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Hilary Putnam

A Half Century of Philosophy, Viewed From Within

IN THIS COUNTRY, THE DEPARTMENTS THAT PRODUCE the majority of Ph.D.'s that will comprise the next generation of philosophy teachers are dominated by a single kind of philosophy, namely, "analytic philosophy." A typical graduate student might envision the history of the last fifty years as follows: until sometime in the 1930s American philosophy was without form and void. Then the logical positivists arrived, and about fifty years ago most American philosophers became positivists. This development had the merit of bringing "high standards of precision" into the subject; philosophy became "clear" and everyone had to learn some modern logic. However, it had other consequences as well. The (supposed) central tenets of the logical positivists[1] were false—according to the stereotype, the logical positivists held that all meaningful statements are either *1)* verifiable statements about sense data or *2)* "analytic" statements, such as the statements of logic and mathematics. They believed in a sharp distinction between synthetic claims (i.e., empirical claims, which they identified with claims about sense data[2]) and analytic statements; they did not understand that concepts are theory-laden[3] or that there are such things as scientific revolutions.[4] They thought that the philosophy of science could be done in a wholly unhistorical way. At the end of the 1940s, W. V. Quine showed that ontological questions, such as whether numbers really exist, make sense[5]—contrary to the logical positivist claim that all metaphysical questions

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are nonsense—and thereby contributed to the revival of realist metaphysics in the United States, even if he (regrettably) retained some positivist prejudices himself. Shortly thereafter, he determined that the analytic/synthetic distinction is untenable.[6] Later Quine showed that epistemology could become a part of natural science,[7] and I helped further demolish logical positivism by proving that the positivist dichotomy of "observational terms" and "theoretical terms"[8] was untenable. This paved the way for a robust metaphysical realism, which I (regrettably) gave up in the mid-1970s.

Although there are elements of truth here, one way in which this story is a distortion is in its account of what the logical positivists believed. The movement was diverse; the positivists did not think that philosophy could be done independently of the results of science.[9] Rudolf Carnap hailed Thomas Kuhn's book, *The Structure of Scientific Revolutions* (which presented a major case for the indispensability of the history of science to philosophy), and he is known to have been instrumental in getting it published.[10] These matters have been set right in the literature, even if the "oral tradition" has it otherwise. But there is a more subtle falsification in this account, namely, the claim that forty or fifty years ago logical positivism was dominant. It is true that if one is interested simply in the *internal* development of analytic philosophy, then the fact that the logical positivist professors were few in number is not important, since the views of many present-day analytic philosophers developed out of a criticism of the views of those few. Nevertheless, if we are not to rest content with a partly fictitious history of American philosophy, it is important to realize that *at the time* when logical positivism was supposed to have been dominant, logical positivists were extremely few and largely ignored. There was Rudolf Carnap (who did not produce a single Ph.D. student in the last ten years that he spent at the University of Chicago), Herbert Feigl in Minnesota, Hans Reichenbach at UCLA, and perhaps a few others. However, these people were quite isolated—Carnap had no intellectual allies at Chicago; Reichenbach had no intellectual allies at UCLA. Only at Minnesota, where Feigl created the Minnesota Center for the Philosophy of Science, was there a little bit of critical mass. Even Quine at Harvard had no permanent allies on the faculty until 1948, when Morton White[11]

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joined the department. Nor were these philosophers regarded as terribly important in the 1940s. At the end of the 1940s, most philosophers would have told their history in a way that few present-day analytic philosophers would be able to recognize. They would have recounted the rise and fall of pragmatism; they would have talked about the New Realists; they would have talked about Critical Realism (led by Roy Wood Sellars, whose son Wilfrid Sellars became one of the most distinguished American analytic philosophers); they would have talked about absolute idealism, which was waning but still had some distinguished representatives; but they would have regarded positivism as a matter of little consequence.

I do not mean to endorse this judgment: logical positivism was a movement that produced not only errors but also insights, and it richly deserved the attention that was later paid to it. But there were also real insights as well as errors in the writings of the American pragmatists, in the writings of idealists such as Josiah Royce, and in the writings of the New Realists and the Critical Realists.

In contrast to this fictitious history, let me cite my own experiences as an undergraduate and graduate student. At the University of Pennsylvania between 1944 and 1948 I know of no class (if we put aside a course taught by a graduate student, Sidney Morgenbesser) in which the writings of the logical positivists were so much as looked at. The department had one atypical pragmatist (West Churchman) but otherwise no one who was associated with a "movement" in philosophy. At Harvard between 1948 and 1949 I also cannot recall any courses in which the logical positivists were read, although I assume that Quine and White must have discussed them. At UCLA, from 1949 to 1951, Reichenbach was the only professor who either represented logical positivism (although he refused the label!) or discussed logical positivism. Harvard had one atypical pragmatist, C. I. Lewis, and UCLA had a Deweyan, Donald Piatt. American philosophy, not only in the 1940s but well into the 1950s, was decidedly unideological. If there were "movements" at individual departments, they were represented by one or two people. The present situation, in which American philosophy is dominated by a movement—a movement proud of how it differs both from what preceded it and from what it sees as the opposing

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tendency ("continental philosophy")—is utterly different from the situation that obtained in the field when I entered it.

1953–1960

Any account of a field over a fifty-year period must be based on an individual's perspective, and I shall continue to use my own experiences to draw a picture of the successive transformations. When I came to Princeton in 1953, the department had three full professors. Ledger Wood was the chairman, and within a few years he brought Gregory Vlastos and C. G. Hempel into the department. His first move towards transforming the department from a sleepy backwater was to hire four young men, myself and three recent graduate students from Harvard University.

Although five years earlier I had studied at Harvard for one year, these three Harvard men came from a milieu that was unknown to me. In a few short years, a group of graduate students at Harvard University had acquired something of a common philosophical orientation. The change appears to have been largely due to the influence of Morton White, who, in addition to assigning Austin and Strawson in his courses, had persuaded a number of the graduate students to spend a year at Oxford. The effect was that Oxford philosophy had reached Harvard, and these young teachers were wedded to something they called "ordinary-language philosophy." The point of that philosophy, as they understood it from reading Austin in particular, was that disaster occurs when philosophers—including philosophers who claim to be "scientific philosophers"—allow themselves to misuse ordinary language and especially to introduce what are in fact very unclearly explained "technical terms" into philosophical arguments. Questions of philosophic method had come to the fore and were the topic of most of our discussions.

At first my reaction was to scoff at "ordinary-language philosophy" and to defend what I called "rational reconstruction," that is, the idea that the proper method in philosophy was to construct formalized languages. Under the influence of Carnap, in particular, I maintained that philosophically interesting terms in ordinary language are too imprecise as they stand and that the task of philosophy is to "explicate" them, to find formal replacements for

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them. This is a view, however, that I quickly gave up because (to tell the truth) I found myself unable to give more than two or three examples of *successful* "rational reconstructions." I can almost remember the exact words that went through my head at that time: "If Carnap is right, then the proper task of philosophy is doing this thing called 'explication.' But what reason is there to think that 'explication' is *possible*? Moreover, even if we could come up with successful explications, *who* except Carnap thinks that scientists would really accept these explications, or adopt this artificial language to resolve controversies, and all that?"

In addition, I rejected the idea that one must *choose* between "rational reconstruction" and "ordinary-language philosophy." I felt that although one could learn a great deal from reading Reichenbach and Carnap on the one hand and Wittgenstein and Austin on the other, the totalistic philosophical methodologies that were being promulgated in their names were unrealistic.

My reasons for thinking that the version of ordinary-language philosophy that was being presented in the United States was unrealistic (when I visited Oxford as a Guggenheim Fellow in 1960, I came to appreciate how much richer the "real thing" was) were as short and simple as my reasons for thinking that "rational reconstruction" was also unrealistic. Reading Austin, I had appreciated the point I mentioned earlier, that confusion is often rampant when philosophers misuse ordinary language. That one should, as far as possible, try to do philosophy *in* ordinary language seemed indubitable. On the other hand, the idea that philosophy should be *about* ordinary language (or about the "ordinary use" of philosophically problematic expressions) was simply a non sequitur.[12] Indeed, I have never been able to believe that philosophy has a definitely delineated subject matter.

I have described a change in the mood of young philosophers, a change from *below*.[13] Older philosophers were, of course, necessarily involved. I have mentioned Austin, Strawson, and Wittgenstein, whose influence from afar had obviously reached Harvard, and Quine, who was going to be at the very center of all the developments in American philosophy for the next two decades and beyond. Indeed, Quine was partly responsible for creating the new climate. I do not mean to suggest that the particular wave of enthusiasm for ordinary-language philosophy that affected Harvard,

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and was later to affect other American institutions,[14] was due to Quine (Quine had no great admiration for ordinary-language philosophy), but Quine's attack on the analytic-synthetic distinction made issues in the philosophy of language central for young thinkers in the field.[15] At any rate, when C. G. Hempel joined the Princeton philosophy department (as I recall, this was in 1955 or 1956) he had already become convinced that Quine's attack on that distinction was certainly correct, and this became a hot topic of discussion among the graduate students. But thinkers of my own generation continued to play a role in this debate. For example, at the end of the decade, in 1959, Noam Chomsky and Paul Ziff both spent a year at Princeton—Chomsky at the Institute for Advanced Study and Ziff as a visitor in the Philosophy Department. Paul Ziff's seminar on the philosophy of language, which Chomsky attended, became a center for discussion of these issues. Chomsky's *Syntactic Structures* had also appeared in 1957,[16] and the Chomskian image of language as a "recursive" system (a system of structures that could, in principle, be listed by a computer[17]) entered all of our philosophical vocabularies, as did the Ziffian image of meanings as a recursive system of conditions associated with the sentences of the language.[18]

My own work also began to influence discussion outside of the Princeton department by the end of the decade. I was in the habit of explaining the idea of a "Turing machine"[19] in my mathematical logic courses in those days. It struck me that in Turing's work, as in the theory of computation today, the "states" of the imagined computer (the Turing machine) were described in a very different way than is customary in physical science. The state of a Turing machine—one may call such states *computational* states—is identified by its role in certain computational processes, *independently* of how it is physically realized. A human computer working with paper and pencil, a mechanical calculating engine of the kind that was built in the nineteenth century, and a modern electronic computer can be in the *same* computational state, with respect to a particular computation that all three carry out, without being in the same physical state. I began to apply images suggested by the theory of computation to the philosophy of mind, and in a lecture delivered in 1960[20] I suggested a hypothesis that was to become influential under the name *functionalism*: that the

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mental states of a human being are computational states of the brain. To understand them (e.g., in a scientific psychology) it is necessary to *abstract* from the details of neurology, as we regularly abstract from the details of the "hardware" when we program or employ computers, and describe mental states entirely in terms of the sorts of computations that they are involved in. Mental states are like *software,* so to speak. I later rejected this hypothesis, but it continues to be popular, and it certainly connected with what was to become a continuing effort of many philosophers to bring philosophy and science into closer touch with one another. At that time, I also decided that one of the positivists' favorite dichotomies, the dichotomy between observational and theoretical terms, was untenable, and I published a paper that was influential in totally rejecting the Carnapian view that in science only "observation terms" need to be "directly interpreted."[21] To explain why this paper was so well received, I must discuss the issue of "realism."

"SCIENTIFIC REALISM"

The prominence that the term "realism" later came to have was, perhaps, presaged by a remark in my essay "What Theories Are Not" to the effect that certain positivist views are "incompatible with a rather minimal scientific realism." At that point, to be a realist was simply to reject positivism. This was the way that I (and most of the analytic philosophers of my generation) thought about realism as late as when I wrote the introduction to *Mathematics, Matter and Method.* In that introduction, dated September 1974, there is a section titled "Realism," which begins: "These papers are all written from what is called a *realist* perspective. The statements of science are in my view either true or false. . .and their truth or falsity does not consist in their being highly derived ways of describing regularities in human experience." What was all this about?

According to most of the positivists, the claim that a scientific theory makes about the world was supposed to be expressible in a language that employed (in addition to logical vocabulary[22]) only such "observation terms" as "red" and "touches." In principle, it was claimed, one could use "sense-datum terms," terms referring

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to "subjective experiences" rather than to physical objects, and still state the entire content of science. The idea is that science is just a device for predicting regularities in the behavior of "observables." Unobservables such as microbes are simply, the positivists claimed, "constructs" we introduce to help predict how observables behave.

It was against this philosophy of science (which sounded, to my ears, a little like Berkeleyan idealism) that I reacted to in "What Theories Are Not" and in subsequent essays, and I was joined by many others, including J. J. C. Smart, with whom I had developed a friendship at Princeton in the late 1950s.

In addition to rejecting positivism, we also emphasized that statements of science are either true or false. The connection was as follows: Since in the positivist view it is only formalized science as a whole that has empirical content, it may well be that certain individual scientific statements S are, in themselves, empty of empirical content in the sense that it would make no difference to what we would predict (given the body of accepted statements) if we accepted S or accepted its negation. For example, it may well be that the scientific theory of a given time, say 1970, is such that if you conjoin to it either the statement that the temperature in a certain place inside the sun is A or the statement that the temperature in that place is B, where A and B are very different temperatures, no new observational prediction results. In that case, on the view that we were criticizing, both those statements would simply lack truth-value, that is, they would be neither true nor false. If a few years later, when scientific theory had changed, those statements had become testable, they would *now* have truth-values, that is, they would now become true or false, depending on what the new observations show. To the objection that the very same statement cannot both possess and lack a truth-value, the positivists would reply, "It is not really the same statement," that the change in theory changed the *meaning* of the term "temperature." (In "What Theories Are Not" and in later papers such as "Explanation and Reference,"[23] I excoriated the positivists for doing violence to all of the notions of sameness of meaning and change of meaning that we have, either in ordinary language or in linguistics, for the sake of protecting their doctrine.) Two things are especially disturbing about such a view. First, if each new theory

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of atoms, genes, or the AIDS virus changes the very meaning of the term "atom" or "gene" or "AIDS virus," then there can be no such thing as *learning more* about atoms, or genes, or AIDS; any discovery that purports to add to our knowledge about one of these things is actually a discovery about something we never talked or thought about before. The only thing that scientists can learn more about is *observables;* theoretical terms are nothing more than prediction devices according to such a view. (That is why in "Explanation and Reference" I characterized the view as a form of idealism.) Second, if we recognize that observational terms are themselves *theory-laden,* then it would follow that they too must change in meaning with every change in theory. This would lead to the Kuhnian conclusion that different scientific theories are *meaning incommensurable,* making it unintelligible that one can so much as *understand* earlier scientific theories.[24]

If what "scientific realism" meant to philosophers like myself at the beginning of the 1960s was simply the rejection of positivism and, more generally, of the idea that the statements of the natural sciences require *philosophical reinterpretation,* within a few years it was to develop into an elaborated metaphysical position, or rather a pair of positions (each of which has many versions). The first position, which I shall call "panscientism," holds that philosophical problems are fated, in the end, to be resolved by the progress of the natural sciences, and that the best the philosopher can do is to anticipate that progress and suggest how the sciences can solve them. The second position, for which I shall employ a term introduced by Simon Blackburn but which I will use in a wider sense, I call "quasi-realism." This position does not claim that all philosophical problems will be solved by natural science, but it does hold that the complete description of reality as it is "in itself" is given by natural science and, in most versions of the position, by *physics.* The idea that there is a sharp distinction between the way things are "in themselves" and how they appear to be, or how we speak of them as being, is characteristic of the position. What distinguishes the second position from the first is the idea that many of the ways we speak—and, indeed, *have* to speak—do not correspond to the way things are in themselves but represent "local perspectives." (As Bernard Williams, who introduced the notion of "local perspectives," uses the notion, a "local

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perspective" may be local in the sense of being the perspective of a particular culture—this is how he understands ethical language—or it may be "local" in the sense of depending on our particular human physiology—a "secondary quality," such as color, is supposed to be "local" in this sense.) To the extent that philosophy has to clarify and help us understand the status of these local perspectives, it has tasks over and above those of the natural sciences. The "local perspectives" do not, however, have any real metaphysical significance; only natural science has that. Paul and Patricia Churchland, Daniel Dennett, and Jerry Fodor, notwithstanding their substantial disagreements, are all representatives of the first position; as representatives of the second position, again notwithstanding substantial philosophical disagreements, I will cite Simon Blackburn and Bernard Williams. Of course, not all analytic philosophers are either panscientists or quasi-realists; but these two attitudes have very much come to dominate the scene in "analytic metaphysics." But I am getting ahead of myself.

OXFORD IN 1960

I spent the fall semester of 1960 at Oxford University. The four philosophers with whom I spent the most time were Elizabeth Anscombe, Philippa Foot, Paul Grice, and James Thomson, and not one of them fit the stereotype of a philosopher preoccupied with "the ordinary use of words." Anscombe was interested in just about every question in philosophy, and although she had been a student and close friend of Wittgenstein's, her own philosophical style is markedly different. At that time, she and Philippa Foot were developing a new approach to ethics, one that stresses the evaluation of character rather than the evaluation of actions (it has acquired the name "virtue ethics").[25] Other Oxford moralists (who mostly combined utilitarianism with noncognitivism in ethics) sneered at the new approach, but it continues to flourish today and has enormously enriched moral philosophy. James Thomson was becoming strongly interested in Chomskian linguistics, and, partly for this reason, I was able to persuade him to join me at MIT, where, from 1961 to 1965, I founded a graduate program in philosophy. And within three or four years, Paul Grice developed an approach to the theory of meaning that remains enormously

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influential to this day. It is sometimes said that "ordinary-language philosophy" began to decline about this time; it would be more accurate, I think, to say that the reality never fit the stereotype and that as time went by, the stereotype disappeared. But the individual *figures* at Oxford—one should, of course, add the names of Dummett, Hampshire, Ryle, Strawson, and still others to the list—not only did not disappear from the scene, but they continue to be discussed to the present day. It is just that, with the exception of Ryle (whose *Concept of Mind,* however, contains insights derived from his early interest in phenomenology!), they ceased to be thought of as "ordinary-language philosophers" and began to be treated as individual philosophers with distinctive contributions.

The later career of Paul Grice deserves a bit of description. When I met Grice, he was still grieving for Austin, who had died a few months before, and was, I believe, consciously trying to be a loyal Austinian; but within the next few years he was to break radically with Austin's way of doing philosophy (he would also leave Oxford for Berkeley). One aspect of that break is especially important. Austin's view represented a kind of *radical pragmaticism,* a view that is today represented by the brilliant work of Charles Travis.[26] According to Austin, the meanings of the words in a sentence do not by themselves determine exactly what is being said in a particular context; many different things may be said by using those same words with those meanings.[27] Grice, whose view is widely accepted today (although I myself agree with Austin's view), held that, on the contrary, there is such a thing as the standard meaning of a sentence and that the various "nonstandard" things we can use a sentence to say are all to be explained by what he called "conversational implicatures."[28] Pragmatics studies these conversational implicatures, while semantics, which is sharply distinguished from pragmatics, studies those "standard meanings."

THE RISE OF PANSCIENTISM

In 1961 I resigned from Princeton so that I could create a new graduate program in philosophy at MIT. If the scene (at least among the junior faculty) at Princeton in the 1950s represented the way in which a new generation of American philosophers was beginning to label itself as "analytic," the scene at MIT in the

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years I was there (1961–1965) represented how the content of that label was already changing. Although there were already philosophers at MIT, including Irving Singer and briefly John Rawls, the core of the new program consisted of myself, James Thomson, Judith Jarvis Thomson, and the two "Jerries": Jerry Fodor and Jerrold Katz. The five of us were close to Noam Chomsky, interested in the new "generative" linguistics, and attracted to the idea that computational modeling of the mind, generative grammar, and "semantics" were destined to solve the problems of the philosophy of mind and the philosophy of language (or at least to reformulate them as straightforward scientific problems[29]).

Quine's influence also played a huge role here, as it has continued to do to the present day. Although we thought that Quine's idea that psychology is the successor subject to epistemology was too simple, Quine's insistence that all philosophical problems are problems about the nature and content of science (because all knowledge is either science or aspires to be science) and the idea that philosophical problems about science are themselves to be solved *within* science appealed to us.[30]

Although I was aware of unresolved difficulties, for many years I too followed Quine in portraying logic (and mathematics) as empirical. For Quine, this does not have the same meaning as it does for a traditional empiricist—e.g., John Stuart Mill—that is, it does not mean that mathematics directly concerns the sensible or physical world. Quine is quite happy to posit a world of separately existing mathematical objects, e.g., sets, functions, and numbers. In this respect he is a Platonist of sorts. What it means, and here Quine parts company with more traditional Platonists like Gödel, is that positing the existence of a separate world of abstract entities is ultimately justified by the utility of the posit in *this* world. In line with this view, I defended Quine's "indispensability argument"[31] in the epistemology of mathematics—the argument that the justification for accepting mathematics is simply that it is indispensable in sciences that are unquestionably empirical, in particular in physics. As for the idea that even *logic* is empirical (in the sense of being revisable for empirical reasons), in 1960 the physicist David Finkelstein had persuaded me that the best interpretation of quantum mechanics involved abandoning a traditional logical law, the distributive law of propositional logic[32]—an

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idea first advanced by one of the greatest authorities on quantum mechanics, John von Neumann.[33] I thought that just as Euclidean geometry had been overturned (shown to be empirically false) by General Relativity, so Aristotelian logic had turned out to be empirically false and had similarly been overturned by quantum mechanics. (I eventually had to give up the idea of interpreting quantum mechanics with the aid of von Neumann logic because of insuperable technical difficulties, but that was three decades later.[34])

I left MIT and joined the Harvard philosophy department in 1965. Although every one of my colleagues at Harvard has influenced my thinking, I propose to focus exclusively on three tendencies that are still represented in the Harvard department and that I believe to be important for the development of philosophy (and not just "analytic" philosophy) as a whole. One of these tendencies is virtually identical with the philosophy of an individual, that is, W. V. Quine. The same is true for the second of these tendencies; it is essentially the philosophy of John Rawls. And the third, which has to do with the continuing interest in Wittgenstein's later philosophy at Harvard, was represented by at least three members of the department when I joined it: Rogers Albritton, Stanley Cavell, and Burton Dreben. I shall talk about these tendencies and figures in turn.

QUINE

I have already spoken of the impact of Quine's rejection of the analytic/synthetic distinction and of his "naturalization" of epistemology on the changing climate of American analytic philosophy. Also important was his famous doctrine of the "indeterminacy of translation," defended at book length in *Word and Object*.[35] In the radical form in which Quine defended it, the doctrine implies that there is no fact of the matter as to what any term in a language refers to.[36] At first the doctrine found few converts (most scientific realists, in fact, simply rejected it outright), but a version of it was later to be defended by Donald Davidson and to be (cautiously) endorsed by Bernard Williams.[37] A fourth Quinian doctrine, however, was enormously influential and introduced a significant change in the whole nature of analytic philosophy,

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especially in the United States. This was Quine's doctrine of "ontological commitment."

To explain this doctrine we have to explain Quine's use of "ontology." The ontology of a theory, in Quine's sense, is simply the objects that the theory postulates. But how are we to tell what objects a theory (or the science of a given time) postulates? (Scientists sometimes talk about "glitches"—is science thereby "committed to an ontology" of glitches?) For that matter, what is to count as an "object"? Quine's answer to these questions is straightforwardly in the tradition of the mathematical logician-philosophers, Frege and Russell: ordinary language, these philosophers claim, is too vagrant and idiosyncratic to reveal when scientists postulate objects and which objects they postulate. To answer such questions we must "regiment" our language, as Quine puts it; we must tidy it up (talk of "glitches" will be eliminated as inessential, for example), and we must standardize our idiom (thus "some particles are charged" will turn into something like "there exist some things such that they are particles and they are charged"). Ideally, we should write out the sentences of science (or of the particular theory whose "ontology" we wish to determine) in the notation of quantification theory, the logic of such expressions ("quantifiers") as "there exists an x such that" and "every x is such that." After this has been done, the "ontology" of a theory will be revealed by the use it makes of the existential quantifier ("There exists something such that"). Implicit in this way of thinking, of course, is the assumption that "exists"—or the existential quantifier that replaces it in "regimented notation"—is a completely univocal notion.

If we accept Quine's view, we have to say that modern science commits us to the thesis that *numbers, electrons,* and *microbes* "exist" in exactly the same sense and are "objects" in exactly the same sense.[38] (An "object" is anything that "exists.") Thus Quine has asserted that numbers are "intangible objects,"[39] his reason being that mathematics postulates these intangible objects, and mathematics is indispensable for the greater part of modern science.

Since the days of logical positivism, "metaphysics" had been pretty much a dirty word; even the new scientific realists, like myself, did not say that they were doing metaphysics. Nor did

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Quine claim this in his essay "On What There Is." But what gradually sank in was that if Quine was right in "On What There Is," then one could not claim any longer that the questions "Do numbers really exist?" and "Do sets really exist?" were "pseudo-questions," as the positivists had. And once the question as to the real existence of numbers and sets had been rehabilitated (and Quine, as I mentioned above, had offered his "indispensability argument" for the answer, "Yes, they do"), it was not long before arguments (sometimes in the same style) were offered in connection with such questions as "Do fictional objects really exist?" "Do possible worlds really exist?"[40] and so on. People became comfortable describing themselves as "metaphysicians," something that would have been incompatible with being an "analytic philosopher" only a few years before, and the expression "analytic metaphysics" began to be heard. American analytic philosophy, and later British analytic philosophy, began to have an "ontological style." A curious reversal of roles took place, in which Anglo-American analytic philosophy, after having conceived of itself as anti-metaphysical during the positivist period, came to be the most proudly metaphysical movement on the world philosophical scene.

RAWLS

After the appearance of his monumental *A Theory of Justice* in 1970,[41] John Rawls began to have a highly significant impact on analytic philosophy. The logical positivists had not regarded ethics as a possible subject at all, although there was a subject called "metaethics" (devoted to showing *why* ethics is not a possible subject). Although a few brave analytic philosophers had continued to do ethics during the intervening years (I have already mentioned the "virtue ethics" of Elizabeth Anscombe and Philippa Foot), the field had been rather in the doldrums. With the publication of *A Theory of Justice,* however—which coincided with enormously important debates in American public life about the rightness or wrongness of the welfare state and over the requirements of social justice—ethics became extremely important, and once again large numbers of graduate students began to specialize in it. There is, however, a sense in which the Rawlsian revolution was quite contained. Insofar as *A Theory of Justice* presupposed an

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epistemology at all, that epistemology centered on the notion of "reflective equilibrium." Rawls credited this idea to Nelson Goodman's proposal that what we have to do in philosophy is to give up the futile search for necessary truths and—observing that the principles we in fact have are always in conflict with the ways in which we resolve some of the cases that we treat as clear in real life—engage in a process of "delicate mutual adjustment." In other words, by simultaneous reflection on both the principles and cases with which we start, we have to gradually (and experimentally) revise both the principles and the "intuitions" about the individual cases until we arrive at a stable equilibrium. As a sensible alternative to apriorism, it is hard to quarrel with this; but philosophers concerned with the questions raised by the logical positivists—"How do we know that ethical sentences are not just expressions of subjective attitudes? How do we know that they can have a truth-value (i.e., be either true or false) at all?"—will want a *philosophical argument,* which, in the nature of the case, would seem to have to come from metaphysics, epistemology, or the philosophy of language, against the positivist claim that such sentences are "cognitively meaningless." "Sure you might arrive at what you call 'reflective equilibrium,'" the positivist will say, "but that is just a fact about *you.* Someone else might arrive at a totally different equilibrium."

In subsequent publications, the most recent of which is *Political Liberalism* (1993) but starting already in his Presidential Address to the American Philosophical Association (1974),[42] Rawls denied the need for such a defense of his methodology from epistemology, metaphysics, or the philosophy of language; today the search is for a set of ethical claims whose "objectivity" consists simply in the fact that, in Western democracies with a certain political history, it is possible to find an "overlapping consensus" on their correctness, or on the correctness of ethical ideals and norms that presuppose them—at least, this is the most that the Rawlsian philosopher tries to demonstrate. (The idea being that if citizens agree to bracket their theological and metaphysical disagreements, they can still find a consensus on a number of specific principles of justice.) A normative ethics that disclaims from the outset any concern with metaphysics or epistemology, and which announces that what it is engaged in is "politics, not metaphysics," does not pose any

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kind of threat to the various self-understandings of analytic philosophy, and in particular not to what I earlier described as the "panscientistic" and the "quasi-realist" understandings of the task of philosophy.

It is not the case, however, that all the philosophers who believe that science delivers the whole truth about reality deny the possibility of true statements in ethics. *Some* of them do (e.g., John Mackie and Gilbert Harman argued in well-known books against the possibility of any such thing as ethical knowledge[43]); some tried to develop "in-between" positions—Bernard Williams has argued that while ethical statements can be "true," their "truth" is not absolute but only reflects the perspective of "some social world or other"[44]—but a group of scientific realists led by Richard Boyd, reviving the old naturalist tradition in ethics, have tried to argue that the predicate "good" does, in fact, pick out a "natural kind," concerning which it is possible in principle to build a scientific theory. However, the great majority of philosophers who interest themselves in normative ethics in Anglo-American philosophy departments today probably follow Rawls's lead in avoiding metaphysical controversy.

WITTGENSTEIN AT HARVARD

Three Harvard philosophers were interested in the philosophy of Wittgenstein when I arrived in 1965.[45] There were, naturally, certain differences in their interpretations, some of which I shall describe. But there were also large areas of agreement. In particular, they quickly convinced me that a version of Wittgenstein's later philosophy due to Norman Malcolm that I had criticized in several papers,[46] which made Wittgenstein little more than a disguised positivist, missed the real thrust of that philosophy. Under the influence of these readers I came eventually to see Wittgenstein as doing something quite different from offering a "philosophical position."

The easiest way for me to characterize the way I now understand what Wittgenstein was doing is by means of an example. For analytic philosophers who believe as Quine does that "exists" is a perfectly univocal notion, the questions "Do numbers really exist?" "How do we know that numbers really exist?" and "Is

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Quine's indispensability argument really a good argument?" are all perfectly clear. We understand the meaning of "exist" when it is used in such mathematical statements as "There exist prime numbers greater than a thousand"; hence we must understand "There exist prime numbers" and "There exist numbers." We can ask whether we are justified in accepting mathematics with its "commitment" to "the existence of intangible objects" (the numbers). For Wittgensteinians, however, the idea that when a mathematician asserts that there is a prime number between ten and one hundred he has *asserted that there is an intangible object* with a certain relation to other *intangible objects* is a piece of confusion. While we use the same formal logical rules in operating with the existential quantifier "there exists" in mathematics as in empirical contexts, nevertheless, the use of "existence" statements in mathematics is *enormously* different from the use of empirical existence statements such as "There exist animals that can echolocate." The idea that when we use "there exists" in mathematics we are talking about objects at all, albeit "intangible" ones, is a confusion.

This conclusion is one to which a logical positivist might also come, but his way of arriving at it would be very different. For the positivists, this conclusion is arrived at by applying the analytic/synthetic distinction and the "verifiability theory of meaning." According to the verifiability theory, there are two and only two (quite different) conditions for a statement being "cognitively meaningful," and hence there are two quite different sorts of cognitively meaningful statements. A statement is cognitively meaningful, according to the positivist's original verifiability theory, if either it can be empirically tested or it is decidable by purely logical and mathematical means.[47] For the positivist, it follows at once that mathematical existence statements belong to a wholly different class than empirical existence statements. The former are "analytic" and the latter are "synthetic" or "empirical" (the positivists treated the latter terms as synonyms). But in his later philosophy, Wittgenstein rejected the idea that there is such a thing as "the" criterion of meaningfulness. The Wittgensteinian has to begin with the sense most of us have that there is something extremely "fishy" about calling the number five an "intangible object" and getting worried about whether it "really exists," and

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explore very carefully and patiently why it is that we feel impelled to talk this way, and feel that unless we can talk this way, then mathematics totters. For the Wittgensteinian, the idea that the "indispensability argument" is *really* analogous to the experimental proofs a physicist offers for the existence of an unobserved particle is just an additional manifestation of the same confusion.[48]

This connects with the question of whether philosophical questions really are similar to questions in empirical science. Like Wittgenstein, I believe that the answer is "no," but after Quine's powerful attacks on the analytic/synthetic distinction, we who agree with this answer will have to show that it is not inconsistent to say that an investigation is conceptual *and* characterized by fallibility;[49] the claim that philosophy is able to arrive at *any* species of *infallible* knowledge is simply no longer credible.

Some interpreters of Wittgenstein, including Burton Dreben, tend to stress the moment in Wittgenstein's philosophy in which a question of traditional philosophy, or a "conclusion" of traditional philosophy, is revealed as a confusion. Their purpose is not, of course, to replace traditional philosophy with a new system, like the many systems of thought produced by logical positivism in the course of its development, but to free us of the illusion that we have here a set of important issues. In so doing, they are, I believe, making a point of fundamental importance, but one that is easy for present-day philosophers to misunderstand. I say "present-day philosophers" because the idea that some philosophical problems are illusory is not a new one in the history of philosophy; it plays a central role in as pivotal a work as Kant's *Critique of Pure Reason*. But for the most part the philosophers who find Wittgenstein's thought difficult to grasp are people who have little time for Immanuel Kant. In *their* memories, the idea that there are "pseudoproblems in philosophy" is inextricably linked to the name of Rudolf Carnap and to logical positivism. Thus, it is natural for them to suppose that the Wittgensteinians' denial of the intelligibility of certain philosophical issues *must* stem from a commitment to the positivist "verifiability theory of meaning," even if they deny that it does. That one can come to see that a philosophical issue is a pseudo-issue by *working through the considerations that seem to make it not only genuine but somehow obligatory,* and not by bringing a "criterion of cognitive significance" to bear

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on it from the outside, is something that can take someone with training in analytic philosophy a long time to see (it certainly took *me* a long time to see), and Dreben has the remarkable ability to convey this Wittgensteinian insight to students (including his colleagues).

Yet there is another, not incompatible but perhaps supplementary, way of seeing the upshot of Wittgenstein's later philosophy. For Stanley Cavell's Wittgenstein, philosophical confusions are not just matters of language gone wrong, but an expression of deep human issues that also express themselves in a variety of other ways—political, theological, and literary.[50]

In this connection, I would remark that many of the problems Wittgenstein discusses have to do with our uneasy relation to the normative. By the "normative" I do not mean just *ethics*. Consider the normativity involved in the notion of following a rule. That there is a right and a wrong way to follow a rule is what Wittgenstein would call a "grammatical" truth; the notion of a rule goes with the notions of doing the right thing and doing the wrong thing, or giving the right answer and giving the wrong answer. But many philosophers feel that they have to reduce this normativity to something else; they seek, for example, to locate it in the brain, but then it turns out that if the structures in the brain lead us to follow rules correctly, some of the time they also lead us to follow them incorrectly. (One can, of course, say with the Chomskians that there is a difference between the brain's "competence" and its "performance," but this is just to say that even in describing the brain we have to employ normative distinctions; what it means to follow a rule *correctly* is not really *explained* by saying, "One follows a rule correctly when one's brain behaves according to its competence, and one follows it incorrectly when one's brain makes a performance error." One is just *restating* the fact that one started with—the fact of the normativity of rule following—in a special jargon.) In the past, philosophers who saw that reductive accounts of rule following did not work either posited mysterious mental powers or Platonic entities to which the mind was supposed to have a mysterious relation. Both in the case of the scientistic reductionist and the old-fashioned metaphysician, the impulse is the same: to treat normativity, that is, the rightness of going one way as opposed to another, as if it were a *phenomenon* standing

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in need of a *causal* explanation (either an ordinary scientific explanation or a, so to speak, "superscientific" explanation). Wittgenstein's response was to challenge the idea that normative talk needs to be "explained" in one of these ways, indeed, to challenge the idea that there is a problem of "explanation" here.

From the outset of *Philosophical Investigations,* comfort and discomfort with the normative are associated with comfort and discomfort with the messiness of language—with the fact that language that is perfectly useful in its context may utterly fail to satisfy the standards of "precision" and "clarity" imposed by philosophers and logicians; indeed, with our desire to deny all of this messiness, to force language and thought to fit one or another impossibly tidy representation. "Cognitive scientists" (or philosophers who think of themselves as such), in particular, often speak as if there were an *essence* of believing, as if, for example, believing something were a matter of "the brain's putting a sentence in its *belief box.*" (I kid you not.) At the beginning of *Philosophical Investigations,* Wittgenstein emphasizes that such words as "believe," "question," and "command" represent (practically speaking) many different things. The desire in contemporary scientific realism to represent all questions as of one kind, as, in effect, empirical questions, and all justifications as of one kind, as empirical justifications, is simply another manifestation of the tendency to force a single representation on what is in no sense one unified phenomenon. Wittgenstein wants not to clarify just our concepts, but to clarify *us*; and, paradoxically, to clarify us by teaching us to live, as we must live, with what is unclear. On such a reading, a concern with Wittgenstein and a concern with personal and social transformation are not only not incompatible, but they can reinforce one another.

"THE MEANING OF 'MEANING'"

The ideas that I have just described did not substantially affect my thinking until the 1980s. But in 1966–1967, first in a philosophy of language class and then in lectures given at an NEH Summer Institute on the Philosophy of Language, I began to develop some new ideas about meaning—ideas not at all inspired by a desire to see how natural science could solve philosophical problems but

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rather by a negative reaction to the views that I had held when I was at MIT. According to those views, speakers' knowledge of the meaning of their words amounted simply to the tacit knowledge of a battery of "semantical rules" stored "inside their heads." What I had come to realize by 1966 was that the whole image of language as something that is entirely "inside the head" of the individual speaker had to be wrong. A number of considerations, which I will not review here, convinced me that the familiar comparison of *words to tools* is wrong, if the "tools" one has in mind are tools that one person could in principle use in isolation, such as a hammer or a screwdriver. If language is a tool, it is a tool like an ocean liner, which requires many people cooperating (and participating in a complex division of labor) to use. What gives one's words the particular meanings they have is not just the state of one's brain, but the relations one has to both one's non-human environment and to other speakers.

While this idea at first fell on more or less deaf ears, when I presented it at much greater length in a paper I wrote at the end of 1972 entitled "The Meaning of 'Meaning,'"[51] it found a surprisingly warm reception (partly because of its consonance with ideas in Kripke's celebrated lectures on "Naming and Necessity,"[52] which had been given in 1970 at Princeton), and at least this much of it—the idea that any complete account of meaning must include factors outside the head of the speaker—may by now even have become "orthodoxy" in the philosophy of language. (Please note, however, that this particular view did not arise from the program of "scientific realism" to which I had previously felt such strong attraction.) Also, starting about 1972, I became preoccupied with a problem with which Quine had long been concerned: how (and Quine would say, *if*) words could have determinate reference at all.

REFERENCE AND MODEL THEORY

The way in which this came to be a problem for me was as follows: like most people who subscribed to the computational model of the mind, I believed that when we see or hear events in the world, what actually happens is that certain visual or auditory sense data are produced in our minds/brains. Those sense data are

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what the mind/brain processes *cognitively*. The relation between the tables and chairs we perceive and the sense data is, on this picture, simply a matter of causal impacts on the retina and on the eardrum, and of causal signals from the retina and the eardrum to processors in the brain; we have no direct *cognitive* relation to the objects of perception. Our sense data are, as it were, the *interface* between our cognitive processes and the world. (This is what Descartes's picture of the mind turns into when the mind is identified with the brain.) The possibility of holding that what we are immediately aware of in veridical perception is genuine properties of external things and not "representations" is one that I categorically rejected. On this neo-Cartesian picture of the mind, there seemed to be no problem as to how the mind (conceived of as a computer) could know the "subjective experiences" (the sense data) the person has, since these were supposed to be events inside the computer itself and thus "available" to the computer. But what does it mean to say that these experiences "represent" objects *outside* the mind/computer?

As we have seen, most analytic philosophers had repudiated the positivist view, according to which a scientific theory is basically a device for predicting subjective experiences. Yet on the philosophy of mind that I and other analytic philosophers found attractive, it was hard to see how the mind's understanding of a scientific theory could really go *beyond* what the positivists would allow. One can understand how the mind, conceived of as a computer, can "understand" a scientific theory in the sense of being able to use it as a prediction device; but how can it understand a scientific theory "realistically" (i.e., understand terms like "atom" and "microbe" as referring to real things) in the way I had been calling for ever since my essay "What Theories Are Not"?

At this point, certain results in mathematical logic occurred to me. Without going into technical details, it turns out that if there is a correspondence between terms in a language and things in the world at all (e.g., the relation of reference that we supposedly all have in mind), then there are infinitely many different ones that *make the same sentences true* (and not only in the actual world, but in all possible worlds!).[53] It at once follows that if there is a fact of the matter as to which correspondence is *the* relation of "reference" between words in my theory and items in the world,

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then that fact cannot be ascertained simply by making predictions and testing them. If A and B are two different correspondences such that it would make no difference to the truth of any sentence (in any possible world) whether A or B is *the* reference-relation, then, in particular, *no empirical test can possibly determine whether A or B is the "right" relation.* The very idea of a "right relation" threatens to become hopelessly metaphysical. Yet Quine's characteristically bold way of dealing with the problem, which is to deny that there is a "fact of the matter" about what our words refer to, has never been one I could accept.[54] On Quine's view, as he himself puts it, when I think I am referring to my cat Tabitha (or to my wife, or to my friend, or to myself) there is no fact of the matter as to whether my words designate Tabitha or "the whole cosmos minus the cat."[55] It has always seemed to me that a view that is so contrary to our whole sense of being in intellectual and perceptual contact with the world *cannot* be right.

Scientific realism seems only to exacerbate rather than resolve these deep problems, because for scientific realists there are only two possibilities: either reduce reference to notions employed in the physical sciences, which seems impossible, or say (with Quine) that it is an illusion that there is a determinate relation of reference. I began to move away from hard-core scientific realism partly for this reason and partly because I was discovering the important work of a philosopher who has always insisted that understanding the arts is as important as understanding science in understanding cognition. That philosopher is Nelson Goodman. I found myself agreeing with Goodman's insistence that the world does not have a "ready-made" or "built-in" description; many descriptions may "fit," depending on our interests and purposes. (This does not mean that anything we happen to like "fits." That more than one description may be right does not mean that every description is right, or that rightness is subjective.) While I could not agree when Goodman went so far as to say that there is not one "world" but many worlds and that these are of our own making,[56] I still find his work a continuing source of stimulation. At this time I also began to take seriously an idea that I had first heard from my pragmatist teachers at the University of Pennsylvania and UCLA: the idea that "value judgments," far from being

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devoid of "cognitive meaning," are actually presupposed in all cognition; fact and value interpenetrate.

This was the context in which I was led to put forward my first attempt at a middle way between antirealism and metaphysical realism ("internal realism") in the 1970s and 1980s.[57] While I still defend some of the ideas that were involved in that attempt (the denial that reality dictates one unique description and the conception of fact and value as interpenetrating rather than discrete are as central to my thinking now as they were then), the project as a whole now seems fatally flawed by its allegiance to the traditional conception of our sensations as an "interface" between us and the world.[58]

I was by no means the only philosopher who was beginning to be troubled by these problems. Michael Dummett was thinking about some of the same issues and attempting to develop a form of verificationism free of the phenomenalistic strain we saw in positivism. And in the same period Richard Rorty broke with scientific realism and moved in a direction that he associated first with Derrida's "deconstruction" and later with American pragmatism.[59] Like Quine, Rorty rejects the idea that there is any determinate reference relation between words and things, but (unlike Quine) he holds that statements of science have no greater right to be called "true" than statements that give us satisfaction in any one of a variety of other ways. "True," for Rorty, is simply an adjective we use to "commend" beliefs we like.

Although I could not accept Dummett's "verificationist semantics," and Rorty seemed to me perilously close to giving up the idea that there is a world out there at all, I was pleased that they saw some of the same difficulties with what had become the standard realist metaphysics in analytic philosophy that I was seeing.

THE HISTORY OF PHILOSOPHY RETURNS

I have described the way in which I came to see that philosophical difficulties about "how language hangs on to the world" are not going to be solved by further investigations in natural science, including computational models of the mind. This is something that the distinguished Canadian philosopher Charles Taylor had

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long argued, and Taylor, in particular, had insisted that those difficulties arise because certain ways of thinking seem obligatory to us. It is his contention that without an investigation into the *history* of that obligatoriness, an investigation that tries to uncover the genealogy of the conceptual changes that made Cartesianism (or Cartesianism *cum* materialism) seem the only *possible* way of thinking about the mind, we can never come to see how *contingent* some of the assumptions that generate our problems are; as long as we do not see *that,* we will remain stuck in those problems. This was, however, something that I was not yet ready to hear at the end of the 1970s when I was writing *Reason, Truth and History.* In 1980, however (influenced by Richard Rorty), I began to make a serious study of William James and was at once struck by James's insistence that the picture of our experiences as "inside" our minds (or our "heads") is an error. Earlier I had been aware of the possibility of denying the interface conception from a reading of Austin's *Sense and Sensibilia* but had rejected the idea. But when I rethought the issue in the 1980s, it became clear to me that (even if James's approach contained some untenable elements) James was right in thinking that the traditional conception must be given up.[60] In addition (with Ruth Anna Putnam) I began to study John Dewey's voluminous writings, which provide a way of thinking about ethical inquiry that avoids many of the standard dichotomies (absolute versus relative, instrumental versus categorical, and so on).[61]

About that time, I became aware that a philosopher I enormously respected, John McDowell, was urging the rejection both of the neo-Cartesian picture of the mind and of the fact/value dichotomy in which analytic philosophy seemed, for the most part, to be stuck. For many years, McDowell's views were available only in the forms of occasional articles and talks, but in 1991 he delivered the John Locke Lectures at Oxford (now published as *Mind and World*). My own Dewey Lectures on a number of related issues were delivered at Columbia University in 1994. In both sets of lectures a non-Cartesian view is spelled out with full reference to the history of philosophy (as urged by Charles Taylor). The long dominance of the idea that "philosophy is one thing and history of philosophy is another" is now visibly coming to an end. Or is this too optimistic an estimate?

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THE (NON-)RECEPTION OF CONTINENTAL PHILOSOPHY

I cannot close without mentioning a feature of Anglo-American analytic philosophy that will not have escaped the notice of even a minimally-informed observer: the exclusion of "continental philosophy." (The leading Ph.D.-granting institutions rarely include texts by Foucault or Derrida in their courses, and the work of Jürgen Habermas has only begun to receive attention—and then usually only in ethics courses—fairly recently.) At first blush, this might seem astonishing; after all, philosophy is classified as one of the humanities, and French "theory" is taken very seriously indeed in the *other* humanities. This indifference of analytic philosophy departments to what interests the other humanities departments is not surprising, however, when one realizes that the self-image of analytic philosophy is scientific rather than humanistic. If one aspires to be a science (even if what one actually writes is closer to science fiction), then being different from the humanities will seem a positive virtue. Of course, not all philosophers in analytic departments are happy with this state of affairs. (A few respected figures, for instance, have studied and taught Husserl's phenomenology, or Habermas's philosophy, or even Heidegger's philosophy for years.) However, most analytic philosophers justify excluding texts by the authors just mentioned on the ground that these authors are "not clear" or that the texts (which they may not actually have read) "do not contain arguments." They do not admit that their own conception of philosophy is scientistic; usually, when analytic philosophy is criticized, its defenders equate their style of philosophy simply with "argument" and "clarity." But the Tractarian doctrine that "whatever can be said at all can be said clearly" has become dogma; since the demise of the notion of "logical form" on which the *Tractatus* was based, I have never heard anyone actually offer an argument for it. Good prose, whatever its subject, must communicate something worth communicating to a sensitive reader. If it seeks to persuade, the persuasion must not be irrational (which does not exclude the possibility that what is involved may be an appeal to *see* something one is refusing to see—say, the appeal of a way of life, or what actually goes on in our linguistic, or scientific, or ethical, or political practices, and not simply a deduction from already accepted premises, or the

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presentation of evidence for an empirical hypothesis). The demand that we only say what can be said in the sort of prose that Bertrand Russell wrote, marvelous as that prose was, will, in fact, necessarily limit what one can talk about.

SHOULD ANALYTIC PHILOSOPHY CONTINUE?

This account of the changes in the character of American philosophy in the half century that I have been able to witness it is, necessarily, from one perspective. I am aware that I have underemphasized some brilliant contributions; the work of Donald Davidson, Saul Kripke, David Lewis, Robert Nozick, and others has, for example, received at best passing mention. In partial exoneration, let me say that my concern has been to trace the rise of what seem to be the currently dominant tendencies and the beginning of a move away from what I see as the errors of those tendencies. Davidson, Kripke, and Nozick have affected those developments but in ways that it would not be easy to describe in a narrative such as this. Because I have been largely critical of the scientific realist tendency and its relatives (e.g., Bernard Williams's materialism *cum* perspectivalism and Blackburn's "quasi-realism"), it may also seem that I am calling for the end of analytic philosophy, and that is a matter about which I must say a word.

If "analytic philosophy" means, simply, philosophy that is informed by a knowledge of science, a knowledge of the achievements of modern logic, and a knowledge of the great works of past analytic philosophers from Russell, Frege, Reichenbach, and Carnap through the present, then I am most certainly *not* calling for an end to it. I am concerned about certain tendencies in analytic philosophy—by the tendency to scientism, the tendency to patronize the history of philosophy, the refusal to *hear* other sorts of philosophy—but fighting those tendencies is not the same thing as fighting analytic philosophy. As a philosopher whose own writing is full of references to Frege, Russell, Wittgenstein, Quine, Davidson, Kripke, David Lewis, and others, I count myself as an "analytic philosopher" in that sense. But, to mention one last tendency of which I disapprove, I see the tendency to think of analytic philosophy as a "movement" (a tendency that has led to the creation of new—and exclusionary—associations of analytic philosophers in

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several European countries) as a bad thing. From my point of view, the only legitimate function for "movements" in philosophy is to gain attention and recognition for ideas that are not yet being received or which have been neglected or marginalized. Analytic philosophy has been around a long time, and it is certainly one of the dominant currents in world philosophy. Making it into a "movement" is not necessary, and it only preserves the features I have deplored. Just as we can learn from Kant without calling ourselves Kantians, and from James and Dewey without calling ourselves pragmatists, and from Wittgenstein without calling ourselves Wittgensteinians, so we can learn from Frege and Russell and Carnap and Quine and Davidson without calling ourselves "analytic philosophers." Why can we not just be "philosophers" without an adjective?

ENDNOTES

[1]The views I describe as the *supposed* central tenets of the movement are defended in A. J. Ayer's famous popularization of logical positivism, *Language, Truth and Logic* (London: V. Gollancz, 1936). They have come to constitute the stereotype of what a logical positivist believed. This stereotype is accurate to this extent: the logical positivists believed that metaphysical statements are nonsense (although they did not agree on just which statements *are* "metaphysical") and that they can be distinguished from "cognitively meaningful" statements (the statements of science) by the fact that they are either empirically testable or decidable by appeal to logic (in which they included mathematics) and definitions (see endnote 47). Ethical statements, and value judgments generally, were also considered to be "nonsense" if they were thought of as expressing truths about the world, although they could be allowed a second-class sort of meaning if considered as "emotive" expressions, i.e., ways of expressing an attitude, appeals to others to share an attitude, and so on.

[2]That all positivists believed that all empirical truths are "about" sense data is perhaps the most persistent misconception. Even Rudolf Carnap's celebrated "Aufbau," in *Der Logische Aufbau der Welt* (Berlin: Weltkreis-Verlag, 1928), only claimed that sense data provide *one* possible way of reconstructing the statements of science, and Hans Reichenbach, *Experience and Prediction* (Chicago, Ill.: University of Chicago Press, 1938), was undisguisedly hostile to this sort of phenomenalism.

[3]In fact, the idea that all scientific concepts—in particular, observational ones—are theory laden figures significantly in papers by Neurath and Reichenbach from the early 1920s on.

[4]This is a misconception for which Thomas Kuhn's influential *The Structure of Scientific Revolutions,* 2d ed. (Chicago, Ill.: University of Chicago Press, 1970) is

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perhaps chiefly to blame. Both Reichenbach and Carnap came into philosophy in the wake of a scientific revolution, the Einsteinian one, and the central question of Hans Reichenbach's *The Theory of Relativity and A Prior Knowledge* (Berlin: J. Springer, 1922; Berkeley, Calif.: University of California Press, 1965) was precisely how to account for scientific revolutions without being forced into the idea that theories before and after such a revolution are "incommensurable," which Kuhn was later to advocate.

[5]"On What There Is" (1948), collected in W. V. Quine, *From a Logical Point of View* (Cambridge, Mass.: Harvard University Press, 1953).

[6]"Two Dogmas" (1950), collected in Ibid. Because the positivist way of allowing some empirically untestable statements—the statements of pure mathematics—into the class of the "cognitively meaningful" while banning "metaphysics" depended on a sharp, analytic/synthetic distinction, Quine's critique of the distinction helped to make the "science/metaphysics" distinction suspect among philosophers.

[7]"The Scope and Language of Science" (1957), collected in W. V. Quine, *The Ways of Paradox* (Cambridge, Mass.: Harvard University Press, 1976); and "On Epistemology Naturalized," in W. V. Quine, *Ontological Relativity* (New York: Columbia University Press, 1969).

[8]This dichotomy figured in Carnap's work after about 1939. It was assumed that "observation terms" (e.g., "blue," "touches") referred only to observables, and that the distinction between statements that function as observation reports and statements that function as theoretical postulates could be drawn as follows: the former contain only observation terms, while the latter must contain at least one theoretical term. In Hilary Putnam, "What Theories Are Not" (1960), collected in Hilary Putnam, *Mathematics, Matter and Method* (Cambridge: Cambridge University Press, 1975), it was shown that both of these assumptions are untenable.

[9]Cf. Michael Friedman, "Logical Positivism Re-Evaluated," *Journal of Philosophy* LXXXVIII (10) (October 1991): 505–519.

[10]See G. A. Reisch, "Did Kuhn Kill Logical Empiricism?" *Philosophy of Science* 58 (1991): 264–277. My thanks to Gerald Holton and Jordi Cat for this reference.

[11]Morton White was not a positivist, but he took positivism seriously, as well as Oxford philosophy and American pragmatism.

[12]I have not seen Wittgenstein as an "ordinary-language philosopher" in this sense for a very long time, and I regard Austin's real importance as quite transcending his allegiance to this idea.

[13]White, who played a role in the change at Harvard, was both the youngest and the most recent arrival in the department (see also endnote 11).

[14]Cornell University, in particular, had a "Wittgensteinian" philosophy department for a number of years.

[15]As mentioned in endnote 6, in the context of the debates over logical positivism, Quine's attack on the analytic/synthetic distinction also undermined the whole idea of a "science/metaphysics" distinction.

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[16]Noam Chomsky, *Syntactic Structures* ('s Gravenhage: Mouton, 1957).

[17]The recursive functions are a class of functions that, according to a thesis ("Church's Thesis") argued for by Alonzo Church and Alan Turing in the 1930s, exactly comprises the functions that a computer can in principle compute. Chomsky's linguistic theory has retained two central contentions in all its various forms: *1)* that the grammatical structures of a natural language are much more complex than traditional grammars ("phrase structure grammars") allowed, but *2)* that these more complex structures can still be described by using a formalism for the theory of recursive functions (and computational processes in general) first devised by Emil Post, an American contemporary of Turing's.

[18]Paul Ziff, *Semantic Analysis* (Ithaca, N.Y.: Cornell University Press, 1960). Today, graduate students generally suppose that this idea originated with Donald Davidson, "Truth and Meaning," *Synthese* XVII (3) (1967), and Ziff's book is, sadly, seldom recalled.

[19]"Turing machines" are abstract devices (at least they existed only as mathematical abstractions when Alan Turing described them in the 1930s) that form the basis for the modern theory of computation.

[20]"Minds and Machines," collected in Hilary Putnam, *Mind, Language and Reality* (Cambridge: Cambridge University Press, 1975).

[21]Putnam, "What Theories Are Not." This influence was partly due to a certain confluence of attacks on the Carnapian view. As Frederick Suppe describes what happened, in the preface to *The Structure of Scientific Theories* (Urbana, Ill.: University of Illinois Press, 1974), there were attacks of two sorts: "First there were attacks on specific features of the received view. . .designed to show they were defective beyond repair [this is how he classifies my attack]. Second, there were alternative philosophies of science advanced [Hanson, Kuhn, and Toulmin] which rejected the received view out of hand and proceeded to argue for some other conception of science and of scientific knowledge." Ibid., 4.

[22]This logical vocabulary was sometimes allowed to include the resources of higher-order logic or, alternatively, set theory. Cf. Rudolf Carnap, "The Methodological Character of Theoretical Concepts," in H. Feigl and M. Scriven, *Minnesota Studies in the Philosophy of Science,* vol. I of *The Conceptual Foundations of Psychology and Psychoanalysis* (Minneapolis, Minn.: The University of Minnesota Press, 1956).

[23]Reprinted as Putnam, *Mind, Language and Reality,* chap. 11.

[24]Donald Davidson's "The Very Idea of a Conceptual Scheme," *Proceedings and Addresses of the American Philosophical Association* 67 (1973–1974), contains a powerful (and celebrated) argument against the coherence of the idea of meaning incommensurability.

[25]Anscombe's "Modern Moral Philosophy" (1958) represented a call for this new approach. This is collected in G. E. M. Anscombe, *Ethics, Religion, and Politics,* vol. 3 of *The Collected Philosophical Papers of G. E. M. Anscombe* (Oxford: Blackwell, 1981).

[26]Charles Travis, *The Uses of Sense: Wittgenstein's Philosophy of Language* (Oxford: Oxford University Press, 1989).

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[27]Consider, for example, the sentence "The table is covered with coffee." This may, depending on the context, mean that there are cups of coffee on the table, or that coffee has been spilled on the table, or that there are bags of coffee on the table. Yet in all of these uses, "coffee," "table," and "covered" have their standard "meanings."

[28]For a powerful critique of Grice's view, see Charles Travis, "Annals of Analysis," *Mind* 100 (398) (April 1991): 237–264.

[29]For example, the traditional mind-body problem was supposed to become simply the problem of the relation of the brain's "software" to its "hardware."

[30]See Quine, "The Scope and Language of Science" and Quine, "On Epistemology Naturalized."

[31]Cf. Hilary Putnam, *Philosophy of Logic* (1971), reprinted as part of Putnam, *Mathematics, Matter and Method,* 2d ed.; and Quine, "On What There Is."

[32]In its simplest form, this says that "p(q or r)" is equivalent to "pq or pr."

[33]See Hilary Putnam, "Is Logic Empirical?" (1968), reprinted as "The Logic of Quantum Mechanics" in Putnam, *Mathematics, Matter and Method.*

[34]For an account of these difficulties, see Hilary Putnam, "Reply to Michael Redhead," in P. Clark and R. Hale, eds., *Reading Putnam* (Oxford: Blackwell, 1994).

[35]W. V. Quine, *Word and Object* (Cambridge, Mass.: Technology Press of the Massachusetts Institute of Technology, 1960).

[36]A famous Quinian thought experiment, often used to illustrate the doctrine, involves the idea of encountering natives who speak a hitherto unknown language and who appear to call rabbits *gavagai.* In *Word and Object,* Quine argued that we could translate all occurrences of *gavagai* in the "jungle language" as an "undetached rabbit part" and make "compensatory adjustments" elsewhere in our translation scheme, and the resulting translation scheme would still fit all possible behavioral data. If we assume that the reference of words is publicly accessible from behavioral data (otherwise how can language be learned?), Quine asked, does this not show that there is "no fact of the matter" as to whether *gavagai* means "rabbit" or "undetached rabbit part"?

[37]In particular, Bernard Williams, *Descartes: The Project of Pure Enquiry* (Harmondsworth: Penguin, 1978), 299.

[38]Since contemporary mathematicians would accept the sentence, "There exist numbers greater than twenty-five," and contemporary biologists would accept, "There exist microbes that cause diseases in humans," Quine's criterion of ontological commitment implies that these scientists are "committed to the existence of" numbers and microbes, and since existence is univocal, numbers and microbes exist (if contemporary science is right) in exactly the same sense.

[39]"Success and Limits of Mathematization," collected in Quine, *Theories and Things* (Cambridge, Mass.: Harvard University Press, 1981), 149.

[40]Cf. David Lewis, *Counterfactuals* (Cambridge, Mass.: Harvard University Press, 1973), 84–91.

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[41]John Rawls, *A Theory of Justice* (Cambridge, Mass.: Belknap Press of Harvard University Press, 1970).

[42]John Rawls, *Political Liberalism* (New York: Columbia University Press, 1993); John Rawls, "The Independence of Moral Theory," Presidential Address to the American Philosophical Association, Eastern Division, 1974, in *Proceedings and Addresses of the American Philosophical Association* 48 (1974–1975): 5–22.

[43]J. L. Mackie, *Ethics: Inventing Right and Wrong* (Harmondsworth: Penguin, 1977); Gilbert Harman, *The Nature of Morality: An Introduction to Ethics* (Oxford: Oxford University Press, 1977).

[44]Bernard Williams, *Ethics and the Limits of Philosophy* (Cambridge, Mass.: Harvard University Press, 1985).

[45]The three are Rogers Albritton, Stanley Cavell, and Burton Dreben; having retired from Harvard, Dreben now teaches at Boston University, but my younger colleague Warren Goldfarb, in addition to doing distinguished work in logic and in the history of analytic philosophy, also helps to continue the Harvard tradition of Wittgenstein studies today.

[46]See Putnam, *Mind, Language and Reality,* chaps. 15, 16, 17.

[47]After the discovery (by Kurt Gödel) that there are undecidable sentences in all systems of pure mathematics, the positivists made various complicated adjustments in their criterion to avoid having to say that any sentences of pure mathematics are cognitively meaningless. However, it is not my purpose to go into those adjustments here.

[48]See "Rethinking Mathematical Necessity" in Hilary Putnam, *Words and Life* (Cambridge, Mass.: Harvard University Press, 1994).

[49]Quine's attacks on the analytic/synthetic distinction were attacks on the positivist idea that a certain class of statements (the analytic ones) is in principle immune to empirical refutation. Philosophers who suppose that Quine disproved the very possibility of a distinction between conceptual knowledge and empirical knowledge (e.g., Richard Rorty) are tacitly assuming that any workable notion of conceptual knowledge must resemble the positivist notion of analytic truth in being *unrevisable.* But in *On Certainty,* Wittgenstein remarked that the distinction between a river and its banks is an important one, even if in time the banks and the river change places and even if some banks are more friable than others. He meant that there is a difference between "grammatical" (conceptual) and empirical claims, even if the status of a given claim changes with time, and even if some "grammatical" claims turn out to be mistaken.

[50]Among Stanley Cavell's recent writings on this theme are *Conditions Handsome and Unhandsome: The Constitution of Emersonian Perfectionism* (Chicago, Ill.: University of Chicago Press, 1990); *In Quest of the Ordinary: Lines of Skepticism and Romanticism* (Chicago, Ill.: University of Chicago Press, 1988); and *Philosophical Passages: Wittgenstein, Emerson, Austin, Derrida* (Oxford: Blackwell, 1995).

[51]Collected in Putnam, *Mind, Language and Reality.*

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[52]Saul Kripke, *Naming and Necessity* (Cambridge, Mass.: Harvard University Press, 1972, 1980).

[53]See Hilary Putnam, *Reason, Truth and History* (Cambridge: Cambridge University Press, 1981).

[54]W. V. Quine, *Word and Object* (Cambridge, Mass.: MIT Press, 1960); W. V. Quine, *Ontological Relativity and Other Essays* (New York: Columbia University Press, 1969).

[55]W. V. Quine, *Pursuit of Truth* (Cambridge, Mass.: Harvard University Press, 1990), 33.

[56]Nelson Goodman, *Ways of Worldmaking* (Indianapolis, Ind.: Hackett, 1978). For a debate about this view among Goodman, Hempel, Scheffler, and myself, see Peter J. McCormick, ed., *Starmaking: Realism, Anti-Realism, and Irrealism* (Cambridge, Mass.: MIT Press, 1996).

[57]See Putnam, *Reason, Truth and History*; see also Hilary Putnam, *The Many Faces of Realism* (LaSalle, Ill.: Open Court, 1987).

[58]Cf. Hilary Putnam, "Reply to Simon Blackburn" in Clark and Hale, eds., *Reading Putnam*, and Hilary Putnam, "The Dewey Lecture 1994: Sense, Nonsense and the Senses: An Inquiry into the Powers of the Human Mind," *The Journal of Philosophy* XCI (2) (September 1994), for what I now think was right and wrong about "internal realism."

[59]Richard Rorty, *Consequences of Pragmatism: Essays, 1972–1980* (Minneapolis, Minn.: University of Minnesota Press, 1982); Richard Rorty, *Philosophy and the Mirror of Nature* (Princeton, N.J.: Princeton University Press, 1979).

[60]Cf. "James' Theory of Perception" (1988), collected in Hilary Putnam, *Realism with a Human Face* (Cambridge, Mass.: Harvard University Press, 1994).

[61]See Putnam, *Words and Life*, pt. III.

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