the question arises, whether such a comprehensive system of the sciences might not hinder the logicalization of unified science (Neurath 1938, p. 25).

Main divisions of traditional systems are for instance: 'non-biological sciences' (opposed to 'biological sciences'), 'abstract sciences' (opposed to 'concrete sciences'), 'mental sciences' (opposed to 'natural sciences'). By such classifications one anticipates the acceptance of and the objections to a great many scientific decisions, for instance to the application of particular scientific procedures to certain disciplines.

Such difficulties can be avoided if one does not make use of these premature presumptions inherent in such an architectonic structure of the sciences, but is satisfied with another type of start: a great many scientific units (many of them very small) might be assembled step by step as systematically as possible. Such an increase of assembling is closely connected with the actual increase of scientific investigation and comprehensive logicalization: chemistry and optics could not be really joined by means of a mere classification.

The usual divisions such as 'Logic', 'Mathematics', 'Physical Sciences', 'Biological Sciences', 'Social Sciences' might be used as titles of handbooks, volumes of an encyclopedia or sections of a library. These names give, as it were, an initial information about the subjects of these handbooks, volumes or sections. A librarian has to find for each single book one and only one bibliographically well-defined place, but such placing of books in different shelves cannot be copied by placing sciences in a corresponding logical framework. Bibliographical cross-connections cannot always be substituted for logical grouping which has its own aims. Difficulties crop up everywhere, for instance: 'Geology' is a subclass of 'Physical Sciences', therefore it is according to the scheme a 'Non-biological Science', but paleontological

Reprint of Neurath 1937/38a [ON 244].

geology is a part of geology and of biological character. Should we introduce 'mixed sciences'? Most sciences might become 'mixed sciences'.

Following the principle only to select scientific units of a relatively well-circumscribed character, without objecting to 'smallness' and 'isolation' of such units, one might select items such as: Heraldry, Criminology, Theory of Business Cycles, Engineering, History of Fine Arts, Phonetics, Comparative Grammar, Procedures of Historical Study, Anthropogeography, Psychiatry, Theory of Achievement and Behaviour, Anthropometry, Historiometry, Mendelism, Procedures of Botanical Study, Ecology, Geology, Astronomy, Cosmology, Physics, Theory of Probability, Vector Analysis. As one starts with the principle only to select relatively well-circumscribed sciences, an overlapping of certain disciplines is at first not avoided by the clear circumscriptions. Particular efforts must be made to show intercorrelations between these fields of scientific study, certain common subject matters of different disciplines, special common logical qualities of certain scientific units and things like these. The assembling of these or newly formed units is a secondary process.

II. NEUTRALITY OF THE INITIAL PROCEDURES

An essential part of Mendelism can be discussed without specific qualities of 'living beings'. One can, for instance, discuss the 'molar behaviour', as it were, of certain groups of elements (beans, etc.) and how their qualities (red, white, etc.) are distributed among them, combined, etc.

When the Brownian motion was discovered, it was regarded as a biological fact. Correct descriptions of this phenomenon have not become invalid by the modern theory that this motion has its place in the framework of statistical microphysics.

Kepler's laws hold good for a tremendous amount of observations; this would not be changed if it were to be found out that the planets are composed of micro-organisms. Kepler's idea was that living beings (angels) were directing the planets and that they were literally acting according to the harmony of spheres. He intended to prove the 'sphere-melody' to be based on melodies composed of musical notes and the system of planets based on the system of Plato's simple geometrical bodies. Keplerian laws remained unchanged as the astronomers stopped using these ideas as measures for scientific work. Kepler's laws are 'neutral' as regards the question whether the astronomical phenomena are biological or non-biological.

One can start with a simple statement, "Astronomy deals with stars,

nebulae, etc.," as one can start with the analogously simple statement, "Geology deals with the structure of the earth". These simple statements do not make any presumption which excludes certain theories and so avoid certain obvious difficulties. One might assign the maximum aggregate of matter which one wants to discuss to 'Cosmology'.

The geologist does not stop analysing coral reefs because they were 'living matter'. If one includes the description of the moon in astronomy, one must consequently also include the description of the earth in terms of 'coral reefs', 'woods', etc. An astronomer might answer that the plants and animals remain for him mere lumps of matter. Is it not a premature presumption, on the side of the astronomer that plants and animals are nothing but matter having gravity? One might assume that certain deviations of the various motions of the earth could be correlated to the amount of living matter and 'life-rays' on its surface. Should it follow from the definition of astronomy as a non-biological science that all these problems must be discussed in another science? Do we know whether all important values discussed in Cosmology do not depend upon cosmic living matter? Is it not better to chose the abovementioned more neutral definition: "Astronomy deals with stars, etc."?

One would arrive at a more neutral discrimination between the sciences if one analyses what one calls 'living matter' without knowing whether all kinds of 'living matter' are of the same character and whether one can analyse an organism in the same way as a stone. Precaution and neutrality might be useful.[1] The smaller those initial scientific units, the less changes are necessary in the scientific descriptions. The general description of whales can remain as it is, even when the general description of 'fishes' has to be changed, because the whales have been removed from the class of fishes. This whole attitude cannot be formulated in a comprehensive rule.

III. UNIFICATION OF SCIENCE

If one starts with a great many special disciplines (some of greater extension, others of smaller) overlapping one another, one might axiomatize all groups of statements ready for it. One might select all cases in which the logical structure of a group of statements is identical with the logical structure of another group.[2] One might find out which common subject matters are essential to seemingly remote disciplines. Another important factor of unification lies in the efforts to show how the statements of astronomy, biology, geology, Mendelism, heraldry or the history of fine arts can be formulated by means of the terms of a 'universal slang' (see 'Protocol Statements', pp. 91–99)

composed of ordinary terms of everday language (certain dangerous terms omitted) and of certain added scientific terms. The reducibility of the terms of astronomy, history of fine arts, Mendelism, etc., to such terms of a 'universal slang' can be based partly on 'simple definitions', partly on 'conditional definitions'.[3] Another question is how to reduce statements and laws of all the sciences to a certain type of statement and law. The programme of 'Physicalism' deals with these problems.

Unification might separate disciplines which were joined in the traditional main divisions and vice versa. It would be wrong to assume that all the subdisciplines of so-called Social Science are based on a universal sociological terminology. Products made by men have been discussed within the framework of Social Science. One might avoid the term 'human being' in many of these subdisciplines without changing their content. A philologist, for instance, can analyse the 'shifting of consonants' without using 'specific' sociological or biological terms. The consonants could be produced synthetically by means of gramophone records without a human tongue. Correlations can be found between chronologically arranged paintings and chronologically arranged buildings without knowing anything about the more comprehensive theories which allow such correlations to be deduced, for instance from statements, which deal with the behaviour of peoples and individuals as one might deduce the 'shifting of consonants' from biological and sociological statements. The analysing scientists might progress from smaller fields to wider fields and find out manifold intercorrelations and combinations forming a very rich logical pattern.

IV. ENCYCLOPEDISM VERSUS 'PYRAMIDISM'

This objection to any anticipating classification of the sciences is an essential element of '*Encyclopedism*' (see 'Encyclopedia as 'Model'', pp. 145–158) the programme of which avoids the dangers of 'THE SYSTEM' in general and therefore also of '*the* system of the sciences', of 'the *optimum* system of the sciences' or of 'the natural order of the sciences', ideas which are often closely connected with metaphysical speculations. Not a few classifications and arrangements of the sciences can be regarded as derivates from the architectonic structure of such metaphysics, even if their creators were interested in empiricism.[4] All these arrangements show us the common features of a 'Pyramidism', which intends to build a symmetrical and complete edifice of the sciences by means of main divisions, subdivisions, subsubdivisions, etc. That is flesh of the flesh of the gigantic comprehensiveness whose roots we

find in Scholasticism and in other all-embracing systems.[5] Encyclopedism is satisfied with a rough bibliographic order for an initial orientation, made by librarians, but not a few librarians are also influenced by the 'Pyramidism'.[6]

Encyclopedism shows at a glance less harmony than its predecessors, based on 'Pyramidism'.[7] It accepts the fact that the vast mass of the groups of statements are, as it were, *in one plane*. Certain coherent forms could be arrived at by means of axiomatization or other procedures and a complicated network gradually created; there is no symmetrically pyramidal edifice. The *mosaic pattern of the sciences* might in the course of the ages show features more and more connected, but always changing, if the scientific attitude will remain at all valid.

The encyclopedic integration of unified science, the character of which is demonstrated above, supports the comprehensiveness of scientific cooperation by means of realistic devices and is far from traditional architectonic fancy. The synthesizing educational effect of such an encyclopedic integration is based on the comprehensive scientific attitude and not on the particular ideal of THE SYSTEM.[8]

What can be achieved by means of this unpretentious integrating programme which avoids all bumptiousness in scientism? One cannot anticipate this by means of explanations, it can only be proved by the work itself.

NOTES

[1] See Niels Bohr, 'Biology and Atomic Physics' in *Celebrazione del secondo centenario della nascita di Luigi Galvani. Congressi Scientifici.* (Sedute plenari. Istituto di fisica, 19 ottobre. Bologna, 1938, page 13: "the impossibilities of regarding an organism as a well-defined system . . . "

[2] Examples taken from different sciences can be given. See the interesting remarks on this problem in Hermann Weyl, 'In memory of Emmy Noether' *Scripta mathematica*, Vol. III. No. 3. 1935, about Gordan's idea of a 'mathematical chemistry' and the possible scientific importance of logical isomorphism.

[3] The progress of the unification of science has been essentially furthered by the systematical introduction of 'conditional definitions'. (See Carnap 1938, p. 49.) Carnap does not object, as is done above, to a presumptuous order of science, but this does not affect his explanations dealing with reducibility and other problems of the unification of science.

[4] Wilhelm Ostwald, *Die Pyramide der Wissenschaften* (Stuttgart and Berlin, 1929). He was under the influence of *Comte* and others. Spencer, Wundt, etc. created systems of a similar type.

[5] See Robert Flint, *Philosophy as* Scientia Scientiarum *and a History of Classifications of the Sciences* (Edinburgh and London, 1904), pages 3, 4, 6: "The sciences are parts of a great whole, the members of a magnificent system . . . (and this) is itself an object of

knowledge ... there must be a science of sciences ... this science is philosophy ... It has to show how science is related to science, where one science is in contact with another, in what way each fits into each, so that all may compose the symmetrical and glorious edifice of human knowledge ... there is but one science, although it has various departments, whereby the incommensurableness of nature is brought down to our capacities ... There are precedence and subordination, order and harmony, among them." Many elements of modern efforts to form an encyclopedic integration of unified science can be found in such a sublime song but one also sees the dangers of premature presumptions common to all 'Pyramidism'. How the 'science of science' revives in modern empiricism see (Charles W. Morris 1938, p. 69).

[6] Henry Evelyn Bliss, *The Organization of Knowledge and the System of the Sciences* (New York, 1929), page 73: "The sciences have definite relations to other sciences; there are groups, or classes of sciences. This, together with the relations involved, constitutes the system of the sciences, which has the coherence or unity of a system." See also his *The Organization of Knowledge in Libraries* (New York, 1933) and his *A System of Bibliographic Classification* (New York, 1935). Bliss analyses some bibliographical systems, also the famous one of Melvil Dewey and its expansion elaborated by *La Classification Décimal of the International Institute of Bibliography* (Paul Otlet, Brussels). The analysis is made partly in respect to ideas on the system of the sciences, partly in respect to very concrete technical problems of a librarian. Dewey's and Bliss' work are useful apart from their ideas of THE SYSTEM. The same can be said of many valuable suggestions made by Paul Oppenheim, *Die natürliche Ordnung der Wissenschaften* (Jena, 1926). He believes, like others, that the demarcation lines of the traditional sciences can be made to meet the requirements of modern logicalization of the sciences. William Maria Malisoff's interesting Disc of Sciences (1937), page 261 might also be too geometrically complete but it can become useful in finding out, for instance, whether certain possible scientific fields could be successfully cultivated.

[7] There is no place to analyse the character of the different structures and façades of the famous systems of the sciences, for instance of the Baconian one or of the strange bifurcations of Ampère.

[8] Some scholars stress the educational importance of scientific integration for cooperation, for instance: John Dewey, 'Introduction' to Bliss (1929) IX. "Specialization has been carried so far that the great need now is that of integration ... a special educational task, which at the present time has become urgent and dominant." See also John Dewey (1938) page 29.

CHAPTER 19

COMMENTS ON THE PAPERS BY BLACK, KOKOSZYŃSKA, WILLIAMS

If one advances analysis by logic of science, especially in the interest of unified science and the development of the individual sciences,[1] then each criticism of unified science or of logical empiricism immediately raises the question whether the critics view the publications treating the development of unified science and of logical empiricism as an expression of a certain scientific procedure or whether they inspect certain individual remarks taken somehow absolutely and in isolation.

Black and Williams seem to regard logical empiricism almost as a new philosophical system; their criticism is not so much directed towards the manner in which its advocates conceive or try to influence the operation of science from which they start, but rather towards certain single expressions that may be more or less adequate, but that often are of minor importance for the total work. On several occasions reference is made to views of Wittgenstein. Though these have influenced the development of the Vienna Circle greatly, we got rid of our atomic statements and other endeavours of a metaphysical bent. The living logical empiricism of today leans towards an analysis of science similar to Mach's, with the application, however, of logical tools that were developed in the meantime.

Mrs. Kokoszyńska's comments may indirectly be very important for the problems of logical empiricism; however, she does not express the fact that an enormous area of science, closed in itself, in any case remains unaffected by any possible application of the suggestions made by Tarski and others.[2]

For the time being it can anyhow be assumed that it is a fertile task to construct a mass of statements, free from contradictions, with the help of a physicalist 'universal jargon'.[3] In these the matter dealt with can be 'the Milky Way' as well as 'names used by certain peoples', 'opinions that occur' and the like, without the need to leave the unified language. For example, there can be the statement, 'Somebody says, "this statement does not conform to the rules laid down in a certain book" ' and the like. In which way this closed area of scientific statements can be enlarged by further formulations using the term 'true' in a certain manner, is a special question.

Translation of Neurath 1937/38b [ON 245].

By this addition the uniformity of the indicated area is not affected. One can surround it by a layer of formulations at it were, add another layer, and so forth.

One cannot say, as Mrs. Kokoszyńska does, that for this area work on 'unified science' and work on 'logical empiricism' coincide. Within this area logical empiricism in itself could be connected with a multiplicity of scientific languages. However, we try our best to restrict ourselves to *one* language [4] within the area in which we work with our universal jargon. The language dualism that is so often demanded by confrontation of 'categories' as 'mental sciences-natural sciences', should above all be overcome, as well as the languages dualism characterised by the confrontation of 'physical and phenomenal language'.[5]

If one aims at creating an area of interlocking scientific statements, the suggestion is close at hand to call the word sequences that cannot be incorporated 'isolated statements' [6] which, however, can form among themselves a logically consistent area. The fact that the isolated statements can form a logically consistent area demonstrates that logical analysis as such does not protect us against metaphysical speculation. Though the carefully used universal jargon helps us to exclude metaphysical speculation, it does not protect us against the coarsest lack of discernment that can perfectly well use empirical terms.

This is not the place to discuss the question, what significance semantics, with which Carnap and others besides the Poles are also concerned, will have for the operation of science, and how many of the formulations of this calculus will still find a place within the unified physicalist universal jargon. The comments above should in no case be understood as if any misgivings should be offered in relation to investigations that contain either no advancement at all, or no immediate advancement, in the way of the logic of science, for the development of positive sciences; the aim is only to show that the work on unified science can continue unerringly within given limits, before the question is decided within which calculi the 'concept of truth' as now suggested by Tarski and others, is of value, and whether it perhaps suggests certain absolutist claims of existence in disguise.

NOTES

[1] See Philipp Frank's speech in memory of Mach, or the programmatic declarations at the first congress for the theory of knowledge of the exact sciences, Prague, 1929 (*Erkenntnis* 1): Hans Hahn, p. 96ff., Otto Neurath, 'Ways of the Scientific World

Conception', pp. 32–47, p. 105 ff., Philipp Frank, p. 126 ff., who all referred to the fundamental ideas that Carnap had developed in his *Logische Aufbau der Welt* without identifying themselves with it in detail.

[2] Mrs. Kokoszyńska's discussions of some special formulations of Carnap are not touched upon here. Some of the ideas that Mrs. Kokoszyńska has developed were also put forward by Karl Popper with reference to Tarski at the second International Congress for the Unity of Science in Copenhagen.

[3] This term – preferred by Bloomfield of Chicago, to 'universal slang' that I used before – is the name of an everyday language that avoids certain phrases and is enriched by certain other phrases. See Otto Neurath, 'Protocol Statements', pp. 91–99, this volume.

[4] Waismann is justified when he demands it should be more closely clarified how '*one*' language should be understood.

[5] When I suggested speaking of the unified science of physicalism, I did so because, among other things, it was important to reject certain considerations that had also come up within the Vienna Circle; for example the question was put as to which is more complex, language or reality. At that time it had to be stressed that the statements are part of 'reality' as well as the rest of 'reality' that was at a certain moment confronted with the statements. At a time when people in the Vienna Circle were trying to say of written statements they were hills of ink, 'ornaments', the Poles, for example, Lukasiewicz, had long spoken of 'arabesques' that are composed of signs. The Vienna Circle became more closely acquainted with the views of the Polish school rather late – about 1930. The ideas of Tarski and others on metalanguages were critically received by Schlick, Waismann and others, because Wittgenstein allowed no accommodation to statements on statements, whereas Carnap, Gödel, Hahn, Menger, Neurath and others, from various points of view, welcomed a view that declared 'statements on statements' 'legitimate'. Just by admitting 'statements on statements' as equal partners of 'statements on other things', unified science was advanced. (Cf. Otto Neurath, 'Physicalism', pp. 52–57; also the articles by Schlick 1935, Carnap 1935a, Hempel 1935 on the language of psychology. Although Schlick was one of those who helped to prepare physicalism and the conception of unified science, he has responded critically to the most recent development of unified science (Schlick 1938).)

[6] This expression, which we have taken over from Reach, is often more suitable than the term 'metaphysical' because the latter characterises the statements concerned, whereas 'isolated statements' only states that they cannot be joined with our scientific statements to enable the creation of further scientific statements. Naturally, statements that are considered isolated today, can be proven scientifically applicable tomorrow.

CHAPTER 20

THE SOCIAL SCIENCES AND UNIFIED SCIENCE[1]

'*Universal Physicalistic Grammar of Science*' (some like this aim but not this heading) may teach us how to assemble empirical studies.[2] Not a few empiricists intend to clean the historically given disciplines by transforming unempirical groupings of words into empiricist statements and defining the purposes of the old-fashioned disciplines without changing the traditional lines of demarcation. They also seem to assume that a pyramid of the sciences could be built up consisting of non-overlapping divisions and subdivisions.

I think on the contrary that we should start with *small scientific units*, analyzing concrete successful scientific investigations and looking at the attempts to build more comprehensive scientific bodies by means of systematization. I see no reason why I should object to the *overlapping of scientific disciplines.*[3]

I should not object to terms like 'chemistry', 'physics', 'geology', 'biology', 'economics', 'social sciences', etc., if they are used as titles of books or lectures. But we should be suspicious, if scientists try to find the lines of demarcation between 'social sciences' and other groups of sciences. I see no reason why some scholars limit the social sciences to human beings. If, for instance, the Human Relations Institute combines research departments devoted to the study of the behavior of rats, monkeys, feeble-minded children, tribes, etc., within one building, it represents a real unification of scientific enterprise which is the background of the Unity of Science Movement. If a scientist finds out experiments which show how 'taboos' can be produced within an animal society, I think we should be glad to learn a lot from such research for anthropological studies.

We can discuss historical and sociological problems in all details without being forced to use the terms 'inner experience' and 'outer experience' or 'opposites' of equivalent scientific significance in forming boundary lines between sciences. That does not mean that we exclude what is called 'inner experience': If we find in an old notebook a line, "I felt so strange in the morning", we can write down, "Tartarine felt strange on the morning of September 5th 1657 – according to his notebook – and was killed in the

Reprint of Neurath 1939/40 [ON 255].

evening of the same day – according to the contemporary chronicle written by Simplicio." We are combining 'observation statements of different persons' (it does not matter that in one case Tartarine speaks about Tartarine, in the other Simplicio about Tartarine) according to the principle of our Physicalism, that's all.[4]

If the scholars are right who maintain that *what we have forgotten is of greater importance for the social life than what we have in our memory,*[5] then we cannot hope to get much information by analyzing our 'consciousness', 'self-experience', 'insight', etc., but more by means of investigations made by third persons.

In the so-called Social Sciences we find a great many well-expressed correlations, some of them in mathematical form; but we cannot deny that many problems are discussed in this wide and multifarious field which lack clearness. It is of more importance to find stronger and sharper formulations for questions than to find boundary lines of the disciplines.[6]

If I assume some empiricists would start with their research without starting at the traditional boundaries of certain 'social' disciplines – I cannot believe that even one of them would create an empiricist discipline the boundary lines of which would be similar to the traditional ones.[7]

Although I presume that it is useful to stress the *continuity of arguing* if we discuss small scientific units, especially if we transform metaphysical problems into empiricist ones, I do not think it is also useful to support without careful analysis the old boundaries of old-fashioned disciplines. Many students hesitate to analyze important questions which do not fit very well into the old pattern; the general opinion about the subject of a discipline about divisions and subdivisions influence many investigations and hinder some ideas.

I doubt whether the term 'law' can get the same peculiar importance within Unified Science as it got within the traditional human speculations. If certain problems of jurisprudence are not further discussed as problems of 'divine law' or 'natural law', but as problems dealing with the consistency of certain sentences, one is on the road to Logical Empiricism, but why should we start such discussion with speculations about the categories 'be' and 'ought to be?'[8]

Another place where we meet difficulties is the discipline 'economics'. There is no doubt that many scientists succeeded in analyzing correlations between the curves of incomes, wages, rents, unemployment, production, etc., given by direct experience. There is further no doubt that not a few succeeded in creating certain 'models' which show us certain changes, for

instance, 'economic depressions', which we can compare with similar changes which appear on the market. There is likewise no doubt that these and the above-mentioned studies can be successfully combined. How are these types of problems to be characterized? If we call this field 'economics', we must, it seems to me, stress the fact, that we are discussing problems within a social order with 'market', 'money' and certain other elements. But it is useful to join this field 'economics' with another field, likewise often called 'economics', where scientists analyze 'production' and 'consumption' including primitive tribes, etc. Another type of 'economics' deals with models which show us the combination of different 'levels of living' and how they depend on certain rules of co-operation, etc. Such problems may be discussed scientifically, but one must avoid unconsciously confusing this analysis with the analysis of markets and applying the principles of a 'measurement in money' in cases in which at first we have to start with a 'measurement in kind', till we show how the two are connected.[9]

To sum up: We should avoid creating a Pyramid composed of divisions and subdivisions both for Unified Science itself and for the Social Sciences. Not a few friends of Scientific Empiricism will criticize this attitude as a lack of systematism. But the anticipation of a pyramid of non-overlapping sciences and subsciences hinders the free evolution of the sciences. We should regard the Social Sciences as a collection of a great many scientific units *which can become combined in very different ways*. That is real '*Encyclopedism*' within the Unity of Science Movement.

NOTES

[1] Paper sent in for the fifth International Congress for the Unity of Science (Cambridge, Mass. U.S.A. 1939).

[2] We have to assemble empirical generalizations with the formulas of theories ('calculus') that we get *predictions* and to compare predictions with *observation statements* (= testing predictions) which are expressed in a *Universal Jargon* (our daily language, to which some phrases and terms are added, some confusing ones being eliminated from it) which contains the *'joinable'* elements by means of which we may form groups of statements as *consistent* as possible. This program was started in: Otto Neurath 1931c, p. 2 and p. 17: "So ist die *Einheitswissenschaft der Schatz aller miteinander verknüpfbaren, also auch logisch verträglichen Gesetze* ["Therefore *unified science is the stock of all connectible and indeed logically compatible laws* . . . " Neurath 1973b, p. 329]. p. 3: "Der *Physikalismus* [will] nur darüber etwas aussagen . . . was er irgendwie auf *Beobachtungsaussagen* zurückführen kann" [Physicalism will affirm only what it can reduce to observation statements]. Cf. Neurath, 'Protocol Statements', pp. 91–92 and Carnap 1932b, pp. 215–228.

[3] Cf. (Neurath 1938, p. 20; Morris 1938, p. 74; and 'The Departmentalization of Unified Science', pp. 200–205.

[4] The criticized type of reflexion may be presented by Frank Knight: 'Relation of utility theory to economic method in the work of William Stanley Jevons and others.' (*Methods in Social Science* edited by Stuart A. Rice, 1931, Chicago p. 67): "The differences between human and physical science arises out of the facts of consciousness and mental communication. In human behavior we have a kind of direct knowledge of motives, whereas we only infer the existence of physical forces from observation of changes specific to each. Hence the irresistible urge to treat motives as real. But furthermore, our knowledge of motives through personal experience and social intercourse shows that (in contrast with physical forces) they do not coincide with observed behavior."

[5] I think many theses of the psychoanalytical theory could be telescoped into this slogan.

[6] The principle of forming a better question may be illustrated: If one asks: "what is the mean between 2 and 8?" and some answer '4' and some '5', it is not the problem to find out which answer fits better, but to find out how to change the question, for instance, into two questions: "what is the arithmetic mean between 2 and 8?"; the correct answer is '5', and "what is the geometric mean between 2 and 8?"; the correct answer is: '4'.

[7] The Empiricalization of disciplines which deal with human activities is much furthered by American thinkers like John Dewey. The old boundaries (metaphysical or non-metaphysical) often remain. The same appears in Europe. Moritz Schlick wrote a book on 'Ethical Questions' with remarkable criticism of many usual errors but one does not get the impression that a similar analysis could deal with railroad timetables or with beehives and the 'felicity' of the bees. Also Karl Menger wrote such a book, a more constructive one, and Viktor Kraft, a more comparative one. Both of them attended the sessions of the Schlick Circle, from which the Vienna Circle started and supported Scientific Empiricism.

[8] Hans Kelsen, supporting Logical Empiricism to some extent, stresses this dualism, while Felix Kaufmann thinks it is not necessary to have the two worlds of 'be' and 'ought to be'. But he fights for the 'inner experience' as the characteristic basis of the Social Sciences, creating a line of demarcation according to his opinion. Alf Ross, Felix S. Cohen and others stand for a pure empirical analysis of ethical and legal problems, but are to a certain extent always entangled with the old lines of demarcation.

[9] The utilitarian ideas often lead to the problem of an 'optimum', which is not always sufficiently analyzed. It may appear connected with a religious basis (Iselin 1784) or without such a basis (Bentham). See Otto Neurath 1937a, pp. 142, 149. The criticized type of confusing may be presented by Raymond T. Bye, *Principles of Economics* (New York, 1932), p. 15: "Utility may be defined . . . as the power of a thing to gratify a human desire". "Human desires are the motivating forces of economic life" (p. 16). "Only transferable things enter directly into the processes of economic life, only they are bought and sold." The last sentence shows clearly that it would be better to form a well-defined discipline which analyzes buying and selling instead of mingling this group of investigations with far-reaching questions, dealing with human desires and motivating forces.

CHAPTER 21

UNIVERSAL JARGON AND TERMINOLOGY

TERMINOLOGY

Logical Empiricism, on which the so-called Unity-of-Science movement is based, stresses the importance of the analysis of our language tools for a comprehensive scientific attitude and for assembling a 'Unified Science.'

In the discussions on the instruments of deduction, on language as a measure of communication and on other matters, many suggestions have been made which deal with alterations of our language. Mach succeeded in doing preparatory work for the theory of relativity, not by introducing new experimental statements but by analysing scientific expressions. Continuing nominalistic and other tendencies, the so-called French 'conventionalists' and the American pragmatists are prepared to look at our language from an 'operational' point of view. The analysis of our language tools (and partly the re-organization of our language) was intensely promoted by Bentham and his followers up to Odgen and Richards, and by the Cambridge School of Analysis. Much of the clumsiness and confusion of argument can be removed by 'formalization': Bertrand Russell presented his classic thesaurus of symbolic tools; Carnap's work shows how symbolism helps us to overcome some difficulties which arise when we seriously try to 'logicalize' and 'empiricalize' our scientific enterprise. All these attempts have been seriously supported by the Polish logicians and other groups. But formalization is no magic sieve, saving empiricism; one can tell highly speculative stories by means of symbols. Sometimes a symbolism may even conceal the ambiguity of certain explanations and lull to sleep the attention of scientists who are accustomed to rely on symbolic argument.

In the discussions on the instruments of our language many suggestions have been made which deal with important alterations of our expressions. I think one could 'canalize' these torrents and rills into one stream. I shall call the doctrines and studies which deal systematically with expressions and terms: 'TERMINOLOGY,' in harmony with the etymology of this word.

Reprint of Neurath 1941 [ON 256].

MAKING LANGUAGE – AN ART

Terminology tries to 'test' all elements of our language and to find out which of them could be altered or dropped altogether when their 'consistency' is discussed. There is a trend to build up a Lingua Franca (Senior (Chicago) suggested this term) which would enable us to pass from the theory of behaviour ('behaviouristics') to geology, biology and mechanics without any alteration of the type of our expressions: moreover to pass from everyday language to scientific language.

Since this is not the place to give a full account of the problems of terminology and their implications, I shall try to show only my own terminological technique, and shall quote some of my articles where more details and examples may be found.

When people are prepared to talk over with me the problem of the 'Universal Jargon' (Otto Neurath, 'Protocol Statements', pp. 91–99) I suggest that we should begin with expressions which, we think, are common to us. On the other hand I suggest that we should desist from using certain expressions during our talk. Some people think that such a procedure reduces dangerously the whole pattern of communication (some are proud of the richness of their vocabulary and not even we scientists are free from a certain romanticism in this respect). Sometimes I shall be in a position to offer some 'substitutes,' but not all my partners in such a discussion will be satisfied by that. I believe that the remainder of the expressions, not common to us, leads to implications which are found neither in accordance with nor in contradiction to our common statements, *i.e.*, the remainder is a body of 'isolated' sentences.

We do not present this reduced and unified everyday language as a complete structure full as an egg. The Universal Jargon will always be in the making, just as our life and our sciences. We shall not be in a position to any large extent to make deliberate conventions. The Universal Jargon will rather be achieved by successive adaptations and compromises, by a kind of 'orchestration,' as Horace Kallen called it.

We must be prepared to speak cautiously whenever we think it necessary, but we should not adopt a pedantic attitude throughout. We are using the terms 'sunset' and 'sunrise' in daily life, and do not use a terminology which is in accordance with the 'orbit of the earth' or rather with the 'relative dislocation of earth and sun.'

Many scientists ask how we should 'test' the advantages of our proposed Universal Jargon. They often forget that all practice of testing is essentially

based on the limitations we give to the respective 'area' in question. The more comprehensive such an 'area' is, the less we are in a position to compare different 'possibilities.' To give an example: how should we be able to compare the 'capacities' of scientific theories?

Our initial observation statements in the sciences are not 'atomic,' but are already imbedded in a body of statements derived from various sources, composed of indefinite terms such as 'microscope,' 'looking through,' 'inconsistent group of observed data.' Consequently no 'unique system of the world' remains, such as the 'rationalists' expected to find behind the screen, but bundles of bodies of statements which all more or less fit into our scientific pattern. Imitating an expression, coined by James, we may speak of 'pluriverse' instead of 'universe,' and consequently we could speak of a 'pluri-moon,' 'pluri-Newton,' and 'pluri-table' in cautious empiricist discussions. But we may not speak of a 'pluri-point,' 'pluri-sphere' and 'pluri-line' in discussing schemes of mathematical physics. It is a special problem how to correlate the 'pluri-statements' with the 'mono-formulæ.'

For limited purposes of analysis 'schemes' and 'models' composed of formulæ may be made which may be so simple that you are able to take an exhaustive view of the deductions derivable from a group of initial formulæ. Then you may define 'simplicity' or another quality of such a scheme and you may find out that one and only one of the schemes in question satisfies certain conditions. But the transfer of this technique of comparison of formalized schemes into the field of empiricist research leads to a relapse into the above characterized 'absolutism.' We have no definite initial primitive atomic statements in the sciences, we have not even definite rules by means of which we could test the totalities of implications of and the inferences from 'competing' scientific theories.

Imagine craftsmen who are building a settlement, with a chest of drawers full of instruments, only part of which are well arranged and the usage of which is only partly known by them; imagine that from behind new instruments are continually put in the drawers, that some instruments are modified by unknown people and that the craftsmen learn to use some of the old instruments in a way hitherto unknown, and now imagine further that the plans of our craftsmen dealing with the building up of settlements are changing too. That resembles to some extent the situation of our scientists.

In practice the situation is rather more complicated than even the 'conventionalists' were accustomed to discuss, because it frequently happens that a certain theory, useful in a determined field, contradicts another theory useful in a different field. We have to compare bodies of statements which are not

yet 'systems.' We should discuss 'model encyclopedias' besides the 'model systems' (Otto Neurath, 'Encyclopedia as 'Model' ', pp. 145–158).

But this indefiniteness is not restricted to the ambiguity of all empiricist statements. Imagine that some people were interested in the invention of a new game of chess which should fulfil certain demands, *e.g.*, in leading to certain stimulating combinations. The combinations of our chess are practically 'inexhaustible.' How can a master player test a certain new opening? His results will depend upon the relative skill of the pairs of players who are applying the new opening. After a long time this perhaps successful idea may be defeated by another master player's countermove, or an already defeated opening may be resurrected. Again and again we are confronted with similar situations. Debaters on comprehensive scientific problems are in the end like lawyers who have to take a side. Each of them intends to strengthen his own arguments and to weaken the arguments of the aggressor – but no judge is in the chair.

However seriously we analyse our case, a 'decision' must be taken, which cannot be substituted by any 'account' (Otto Neurath, 'The Lost Wanderers of Descartes and the Auxiliary Motive', pp. 1–12).

What engineer would be able to account and survey the multiplicity of 'possible' steam engines and all their qualifications as he is able to survey scientifically all 'possible' simple levers? This 'completeness' is an essential difference between Mechanics and Engineering. What painter would be able to survey the multiplicity of 'possible' variations of his work and the 'possible' impressions it could make on the onlookers? 'Making a steam engine' therefore may be called an 'art' insofar as it has a common quality, just as 'making a painting' may be called an 'art,' but then 'making a science' should be called an 'art,' too. How much more we are entitled to call 'making language' an 'art.'

Comparing 'model languages' is of course stimulating for many purposes just as it is stimulating to compare scientific 'model systems,' but there are limits in both cases, and building up our Universal Jargon is rather more involved than building up our science. Moreover, building up a Universal Jargon needs a comprehensive training, which is connected with an alteration of our whole attitude. One is hardly able to apply alternatively an empiricist's language with all its implications and a non-empiricist's language with its implications. What comes from an 'experiment' with a modified scientific language will be analysed by a man who is modified by this 'experiment,' which is more than an experiment: it performs a kind of self-education.

DANGEROUS TERMS

I started in my university days rather primitively by making a collection of 'dangerous terms.' Before I started making this collection (I sometimes called it as a joke my 'Index Verborum Prohibitorum' (Neurath 1933)), I tried to criticize books and articles. Particularly I was busy with reading Adam Smith's *The Wealth of Nations* along the lines of an analysis of language. I found out that such butchering criticism lacks constructive power and that a long self-education has to be the first step. I altered successively my own terms in all my articles and books in accordance with my increasing Index by eliminating 'emotional,' 'concealing' and 'confusing' terms.

I learned much from Mach's writings, from Poincaré, Duhem, Enriques, Avenarius, later on from Jevons, Abel Rey, James, Karl Pearson and Bertrand Russell. I think through Ferdinand Toennies (Welby Prize essay on 'Philosophical Terminology') I heard of 'significs' for the first time. I am highly obliged to the members of our Viennese Circle for many stimulations given to me in the field of terminology, and to friends of our circle, such as Tarski, Hempel, and Nagel. Above all I was induced by Gregorius Itelson to be cautious in the use of expressions. Mach's friend Popper-Lynkeus, interested not only in physics but also in the humanization of social life, was open in his criticisms of the tendency to conceal the unpleasantness of historical events by using certain fashionable vague terms and well-sounding phrases. I think he strengthened my attitude very much. This whole business of criticizing language was in the air and has increased in the last decades. In the United States popularizing books such as those written by Stuart Chase and Hugh Walpole get an increasing circle of readers. It is not only by accident that L. Susan Stebbing wrote on the one hand a book criticizing highly metaphysical speculations of modern physicists and on the other hand her *Thinking to Some Purpose* and her *Ideals and Illusions*. It will be stimulating when we new critics of our language will be criticized by means of the procedures we proposed.

When I call a term 'dangerous' I cannot pretend that no new definitions could be proposed which would avoid a particular danger, but personally I do not like to act as a terminological rope-dancer, and therefore I prefer a strong reduction of my vocabulary, where I try to speak cautiously. Successively certain rules come out of such a job, the nucleus, so to speak, of a future terminology, as a particular discipline.

I avoid in empiricist discussions (some of the terms may be used in discussions of models and schemes after limiting definitions) terms such as: 'mental

world,' 'true,' 'meaning,' 'verification,' 'progress,' 'pathological,' 'motive,' 'value,' 'thing in itself,' 'observation' (but 'observation-statement' is not dangerous), 'perception,' 'reality,' 'existence,' 'thing,' 'experience,' 'theory of knowledge.'

Therefore I cannot use, within the framework proposed by me, sentences such as: "there is a need for us to justify our belief in the existence of material things," "the events are the evidence for the truth of the sentences," "we do not know our present experiences," "no chronometer is exactly right," "we can arrive at considerable knowledge concerning the structure of the world," "these rules are not unambiguously determined by the facts." These sentences are for me 'isolated' ones.

Let me add that this whole problem of making a Universal Jargon was put before me in a different shape when I was working out together with my collaborators an International Picture Language for educational purposes (Neurath 1936f). The rules of picture writing are different. Starting with 'icons' implies far-reaching limitations of language, but these limitations sometimes eliminate much danger. We are, *e.g.*, not able to create an analogy to 'a through is walking through a through' in our picture language. The working out of such a picture language needs years, and I see no way how to compare this one picture language with other alternative picture languages which are based on different rules, insofar as we do not restrict our comparison to the very few and 'accidental' picture languages we find in use somewhere or to reduced schemes for which we cannot simply deduce consequences applicable to a more comprehensive picture language.

AGGREGATIONAL TERMS OF PHYSICALISM

Mach made the important remark: one should at least mention all moments which cannot be removed during an experiment. This advice taken seriously implies that we have to formulate all laws of mechanics, biology, sociology as laws of a respective 'cosmic aggregation,' as I propose to call a rather indefinite mixture of sun, moon, earth, plants, animals, men, streets, houses, telescopes, watches, etc. In spite of strikingly different starting 'cosmic aggregations' the behaviour of simple levers of a certain type was more or less similar. When one speaks of 'identical' starting situations at different times, one often overlooks that the term 'time' implies the measuring of time, *i.e.*, different 'cosmic aggregations.' It is astonishing that within such a perplexing mixture such simple laws are possible.

The terms of an 'aggregational language' are not only to fit into this

comprehensive pattern, but they have to stress always that they are 'pluri-terms.' It is misleading to speak of the 'exactness' of data within the 'aggregational' discussions, because we have no standard of exactness within the 'pluri-terms,' only within the schemes and their formulæ may we speak of 'exactness.' The terms 'finite' and 'infinite' cannot be used within the 'aggregational' disciplines, but within the framework of schemes. The terms of the probability calculus cannot be used within the 'aggregational' area – the question is, how to relate the 'aggregational' statements to the formulæ of the schemes, with their dichotomy 'finite' – 'infinite,' etc. We may discuss in this way 'aggregational geometry' and 'mathematical geometry,' and see what the implications are of this point of view in the classification of the sciences (Otto Neurath, 'The Departmentalization of Unified Science,' pp. 200–205).

When we start with 'aggregational' terms we are not in a position to divide the sciences into biological and non-biological sciences because they are all together sciences of the 'cosmic aggregation.' Sociology, according to our proposal, discusses an aggregation of men, streets, houses, books, paintings, soil, plants, etc., within the 'cosmic aggregation.' The law of falling bodies remains unaltered whether we make a statement on a falling stone or a falling cat; it is therefore misleading to say that physics has to do with non-living bodies.

Therefore, I propose to start with an 'aggregational language.' Acknowledging an expression as an 'aggregational' one seems to be the first step. By this acknowledgment of expressions as 'aggregational' we get a substitute for Carnap's 'confirmability' of statements. An essential difference is that according to Carnap and Schlick a statement may be admitted which either (1) is based on observation statements and can be tested by observation statements, or (2) is not based on observation statements but can be tested by observation statements, or (3) is based on observation statements but cannot be tested by observation statements. I see no objection to 'acknowledging' the 'expressions' of a statement which is (4) not based on observation statements, and cannot be tested by observation statements (Neurath 1937c). Whether we are prepared to waste our time by seeking ways how we could fit such statements of type (4) into our pattern is a different question, but the terminological basis is given for them and that is an important quality of a sentence and saves it from being regarded as an 'isolated' one.

'Aggregational expressions' are spatio-temporal ones and therefore we can relate them to schemes composed of spatio-temporal expressions. That is the reason why we call the 'aggregational' language a 'physicalistic' one. Terms

such as 'thinking man,' 'observing man' are 'aggregational' ones, whereas terms such as 'thought,' 'observation' and similar nouns (particularly when one regards them as 'factors' within a so-called 'epistemological' discussion) do not fit into the pattern of 'physicalism.'

PROTOCOL STATEMENTS AS AGGREGATIONAL STATEMENTS

With regard to my proposal always to maintain the 'aggregational' character of our initial statements we should put into the observation-statements the observer-name, if necessary the instrument-name and other 'aggregational' terms. I use the term 'observer-name' instead of 'name of an observer,' stressing that I do not speak of two items 'name' and 'observer' but only of the term 'observer-name.'

A 'protocol-statement' may run as follows: "Otto's protocol at 3.17 [Otto was word-thinking at 3.16 (in the room at 3.15 was a table perceived by Otto)]." Statements of this type are neither 'simple' nor 'primitive' but they are of the type which is used by scientists and by the man in the street when they are discussing 'factual statements,' 'hallucinatory statements' and other items of this type.

When a man wrote, "I have seen a zebra in the zoo," then somebody may say: "There is a zebra in the zoo, but you are a liar as you did not visit the zoo." Such trivial statements we can express by means of our 'involved' protocol statements. Let us call the four 'parts' of our protocol statement: A (protocol), B (word-thinking), C (zebra), D (person perceiving).

A, B, C, D, accepted	A, B, C, D, accepted
B, C, D, accepted	B, C, D, rejected
C, D, accepted	C, D, accepted
D, accepted	D, rejected
'factual statement'	'type of lying'

A, B, C, D, accepted
B, C, D, accepted
C, D, rejected
D, accepted
'hallucinatory statement'

'Aggregational language' uses expressions such as: 'the moon,' 'spectral line in a certain microscope,' 'periodicity of colours on the surface of a liquid,' 'periodicity of pain,' 'periodicity of sounds,' whereas in the formulæ of mathematical physics (regarded as a section of mathematics) we find, *e.g.*,

periodicity-symbols of the calculus which can be related to aggregational periodicity-terms of our language.

We can apply the same statements of mathematical physics to observation-statements made by deaf-mutes without using sound terms, and to observation-statements made by blind persons without using colour-terms. It is not difficult to imagine blind persons who do not only discuss the problems of optics but also use their own experiment-statements. One can make optical experiments by means of selenium-cells and similar devices.

According to our point of view optics has no more to do with 'seeing' than with 'hearing.' (Mach, on the contrary, linked each branch of physics with a correlating type of observation-statements.) I think 'pain-statements' cannot add details to 'sound'-statements when the building up of physics is intended, and also smell-statements do not add very much, but together with other statements we are using 'pain-statements' and 'smell-statements' in building our 'aggregational cosmology.' I see no reason why we Logical Empiricists should treat differently the pain-statements and the smell-statements, why we should put the 'higher-sense' statements into a particular drawer. Karl Popper and others try to avoid these statements as 'mental' ones. They are responsible for the widely spread opinion that physicalism leads to abandonment of all nuances and of the richness of our life. Why should it do that? I have no objection to use all shades of a painter's or a connoisseur's stories, when we transform them into a proper 'physicalistic' shape. Statements of the type: "this entrance hall of a building thrills me" can be regarded as physicalist ones because they are observation-statements.

TRUTH TERMS

When somebody makes a statement, I propose we, as empiricists, should ask him: how can one make this statement plausible by observation-statements which we are prepared to accept? In this we are able to get a set of 'accepted' statements, a kind of an encyclopedia.

How can people, who 'accepted' different encyclopedias, written already in our 'aggregational' language of physicalism, co-operate? Each group can try to make the choice plausible to another group and then wait for success. How could they discuss 'THE TRUTH' of competing arguments? I propose to say that scientists of a certain group at a certain period are 'accepting' or 'rejecting' statements or they are 'suspending their decision.' I propose to avoid the expressions 'verification' and 'refutation' altogether.

Each statement could be rejected, also 'protocol-statements.' Let us

imagine Kalon writes with his left hand a protocol statement 'p' and with his right hand a 'protocol-statement,' let me say 'q,' which is contradictory to 'p.' We might reject both of them or at least one of them if we did not find a way to speak of a 'split personality.' Others think that protocol-statements written by two different persons could not appear to be contradictory. Let us imagine Otto's protocol-statement states that Otto wrote a protocol at 3.17 p.m. whereas Arthur's protocol-statement states that Otto was asleep at 3.17 p.m. First step: acknowledgment of expressions – problem of language-making and therefore of 'TERMINOLOGY.' Second step: acceptance of statements – that needs 'decisions' after comparing various possibilities, even when we make simple 'generalizations' telling, *e.g.*, that the data of the astronomers permit formulæ according to which the historically given positions of a planet may be regarded as points of an ellipse. 'Decisions' are the more needed when we try to make 'extrapolations,' *i.e.*, 'inductions.' The 'induction' is not based on an account even when we may decide that the 'extrapolation' (or 'interpolation') of an account may be accepted.

In accordance with our traditional language we may say that some statements are accepted at a certain time by a certain person and not accepted by the same person at another time, but we cannot say some statements are true today but not tomorrow; 'true' and 'false' are 'absolute' terms, which we avoid.

We are prepared to show that a certain theory is more 'plausible' than another theory (we avoid the term 'probable' which we reserve for the probability calculus) and we may 'corroborate' a theory or 'weaken' it. I have to stress that we may assume that we are always able to say of a theory it is more plausible than another theory without assuming that we can build a scale of plausibilities and speak of 'degrees' of plausibility (Otto Neurath, 'Pseudorationalism of Falsification', pp. 121–131). I cannot agree with Carnap when he adds to my proposal dealing with 'corroborating' and 'weakening' his proposal dealing with 'degrees' of confirmability (Carnap 1936–1937; in this article he explains the importance of 'reduction' *i.e.*, 'conditional definition;' see Carnap 1938).

VALUE TERMS

That Logical Empiricists 'relativize' terms such as 'just,' 'right,' 'beautiful,' needs no particular discussion. 'Just' may be replaced by 'just in concordance with regulations made by somebody'; it does not matter whether one interprets 'somebody' as the person in question ('own conscience') or as

'the community in which somebody is living.' Difficulties may arise when we start with comparative studies, *e.g.*, asking to what other items 'justice' is correlated. The usage of the word may be unstable within the same community and we need another measure for testing, besides the usage of the term, 'usage' within a community. But we do not see how we should make our comparative studies when the word – or an equivalent – is not in usage. Anthropologists tell us of tribes whose language does not provide a translation of the term 'justice.' Another question remains, to what extent they have a behaviour sufficiently similar to the behaviour of the Westerner – so to speak, a 'justice-behaviour,' without having a verbal correlate to it. It is not immediately apparent that we should not be able to find such a behaviour analogy. We may speak perhaps even of 'dogs loving their puppies,' of 'obeying-dogs' of 'dogs, conscious of guilt' within a careful terminology. In all these cases we are led to a more comprehensive terminology, which does not depend upon the 'accidental' terminology of historically given notions but upon terms created by us.

'Genius' is an empiricist term, but many discussions on human genius are handicapped because we have mostly to use a selection made by people who apply the term to certain persons in accordance with certain historically determined situations. A comparative study would be confronted with problems which Edgar Zilsel discussed (Edgar Zilsel 1926).

I am doubtful whether the value-terms allow at all a modification which would enable us to put them into our universal terminological pattern. I avoid, if possible, 'asymmetric' terms in such cases. Why should we anticipate that 'a picture A has to be beautiful for a person B,' why should 'having the feeling of beauty' not be the start (it is even difficult to find a traditional expression for that), such as we today classify a certain behaviour as 'fear' without the assumption that this fear has to be regarded as a 'fear of something.' The physicalism, far from eliminating value-questions, or problems connected with what is called 'moral questions,' tries to find a terminology which enables us to discuss as many of these problems as possible by means of universal terms.

What is called 'violation of a law' in one community may be called 'violation of a custom' in a different community and in a third one we may find people who are in fear and anger after certain actions, of which we could not say that they are violating any formulated regulations, but that they are apparently in a state which is usually correlated to 'violation of something.'

The introduction of the term 'taboo' by anthropologists was of great importance (Radcliffe-Brown, *Tabu*, Cambridge University Press, 1940),

and I think the introduction of similar terms will be useful for the social sciences.

The so-called 'theory of law' will hardly remain unaltered. Laws as 'imperative sentences of a god' are sufficiently defined, but it is difficult to imagine that a science should be particularly concerned with 'regulations formulated by groups of men.'

When we regard 'medicine' as a part of 'biological engineering,' then we may be interested in the 'art' of 'making an efficient hospital.' We may analyse the operations performed and the procedures of surgeons and nurses as elements of 'hospital technique.' In analogy with that I could imagine scientific studies dealing with 'social engineering,' 'state-making technique' a part of which are the 'regulations' of course. I do not think that we should separate the 'regulations' in the hospital from other elements of the hospital technique. Procedures not covered by regulations may be of the same interest for us as others covered by them. Why should it be of interest whether certain regulations are 'officially' introduced by the ruling body or by physicians who are in charge of certain departments? Questions of this type are seriously discussed in the law literature.

Similar difficulties arise in the field of 'economics.' The terms of this discipline are not very universal and do not form a homogeneous pattern. Value terms of the older type are often removed, but their pattern remains, no longer with any justification, a situation not so rare in the sciences (Neurath 1935c).

CAUSATION TERMS

The intention to form a store of 'aggregational' expressions continues the historical trend of empiricism, as it definitely appeared in Hume's criticism of the traditional causation statements. Mach proposed to use the term 'functional relations' ('functional' used as in mathematics) to avoid the 'asymmetry' of 'cause' and 'effect.' The terminology of 'causation' is full of man-traps (Philipp Frank 1932). Therefore I propose to drop the causation terms altogether.

Certain languages without the causation terms seem to be suitable for the practice of life: D. D. Lee (1940) explains that the Trobrianders have no substitute for the causation terms. Such historical information makes it easier for many students to abandon the causation terms, which they often regard as indispensable on lower stages, at least, of scientific arguing. We could use terms such as 'arising from' or 'coming out of.'

We know the endless discussions of the difference between 'cause' and 'occasion.' How often social scientists try to find out how 'social factors' are influencing one another. It is not useful to speak of 'social factors' as long as we have no theory determining this kind of classification. Sometimes 'intellectual and ethical forces' are brought into the picture, the 'world of religious movements' with 'commerce,' 'finance,' and 'industry' (Tawney 1930).

The Marxist literature tries to avoid the traditional metaphysical antithesis 'ideas' and 'reality' by substituting the antithesis 'superstructure' and 'substructure,' a terminology, which is based on the causation terminology stressing a kind of 'priority' of the 'substructure.' One could ask whether one can better predict alterations of production organization than alterations of music, painting, etc., and whether one can better predict the state of music and painting when we know the state of production organization, than *vice versa*. This possible inequality of deduction possibilities would be the small remainder of the 'asymmetric superstructure-substructure terminology' with its strange 'priority' qualification which we meet in so many other doctrines, too.

IDEAL TYPE TERMS

The 'absolutism' of terms appears particularly in terms which speak of certain 'standards' – Max Weber, *e.g.*, wants to estimate the 'irrationality' of a certain behaviour by comparing it with the behaviour which would appear, if a man would know completely all elements of a situation and all possible ways and therefore the most effective way. This tendency to create an 'ideal type' is connected with the old strain of rationalism. The '*homo economicus*' and similar beings have been created in this way. Laplace imagined a superhuman being which should be able to predict all future dislocations of all bodies by knowing their present positions and the formulæ which describe their actions. When Newton imagines the space as '*sensorium dei*' for explaining his theories, he admits that his terminology is not an empiricist one in this direction. He starts with an 'ideal type,' not with what Avenarius calls 'Natürlicher Weltbegriff.' This latter start is used by Mach and Einstein systematically. Our proposals continue this kind of 'secularization.'

The 'idealization' of social patterns is at first dangerous because we do not know, generally, to what extent certain social elements are inseparable. An analogy: What should it help us to imagine a human being who is very tall but has no weight, or something like that?

ONTOLOGICAL MISINTERPRETATIONS

When we suggest we should alter the terminology of 'good' and 'evil,' not a few of our critics say, "Logical Empiricists declare there exists no good and evil at all" and they continue "there is no difference between brute force and kind love for these Logical Empiricists who are destroying moral feeling." We propose, on the contrary, to find a way of saying all that seems of importance for us not less than for others – and kindness, *e.g.*, is certainly of importance – in a manner which enables us to discuss these problems with people of a very different faith and attitude.

I cannot deny, on the other hand, that this altering of our terms may be related to alterations of our social life which are going on for decades, for centuries, for millenniums. The mathematician may exchange symbols without altering his deductions and this exchange may not be related to apparent changes of our community. But when we alter everyday terms, then we have to expect that they are related to other changes, and I think that is a serious problem of our community, but it has nothing to do with the interpretations given by 'ontological misinterpretations.'

Logical Empiricists admit by their term 'proposal' that they expect competing proposals. They are in principle prepared for a tolerant attitude. They lack the unambiguity of traditional rationalism. Not a few thinkers are of the opinion that at least a certain remnant of this attitude is needed as a stimulus (Giorgio de Santillana 1941, on Enriques' 'infinitesimal apriori'). I do not think that this stimulus is indispensable (there was a period in which theology, or at least a remnant of theology, seemed to be an indispensable element of scientific research) – that is a question to be dealt with in a chapter, 'behaviouristics of scientists.'

I think it is more serious that a child learns a language as a being reflecting the behaviour of other people, adopting thousands of taboos. No Logical Empiricist can prevent a child from being conditioned in some way, he can only attempt an alteration of a particular type of conditioning.

The 'ontological misinterpretations' are not limited to thinkers who are totally opposed to our enterprise, but also a scientist such as Bertrand Russell, whose books were of great influence within the Unity-of-Science movement and in the analysis of language, treats our attempts in this way. I shall exemplify my remarks on this point with quotations from his latest book, *An Inquiry into Meaning and Truth*, in the Preface of which he declares: "I am, as regards method, more in sympathy with the logical positivists than with any other existing school."

Russell says (p. 148), "Neurath's doctrine, if taken seriously, deprives empirical propositions of all meaning. The purpose of words, though philosophers seem to forget this simple fact, is to deal with matters other than words. The verbalist theories of some modern philosophers forget the homely practical purposes of everyday words, and lose themselves in a neo-neo-Platonic mysticism; I seem to hear them saying 'in the beginning was the word,' not 'in the beginning was what the word means.' It is remarkable that this reversion to ancient metaphysics should have occurred in the attempt to be ultra-empirical."

I do not know which of the multifarious features of neo-Platonic mysticism Russell referred to when he wrote this statement. Apparently he transforms our statements which deal with terms into 'ontological' metaphysics dealing with the 'existence' of words, which therefore he opposes by means of an 'ontological' statement "in the beginning was what the word means." It may be pointed out that Russell always uses terms in his answer I do not use at all. I did not formulate a statement even similar to "in the beginning was the word."

Russell polemically says: "If I go into a restaurant and order my dinner, I do not want my words to fit into a system with other words, but to bring the presence of food." I agree with Russell, but I did not say that I want to fit my words into a system with other words when I want to get chicken. I only made a proposal how to establish a discussion. I proposed not to use the term 'comparison' in very different cases: on the one hand when we say 'one statement contradicts another statement' and on the other hand when we say 'the bringing of a rabbit instead of a chicken does not satisfy Russell's desire expressed by his order.' I propose only that we should transform the expression into: "the word-thinking of Russell, 'A chicken will appear' " (in connection with his order) seems to be contradictory to his word-thinking: "no chicken appeared." That is all.

Russell tries to create something absolute, a 'reality,' a 'personality' or whatever it is. Therefore he, like Schlick, compares the 'memory statements' with the 'instantaneous thinking,' whereas I would propose to say we are comparing our 'first memory statements' with our 'latest memory statements' (Otto Neurath, 'Radical Physicalism and the 'Real World' ', pp. 100–114).

Russell seems to want, at least, the personality as a stable permanent element of his discussions, so to speak, as an *a priori* assumption. In this connection he stresses the dichotomy 'mental world' and 'non-mental world' and he seems, like Wittgenstein, with whose opinions he is not always in agreement, to imagine a kind of 'immaterial' proposition as a bearer of

significance behind the statements. I think one could call statements 'representing the same proposition' when they are exchangeable with respect to their respective positions within human communication.

My proposal is to treat all observation statements democratically, irrespective of whether they are made by the same person at different times or by different persons, and then I propose to make no difference in principle between observation-statements made by a person a few seconds before or years ago. Why should we not start with all these 'ipseities' without making a difference? We could so continue an old philosophical tradition, which was interested in 'solipsism.' I think it was not so strange that a kind of 'solipsism' was more than once closely connected with empiricism. The problem is, how to keep, on the one hand, the 'ipseity' of empiricist statements and on the other hand to remove the 'EGO' from the traditional pedestal. We plead, so to speak, for a kind of 'pluripsism.' We avoid the 'asymmetry' of the solipsism but keep its 'autopsy' (see Karl Britton's remarks on 'Asymmetry' (1939, pp. 62–63), and L. J. Russell's (1934)).

A discussion of Robinson Crusoe with himself is full of 'operational' problems dealing with social implications of language. Russell is of a different opinion (p. 186): "I think that, in fundamental discussions of language, its social aspect should be ignored, and a man should always be supposed to be speaking to himself, or, what comes to the same thing, to a man whose language is precisely identical with his own. This eliminates the concept of 'correctness.' What remains – if a man is to be able to interpret notes written by himself on previous occasions – is constancy in his own use of words; we must suppose that he uses the same language today as he used yesterday." It is remarkable how many expressions Russell here uses, which I regard as dangerous, and how he imagines the 'ideal' constancy of language, how he images two people or a man at different periods, using identical languages. I would not object to saying that many people alter their language and that even on the same day people use a mixture of languages, sometimes called '*quaternio terminorum*.'

This tendency of Russell to imagine a solitary thinker of absolute constancy of personality explains perhaps his opposition to our proposal that all statements should be regarded as historical elements. Russell writes ironically, "In a different culture another body of propositions may be accepted; owing to this fact, Neurath is an exile. He remarks himself that practical life soon reduces the ambiguity and that we are influenced by the opinions of neighbours. In other words, empirical truth can be determined by the police. This doctrine, it is evident, is a complete abandonment of empiricism, of which

the very essence is that only experiences can determine the truth or falsehood of nontautologous propositions." Does Russell think of an 'absolute truth in itself'? But even if such a truth exists, somewhere the human beings discuss this truth only by means of our human statements, and they fight one another. Who knows the truth? Perhaps the imagined 'solitary thinker of absolute constancy of personality'? I do not think that we can describe a fight between 'error and truth,' but only between different groups of thinkers. Their statements may be correlated with 'cosmic aggregations,' with sunshine, floods, industry, churches, paintings, and last but not least with the police. The police do not 'determine' our statements directly, predominantly they influence their public discussions. More important are the indirect 'determinations' – people are not only prevented from saying many things but they are so altered that they even in private discuss strange observation statements. It would be not without interest to find out how Russell's imagination of the solitary thinker and my own proposals grew up from the situation decades ago. No judge is in the chair who says which of us has THE TRUTH.

Our proposals lead to history and sociology of the sciences (Otto Neurath 1935d) and to a stressing of the social implications of language. This is particularly in accordance with the leading intentions of C. S. Peirce, G. H, Mead, John Dewey and others (Charles Morris 1938).

The 'pluripsist' attitude of 'physicalism' gives no place from which to move the earth. The diaries of many people are co-operatively used for building up the Unified Science. How can we form a Universal Jargon for this purpose which may prepare a Lingua Franca for the sciences? That is far from making any 'ontological' statements about the 'essence' of the 'world.'

We try to start as analysing scientists in the same empiricist way as we are accustomed to start in the practice of the sciences, which form a part of our social life. I cannot deny that many scientifically minded people do not like such a start full of vagueness; they would prefer – as I would prefer too, if I did not regard this wish as a utopian one – to start with exact initial definitions and atomic simple elements.

Others who do not like scientific attitudes in comprehensive discussions are against our start because it is scientific and not a metaphysical one. That is as it may be. Finally we find ourselves all together in the same ship and are co-operating even when we think we are fighting one another.

CHAPTER 22

THE ORCHESTRATION OF THE SCIENCES BY THE ENCYCLOPEDISM OF LOGICAL EMPIRICISM

I

I took the term 'orchestration' from Kallen's lively lecture, read at our fifth Unity-of-Science Congress (Harvard, 1939), as one of my pet words and have since used it frequently. Thus, I gladly accepted the editor's friendly invitation of starting a discussion on Kallen's paper 'The Meaning of 'Unity' among the Sciences' (*Educational Administration and Supervision*, February, 1940, pp. 8–97) in the pages of this periodical.

First of all, I want to express my sympathy with Kallen's general attitude towards the scientific breed of totalitarianism. I do not think, however, that he deals properly with our 'logical empiricism,' the main features of which are of an anti-totalitarian character. The Unity-of-Science movement is multifarious, and as the membership lists of our congresses show, people with a great variety of opinions come together there; we logical empiricists are only one group among many. We intentionally rejected the plan of forming anything like a programme, and we stressed the point that actual cooperation in fruitful discussion should demonstrate how much unity of action can result, without any kind of authoritative integration.

This makes it difficult to give a short comprehensive account of this movement; but since Kallen, who himself belongs to the movement, apostrophizes me as a representative of it, I will speak here for myself, and I shall assume that some of my scientific friends, even if they should not agree with some of my peculiarities, will not object to my description of our general trend.

We are here analyzing human traits and attitudes, not the consistency of arguments. I shall therefore try to describe how I myself, as a logical empiricist, developed my attitude towards the sciences and their unity. Many of us, besides myself, have been brought up in a Machian tradition, e.g., Frank, Hahn, von Mises. Because of this, we tried to pass from chemistry to biology, from mechanics to sociology without altering the language applied to them. We, as many others all over the world, were also influenced by scientists such as Poincaré, Duhem, Abel Rey, William James, Bertrand

Reprint of Neurath 1946a [ON 272].

Russell, and I, in particular, by Gregorius Itelson. I think that Poincaré and Duhem made me realize that wherever one hypothesis can be elaborated, it is possible to elaborate any number (cf. my 'On the Foundations of the History of Optics' (1973e)).

This Vienna enterprise was pushed on by Schlick and Carnap in a most intensive way. They particularly supported the analysis of the scientific language as such. Of all the refinements Carnap brought to us, one point impressed me especially, namely, that we should not only distinguish between sentences we want to use and those we want to eliminate, but also between sentences we want to eliminate because they are contradictory and those we want to eliminate because they do not fit into our scientific language at all, 'meaningless' sentences within the language in question.

Eliminating 'meaningless' sentences became a kind of game, and I enjoyed it when, at our meetings on Wittgenstein's book, to which we owe so much, I could call sentences 'meaningless.' But I very soon felt uneasy, when members of our Viennese Circle suggested that we should drop the term 'philosophy' as a name for a set of sentences (all 'meaningful' sentences being scientific, and the remainder 'meaningless') but use it as a name for the activity engaged in in improving given sentences by 'demetaphysicalizing' them, as it were. I objected to that, as a rather negative activity, and remarked jokingly that it would force us one day to invent a metaphysic to enable us to weed that out. Thus I came to suggest as our object, the collection of material, which we could accept within the framework of scientific language; for this I thought the not-much-used term 'Unified Science' (*Einheitswissenschaft* – friends and I, later on, edited a series of books under this title) a suitable one.

But now the question arose, what would such a unified science be like? My 'pluralist' attitude, described above, immediately objected to all absolutist attempts to speak of one comprehensive 'world picture' as the best, or as other absolutists would say, the 'relatively best,' even if one were to look at it as an 'ideal' only. I would not think of '*the system*' as a 'model' and stressed that one had to remember the 'gaps' and 'gulfs' from the beginning (cf. 'Pseudorationalism of Falsification' pp. 121–131).

I knew very well that in any consistent system of statements a single contradiction would 'infect', as it were, the whole body and would enable one to infer anything one pleased. I also knew that in the practice of scientific behavior, occasional contradictions did not destroy the work. How should one look at this? My father, an economist, used to ask: "What would happen if someone were to force scientists to follow up consistently all

the declarations they make. Fortunately," he would add, "they compare their deductions again and again with their experiential material." So it is; our scientific practice is based on local systematizations only, not on overstraining the bow of deduction. Very often scientists know perfectly well that certain principles applied to a certain area are very fruitful, while contradictory principles applied to a different area also appear to be fruitful. It would, of course, be nice to harmonize the demonstrations in both areas, but, in the meantime, scientific research progresses successfully. I thought it in accord with the historically given situation to acknowledge these 'localized' contradictions, and to think of an 'encyclopedia as a model' (cf. my 'Encyclopedia as 'Model'', pp. 145–158) as intentionally opposed to the 'system as a model.' Let me call this approach 'encyclopedism.'

Thus I stressed that not even 'all-comprehensive consistency' could rule scientists in action. I have, of course, always appreciated any kinds of 'instances' brought forward to 'shake' or 'support' a hypothesis, but I did not acknowledge that there existed isolated single definitely negative instances, which could destroy any general empiricist assumption. We do not discuss 'isolated' items in chemistry, geology, or history; but every item is, as it were, an element of a 'cosmic aggregation' (cf. my "Universal Jargon and Terminology," pp. 213–229), and any experiential statement has to be regarded as a statement dealing with a 'cosmic aggregation'; each chronological 'date' given in a paper implies the connection of a certain item with the 'cosmic aggregation' to which it belongs. This 'aggregational attitude' has certain far-reaching consequences.

It leads, for example, to certain reflections on 'unpredictability,' usually overlooked by scientists, who discuss this prediction business, and who mostly only concede that our incomplete knowledge of the situation makes prediction incomplete. I try to show that even this assumption is derived from the absolutism of Laplace, who imagined a spirit who would be able to predict the constellation of all bodies in the future by knowing their position and the direction of their movement in the present. This is a problem discussed in detail by Philipp Frank. The unpredictability within aggregational discussions is a hard thing to swallow, even for many of my friends (cf. my *Foundations of the Social Sciences*, 1944).

"So far so good," Kallen and his friends will perhaps say, "but where is the famous 'unity of empiricism'?" Wait and see! As I have already shown, the Machian suggestion of one scientific language, supplemented by Carnap and others, formed the backbone of my scientific attempt to do something for the 'unification' of our scientific enterprise. As a sociologist I disliked

all this talk about 'the national spirit,' 'mentality of a ruler,' etc. Why should we not speak here in the same simple way as in the laboratory? And, as an empiricist I asked myself how we might start from simple observation-statements, on which to base all further scientific discussions. So I developed my suggestions dealing with 'protocol-statements' (cf. my 'Protocol Statements', pp. 91–99), frequently discussed since then by various people. I disliked starting from a vague statement of 'something red' floating somewhere in the air and therefore I asked for a more exact formulation. Such a formulation always gives the name of the 'protocolist' first and then adds his sayings. "Charles told us he had seen a red table in his room on March 4th" seemed to me a fair start, which enabled us to ask the question, "When, where, and how?" which we are accustomed to ask when we make an astronomical or chemical statement. With one stroke, I thought, I could overcome a certain cleavage always felt when scientists want to pass from 'sensual elements' to descriptive statements on stars and stones.

My suggestion seemed to have the advantage that the "when, where and how" attitude could be maintained from the bottom to the top. This I call the 'physicalist' approach, which has nothing to do with 'mechanism' or anything like that; it only pretends that we can use the everyday language which we use when we talk of cows and calves throughout our empiricist discussions. This was for me the main element of 'unity.'

Of course, besides the unity of the chemical, biological, and historical language, one can also analyze the 'unity of laws,' a problem Carnap tackled more than once, developing to a high level the relatively poor attempts made before (e.g., Hjort, *The Unity of Science*, Gyldendal, London, Copenhagen, Leclerc du Sablon, *L'Unité de la Science*, Alcan, Paris). But I myself stressed that we can start from everyday language after dropping some expressions, derived from magical, theological, or metaphysical speculations.

My thesis is that this start is common to human beings, past and present, all over the world. We are not presenting them with some new unity; not at all, we only want to say that wherever people speak to one another, for example, marooned men on an island coming from different parts of the world about fishes and trees, drink and sleep, pain and pleasure, they will have no particular difficulties in communicating through gestures, pictures, and words, which they may translate from one language into another. Difficulties will usually appear when they *want* to tell each other of their different magical expressions, theological sentences, or metaphysical formulations; whereas, and this is our point, in putting forward the principles of

the relativity theory we may start from the bulk of everyday sentences that all these people have in common. This agrees with a saying by Gregorius Itelson: 'What one cannot explain in principle to a taxidriver in his language must be somewhat twisted."

And thus I have tried to discover in agreement with my scientific friends what expressions might form the elements of a set of terms which could serve to create such a worldwide contact. This implies that one presents a set of empiricist expressions together with the rules for their application and asks for a convention concerning scientific communication. Such a set, suitable for discussions of any kind, I called the 'Universal Jargon' of each language in question. The English Universal Jargon would, therefore, be translatable into the French Universal Jargon or into the Esperanto Universal Jargon. But it was not intended to create a new language as such.

I myself think that many people do not realize sufficiently that the language we use bears in itself the scientific attitude we maintain and therefore research workers should not regard their respective phraseologies as something they just take over from literature, but as tools, anticipating partly the function they are to perform. Therefore I suggest that we speak of 'Terminological Empiricists' wherever people acknowledge this new approach to the sciences. That is in opposition to people who think they are fine empiricists when they drop 'mind' and praise 'matter,' since in our Universal Jargon both words disappear simultaneously, without any substitutes such as 'inner' or 'outer' experience.

Within the framework of this approach we value the empiricist basis highly, but on the other hand we also value the deductions we may use for connecting the various empiricist statements with one another. This combination is relatively new. Traditionally one looked at 'empiricism' as something crude and coarse, not interested in any refinement of arguing, and at 'rationalism' as something creating wonderful pyramids of arguments, deducing any detail on the basis of a nicely elaborated set of assumptions or *a priori* declarations. I suggested the expression 'Logical Empiricism' for stressing the combination of empiricism and highly evolved deduction. Since in Latin countries there is a fine tradition of 'rationalism,' I suggested the use of the term 'Empirical Rationalism' (used by Itelson) as synonymous with 'Logical Empiricism.'

I think it will help our debate if I give the reasons why I suggested the term 'Logical Empiricism.' Schlick and others have been fond of 'Radical Empiricism,' a term used by William James. I know very well how much we owe to James, but, on the other hand, he introduced this term, speaking

of 'the substance of reality' as other people perhaps speak of the 'area of New York.' James is also a supporter of a very Bergsonian attitude, which is foreign to empiricism. I succeeded in getting the word 'radical' dropped entirely, as far as I can see; but I have been less successful in promoting 'Logical Empiricism' instead of 'Logical Positivism,' a term much liked by many friends and critics. Not being a pedant I can bear that. But I think from a pedagogical and Kallenian point of view we should solemnly cut the strings which connect us with the positivism of the past. Comte and some of his followers, in their arguing and in their social approach tried to create a definite system of universal morality not based on the consensus of mankind, but on the deductions brought forward by the positivists. Their praise of medieval Catholicism is connected with what Kallen would call an imperialist attitude, which led them to create a kind of positivist church, of which something has remained in Great Britain. Their anti-pluralist attitude induced me to drop, wherever possible, the term 'positivism.' I know that this endangers von Mises' witty remarks on 'positive' and 'negative' attitudes, but I hope he will not object to my not very powerful action.

The first step of our Unified Science as an Encyclopedia is that we 'acknowledge' the elements of our Universal Jargon. That is nothing definite; tomorrow we may reach another convention. Within the acknowledged field, then, we 'accept' certain statements as being in harmony with protocol statements, 'reject' others, and remain undecided in some cases. We then try to collect statements which we have in common and to build up hypotheses and theories. Let me stress that sometimes people do not even harmonize in selecting protocol-statements even when they agree that only protocol-statements may act as last instances in discussions between them. One can learn from this that we have no possibility of discussing the 'truth' of anything, since there is no imagined arbitrator in the chair. Therefore I suggested that we drop the term 'truth' with the whole of its large family. Everything will then be based on the comparison of statements with protocol-statements, leaving open the many ways in which such a comparison can be made. It is essential that all statements should be 'connectible,' as von Mises happily puts it.

If people ask how we test our Universal Jargon as the best (people who want to conceal their absolutism speak of the 'relatively best'), we have to answer that such a test cannot be carried out. Of course, we do not toss a coin (though even that is sometimes better than a pseudo-scientific fairy tale, which tells of *the one solution*,) but we listen to various 'instances,' and in the end we have to reach a 'decision' not based on a calculus. One

cannot test the future usefulness of a scientific technique beforehand: unpredictability here plays its part. Some people think that 'testing' in social life is a rather democratic activity and deplore the fact that we do not apply it here. But even that is doubtful. Who can test the future social implications derived from applying certain tests? And first of all who tests the testers? The tyranny of the bosses may be supplanted by the tyranny of the testers. Poor man, whom they have 'tested down' to a 'nothing, not good for anything.'

I think that this gives a picture of the democratic attitude of the Unity-of-Science movement, which acknowledges from the start a multiplicity of possibilities. It is the problem of any democracy, which any actual scientific research organization has also to solve: on the one hand the non-conformists must have sufficient support; on the other hand, scientific research needs some cooperation. This implies that on the one hand we have to leave something to chance, and that on the other hand we have to find some loyal compromise for actual collaboration, without suppressing personal convictions. What can we call this democracy of cooperation within the 'encyclopedism'? I have no better word for that than Kallen's 'orchestration.'

II

That is that. Now let us pass to an all-round analysis of some of Kallen's remarks. He tries to create an unfriendly atmosphere against any kind of unification. I am surprised that my kind friend Kallen speaks of our human brethren as men combatting or confirming variance. I know what terrible things we human beings sometimes perpetrate, but I also know that cooperation forms a considerable part of our private and social way of life.

Further, Kallen thinks of unity as something imperialistic. I confess that when studying the history of Visual Communication, I had to stress just the opposite. If priests and rulers have a language of their own, they become separated from the ruled masses, and it is just the unification of language that is a step forward to some democratic possibilities. When I created 'Isotype,' together with my collaborators, as an international technique of visual information, I was thinking mainly of the masses, who could now grasp something more than before of the present knowledge of mankind.

A Universal Jargon – I think it is good to take from Kallen the term 'Logpu' – would be an advantage from the point of view of popularizing human knowledge, internationally and democratically. Chinese Logpu, Russian Logpu. Basic Logpu, Interglossa Logpu accompanying Isotype charts seems

to me something anti-totalitarian, as long as people have an opportunity to look at various points of view appearing in such information.

Kallen and others seem to think of Logpu as something highbrow and complicated, whereas it is just our point that Logpu starts from everyday language, using such words as 'tree,' 'red,' 'fence,' 'cow,' and 'man,' but not such words as 'matter,' 'mind,' 'dialectics,' '*élan vital*,' and 'reality.' I should like to make an experiment when lunching again with my friend Kallen in a New York restaurant. I think everything will go smoothly as long as we use the words 'turkey,' 'crackers,' 'cold,' 'hot,' 'happy,' and so on. Even if a Melanesian friend appeared, I do not think that particular difficulties would arise; perhaps the interpreter, if we could not manage with a dictionary, would translate 'turkey' as 'some fowl' in Melanesian. But difficulties would arise if Kallen started talking to us about 'causality,' 'inner experience,' or something like that. I should not grasp the point as a hard-boiled Machian, and the Melanesian guest, or Host, perhaps, would not, being just a Melanesian, who is not accustomed to speak in terms of 'causality.' But in our Logpu language we could tell one another of fishing in the Pacific and in Alaska, of an airplane – the big bird made of metal – and so on.

I do not deny that, as Kallen states it, our scientific environment is as full of totalitarian danger as our whole social environment, but the unity of Logpu, as such, all over the world, seems to be untotalitarian. Predictions made in Logpu could be totalitarian in a book with an absolutist approach; if an author like Spengler predicts something as 'unavoidable,' then he pretends a totalitarian power nobody has. A logical empiricist who likes to think of many possible predictions would tell everybody that the pluralism of encyclopedism enables us to try something, just because we have no reason to expect anything to be definitely 'unavoidable.'

Logical Empiricism is fighting 'metaphysical idealism' along the whole line. It is just this set of phantasmagoria, allowing terrible means to lofty ends, which very often reduces the preparedness of people to object to the mercilessness of totalitarianism. Think of Plato's *Republic*, for example, where the Nazis found fine arguments for persecution, for destruction of mentally or bodily weak people and for teaching children to be cruel ("blood lust like young hounds" as Plato puts it within his idealist deductions). We recognize that atmosphere in the period when crusaders annihilated the Albigenses lock, stock, and barrel, and a highly praised emperor, Frederick II of Sicily, introduced the persecution of heretics into the secular law and started forcing the Jews to wear humiliating badges and to dismiss Christian servants. We recognize it in the period when Jews and Moors were expelled

from Spain and Torquemada organized the Inquisition. We know of Fichte's lofty so-called idealism, which permitted him to ask for the degradation and expulsion of the Jews and for using 'Barbarians,' outside and even inside Europe, as targets for the warlike practices of the highly cultured youth; Fichte wanted to educate his youth as Plato educated the youth of the guard formations of his Republic.

Since the encyclopedism of logical empiricism challenges any intellectual authority which pretends to preach the truth (whether it identifies the truth with the leader's intuition, with the interest of the state or any other human group, with the decision of a deity, or with anything else) it is out of the question that it should not challenge any attempt to misuse any kind of distorted empiricism for creating a similar authority.

I cannot but agree with Kallen when he thinks that many people today try to use for totalitarian purposes the popular drive to reach a higher standard of living by planning, but I think that Kallen wholly overlooks the fact that there is a possibility of 'planning for freedom' as suggested by various people. I myself think that one can hardly get more freedom by non-planning than by planning for freedom with all its muddle and whimsicalities, though planning can suppress freedom to an extent hardly imagined before (cf. my 'International Planning for Freedom', 1942).

I think the sceptic pluralism of our empiricism is, in itself, hardly a suitable tool for suppressors. He who is full of 'zeal,' may spread, without knowing it, some danger, even if his propaganda deals with nice things; because it accustoms people to focus on one particular, over-estimated, and emphasized point that may later be transformed into a super-human being, *the* state, *the* leader, or something else. I try to imagine the fate of many Germans in this way. They are, as individuals, on an average not less friendly than other people, but the German atmosphere is full of enthusiasm and exaltation, more than, for example, the Anglo-Saxon one; that is, full of preparedness to admire self-sacrifice, and to desire death in war for the highest good; and, from self-sacrifice it is but a short step to realizing that sacrifice of others is unavoidable for *the* cause. In the Anglo-Saxon atmosphere, people think more in terms of the little happiness of all little men in a human environment, and even the people on top regard themselves more often as people who like to have their weekends and who therefore could become potential victims of a future totalitarianism, rather than as people who are permanent commanders of guard formations. This would agree with the prevalence of empiricist utilitarianism in the Western countries and of metaphysical idealism in Germany.

Voltaire and Zola grew up in this Western, humane atmosphere, full of sympathy wherever people suffered from persecution; Zola was just preparing a book on Zionism when he died. They did not concentrate on any particular enthusiasm, but tried to be humane whenever the occasion demanded it, like the devil's disciple in Shaw's witty play of that name, acting, not even for a moment, as under pressure from some authoritarian categoric imperative.

I do not think the line of division runs between people with secular and those with transcendental creeds, but rather between people with a centralized and dominating zeal which may possibly lead to self-sacrifice and the sacrifice of others, without tolerance on principle, and people who are tolerant on principle, having perhaps some transcendental creed, or because they, as empiricists, see the multiplicity of all arguing. People of the totalitarian kind may try to make scientists the leaders of a new society, like the magicians, nobles, or churchmen of former societies. The encyclopedism of logical empiricism does not see why scientists, trained to discover as many alternatives as possible, should be particularly able to select one alternative only (one that never can be based on calculation) by making a decision or performing an action for other people with different desires and attitudes.

On the other hand, I should not say with Kallen that the sciences as such are 'centrifugal,' since even the alternatives mentioned can only be reached by the cooperation of scientists throughout the world, who already use some parts of our Logpu, without knowing it. I think the 'exaltation of diversity' is rather the concern of the metaphysicians, who are very proud of being different. Since they have no work in common, they can build their ivory towers *ad libitum*. For these metaphysicians, Kallen's "one man's meaning may be another man's nonsense" may be valid; it seems rather a Spenglerian overstatement. I should like to be given some examples from the history of the sciences which prove his statement. When we discuss gardening or optics, lunching or history, should our starting point appear to be 'nonsense' to other people?

I agree with Kallen that if we wanted to be very careful, we should translate sentences formulated in our own language of fifty years ago into different sentences of our own language of the present; being consistent, we should have to do the same with our sentences of yesterday and today, with my statements and Kallen's answers, even if they sound identical. Nevertheless, we find that we get on quite well, within limits, without such finesse. The New York lunch with our Melanesian friend will, I hope, go off perfectly smoothly.

Of course I am prepared to support Kallen in attacking people who want

to impress others by speaking of the 'exactness' of certain results, and by praising the 'truth' they could prove. I myself stress, as I mentioned above, that we should never overlook the 'cosmic aggregation'; within that we have to locate all items of discussion which have to bear the indefiniteness of this comprehensive enterprise of an aggregational approach.

I do not think it a well-chosen example when Kallen speaks of "indivisible units of matter" as a term dropped by scientists. It never did play a part in the experiential discussions and formulas of the physicists. It sounds rather classical, that is all. I know that there are books in which people complain that the many possible logics and geometries now make life so difficult. I must confess that I have read many books on various subjects, and I have never met this difficulty. On the contrary, many of the difficulties I met with as a boy and as a student have now disappeared from serious literature. But the problems Kallen mentions occurred when famous physicists such as Jeans and Eddington, whom Kallen quotes, made up fairy tales of an evening, as Lewis Carroll did, when, tired of mathematics and logic, he told a small girl the story of Alice in Wonderland. Unfortunately, many people take the fairy tales of physicists as seriously as others took Newton's interpretation of some biblical books. Philipp Frank and Susan Stebbing have dealt sufficiently with these disturbing books by physicists, which do not tell the man in the street how physicists make their discoveries but rather conceal it.

Just these physical fairy tales have a totalitarian touch, insofar as they try to create the feeling of something miraculous around us, whereas we logical empiricists want to show people that what physicists and astronomers do is only on a grand scale what Charles and Jane are doing every day in the garden and the kitchen. In the same way, the language in which scientists speak to one another is of the same kind as Charles' and Jane's Logpu, which we want to promote as the universal language of all of us.

Why is Kallen so angry with the people who invent new names for the new departments of science which they have started? Does he also object to the fact that people now speak of 'television,' 'stratosphere' and similar things? It would be difficult to find one's way through all these wonderful new calculi, without *proper* names.

Why does Kallen mention Spinoza together with Galileo? Because both were victims of heresy hunters in one way or another? Galileo's writings have been partly incorporated into modern science and are full of Logpu, whereas Spinoza's works, though they stimulated many people and helped them to fight for freedom, are not suitable for a similar incorporation, since

they are full of expressions we can hardly use in Logpu and also full of twisted arguments.

"Crossing logistics with a pragmatic theory of meaning," what is that? Is that me? Surely not. I myself do not use the term 'meaning,' nor a substitute for it, in a context which would allow the application of this remark, made by Kallen on my writings. I did write, many years ago, some papers on logistical subjects, but I never applied such enterprise to my modest scientific arguments. Perhaps I have learned a lot more in this way about careful arguments, but that is another story.

Repeating some of his arguments, Kallen again speaks of the increasing multiplicity of arguing. I think that multiplicity was greatest where magic, theology, and metaphysics ruled the show. But the scientific realm forces us to cooperate and to begin again and again from the protocol-statements which we have in common. The highbrow philosophers concentrate on the divergencies in the 'highest levels' of argument, as I explained above. Kallen asks how, when labeling something as science, we judge 'telepathy.' Were I to imagine Kallen and his friends choosing teachers for a school and subjects for a curriculum with me, I think we should agree about chemistry, gardening, astronomy, cooking, geography, history of arts, technology, etc. If we were to discuss the Nazi literature on race, as far as it is based on a purely empiricist language, I think we would agree about its undesirability, because we did not think the approach sufficiently serious, but rather frivolous. In the same way, introducing this argument we can also consider the telepathy business. As far as Logpu was concerned its qualifications might be all right; but then we have to consider the 'seriousness' or 'frivolity' of this field of research. I should like to know what Kallen thinks about my attitude towards seriousness.

There is some irony in Kallen's talk about Logpu. He treats us logical empiricists as though we were in competition with Kantians, Hegelians, phenomenologists and so on. He finds himself in good company. L. J. Russell, analyzing communication and verification, imagined in a similar way, "one group purely physicalist, another group theological," giving different reports. I repudiate this 'symmetrical' approach of Kallen and Russell from the start. Almost all people, even Kantians and phenomenologists included, have with us and other simple people some part of the language in common; they all have in common statements such as "we saw some airplanes yesterday" or "I met Jane in Charles' garden" and so on.

We use this common part of the language and these common protocol-statements for building up our whole science. We think that the Kantians,

Hegelians, phenomenologists and theologians add something peculiar we do not. We are not selling a new philosophy in competition with other philosophers, therefore Kallen's and Russell's demands for equal treatment of us and the metaphysicians are not in harmony with the situation.

That is the reason why I think that Kallen is overstating when he imagines only a *force majeure* could lead to the general use of our good and trusty Logpu. Why should a *force majeure* be necessary when Kallen, our Melanesian friend, and I want to have lunch and talk together in New York? I think of the humanization of scientific information in terms of such a meeting – mankind forming one large family with many different approaches but using some common elements of Logpu. Otherwise – that is our theme – they cannot cooperate at all.

Kallen asks whether I, 'generous hearted' as I am, have ever thought of the problem that there is also a danger of 'infection'; in the same way that scientific 'improvement' spreads, so also scientific 'muddle' may spread. I have indeed thought of this danger. But, as a sociologist I did not look at it as a problem 'in principle' only. I asked myself, what can history show us. The spreading of muddle does not seem to be as simple as the spreading of a successful technique. The frivolity of the race theory developed by the Nazis in many books on character, physiognomics and heredity, did not even infect the mathematics, astronomy, chemistry, and physics of the Nazis very much. If it had, the firing capacity of their machine-guns might have been reduced. Just because our whole life is connected with scientific activities, muddle must find itself difficult to spread. But, of course, I do not deny that such a danger exists in principle.

I think one point is clear. The unity we have before us, as a goal for the encyclopedism of logical empiricism, is based on the actual store of expressions which people have in common all over the world. Its evolution would be based on conventions which could never be definite or authoritative as far as the aspirations of conscientious logical empiricists are concerned. Pluralism is the aura of this scientific world community of the common man. The encyclopedism of logical empiricism (which, not only I myself, but so many of my scientific friends, promote, perhaps in different words) with the unified science encyclopedia are the children of the tolerant approach of democratic cooperation. It competes with no philosophy, and is anti-totalitarian through and through. Were I to give a name to this kind of cooperation of the man in the street with the scientific expert, I should again use the Kallenian expression, "orchestration."

CHAPTER 23

PREDICTION AND INDUCTION [1]

What is called the problem of 'Induction' has interested representatives of very different groups of thinkers again and again. Hume's renunciation of the induction and causality business did not satisfy research workers, who wanted to have a kind of 'justification' for their everyday technique. I should like to call their attitude 'pseudo-rationalism' – starting from the assumption that the one may be right, the other wrong, and by some effort they may get nearer to the truth. That is just what Logical Empiricism does not accept. There is no judge in a chair who decides who is nearer to the truth. There is no way of 'impartiality' or 'scientific objectivity', there is no point outside our life, from which we may finally decide what is 'impartial' or 'scientifically objective' – we do not see such a point.

Pseudo-rationalism does not sufficiently acknowledge that all our behaviour starts with the folklore of our youth and of our ancestors . . . Our ancestors seem to have feared that the spring sun might never rise again. It does not seem unlikely that human beings in almost all regions of a certain climate tried various devices to get the spring sun back. Then a kind of Couéism entered human society, and people repeated: "The sun will come again, the sun will come again – we know the sun did come again formerly."

Scientists are no better off than the men in the street as far as their predictions are concerned. They only collect more material dealing with past uniformities. The famous Kepler's laws may be regarded primarily only as statements which deal with certain star positions given by Tycho Brahe and other astronomers. And one would call it a tremendous discovery if Kepler had only said: "All known positions of Mars may be regarded as points of a certain ellipse." This statement does not even assert that other points of this ellipse are also likely to be points of the orbit of Mars. That would be an extension of this statement and would at first not be backed by observation-statements. Perhaps one would afterwards find other astronomers' protocols with respective data, and they might fit into this ellipse too.

The statement on the fitting of the known positions of Mars into an ellipse is a purely mathematical one, but the statement that we also expect other

[1] Reprint of Neurath 1946b [ON 274].

positions of Mars to fit into this ellipse is an 'induction' proper; and also the expectation that the orbit of Mars will be an ellipse in the future may be called an 'induction' proper.

The induction, if scientifically made, is based on some 'supplementation' (I suggest using the term 'supplementation' because I have no reason here to distinguish between extrapolation and interpolation, for which we have no common name); but there are always many such 'supplementations' in harmony with our *Encyclopedia*, and therefore it remains a matter of decision what kind of 'supplementation' we prefer. There are scholars who try to find certain rules for making inductions; but I can hardly imagine how these could be a substitute for decision.

There is nothing strange in the assumption that a group of scientists agree about all observation-statements and nevertheless disagree about statements which try to fit these observation statements together, particularly when making predictions on continuation of some correlations. From the start, we have to consider many possible unified sciences, even if there were full agreement on observation-statements. This implies that even one individual on an island could create a 'legitimate' multiplicity of theories among which he could not make a choice based on a 'higher' analysis. Should he apply one of these possible hypotheses in his daily life, he would have to make a 'choice' before reaching a 'decision'. Decision and action . . .

I see no way either to ascertain or to reject any hypothesis by means of any scientific means. Of course people may introduce the elimination of multiplicity by means of lots or by declaring that the statements of certain people with a certain prerogative is decisive; but these are not scientific means. Therefore there remain many possible decisions; the Logical Empiricism as presented here is something pluralist in its start. When other people speak of 'facts', the 'truth' of which is 'objective' or 'absolute', Logical Empiricists speak of accepted protocol-statements as the basis of further discussions. It is sufficient that we discover a common basis in protocol-statements; in many fields it is difficult even to have such a basis. Speaking of 'reality' is somewhat dangerous, because it gives the impression as if at least some statement exists which is unpluralistic. And we have no such statement. There is a contradiction even in 'rule of reason' (or arguing), as arguing needs a matter to argue on.

It is a strange phenomenon that more and more people think the 'probability' calculus would help them in making inductions, whereas the only thing we can say is that not all predictions deal with definite data of certain items, but some with groups, and they are statistical predictions. We cannot generally

say that the behaviour of masses is more easily predictable than the behaviour of individuals. We can sometimes predict the behaviour of an ant community, because we have seen many ant communities – but sometimes we can predict the behaviour of single ants better, because we know many more ants than ant communities. On the other hand: if somebody found that the mortality rate of a population has for years been 12 per thousand, then he may perhaps predict (within a network of hypotheses) the continuance of this uniformity; but he would not be able to predict the dying of a certain individual of this population. And again: if we wanted to predict the behaviour of the various nations after this war, or something concerning the persecution of the Jews in Germany, where can we find material to support any hypothesis dealing with this matter? Let us be cautious in stressing the difference between the predictability of groups and individuals.

Social scientists sometimes think of physics and astronomy as of an El Dorado of exactness and definiteness, and they assume, frequently, that in this field any kind of contradictions are fatal to hypotheses. But certain defects, e.g., well-described contradictions, do not always induce scientists to discard a hypothesis. They may maintain that this hypothesis is often useful and that there is no other more attractive hypothesis. Newton's gravitation hypothesis has been used in spite of the fact that for about a hundred and fifty years scholars have felt again and again that there were contradictory and ambiguous elements in it, but it appeared so successful in analyzing the movements of bodies that only a few scholars really criticized the defects of this hypothesis.

Wherever we may start, we have to expect gulfs and gaps, together with unpredictability, incompleteness, and one-sidedness of our arguing. We cannot think of a 'system' as a limit, but have to look at our scientific work from the viewpoint of encyclopedism. We could speak of a 'model-encyclopedia' as one formerly spoke of a 'model-system'.

It seems to me important that 'unpredictability' plays its part within Empiricism. I personally hope that this item will be discussed carefully in the near future. Sometimes the behaviour of human groups may be connected with some changes which appear 'by chance'. Moreover, if a writer makes a prediction, he has also to take into account the making of the prediction as a social item. Prophets have sometimes been called "men who created the world in harmony with their prophecies." Imagine somebody wants to make predictions such as the following: "The statement that the formula x may be deduced in a certain way from the formula y cannot be made sooner than in the next century." It is obvious that one cannot accept this statement,

because through making the prediction this man also makes the very statement of which he intended to say it will not be made sooner than in the next century. These and similar remarks lead to further puzzling results, e.g., that some geological and astronomical connections may also become unpredictable. A decision told to other people may be connected with geological alterations (building of dams, etc.) which may lastly be connected with an alteration of the orbit of the earth. We learn from this that the unpredictability of certain statements may be connected with the unpredictability of geological or even astronomical changes.

It would be worth studying these and similar problems within the framework of Logical Empiricism without any slip into speculative metaphysics. Nevertheless, we have to maintain that these limitations do not touch our language or our scientific procedures. Even where sociologists cannot make predictions, they may provide men of action or meditation with empiricist material. We argue differently and act differently, when we know the material provided by the social sciences. One sees immediately how important it is to have proper descriptive statements. And even our restricted scientific work may be dovetailed into our social and private life in various ways. The pluralist attitude of arguing enables people who like traditions to bring forward the lack of predictability: who knows what happens when we abandon this or that – and people who want to alter social and private life to declare: nobody is able to predict that the happier future we pursue will remain unreachable.

The difficulty is that, whatever changes we accept, it is a kind of venture ...

NOTE

[1] [This article was compiled by Marie L. Neurath from notes found on Otto Neurath's desk after his death, and from quotations from his 'Foundations of the Social Sciences' (International Encyclopedia of Unified Science, Vol. II, No. 1, The University of Chicago Press, 1944).]

CHAPTER 24

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BIBLIOGRAPHY C: NEURATH IN ENGLISH

The following list of Otto Neurath's writings in English – those written in English as well as published translations – is arranged chronologically according to the publication date of the original work. The titles and dates of the original works are given below; full bibliographical information for these works is to be found in the numbered list of Neurath's writings that forms Chapter 12 of *Empiricism and Sociology* and its supplement in this volume, *Philosophical Papers*, pp. 255–258. The numbers, *viz.* [ON 1], refer to those lists.

There are three major collections of Neurath's work in English: *Empiricism and Sociology*, referred to below as *Emp. Soc.*; *Philosophical Papers* (*Phil. Pap.*); and *Economic Writings* (*Econ. Writ.*). Complete bibliographical information for these three volumes will be found at the end of this list.

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