STRATA OF EXPERIENCE[1]

Discussions concerning the relation between science and philosophy are still likely to be carried on in terms of the controversy as to whether all knowledge springs from (or has its source in) experience. Yet it is becoming more and more apparent that the solution of this controversy will depend not on any fact finding but on a thorough analysis of the meaning of the terms "knowledge" and "experience." Furthermore, it is generally admitted that the definition of these terms should not be arbitrary but in line with their usage among scientists.

When we realize that the question of the *source* of knowledge aims at a proper determination of the *meaning* of "knowledge" it turns out that philosophical or *a priori* knowledge as contradistinguished from scientific or *a posteriori* knowledge is concerned with the presuppositions involved in the meaning of objective knowledge.

The first philosopher to make this point clear was Kant. He called his analysis of the meaning of scientific experience "transcendental method." In Husserl's transcendental phenomenology the problem of the analysis of experience is attacked even more radically than it was in the *Critique of Pure Reason.* One of Husserl's fundamental points is that logical analysis in the strict sense is only one level, the uppermost level, in the analysis of meanings. It is this point which I wish to clarify in this paper by starting with a logical or methodological analysis of the meaning of scientific experience. I shall confine myself to natural science but this is only for the sake of simplicity. The problems treated here are invariant to the variations of subject matters in science.

The classical idea of science as conceived by Aristotle is that of a deductive system. A science consists of propositions which can be divided into two disjunctive classes, axioms and theorems. The axioms are regarded as self-evident. They need not and cannot be proved. The truth of the theorems on the other hand is proved on the basis of the axioms. To prove a theorem or to deduce it from the axioms means to show by application of well-defined operational rules that this proposition asserts nothing else than is asserted by the axioms. Geometry as presented in Euclid's *Elements* is—if certain minor deficiencies are removed—the prototype of science in this sense.

There were two chief objections raised against this conception of science. The first one is directed against the claim of self-evident

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truth for the axioms. When non-Euclidean geometries had been constructed and their applicability to physics demonstrated in Einstein's theory of relativity this objection could no longer be refuted by any convincing argument. Even before this time, however, science had been conceived as a *hypothetico*-deductive system. The axioms were no longer regarded as eternal truths but rather as hypotheses which had to be replaced by others if they did not stand control.

But even the conception of science as a hypothetico-deductive system is open to a more radical objection, namely, that one misinterprets science by thinking of it as a system. Such a view, it is argued, arises from Eleatic metaphysics, accepted by Plato and Aristotle, which holds that true reality is changeless and can therefore be aptly described by a closed system established for eternity. Ours, however, is a world of continuous change, science describing it is a continuous self-correcting process. This is the view taken by the pragmatists. In John Dewey's *Logic* it has found its most elaborate expression.

Yet this argument is apt to cause some bewilderment. Granting that Aristotle's idea of science does not account for any change in the body of knowledge other than the drawing of new conclusions from a set of given premises one wonders how the concept of change can be applied to propositions which must be clearly distinguished from the acts of thinking them.

It seems to me that we can get rid of this apparent difficulty by applying certain ideas of modern postulational theory, with proper modifications, to the analysis of the meaning of empirical science. As a matter of fact the interpretation of the meaning of empirical sciences which I am going to offer did originate from an analogy with the so-called metalogic dealing with the analysis of logical calculi, but it can be understood without any reference to metalogic. Here I shall have to restrict myself to the outline of some basic ideas but the problem will be dealt with in more detail in a forthcoming book.

If we reflect upon what we mean by saying that a certain scientific hypothesis has been well established or that a hypothesis had to be dismissed because it did not stand the observational test, it becomes clear that implicit reference is made to rules of scientific procedure. At first one may be inclined to conceive of all these rules of procedure as technical devices for the craft of scientists, indicating what means they should apply towards the attainment of a goal given irrespective of these rules. This goal is held to be the finding of true propositions in terms of which the sphere of reality with which a particular science deals can be described. This view presupposes that "truth" can be defined without any reference to procedures of acquisition of knowledge.

But it can no longer be upheld when reference to a world of things in themselves, transcending all possible human experience, is excluded. The definition of "truth" as *adaequatio rei atque intellectus* is then found inadequate; instead "truth" is defined in terms of agreement or coherence between possible human experiences.

It is misleading, however, to speak in this connection of a coherence-*theory* of truth as opposed to a correspondence-*theory* of truth. A theory is a set of general assumptions which may be confirmed or refuted by experience. Here, however, one is concerned not with two conflicting theories but with the analysis of the meaning of "truth." For the same reason it is misleading to say that truth *depends* on knowledge. This would mean that knowledge of propositions is one of their truth-conditions, which is obviously not the case. The same applies to the concept of reality. It is *defined* in terms of possible human experience but this does not imply that reality *depends* on experience. Later on we shall discuss the fallacy involved here in more detail.

The statement that "truth" is defined in terms of agreement between experiences is rather vague, however, as long as it is not determined what kind of agreement is aimed at. To do this one must refer to the rules of procedure. Only by reference to these rules does it become clear, for instance, what it means to say that a certain expectation has been fulfilled.

Bearing this in mind one realizes that the aim of science to find true propositions must be defined in terms of the rules of scientific procedure. An analogy will facilitate understanding and lead us to the next step in our reasoning.

In a chess game it is the goal of each of the players to checkmate his opponent. This goal is defined in terms of the basic rules of the game including the different types of permitted moves on the chessboard characteristic of the different types of chessmen, the number of chessmen of each type, and their initial position on the chessboard. Given these basic rules of the game and the goal defined in terms of them, other rules which we may call "preference rules" or "technical rules" can be introduced by which for given situations moves conducive to the attainment of the goal are selected. Here we shall not be concerned with the analysis of preference rules in science. What matters for our analogy is that "chess game" and the goal of "checkmating the opponent" are defined in terms of rules of the game.

Similarly the concept of a particular science S, e.g., physics, may be defined in terms of certain basic rules of method,[2] stating under

2. The term "rules of method" is meant to include two disjunctive classes of rules, viz., "rules of language of a science" and "rules of procedure of a science."

what conditions propositions may belong to S. Such a definition is not circular as it may seem to be at first sight; we simply define the term "S" by defining the term "proposition of S." The mathematician who defines a set by establishing under what conditions objects are called elements of this set is familiar with this type of definition.

Once it is realized that this definition of "science" agrees with the actual usage of the term, it turns out that "truth" is not a constitutive but a regulative principle of science, an ideal in the sense in which Kant uses this term. One aims at propositions which once accepted will stand any control to which they may be subjected. But since the process of control is—in principle—never-ending, we can never declare that we have definitely attained truth.

To get a clearer idea of the rules of procedure we have to consider that the set of propositions belonging to a particular science at a given time cannot be altered arbitrarily; *reasons* must be given for any change in the corpus of a science. Further, considering that such a change may consist either in the acceptance of a new proposition or in the elimination of a proposition previously accepted and that the reasons for the change are determined by the rules of procedure, we may say that the rules of procedure establish the conditions under which the acceptance or elimination of propositions is *permitted*. If a change in the corpus of science is permitted it may be *required* as well, but it need not be. This can easily be seen. The acceptance of a proposition which reports the result of observations performed in order to check a given hypothesis must not be ignored, and as a consequence the elimination of a hypothesis may be required. We point to these rules of procedure if we say that a hypothesis must be in conformity with the facts. The rules of inductive inference on the other hand never *oblige* us to accept a universal proposition; they only permit its acceptance under certain conditions. Let us call the acceptance of a given proposition or the elimination of a given proposition *one step* in a scientific procedure. Then we may say that the rules of procedure determine whether or not a given step is correct.

By examining the conditions for the acceptance or the elimination of propositions as referred to in the rules of procedure one finds that they can be divided into two disjunctive classes, namely those which *include* the observational test and those which do not. The latter are usually called "logical rules." A closer analysis of the two types of

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The former rules deal exclusively with the meaning of propositions irrespective of their empirical validity and delimit the subject-matter of a particular science; its meaning is presupposed as "given" in the rules of the second class by which acceptable propositions belonging to the subject-matter of the science under consideration are selected. Here we are concerned with rules of procedure. Something more about the rules of language will be said later.

rules reveals the paramount importance of their interrelations for scientific procedure. But it would lead us too far afield to discuss this point.

It becomes clear from the above analysis that we must distinguish between two meanings of the term "science." On the one hand science in general or any specific science is defined as a process going on indefinitely in accordance with a set of given rules. If on the other hand, we speak of a science as consisting of propositions we point to the result of this process at a given time, we give—metaphorically speaking—a cross-section of the process. To avoid the ambiguity involved we may say that the rules of method define the *structure* of a science. To this structure corresponds a certain *corpus of knowledge* varying in time. Aristotle's conception of science as a deductive system may then be reinterpreted as implying the ideal of a corpus of knowledge invariant in time. It should be noted, however, that this ideal applies only to universal propositions called "laws," but not to statements about facts in the strict sense. During the last decades the corpus of such knowledge pertaining to certain disciplines of physics has been arranged in deductive systems but there is always the possibility that these systems will not stand future controls.

The relation between the structure of a science and the corpus of knowledge belonging to this science at a given time is isomorphic with relations significant in other fields. Just think of the problem of how a society or state may still remain the same when a part or even all of the members have changed. Another instance is the concept of legal order. Its structure is defined in terms of the processes by which valid legal norms are established. To this process there corresponds a definite body of norms comprising a particular legal order at a given time. Here the analogy with science goes still further than in our previous example, for there is a strong formal resemblance in the conditions for the acceptance or elimination of elements insofar as they depend on the corpus of knowledge or on legal norms established at the time of the decision.

Still more significant for us in this analogy is that it suggests the question of whether all the rules of scientific procedure are invariable in time. Obviously the rules defining the procedures of establishing positive legal norms are not. Congress may extend or restrict its own legislative authority and the authority of other bodies, e.g., that of government departments to issue legal decrees. But this procedure of creating or abolishing laws is itself regulated and points back to invariable rules in terms of which the *unity* of the legal order is defined. It turns out that the same is true of the structure of scientific procedure. Allowance is made for (slight) changes in the rules of pro-

cedure; but there are invariable rules in terms of which the *unity* of procedure is defined. Otherwise one could no longer speak of the *same* science when rules of procedure change.

If we thus define "science" in terms of a complexly interrelated system of rules, the question is bound to arise: How are the rules themselves given? It seems that the answer to this question is of even higher importance for the understanding of the meaning of "science" if rules of procedure are no longer supposed to be technical devices for the attainment of a goal given irrespective of these rules.

Two answers have been offered to this question which at first sight seem to be mutually exclusive. The first declares that the rules are *a priori* given and discovered by reason, the second that they are conventions. It seems to me, however, that the apparent incompatibility between the two views is largely due to the ambiguous use of the two terms "a priori" and "convention." The rules of scientific procedure may be called *a priori* in contrast to the propositions of science if this is not taken to mean anything else than (a) that they are not subjected to any observational test (which is itself one of the rules of scientific procedure) and (b) that they are *presupposed* in science, the concept of science being defined in terms of them. If, however, the connotation of absolute truth is connected with the term *a priori* and if furthermore, truth is held to be a property attributed exclusively to statements about reality then the rules of procedure cannot be called *a priori,* since they are not statements about reality, but *definitions.* It may then seem appropriate to call the rules of procedure "conventions." As a matter of fact one says that a definition is a convention concerning the use of a term. If one does call them "conventions," however, one will have to distinguish much more carefully than has been done so far between these conventions, in terms of which "science" is defined and the conventions *in* science, e.g., the choice of cm, gram and second as measuring units in physics. Conventions of the latter type are regarded as non-essential for scientific procedure. They may be replaced by others for purely technical reasons, whereas the former define this very procedure. If these rules are changed in a way which has not been provided for in their hierarchical order, we cannot say that the meaning of "science," defined in terms of the *new* system of rules is the same as that defined in terms of the old system.

We have already mentioned that the rules of procedure in each field of inquiry should be so defined as to be in agreement with the criteria for the correctness of assumptions actually applied by scientists in this field. There is no logical circle involved in this postulate although at first sight there may seem to be. Only a very broad characterization of scientific inquiry is needed for the definition of the

term "scientist in a certain field." A strict definition of the science in question is given in terms of rules of procedure agreed upon by the scientists. There is of course no reference to the fact of this agreement between scientists in the formulation of the rules.

Summing up this point we may say: That the rules of procedure of a certain science are "given" does not mean anything else for the methodologist than that the correctness of procedural decisions in this science is defined in terms of these rules. We should not simply say that they are given, but that they are given *for* this procedure. The term "given" has here a meaning similar to the one it has for the mathematician. If he says: "Given the side of a square, the length of the diagonals is to be determined," he means that the length of the diagonals should be expressed in terms of—or, as one usually says, as a function of—the length of the sides. In a similar sense a certain law is said to be given to a judge who has to decide a particular case, if his decision is to be based upon this law, i.e., if its correctness is defined in terms of agreement with this law.

Psychological, sociological, or historical aspects of givenness are by no means excluded, but no methodological conclusions can be derived from them. Yet as far as psychology is concerned the following objection to this statement may be expected: To incorporate a proposition into science—so it may be argued—implies the belief that it is true; therefore a psychological explanation of beliefs will be the proper foundation for an analysis of scientific procedure. But this objection misses the point. It has been stressed time and again in discussions of psychological interpretations of logic—most convincingly in the first volume of Husserl's *Logische Untersuchungen*—that logic is not concerned with the explanation of the process of thinking or believing but rather with the criteria of correct thinking, of warranted belief. All those who declared that logic is normative, that it does not say how man actually thinks but how he ought to think, stressed this point.

It may seem that the psychologistic fallacy has become rather obsolete and no longer causes any trouble in methodological analyses. Unfortunately it still does. I shall give you an example. It is still widely held that the concept of probability should be defined in terms of "partial ignorance" and that degrees of probabilities correspond to degrees of intensity of beliefs culminating in the feeling of absolute certainty. But partial ignorance and degrees of intensity of beliefs are methodologically irrelevant. Each step in scientific procedure concerns the acceptance or elimination of a given proposition and one cannot accept or eliminate a proposition partly or in different degrees. Probability statements differ in their contents from other statements

in being concerned with relative frequencies in large series of events of a particular class. No problems of modality are involved here. This is recognized in principle by the majority of leading theorists in this field but even they are more or less inclined to declare that all our knowledge about reality is "only probable" and thus to link the concept of probability with the idea of fallibility.

This, however, is not in order. The mathematician may err as well as the empirical scientist. If nevertheless knowledge in pure mathematics is called certain, as opposed to the merely probable knowledge in empirical sciences, the difference between "certain" and "probable" as the terms are used here must be sought elsewhere. What is really meant is that a once correctly proved formula in mathematics has been proved forever, whereas an empirical proposition which had been correctly accepted in a given situation of inquiry may have to be eliminated later. If one gives the term "error" the strict meaning of violation of given rules applicable in a particular situation of scientific inquiry it becomes clear that we cannot speak of an error committed in such a case.

To summarize: In pure mathematics as well as in empirical science, correct steps in the procedure are defined relative to a certain procedural situation. The difference, however, is that in contrast to mathematics correct steps in an empirical science may have to be reversed. One points to this difference by opposing the certainty of mathematical knowledge to the mere probability of empirical knowledge. This terminology, however—though sanctioned by philosophical tradition—is not advisable because it is not in agreement with the usage of the term "probability" in modern science. I have dealt with that example at some length since it shows well the significance of a logical analysis of the meaning of "science" for the discussion of actual methodological problems.

Up to this point the analysis has been strictly methodological. But this is only half the story for the philosopher. If on the one hand the meaning of "scientific experience" or "knowledge" is defined in terms of procedural rules these very rules presuppose on the other hand "experiences" in terms of which they are formulated. To make this point clear it will be in order to say first a few words about objective meanings in general and to point then to a widespread fallacy connected with the analysis of meanings.

Rules of procedure are concerned with propositions and accordingly meanings of propositions (rules of language) are presupposed as given in the procedural rules. The meaning of a proposition is determined by the meaning of the terms of which it consists and the way in which these terms are connected. All meanings of terms can

be traced back to a set of fundamental meanings. As far as the physical world is concerned these are the meanings of space and time on the one hand and systems of so-called sense-qualities on the other, and furthermore some formal concepts such as "not," "and," "all," which function as a sort of link between the above meanings. Here of course fundamental logical problems are involved but we cannot consider them now. In a proposition space-time points and qualities are correlated. A simple example is: "There is blue at place p at time t."

But what about relations, it may be asked. The answer is that the meanings of the so-called *external* relations can be traced back to the just-mentioned fundamental meanings. On the other hand analysis of the fundamental meanings reveals *internal* relations. By speaking of systems of sense-qualities we have already referred to them. One points to internal relations by stating that it is impossible for two qualities belonging to the same system, such as blue and yellow, to be at the same place at the same time.

It would be a great mistake, however, to infer from what we said about external relations that there is a primacy of qualities over external relations in scientific procedure. As a matter of fact just the opposite is the case. Unfortunately it must be said that neither in the Aristotelean system nor in the attacks upon it have these two points been distinguished clearly enough. We have here before us a particular instance of a fallacy which has been of such significance in the history of philosophy that we do not pay it excessive tribute in calling it the philosopher's fallacy.

It is the confounding of problems concerning statements about reality with problems related to the analysis of meanings. To recognize that the physical laws are about invariant relations rather than about invariant qualities is one thing and to recognize that the meanings of relations can be traced back to the fundamental meanings of space, time, and sense-qualities is something else. There is no incompatability between these two statements. But aprioristic philosophers are inclined to attempt the explanation of external relations between facts in terms of internal relations between meanings, whereas radical empiricists like J. S. Mill go the opposite way. The two ways are equally wrong. By distinguishing between matters of fact and relations between ideas Locke and Hume showed a keen eye for the discovery of this fallacy but they did not show equal keenness for the range of the problems related to the analysis of meanings. Here they were hindered by sensualistic prejudices.

That the sensualistic interpretation of the meaning of the so-called sense qualities is inadequate can easily be seen if we consider that it does not enable us to understand the internal relations between

these meanings. It is inconceivable according to this interpretation why blue and yellow at the same place and time should be impossible. I would only mention in this context that Husserl's analysis of acts, centered around the concept of intentionality, entails a devastating criticism of sensationalism.

Nevertheless there is certainly something right in the sensualistic view that the meaning of sense qualities has its source in experience and this leads us to the heart of our argument. If asked, "What do you mean by 'blue'?" one will answer, "This can't be defined but only shown, look there, something like that." Thereby it is presupposed that the meaning of "blue" refers back in a certain way to the contents of acts of perception though one should not say that the meaning is *contained* in them. We are then confronted with the following situation: The meaning of "objective experience" is defined in terms of certain rules of method including presuppositions concerning given fundamental meanings. These meanings in turn point back to experiences as constitutive elements. *Obviously the meaning of "experience" cannot be the same in both cases.*

To tell them apart it may be well to distinguish between different "strata of experience." It seems to me that lack of this distinction combined with the philosopher's fallacy has been largely responsible for controversies between philosophical doctrines. One example is the controversy between realists and nominalists about whether universals are *ante rem* or *post rem.*

If we extrapolate the philosopher's fallacy inherent in this controversy the problem may be formulated as follows: Is the meaning of universals a constitutive element in the meaning of the experience of things or is the latter meaning a constitutive element in the former? Obviously meanings of universals are constitutive elements in the meaning of any thing-concept and in this sense they are *ante rem.* There is, however, on the other hand a certain meaning of "concrete" experience which is presupposed in the meanings of universals.

The distinction between different strata of experience cannot, of course, be regarded as a solution of the problems involved, but rather as a small step in the approach to their formulation. These problems are, as I see it, the very problems of philosophy. If it is the goal of philosophy to make our thoughts clear and distinct, then the analysis of meanings is the chief task of the philosopher. As far as this analysis remains *within* the stratum of objective meanings it is logical analysis; as far as it transcends this stratum it may be called transcendental analysis.

The problems of the constitution of meanings are the central problems of Husserl's transcendental phenomenology. This becomes

more and more obvious in his later works, the *Méditations Cartésiennes,* the *Formale und transcendentale Logik,* and the posthumously published book, *Erfahrung und Urteil.*

There one may find problems like the following: One conceives of the physical world as a world for everybody, that is, for any man. Thus the meaning of "man" is presupposed in the meaning of "physical world." On the other hand, however, we think of man as a being living in a spatio-temporal world. It follows that the two meanings of "man," the one which is presupposed in the meaning of "physical world," and the other which presupposes the meaning of "physical world" cannot be identical. Thus the problem arises of separating the different constitutive levels of the meaning of "man." It appears to be a tremendous problem, requiring for its solution in the first place a much more radical analysis of "time" than had been hitherto undertaken. Husserl had already realized this at a relatively early stage of his philosophical development. Unfortunately only a small part of his work in this field has been published so far. It shows merely the promising start, not the major part of the way nor the results which he eventually obtained.

When we seek to lay bare in the proper order the levels of the constitution of an objective meaning so that the hierarchy of the presuppositions may come to light, the problem arises of revealing what is presupposed in all presupposing, what is constitutive for any meaning whatsoever. Husserl called this fundamental presupposition "transcendental ego." In his *Méditations Cartésiennes* he traced this conception back to Descartes' "ego cogito"; as a matter of fact it even can be traced back to Plato and the Neo-Platonists.

With all these thinkers, however, the analysis has been vitiated by the philosopher's fallacy. Husserl and others have shown that even Descartes has not escaped it though his program of universal doubt if consistently followed would have excluded it. Transcendental phenomenology starts with a radicalized Cartesian doubt. The phenomenological epoché does not only preclude all references to assumptions about matters of fact but even all references to already constituted meanings—including the concept of man as an inhabitant of a spatio-temporal world. A step by step reconstitution of meanings in their proper order is posed as the task for the phenomenologist.

This should be borne in mind by all critics of phenomenology. It will be well for philosophers opposed to phenomenology to try to show that Husserl did not succeed in solving this problem. It will not even be amiss for them to try to show that the problem has not been properly posed by him. But two misinterpretations should be avoided, namely (a) that phenomenology is a revival of conceptual realism

and (b) that stripped of all its metaphysical connotations it is nothing more than descriptive psychology. Unfortunately it is chiefly these two ever-recurring misunderstandings which are responsible for the fact that discussion of Husserl's phenomenology has contributed but little to a clarification of the issue involved.

I would suggest that future discussions—particularly discussions in the near future—should deal first of all with the fundamental issue of different levels of meanings or strata of experience. Once it is established that we do have to distinguish between different levels in the constitution of meanings the problem of a philosophical foundation for positive science can be clearly formulated and the stage is set for an unprejudiced analysis of Husserl's scheme of constitution.

Such a discussion should not be sidetracked—as it has so long been—by the claim made by one side that philosophy in contrast to science can yield absolute truth, and its rejection by the other. Once it is realized that philosophy does not compete with science in making statements about reality this controversy loses significance.

It may be asked, of course, whether the results of philosophical reflection should be called "absolute truths." There are historical reasons for doing so and methodological reasons against doing it. But the essential problem implicit in the controversy about philosophical truth is whether there are genuine philosophical methods and results at all.

It seems to me that the answer to this question will depend chiefly upon whether or not the problems connected with the concepts "strata of experience" or "levels in the constitution of meanings" are found to be pseudo-problems. If it should turn out that they are, we might do well to reconsider the scope of our Phenomenological Society. But I am quite confident that it will not turn out so.

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