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Genetic epistemology attempts to explain knowledge, and in particular scientific knowledge, on the basis of its history, its sociogenesis, and especially the psychological origins of the notions and operations upon which it is based. These notions and operations are drawn in large part from common sense, so that their origins can shed light on their significance as knowledge of a somewhat higher level. But genetic epistemology also takes into account, wherever possible, formalization—in particular, logical formalizations applied to equilibrated thought structures and in certain cases to transformations from one level to another in the development of thought.

The description that I have given of the nature of genetic epistemology runs into a major problem, namely, the traditional philosophical view of epistemology. For many philosophers and epistemologists, epistemology is the study of knowledge as it exists at the present moment; it is the analysis of knowledge for its own sake and within its own framework without regard for its development. For these persons, tracing the development of ideas or the development of operations may be of interest to historians or to psychologists but is of no direct concern to epistemologists. This is the major objection to the discipline of genetic epistemology, which I have outlined here.

But it seems to me that we can make the following reply to this objection. Scientific knowledge is in perpetual evolution; it finds itself changed from one day to the next. As a result, we cannot say that on the one hand there is the history of knowledge, and on the other its current state today, as if its current state were somehow definitive or even stable. The current state of knowledge is a moment in history, changing just as rapidly as the state of knowledge in the past has ever changed and, in many instances, more rapidly. Scientific thought, then, is not momentary; it is not a static instance; it is a process. More specifically, it is a process of continual construction and reorganization. This is true in almost every branch of scientific investigation. I should like to cite just one or two examples.

The first example, which is almost taken for granted, concerns the area of contemporary physics or, more specifically, microphysics, where the state of knowledge changes from month to month and certainly alters significantly within the course of a year. These changes often take place even within the work of a single author who transforms his view of his subject matter during the course of his career.

Let us take as a specific instance Louis de Broglie in Paris. A few years ago de Broglie adhered to Niels Bohr's view of indeterminism. He believed with the Copenhagen school that, behind the indeterminism of microphysical events, one could find no determinism, that indeterminism was a very deep reality and that one could even demonstrate the reasons for the necessity of this indeterminism. Well, as it happens, new facts caused de Broglie to change his mind, so that now he maintains the very opposite point of view. So here is one example of transformation in scientific thinking, not over several successive generations but within the career of one creative man of science.

Let us take another example from the area of mathematics. A few years ago the Bourbaki group of mathematicians attempted to isolate the fundamental structures of all mathematics. They established three mother structures: an algebraic structure, a structure of ordering, and a topological structure, on which the structuralist school of mathematics came to be based, and which was seen as the foundation of all mathematical structures, from which all others were derived. This effort of theirs, which was so fruitful, has now been undermined to some extent or at least changed since McLaine and Eilenberg developed the notion of categories, that is, sets of elements taken together, with the set of all functions defined on them. As a result, today part of the Bourbaki group is no longer orthodox but is taking into account the more recent notion of categories. So here is another, rather fundamental area of scientific thinking that changed very rapidly.

Let me repeat once again that we cannot say that on the one hand there is the history of scientific thinking, and on the other the body of scientific thought as it is today; there is simply a continual transformation, a continual reorganization. And this fact seems to me to imply that historical and psychological factors in these changes are of interest in our attempt to understand the nature of scientific knowledge.*

I should like to give one or two examples of areas in which the genesis of contemporary scientific ideas can be understood better in the light of psychological or sociological factors. The first one is Cantor's development of set theory. Cantor developed this theory on the basis of a very fundamental operation, that of one-to-one correspondence. More specifically, by establishing a one-to-one correspondence between the series of whole numbers and the series of even numbers, we obtain a number that is neither a whole number nor an even number but is the first transfinite cardinal number, *aleph zero*. This very elementary operation of one-to-one correspondence, then, enabled Cantor to go beyond the finite number series, which was the only one in use up until his time. Now it is interesting to ask where this opera-

* Another opinion, often quoted in philosophical circles, is that the theory of knowledge studies essentially the question of the validity of science, the criteria of this validity and its justification. If we accept this viewpoint, it is then argued that the study of science *as it is*, as a fact, is fundamentally irrelevant. Genetic epistemology, as we see it, reflects most decidedly this separation of norm and fact, of valuation and description, We believe that, to the contrary, only in the real development of the sciences can we discover the implicit values and norms that guide, inspire, and regulate them. Any other attitude, it seems to us, reduces to the rather arbitrary imposition on knowledge of the personal views of an isolated observer. This we want to avoid.

tion of one-to-one correspondence came from. Cantor did not invent it, in the sense that one invents a radically new construction. He found it in his own thinking; it had already been a part of his mental equipment long before he even turned to mathematics, because the most elementary sort of sociological or psychological observation reveals that one-to-one correspondence is a primitive operation. In all sorts of early societies it is the basis for economic exchange, and in small children we find its roots even before the level of concrete operations. The next question that arises is, what is the nature of this very elementary operation of one-to-one correspondence? And right away we are led to a related question: what is the relationship of one-to-one correspondence to the development of the notion of natural numbers? Does the very widespread presence of the operation of one-to-one correspondence justify the thesis of Russell and Whitehead that number is the class of equivalent classes (equivalent in the sense of one-to-one correspondence among the members of the classes)? Or are the actual numbers based on some other operations in addition to one-to-one correspondence? This is a question that we shall examine in more detail later. It is one very striking instance in which a knowledge of the psychological foundations of a notion has implications for the epistemological understanding of this notion. In studying the development of the notion of number in children we can see whether or not it is based simply on the notion of classes of equivalent classes or whether some other operation is also involved.

I should like to go on now to a second example and to raise the following question: how is it that Einstein was able to give a new operational definition of simultaneity at a distance? How was he able to criticize the Newtonian notion of universal time without giving rise to a deep crisis within physics? Of course his critique had its roots in experimental findings, such as the Michaelson-Morley experiment—that goes without saying. Nonetheless, if this redefinition of the possibility of events to be simultaneous at great distances from each other went against the grain of our logic, there would have been a considerable crisis within physics. We would have had to accept one of two possibilities: either the physical world is not rational, or else human reason is impotent—incapable of grasping external reality. Well, in fact nothing of this sort happened. There was no such upheaval. A few metaphysicians (I apologize to the philosophers present) such as Bergson or Maritain were appalled by this revolution in physics, but for the most part and among scientists themselves it was not a very drastic crisis. Why in fact was it not a crisis? It was not a crisis because simultaneity is not a primitive notion. It is not a primitive concept, and it is not even a primitive perception. I shall go into this subject further later on, but at the moment I should just like to state that our experimental findings have shown that human beings do not perceive simultaneity with any precision. If we look at two objects moving at different speeds, and they stop at the same time, we do not have an adequate perception that they stopped at the same time. Similarly, when children do not have a very exact idea of what simultaneity is, they do not conceive of it independently of the speed at

which objects are traveling. Simultaneity, then, is not a primitive intuition; it is an intellectual construction.

Long before Einstein, Henri Poincaré did a great deal of work in analyzing the notion of simultaneity and revealing its complexities. His studies took him, in fact, almost to the threshold of discovering relativity. Now if we read his essays on this subject, which, by the way, are all the more interesting when considered in the light of Einstein's later work, we see that his reflections were based almost entirely on psychological arguments. Later on I shall show that the notion of time and the notion of simultaneity are based on the notion of speed, which is a more primitive intuition. So there are all sorts of reasons, psychological reasons, that can explain why the crisis brought about by relativity theory was not a fatal one for physics. Rather, it was readjusting, and one can find the psychological routes for this readjustment as well as the experimental and logical basis. In point of fact, Einstein himself recognized the relevance of psychological factors, and when I had the good chance to meet him for the first time in 1928, he suggested to me that is would be of interest to study the origins in children of notions of time and in particular of notions of simultaneity.

What I have said so far may suggest that it can be helpful to make use of psychological data when we are considering the nature of knowledge. I should like now to say that it is more than helpful; it is indispensable. In fact, all epistemologists refer to psychological factors in their analyses, but for the most part their references to psychology are speculative and are not based on psychological research. I am convinced that all epistemology brings up factual problems as well as formal ones, and once factual problems are encountered, psychological findings become relevant and should be taken into account. The unfortunate thing for psychology is that everybody thinks of himself as a psychologist. This is not true for the field of physics, or for the field of philosophy, but it is unfortunately true for psychology. Every man considers himself a psychologist. As a result, when an epistemologist needs to call on some psychological aspect, he does not refer to psychological research and he does not consult psychologists; he depends on his own reflections. He puts together certain ideas and relationships within his own thinking, in his personal attempt to resolve the psychological problem that has arisen. I should like to cite some instances in epistemology where psychological findings can be pertinent, even though they may seem at first sight far removed from the problem.

My first example concerns the school of logical positivism. Logical positivists have never taken psychology into account in their epistemology, but they affirm that logical beings and mathematical beings are nothing but linguistic structures. That is, when we are doing logic or mathematics, we are simply using general syntax, general semantics, or general pragmatics in the sense of Morris, being in this case a rule of the uses of language in general. The position in general is that logical and mathematical reality is derived from language. Logic and mathematics are nothing but specialized linguistic structures. Now here it becomes pertinent to examine factual findings. We can look to see whether there

is any logical behavior in children before language develops. We can look to see whether the coordinations of their actions reveal a logic of classes, reveal an ordered system, reveal correspondence structures. If indeed we find logical structures in the coordinations of actions in small children even before the development of language, we are not in a position to say that these logical structures are derived from language. This is a question of fact and should be approached not by speculation but by an experimental methodology with its objective findings.

The first principle of genetic epistemology, then, is this—to take psychology seriously. Taking psychology seriously means that, when a question of psychological fact arises, psychological research should be consulted instead of trying to invent a solution through private speculation.

It is worthwhile pointing out, by the way, that in the field of linguistics itself, since the golden days of logical positivism, the theoretical position has been reversed. Bloomfield in his time adhered completely to the view of the logical positivists, to this linguistic view of logic. But currently, as you know, Chomsky maintains the opposite position. Chomsky asserts, not that logic is based on and derived from language, but, on the contrary, that language is based on logic, on reason, and he even considers this reason to be innate. He is perhaps going too far in maintaining that it is innate; this is once again a question to be decided by referring to facts, to research. It is another problem for the field of psychology to determine. Between the rationalism that Chomsky is defending nowadays (according to which language is based on reason, which is thought to be innate in man) and the linguistic view of the positivists (according to which logic is simply a linguistic convention), there is a whole selection of possible solutions, and the choice among these solutions must be made on the basis of fact, that is, on the basis of psychological research. The problems cannot be resolved by speculation.

I do not want to give the impression that genetic epistemology is based exclusively on psychology. On the contrary, logical formalization is absolutely essential every time that we can carry out some formalization; every time that we come upon some completed structure in the course of the development of thought, we make an effort, with the collaboration of logicians or of specialists within the field that we are considering, to formalize this structure. Our hypothesis is that there will be a correspondence between the psychological formation on the one hand, and the formalization on the other hand. But although we recognize the importance of formalization in epistemology, we also realize that formalization cannot be sufficient by itself. We have been attempting to point out areas in which psychological experimentation is indispensable to shed light on certain epistemological problems, but even on its own grounds there are a number of reasons why formalization can never be sufficient by itself. I should like to discuss three of these reasons.

The first reason is that there are many different logics, and not just a single logic. This means that no single logic is strong enough to support the total construction of human

knowledge. But it also means that, when all the different logics are taken together, they are not sufficiently coherent with one another to serve as the foundation for human knowledge. Any one logic, then, is too weak, but all the logics taken together are too rich to enable logic to form a single value basis for knowledge. That is the first reason why formalization alone is not sufficient.

The second reason is found in Gödel's theorem. It is the fact that there are limits to formalization. Any consistent system sufficiently rich to contain elementary arithmetic cannot prove its own consistency. So the following questions arise: logic is a formalization, an axiomatization of something, but of what exactly? What does logic formalize? This is a considerable problem. There are even two problems here. Any axiomatic system contains the undemonstrable propositions or the axioms, at the outset, from which the other propositions can be demonstrated, and also the undefinable, fundamental notions on the basis of which the other notions can be defined. Now in the case of logic what lies underneath the undemonstrable axioms and the undefinable notions? This is the problem of structuralism in logic, and it is a problem that shows the inadequacy of formalization as the fundamental basis. It shows the necessity for considering thought itself as well as considering axiomatized logical systems, since it is from human thought that the logical systems develop and remain still intuitive.

The third reason why formalization is not enough is that epistemology sets out to explain knowledge as it actually is within the areas of science, and this knowledge is, in fact not purely formal: there are other aspects to it. In this context I should like to quote a logician friend of mine, the late Evert W. Beth. For a very long time he was a strong adversary of psychology in general and the introduction of psychological observations into the field of epistemology, and by that token an adversary of my own work, since my work was based on psychology. Nonetheless, in the interests of an intellectual confrontation, Beth did us the honor of coming to one of our symposia on genetic epistemology and looking more closely at the questions that were concerning us. At the end of the symposium he agreed to co-author with me, in spite of his fear of psychologists, a work that we called *Mathematical and Psychological Epistemology*. This has appeared in French and is being translated into English. In his conclusion to this volume, Beth wrote as follows: "The problem of epistemology is to explain how real human thought is capable of producing scientific knowledge. In order to do that we must establish a certain coordination between logic and psychology." This declaration does not suggest that psychology ought to interfere directly in logic—that is of course not true—but it does maintain that in epistemology both logic and psychology should be taken into account, since it is important to deal with both the formal aspects and the empirical aspects of human knowledge.

So, in sum, genetic epistemology deals with both the formation and the meaning of knowledge. We can formulate our problem in the following terms: by what means does the human mind go from a state of less sufficient knowledge

to a state of higher knowledge? The decision of what is lower or less adequate knowledge, and what is higher knowledge, has of course formal and normative aspects. It is not up to psychologists to determine whether or not a certain state of knowledge is superior to another state. That decision is one for logicians or for specialists within a given realm of science. For instance, in the area of physics, it is up to physicists to decide whether or not a given theory shows some progress over another theory. Our problem, from the point of view of psychology and from the point of view of genetic epistemology, is to explain how the transition is made from a lower level of knowledge to a level that is judged to be higher. The nature of these transitions is a factual question. The transitions are historical or psychological or sometimes even biological, as I shall attempt to show later.

The fundamental hypothesis of genetic epistemology is that there is a parallelism between the progress made in the logical and rational organization of knowledge and the corresponding formative psychological processes. Well, now, if that is our hypothesis, what will be our field of study? Of course the most fruitful, most obvious field of study would be reconstituting human history—the history of human thinking in prehistoric man. Unfortunately, we are not very well informed about the psychology of Neanderthal man or about the psychology of *Homo siniensis* of Teilhard de Chardin. Since this field of biogenesis is not available to us, we shall do as biologists do and turn to ontogenesis. Nothing could be more accessible to study than the ontogenesis of these notions. There are children all around us. It is with children that we have the best chance of studying the development of logical knowledge, mathematical knowledge, physical knowledge, and so forth. These are the things that I shall discuss later in the book.

So much for the introduction to this field of study. I should like now to turn to some specifics and to start with the development of logical structures in children. I shall begin by making a distinction between two aspects of thinking that are different, although complementary. One is the figurative aspect, and the other I call the operative aspect. The figurative aspect is an imitation of states taken as momentary and static. In the cognitive area the figurative functions are, above all, perception, imitation, and mental imagery, which is in fact interiorized imitation. The operative aspect of thought deals not with states but with transformations from one state to another. For instance, it includes actions themselves, which transform objects or states, and it also includes the intellectual operations, which are essentially systems of transformation. They are actions that are comparable to other actions but are reversible, that is, they can be carried out in both directions (this means that the results of action *A* can be eliminated by another action *B*, its inverse: the product of *A* with *B* leading to the identity operation, leaving the state unchanged) and are capable of being interiorized; they can be carried out through representation and not through actually being acted out. Now, the figurative aspects are always subordinated to the operative aspects. Any state can be understood only as the result of certain transformations or as the point of departure for other

transformations. In other words, to my way of thinking the essential aspect of thought is its operative and not its figurative aspect.

To express the same idea in still another way, I think that human knowledge is essentially active. To know is to assimilate reality into systems of transformations. To know is to transform reality in order to understand how a certain state is brought about. By virtue of this point of view, I find myself opposed to the view of knowledge as a copy, a passive copy, of reality. In point of fact, this notion is based on a vicious circle: in order to make a copy we have to know the model that we are copying, but according to this theory of knowledge the only way to know the model is by copying it, until we are caught in a circle, unable ever to know whether our copy of the model is like the model or not. To my way of thinking, knowing an object does not mean copying it—it means acting upon it. It means constructing systems of transformations that can be carried out on or with this object. Knowing reality means constructing systems of transformations that correspond, more or less adequately, to reality. They are more or less isomorphic to transformations of reality. The transformational structures of which knowledge consists are not copies of the transformations in reality; they are simply possible isomorphic models among which experience can enable us to choose. Knowledge, then, is a system of transformations that become progressively adequate.

It is agreed that logical and mathematical structures are abstract, whereas physical knowledge—the knowledge based on experience in general—is concrete. But let us ask what logical and mathematical knowledge is abstracted from. There are two possibilities. The first is that, when we act upon an object, our knowledge is derived from the object itself. This is the point of view of empiricism in general, and it is valid in the case of experimental or empirical knowledge for the most part. But there is a second possibility: when we are acting upon an object, we can also take into account the action itself, or operation if you will, since the transformation can be carried out mentally. In this hypothesis the abstraction is drawn not from the object that is acted upon, but from the action itself. It seems to me that this is the basis of logical and mathematical abstraction.

In cases involving the physical world the abstraction is abstraction from the objects themselves. A child, for instance, can heft objects in his hands and realize that they have different weights—that usually big things weigh more than little ones, but that sometimes little things weigh more than big ones. All this he finds out experientially, and his knowledge is abstracted from the objects themselves. But I should like to give an example, just as primitive as that one, in which knowledge is abstracted from actions, from the coordination of actions, and not from objects. This example, one we have studied quite thoroughly with many children, was first suggested to me by a mathematician friend who quoted it as the point of departure of his interest in mathematics. When he was a small child, he was counting pebbles one day; he lined them up in a row, counted them from left to right, and got ten. Then, just for fun, he counted them

from right to left to see what number he would get, and was astonished that he got ten again. He put the pebbles in a circle and counted them, and once again there were ten. He went around the circle in the other way and got ten again. And no matter how he put the pebbles down, when he counted them, the number came to ten. He discovered here what is known in mathematics as commutativity, that is, the sum is independent of the order. But how did he discover this? Is this commutativity a property of the pebbles? It is true that the pebbles, as it were, let him arrange them in various ways; he could not have done the same thing with drops of water. So in this sense there was a physical aspect to his knowledge. But the order was not in the pebbles; it was he, the subject, who put the pebbles in a line and then in a circle. Moreover, the sum was not in the pebbles themselves; it was he who united them. The knowledge that this future mathematician discovered that day was drawn, then, not from the physical properties of the pebbles, but from the actions that he carried out on the pebbles. This knowledge is what I call logical mathematical knowledge and not physical knowledge.

The first type of abstraction from objects I shall refer to as simple abstraction, but the second type I shall call reflective abstraction, using this term in a double sense. "Reflective" here has at least two meanings in the psychological field, in addition to the one it has in physics. In its physical sense reflection refers to such a phenomenon as the reflection of a beam of light off one surface onto another surface. In a first psychological sense abstraction is the transposition from one hierarchical level to another level (for instance, from the level of action to the level of operation). In a second psychological sense reflection refers to the mental process of reflection, that is, at the level of thought a re-organization takes place.

I should like now to make a distinction between two types of actions. On the one hand, there are individual actions such as throwing, pushing, touching, rubbing. It is these individual actions that give rise most of the time to abstraction from objects. This is the simple type of abstraction that I mentioned above. Reflective abstraction, however, is based not on individual actions but on coordinated actions. Actions can be coordinated in a number of different ways. They can be joined together, for instance; we can call this an additive coordination. Or they can succeed each other in a temporal order; we can call this an ordinal or a sequential coordination. There is a before and an after, for instance, in organizing actions to attain a goal when certain actions are essential as means to attainment for this goal. Another type of coordination among actions is setting up a correspondence between one action and another. A fourth form is the establishment of intersections among actions. Now all these forms of coordinations have parallels in logical structures, and it is such coordination at the level of action that seems to me to be the basis of logical structures as they develop later in thought. This, in fact, is our hypothesis: that the roots of logical thought are not to be found in language alone, even though language coordinations are important, but are to be found more generally in the coordination of actions, which are the basis of reflective abstraction. For the sake of completeness, we might add that naturally the distinction between individual actions and coordinated ones is only a gradual and not a sharply discontinuous one. Even pushing, touching, or rubbing has a simple type of organization of smaller subactions.

This is only the beginning of a regressive analysis that could go much further. In genetic epistemology, as in developmental psychology, too, there is never an absolute beginning. We can never get back to the point where we can say, "Here is the very beginning of logical structures." As soon as we start talking about the general coordination of actions, we are going to find ourselves, of course, going even further back into the area of biology. We immediately get into the realm of the coordinations within the nervous system and the neuron network, as discussed by McCulloch and Pitts. And then, if we look for the roots of the logic of the nervous system as discussed by these workers, we have to go back a step further. We find more basic organic coordinations. If we go further still into the realm of comparative biology, we find structures of inclusion ordering correspondence everywhere. I do not intend to go into biology; I just want to carry this regressive analysis back to its beginnings in psychology and to emphasize again that the formation of logical and mathematical structures in human thinking cannot be explained by language alone, but has its roots in the general coordination of actions.