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OBJECTIVE KNOWLEDGE IN SCIENCE AND THE HUMANITIES

Philosophy of science is still, in the minds of many, identified with positivism. This is understandable since twentieth century philosophy of science originates with the work of the Vienna Circle. Positivism is most famous for the verification theory of meaning, the doctrine that the meaning of any proposition is the method by which it is verified, and that any nonanalytic locution which cannot be proven or disproven by some empirical test has no cognitive significance. Positivism is an attempt to construct a "scientific philosophy" in the worst sense: it is maintained that (with the exception of propositions which are analytic and thus vacuous) only those propositions which occur in the sciences are meaningful, all other discourse having at best some emotive value, but no cognitive content, and all of this is maintained within the confines of an exceedingly narrow notion of scientific knowledge. This notion is so narrow that its advocates found themselves in danger of having to relegate most of physics to the realm of nonsense since physics contains many statements which are strictly universal and thus cannot be conclusively verified.

Note the logical situation in which the positivist finds himself: he has a theory of knowledge which includes a set of claims about what sort of utterances are meaningful while he also has an area of human experience which is intended to serve as a paradigm case of his theory and to receive elucidation from that theory. But these theses are in conflict and the positivist must either give up the meaningfulness of science, modify some part of his philosophy, or find some new maneuver which will allow him to reconcile the two sides and thereby show that there is no genuine contradiction. A few positivists took the last route, arguing that the universal statements of science are not propositions after all but material rules of inference, and that they therefore do not come under the strictures of the verification theory of meaning. Most positivists, however, preferred to modify their theory of meaning by weakening the demand for strict verification to the demand that empirical evidence be relevant to the confirmation or disconfirmation of theories. None took the third option, that of declaring all universal scientific claims to be meaningless, an option which one would expect to be quite popular if epistemology is an a priori discipline. In practice the positivists treated their epistemology as a set of substantive claims about science which are subject to modification as a result of tests against science itself.

By the end of the 1930's the mainstream of philosophy of science had shifted to the somewhat more moderate version of positivism generally referred to as "logical empiricism." Logical empiricism is best understood in terms of a set of theoretical commitments and a series of research projects generated by those commitments. Two commitments are central: that scientific knowledge is wholly based on sensory experience in that experience provides the data for confirming or disconfirming theories as well as the source of meaning of all concepts; and that the task of the philosopher is the logical analysis of science. These commitments generate such problems as the need to construct a logic of confir-

[1] With the benefit of hindsight we can expand this problem considerably. Consider a *true* universal proposition. Since it is true it cannot be falsified and since it is universal it cannot be verified, therefore it is meaningless. If it were false it would at least be falsifiable and thus meaningful. Similarly, from the point of view of logical form an existential proposition can be verified but never falsified, so all false universal statements become meaningless. In addition there is a variety of mixed statements, such as "There exists a metal which is always a gas at twenty degrees centigrade," which, by virtue of their form alone, can be neither verified nor falsified.

mation, to show in detail how experience confers meaning on concepts, especially the high level theoretical concepts of modern physics (or, alternatively, to show that these concepts can be eliminated), and to analyze the logical structure of such fundamental scientific activities as explanation and prediction. To be a logical empiricist is to commit oneself to the attempt to solve these problems using the tools provided by symbolic logic, probability theory, and empiricist epistemology. I have examined the various attempts to solve these problems and the development they underwent elsewhere.[2] It is sufficient for our purposes to note that none of these attempts has been successful and that there is still a substantial number of philosophers busily at work seeking new approaches to their solution.

The first major break with positivism came in Vienna itself in 1935 with the publication of Karl Popper's *Logik der Forschung*.[3] Although Popper shares many ideas and techniques with the positivists, the degree to which he rejected their basic approach can be seen by noting that he rejected the verification theory of meaning on two counts: first he rejected the view that the theory of meaning is fundamental to philosophy and argued instead that while there is a crucial difference between science and all other disciplines, it is not a distinction between meaningful discourse and nonsense. Metaphysics, in particular, is not meaningless for Popper, it is just not science, although this does not mean that it is irrelevant to science for, as Popper continually emphasizes, metaphysics has often been a vital source of scientific ideas. Atomism, for example, was, until relatively recently, a metaphysical theory which had a definite and generally salutary influence on the development of science. Second, Popper rejected the view that scientific theories can be confirmed. Lest a misunderstanding be generated, let me recall that when the positivists spoke of a theory as "verifiable" they meant that it could be either confirmed or disconfirmed by some empirical test; Popper's thesis is that although it is possible to disconfirm theories on the basis of experience, it is not possible to confirm them. Confirma-

[2] *Perception, Theory, and Commitment: The New Philosophy of Science*, forthcoming.

[3] Translated as Karl R. Popper, *The Logic of Scientific Discovery*, New York, Harper Torchbooks, 1968.

tion, Popper argues, requires induction and Hume has shown that no inductive arguments are valid. Thus there is no valid argument which will get us from statements describing observable states of affairs to any determinable degree of support for a universal claim. However, in the case in which a theory entails that something should be observed under specified circumstances and that something does not occur, we can infer by a valid *deductive* argument that the theory is in error. Scientific research is viewed, then, as a process of conjectures and refutations: a scientist offers an hypothesis and attempts to refute it by experimental test. As long as a conjecture passes these tests we are justified in not rejecting it, although it does not thereby accumulate confirmation points, and once it has failed a single test it must be rejected. We can never be in the position of having proven a theory true, but by rejecting theories we can at least learn from our mistakes.

Popper's approach gives a very different picture of the development of science from that of the positivists, for although the latter did not write much about the history of science, there is a clear view as to how this development must have taken place implicit in their epistemology: as a result of scientific research theories are either rejected or confirmed and those which are confirmed gain steadily increasing support as further evidence is gathered, so that by now there is a fairly substantial body of firmly established scientific knowledge, a body of knowledge which, it is generally assumed, dates from Copernicus. For Popper, on the other hand, the history of science is full of false theories which are, nonetheless, scientific. For if the distinguishing characteristic of a scientific theory is that it is falsifiable, then any theory which has in fact been falsified is scientific. Thus to label a theory "scientific" is something quite different from labelling it "true." To find true theories does remain the goal of science, but we have no way of proving that any of the theories we currently hold are true and no way of determining how closely they approximate the truth. All we can do is seek to refute them and, when we succeed, replace them with new theories which account for all of the empirical data that the refuted theory accounted for, as well as the data that led to the rejection of that theory. In addition, the new theory must entail new results

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which will provide new tests for that theory. The classic example of this process is the rejection of the Newtonian theory of gravitation and its replacement by general relativity as a result of the measurement of light deflection by the gravitational field of the sun. For, the story goes, Newtonian mechanics predicted a very different result than did Einstein's theory and the observation was consistent with relativity and contrary to classical mechanics. Having been proven false, classical mechanics does not cease to be a scientific theory, and we need not conclude that Newton, Laplace, Lagrange, etc. were not scientists after all. Indeed, it is quite possible that at some time in the future general relativity will be overthrown and replaced by a theory which no one has yet thought of, but it will still be a scientific theory. This is not to deny that Newtonian mechanics is still useful, but it is useful only because it gives sufficiently accurate results in a wide range of situations, although it gives them for the wrong reason. We have here a case of correct (or almost correct) conclusions being validly deduced from false premises, a situation which is by no means unfamiliar to the logician. One advantage of having general relativity at our disposal is that it allows us to determine exactly how reliable Newtonian theory is and to show in detail why it provides acceptable results for some situations.[4]

Perhaps the most radical upshot of Popper's approach is its separation of the notions of "science" and "truth," for, at least since Aristotle, to claim that a view could be both scientific and false has seemed a contradiction in terms. This should not trouble us excessively, however, for Popper is proposing a new way of looking at knowledge and we must expect that such an attempt will require us to use language in some new ways. This is typical of the development of knowledge, as may be noted by recalling the sorts of contradictions in terms involved in the Copernican thesis that the sun is a star or in Freud's talk of unconscious thought. New views in science or in philosophy can generally not be expressed comfortably in existing language (which is not to say that they cannot be expressed at all); one result of the acceptance of new theories is often a change in what

[4] Note that this opens up the possibility that pre-Copernican theories such as Ptolemaic astronomy and Aristotelian dynamics, as well as the later, much maligned phlogiston chemistry, are scientific.

can be comfortably expressed, a change which, as the sun-star case illustrates, can be so extreme that locutions which were previously contradictions become tautologies.

There are, however, serious difficulties in the Popperian theory of science. Indeed, the difficulties are so severe that during the past two decades yet another view of the nature of science has emerged—but it is a view which accepts Popper's separation of the notions of science and truth and, in fact, requires that this separation be made even sharper. The difficulties can best be seen by turning our attention away from the logic of theory testing and looking instead at the actual development of science, for this is the touchstone against which any philosophy of science and, as was argued earlier, any epistemology, must prove its mettle.

I have already mentioned one test that led to the overthrow of classical mechanics, its failure to predict the correct value for the deflection of light by the gravitational field of the sun, and according to Popper one such counter-instance should be sufficient to overthrow a theory. But this was by no means the first discovery which could, logically, have been taken as a counter-instance. During the nineteenth century it was well known that the orbits of Mercury and Uranus were not in accord with the predictions of Newtonian theory, yet neither of the anomalies was considered a counter-instance, although the full story turns out somewhat different in each case. In the case of Uranus, rather than rejecting Newtonian theory, scientists turned to this theory itself for an explanation of the perturbations in the planet's orbit. Newtonian theory does tell us that each planet has a gravitational attraction which plays a role in determining the orbit of every planet; thus instead of taking the orbit of Uranus as a counter-instance, it was possible to assume that Newtonian theory was correct and take the perturbations in the orbit of Uranus as evidence for the existence of another, as yet unobserved, planet. This is exactly what Leverrier and Adams did. On the basis of the deviations of the observed orbit from the computed orbit they succeeded in calculating the mass and orbit of Neptune and thereby produced a major corroboration of Newtonian mechancs. Clearly, to have rejected Newtonian mechanics because it seemed to give an incorrect orbit for Uranus would have been a mistake, but there is no rule of scientific method and no valid argument

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of formal logic that could have guaranteed this. The point can be underlined by comparing the case of Mercury. Leverrier used exactly the same method to compute the mass and orbit of another planet, Vulcan, in order to account for the discrepancies in the orbit of Mercury, but no such planet exists and the failure of Newtonian mechanics to give a satisfactory orbit for Mercury eventually became a counter-instance to that theory, *but only after the orbit had been successfully computed using general relativity.* We should note that the same technique, now based on perturbations in the orbit of Neptune, led to the discovery of Pluto although it is now known that the mass of Pluto is not large enough to account for the perturbations which led to its existence being predicted. And we should note finally that all of these problems exist only because scientists were studying the heavens within the framework of Newtonian theory. Without some theory which tells us how the planets ought to behave, no observed behavior could be recognized as problematic.

The upshot of the story is that even though Popper is correct in pointing out that falsification requires only deductive logic, logic and experiment alone cannot tell us when it is time to abandon a theory.[5] By the exercise of sufficient cleverness on the part of the theorist an apparent counter-instance can often be shown to be no counter-instance at all and perhaps even, as in the case of Neptune, be turned into a major triumph for the theory. Some further examples from the history of astronomy will lend support to this point.

The Copernican thesis that the earth moves was clearly refuted by overwhelming empirical evidence when it was proposed during the sixteenth century. The most important counter-instance was the failure to observe the stellar parallax which should result from the annual motion of the earth. Indeed, this same observational test had resulted in the rejection of the moving earth thesis when it was proposed by Aristarchus in the ancient world. In addition, the Copernican view entailed that Venus should show phases of the same sort as the moon, phases that Copernicus knew were not observed, and that the variation in the apparent size of

[5] Cf. Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2nd ed., Chicago, University of Chicago Press, 1970, p. 94.

Venus from conjunction to opposition should be forty times that actually observed and the variation for Mars should be sixty times that observed. The observational situation at the time is expressed most forcefully by Galileo when he states his unbounded admiration for "the outstanding acumen of those who have taken hold of this opinion and accepted it as true; they have through sheer force of intellect done such violence to their own senses as to prefer what reason told them over that which sensory experience plainly showed them to the contrary." [6] Galileo is plainly expressing the viewpoint of a thinker who has committed himself to a theory, recognized its problems, and set out to solve them within the structure of that theory. In this respect the only difference between Galileo, Copernicus and Kepler on the one hand and Leverrier and Adams on the other is that the former group were defending a new view while the latter were defending an old "established" view, although one which was soon to be overthrown. It must be emphasized, however, that in all of these cases we do not have a mere dogmatic assertion of some thesis, but rather a recognition of the problems the thesis faces and a commitment to attempt to solve these problems. Indeed, three of the problems that Copernicus could not solve, the variation in the apparent size of Mars and Venus and the missing phases of Venus, were removed by Galileo in a manner which Copernicus could never have envisaged, i.e., by observations with the newly invented telescope. Galileo did not, however, succeed in observing an annual stellar parallax and the reasons for this deserve special mention.

The prediction that there will be an *observable* stellar parallax requires an additional premise besides the claim that the earth moves. The amount of parallax is a function of the distance to the stars and if this distance is too large no parallax will be observable without extremely sophisticated instruments. Now Aristarchus, who had no concept of improving our vision by means of instruments, responded to the observational refutation of his conjecture by proposing that current estimates of the distance to the stars were much too low, that the stars are so

[6] Galileo, *Dialogue Concerning the Two Chief World Systems*, trans. Stillman Drake, Berkeley, University of California Press, 1967, p. 328.

far away as to make the parallax unobservable, and that the failure to observe parallax did not, therefore, refute his proposal. His contemporaries responded as good Popperians: this was an *ad hoc* hypothesis introduced to protect an initially implausible claim from refutation and thus scientifically unacceptable. Now exactly the same logical situation obtained when Copernicus revived the moving earth thesis and even those who attempted to defend Copernicanism by increasing the distance to the stars grossly underestimated this distance. Thus Galileo proposed an observation technique using the telescope which he believed would have revealed stellar parallax no matter how small, although he never tried the observation himself—and it would have failed. Stellar parallax was first observed by Bessel in 1838; this means that there was a period of well over one hundred years during which the moving earth had been universally accepted by astronomers and the failure to observe stellar parallax served not as a counter-instance but as a research problem.

The Popperian approach fails because it demands clear-cut tests which will lead to the falsification of theories, but there is no such test which will determine if a particular anomaly is a genuine counter-instance. The above discussion provides three reasons why this is the case: (1) the observation may not be contrary to the theory after all, as further work within the theory will show; (2) the observations themselves may be rejected as a result of further refinements in observation techniques; (3) the theory that a body of observations contradicts is generally complex and it may not be at all clear which part of this theory is to be rejected.[7] It is considerations such as these which have led a number of philosophers and historians in recent years to propose yet a third model of scientific knowledge.[8] I will sketch this model briefly.

The central idea is that, for the most part, scientists work

[7] This last point is known as the Duhem-Quine thesis. Cf. Pierre Duhem, *The Aim and Structure of Physical Theory*, trans. Philip Wiener, New York Atheneum, 1962, and Willard Van Orman Quine, "Two Dogmas of Empiricism," *From a Logical Point of View*, New York, Harper Torchbooks, 1963.

[8] The major figures in this group are Paul K. Feyerabend, Norwood Russell Hanson, Errol E. Harris, Thomas S. Kuhn, Imre Lakatos, Michael Polanyi, and Stephen Toulmin although the verson to be sketched does not coincide with the views of any one of these thinkers.

within the structure of an accepted theoretical framework and that observations which contradict this framework provide research problems, not refutations. The theory itself determines what observations are worth making, how they are to be understood, what situations are problematic, and what counts as an adequate solution of a problem. The starting point from which research takes off is never established as true, although this does not mean that there are no good reasons in support of it, and no theory is rejected because it has been unequivocally proven false, for no matter how many problems a theory has, it is always possible that further research within that theory will lead to the resolution of these problems. Theories which pile up a large number of unsolved problems are rejected and replaced by new theories without the old theory having been proven false or the new one true in any traditional sense of the notion of *proof*. Theory change takes place because a number of creative thinkers judge that it is time to strike off in a new direction and succeed in constructing a new starting point which interests enough researchers to take over the field. And note, as we saw in the case of the orbit of Mercury, an old fundamental theory will not be rejected until there is a new one available to take its place, for there cannot be significant research without a theory to guide it.

Perhaps the most dramatic change in our conception of knowledge entailed by the approach described is a shift from the view that it is proven results that are constitutive of science to the view that it is on-going research under the guidance of a theory that is fundamental. This view raises many new problems and I wish to consider a particularly crucial one here. We have seen that theory plays a fundamental role in determining what the scientist chooses to observe and how these observations are understood, and this leads us to ask just how great this role is. An extreme answer is given by Feyerabend: "what is regarded as 'nature' at a particular time is *our own product* in the sense that all the features ascribed to it have first been invented by us and then used for bringing order into our surroundings." [9] If

[9] Paul K. Feyerabend, "Explanation, Reduction, and Empiricism," *Minnesota Studies in the Philosophy of Science*, III, ed. Herbert Feigl and Grover Maxwell, Minneapolis, University of Minnesota Press, 1962.

this is the case, if what we take to be nature is wholly created by our theories, then constructing a scientific theory would be akin to writing a fantasy in which the landscape and characters may be totally different from anything previously encountered. Any proposed theory will be as "scientific" as any other, there will be no independent basis against which theories can be tested, and we will only be able to choose between theories on aesthetic grounds. Clearly, this would rob the sciences of any claim to objectivity, but Feyerabend's claim is not an adequate description of scientific procedure. Scientists no more create fantasies in their attempts to make sense of nature than we do in our everyday attempts to get around in the world, and Feyerabend in effect acknowledges this when he states that the invented aspects of nature are used to bring order into our *surroundings*. To understand the source and limits of scientific objectivity we require a threefold distinction between (1) the conceptual structures that science creates; (2) our surroundings, the physical world that exists independently of our experiences and which we are attempting to understand when we construct theories; (3) nature, i.e., the world as we experience it, which is constructed out of our surroundings by the imposition of our conceptual systems.[10]

Scientific theories are created to make sense out of an independent reality. For them to be able to do this job there must be a considerable degree of meshing between the structure of the theory and the structure of that reality. This is not a throwback to the early Wittgensteinian idea that the logical structure of language is identical with that of reality and thus that there can only be one logically correct language. Rather, the structure of reality underdetermines the structure of our theories so that there is a multitude of theories that are admissible for the organization of our surroundings, but not every theory that one might propose is admissible since not every theory will provide even an approximate fit to reality. Nature as we experience it is

[10] The parallel between this distinction and Kant's distinction between a priori forms of intuition and categories, noumena, and phenomena is no coincidence although I reject the claims that the categories are a priori and that the noumena are unknowable. Cf. my "Idealism, Empiricism, and Materialism," *New Scholasticism*, 47, 1973, pp. 311-323 and "Paradigmatic Propositions," *American Philosophical Quarterly*, 12, 1975, pp. 85-90, and Kuhn, *Structure*, pp. 111-112.

a result of mapping our theories onto an indipendent world and includes contributions from both; our theories are capable of providing objective knowledge exactly because we attempt to fit them to a world whose existence and properties in no way depends on our theories.[11]

A great deal of light can be thrown on the relation between our knowledge and an independent reality by considering an analogy with what happens when one reads and interprets a text. The text itself, the paper with ink marks on it, is a physical object with a definite set of properties which exists independently of anyone's reading it. But the text itself is not sufficient to permit reading to occur; in order to read I must be equipped with the knowledge of a language which will allow me to make sense of the text. The meaning of the text that I learn by reading it depends on the presence of both these factors. There is a difference between reading a text and making one up for myself, for although I must bring my knowledge of the language to the text, my use of this language must be guided by the physical structure of the text. If I hold a copy of the *Critique of Pure Reason* before my eyes and proceed to recite from *Gravity's Rainbow*, I am not reading.

But the situation is considerably more complex than this. Reading a text requires more than just looking it over and recognizing each of the letters, it requires that I understand the meaning of the text; this is a much more ambiguous and uncertain process than merely recognizing letters. It requires making sense of an entire linguistic structure and this, as we all know, can lead to widely varying interpretations of the same text. But we must not conclude from this that the process of interpreting a text is wholly arbitrary. For the fact that various interpreters may legitimately disagree (and this is not to suggest that *all* disagreements are legitimate) does not negate the fact that any interpretation must deal with the actual text. There is no need here to attempt to draw a sharp line between what constitutes legitimate interpretive disagreement and what does not; it is sufficient for our purposes to recognize that there is a difference,

[11] A detailed defense of this claim is possible although it requires a considerably longer work than this paper.

that the *Critique of Pure Reason* is not *Gravity's Rainbow*, and that this serves to illustrate the role that the physical text plays in delimiting the range of possible interpretations.

This notion of variability within limits has been pushed to its logical conclusion by Borges in "The Library of Babel." [12] Borges imagines a vast, possibly infinite, library in which the books are mostly gibberish with the exception of an occasional meaningful word or phrase. The narrator, one of a multitude of librarians, offers a theory according to which the books consist of all possible permutations of the typographical characters. This would make the number of books, which are all of the same size, enormous, but it would also guarantee that all of the works which have been or could be written are in the library. More importantly for our purposes, any book which the librarian takes to be gibberish may be a completely intelligible book in some possible language, and the library will include all information needed to translate from that language to any other actual or possible language. Any book, then, may have an enormous number of different readings in an enormous number of different languages. The book that the librarian is now holding, which does not include a single recognizable word in any language he is familiar with, may be a translation of *Gravity's Rainbow* into some language and of the *Critique of Pure Reason* into another, while my copy of *Gravity's Rainbow* may be the *Critique* in some possible language and the latest pornographic potboiler in yet another.

Pushing the idea like this might seem to remove the significance of the text in limiting possible readings, but this would be an incorrect conclusion. For while it does show that a vast number of readings of any given text are logically possible, it also shows that a vast number are excluded. No matter what I might be able to find in my copy of *Gravity's Rainbow* by inventing an appropriate language or code, I still cannot find the German text of the *Critique* there—in fact, I cannot find any recognizeable

[12] Jorge Luis Borges, "The Library of Babel," trans. Anthony Kerrigan, *Ficcones*, New York, Grove Press, 1962. The use of Borges story to illustrate my theory of objectivity can be taken as a further example of the kind of alternative interpretation with which I am concerned. It is clear that Borges reads his story as a metaphor on the structure of time.

text in any language I am familiar with other than English, and even in English there is only one text I can find on these pages, that of Pynchon's *Gravity's Rainbow*.

A language can be a suitable vehicle for interpreting a text only if the structure of the language and of the text mesh, i.e., only if the text is written in that language. Thus the physical properties of the text still determine what languages it can be read in and, to a large extent, how it is to be read in any given language. It is logically possible to find a new reading of a given text by inventing a new language, but this is no easy task and it approaches impossibility as the length of the text increases.

Let us return to Borges' librarian. I want to argue that the theory of the library that the librarian develops in the story has the main features of an objective scientific theory—although we must remember that this does not entail that it is "the true theory" in any traditional sense of this notion. The librarian is faced initially with a set of experienced phenomena, the bewildering variety of meaningless books in the library, to which he wishes to bring some order. He attempts to do this by offering a theory, a set of claims about the underlying structure of the library which will account for the phenomena he is concerned with. The library is taken to be something which exists independently of the librarian of his theories and the theory of the library is an attempt to say something about this entity. Thus the theory meets the first demand of an objective theory. Note secondly that the theory results in a new way of conceiving the library and its contents. This is also to be expected since we only experience the things around us in terms of the theories we hold about them; a change in our theories about some set of objects results in a change in the way we experience these objects. Suppose the librarian initially believed that all of the books in the library were intended to make sense. Picking up a book he would search it for recognizable words, phrases, or sentences and cherish those books which contained meaningful segments, but most books would show a bewildering failure to fit any intelligible pattern. Examining the books from the viewpoint of his new theory, new patterns emerge. Books that previously had no meaning—not in the literal sense of being unreadable, but in the extended sense of fitting no intelligible pattern—now do have a meaning. The occurrence of sense in the midst of nonsense is explained, the existence of vast quantities of nonsense is explained, one need no longer be concerned about the zealots who destroyed millions of books since the new theory assures us that for every book destroyed there exist millions of duplicates which differ only in the location of, say, one comma (note that this involves a further implicit theory about what is significant in books, a theory which would not be shared by, for example, many poets), and most importantly, the theory provides a new guide for further study of the books. The librarian now has good reason to spend large amounts of time, energy, effort, and perhaps even money searching for books which are mostly intelligible since the theory guarantees that they exist—just as the expenditure of large amounts of time, energy, effort, and money building particle accelerators is scientifically justifiable because current theory tells us that this is the way to gather significant new information, while the fact that this approach has consistently turned up important surprises is one of the strongest defenses of current theory. In the case of our librarian the new theory redirects his research even more narrowly to the search for certain specific intelligible books: the dictionaries and grammars which will enable him to read more books, the catalog of the library, and a book that provides a compendium of all the other books.[13] Success in finding the kinds of books that the theory directs us to look for will provide good reasons for continuing to do research under its guidance while failure to find those objects will provide grounds for doubting the theory and attempting to produce an alternative. But there is no test which can force us to conclude either that the theory has been finally proven or that it has been refuted.

I will now use the theory of objective knowledge developed above to evaluate the epistemic status of three areas of the humanities: history, literature, and one sub-discipline of philosophy, epistemology.

[13] Unfortunately, while the theory guarantees the existence of millions of copies of each of these books with only minor variations, it also guarantees the existence of an untold number of misleading catalogs and erroneous compendia and provides no criteria for distinguishing them. This means that in addition to empirical research the theory provides problems for further theoretical research too.

In the case of history objective knowledge is possible because the historian's job is to understand documents, artifacts, etc. which exist independently of any interpretations developed. I cannot enter here into disputes over the proper function of historical research, but I will assert that I do not take the view that the historian simply compiles "facts." The historian's job is interpretive throughout, requiring some set of theoretical guidelines for selecting and making sense of his data. But even in the case of an idealist historian who wishes to rethink the thoughts of historical figures, the rethinking process is not the construction of a fantasy; it must be based on a detailed knowledge of the person involved and his historical circumstances, knowledge which must be supplied by the study of objects such as documents which exist independently of the historian. It is, of course, notorious that historians disagree on the interpretation of events, but the range of their disagreements is limited by the evidence—historians who disagree about some aspect of Napoleon's personality do not confuse Napoleon with Socrates. It is the proper interpretation of the evidence that provides the subject of the historians' disagreements; without an intimate tie to independent evidence the types of debates historians engage in would not occur. By contrast, novelists do not debate about the physical characteristics, personalities, or events in the lives of their characters, nor do they do research to discover these. They create their characters and attribute to them whatever characteristics or "histories" they choose. Literary critics, on the other hand, debate about the plausibility, depth, and coherence of a character in a novel, but the critic, unlike the artist, is dealing with the novel, an object which exists independently of any interpretation of it. All of which brings us to the question of the status of literature in the scheme I have been developing.

I want to argue that literature (and art in general, but I will confine myself here to literature) does not provide objective knowledge since the artist creates his characters and situations and is quite free to invent any set of circumstances which his art demands. This will obviously meet with strong objections: art, we will be told, takes us to the essence of things, it grasps the universal in a particular situation, it provides, as it has been argued here that science does, new ways of understanding experience. But these claims miss an important point. Whatever else it may be, a great novel or play is highly seductive. It is always tempting to believe that characters and situations which are well drawn and powerfully described are real, or at least catch the essence of the real, and it is equally easy to believe this whether we are reading Conrad or Tolkien. But of course not *all* writers will strike each of us as providing truth, and what we are implicitly doing when we make this judgement is evaluating the fictional situation in terms of information we have, or think we have, from other sources. What the novel provides is not objective knowledge but an *illustration*, often a powerful one, of claims that must gain their epistemic warrant on other grounds. It is epistemologically vital to distinguish the evidence for a view from an illustration of that view.

The point can be clarified by reflecting further on the way in which I used Borges' story as an aid in expounding my analysis of objectivity. There is no respect in which the story can be taken as grounds for accepting the theory of objectivity developed, its only legitimate epistemic function is to provide clarification of my thesis and some of its ramifications. Thus one must be rather wary of any use made of a literary example in an epistemic context, for Borges' story presents an intriguing development of a set of ideas in a much more elegant way than I am capable of providing—which is why I took the liberty of borrowing it. All I am capable of providing are some rather dry arguments, but it is only arguments that provide any grounds for accepting a theory.

Finally, let us consider the status of epistemology in this scheme. Many contemporaries maintain that philosophy is a second order activity, that it is not directly about the world but concerned with the analysis of what others have to say about the world. Now in the case of epistemology I accept this thesis with one major caveat. Those who originally promoted this view generally went on to argue that the proper object of philosophic study is language; I believe that this particular strait-jacket is too narrow. In the case of epistemology our subject is the nature of knowledge. If we are to attempt to construct an objective epistemology we must do so on the basis of some data, some examples of knowledge which we study. My thesis is that the best data we have is provided by the history of the sciences so that we should take the detailed story of the development of science to be the main evidence for our view and proceed by constructing theories which can be tested against this data in much the same way that the natural scientist constructs theories which are tested against the physical world. All of the problems about the theory-ladenness of our data and the grounds for theory acceptance and theory change that arise in the analysis of the sciences are again for the epistemologist. To accept the sort of epistemology I have outlined here is to commit oneself to working on these problems, but epistemology remains a second-order discipline because its data is provided by the results of first-order disciplines. Epistemology is thus capable of providing objective knowledge in exactly the same sense as and using the same methodology as the first-order disciplines. I have attempted in this paper to provide an example of this sort of epistemology.