The UK edition of this book by Harvester Press was pulped under threat of legal action on comments on a book from Cambridge Univ press (p 269).

[illegible] the swamp of unreason. What are the philosophers doing to help?

IN PURSUIT OF TRUTH

Essays on the Philosophy of Karl Popper on the Occasion of His 80th Birthday

Edited by
PAUL LEVINSON

with Forewords by
Isaac Asimov
and
Helmut Schmidt
Chancellor,
Federal Republic of Germany

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hope and no proof is Stoicism and Judeo-Christian religion, in which irrationalism and stoicism alternate. Yet there is no inconsistency in the philosophy of hope without proof, especially as a moral injunction to offer the benefit of doubt wherever at all possible. Now, the philosophy of hope without proof is not *exactly* Popper's. It began with T. H. Huxley, the famous militant Darwinian, and H. G. Wells. Its best expression is in Bertrand Russell's "Free Man's Worship," the manifesto of hope born out of despair, clinging to both despair and reason most heroically. This famous paper, published in Russell's *Philosophical Essays* of 1910, is a sleeper. It was hardly noticed by contemporaries and followers, yet now it is the expression of current religion, of current scientific ethos. Here, I think, Popper's philosophy of science gave it substance.

Or perhaps Russell's "Free Man's Worship" gives Popper's philosophy of science its proper bearings, its proper metaphysical framework. I cannot say. For my part, I do not mind that I have no sense of security, no proof, no anchor, but I do not like the sense of not seeing the wood for the trees, so I like to have some idea of the wood, however fanciful, and attend only the details that my idea directs me to or that are used by critics of this idea. This gives me a great sense of peace in my lack of orientation: I may be moving in the world with the wrong map, but I am eager to improve it if and when I can, and I seek friends who can criticize me. For this I am grateful to Karl Popper.

NOTES

0. I wrote this essay while I was an Alexander von Humboldt Stiftung senior fellow resident at the Zentrum für Interdisziplinare Forschung, Bielefeld, Germany.

A Popperian Harvest[0]

W. W. BARTLEY, III

> It is by its methods rather than its subject-matter that philosophy is to be distinguished from other arts or sciences.
>
> A. J. Ayer, 1955[1]

> Philosophers are as free as others to use any method in searching for truth. *There is no method peculiar to philosophy.*
>
> K. R. Popper, 1958[2]

I. A Difficult Man

1. Late one afternoon in the early winter of 1960, I was sitting with Karl Popper in the waiting room of his doctor's office on Harley Street in London. Popper loved to spend time with his students. To cram in conversation with us, he used his spare moments to the full. So we would tag along with him everywhere—to the doctor's and dentist's offices, back and forth to the train station, on walks, in taxis or on the underground—talking philosophy incessantly.

That afternoon we had been talking heatedly about the pre-Socratics. There was a lull in the conversation, and I could see Popper's brows darken as an extraneous thought flickered across his awareness. He turned to me:

"Bill, people say that I am a difficult man. Am I a difficult man?"

The reply bolted out of me unhesitatingly: "Karl, only a difficult man would ask a question like that!"

2. I first heard what a difficult man Karl Popper was from my teachers at Harvard College. When, in the spring of 1958, I told them that I would go to London to study with Popper, they strongly discouraged me, warning me that I would regret it. Later, when they learned that I did not regret it, they became very angry with me.

By late September of that year, I was being interviewed by the Graduate Registrar at the London School of Economics and Political Science (LSE). She informed me of two things: that I could have the Ph.D. if I would stay at least two years at the School and "write a very good book," and that my adviser would be Professor . . . let us call him "X." I told her that I would be happy to write the book, but that I had come across the ocean to study with Popper, and that if he

were not to be my adviser I would return to America on the morrow. She scolded me and told me that I was being difficult.

The following morning I met my new adviser, Karl Popper, for the first time. He was striking in appearance. The upper part of his body was well-proportioned around a broad chest, but his legs were very short. On top of this peculiar body there perched an imposing high-domed forehead framed by the largest pair of ears I had ever seen. They were pointed, almost elflike. But between them there was a lovely face, handsome, proud, kindly, and very serious. His eyes were trained directly on me, almost discomfortingly so. And I felt immediately—and ever after—that I had his whole attention: there was nothing routine about this interview.

We did not discuss the weather or life in London. He began the interview by telling me that he disagreed utterly with the philosophical views of my teachers at Harvard; and he summed up these differences succinctly. He then decreed, in his thick Viennese accent, and constructing his sentences in the German manner, that I wrote very badly (I had been asked to submit an essay and had turned in one for which I had been awarded a prize) and that I would need to learn to write better before I could expect to make any progress in philosophy. He went on to explain exactly what was wrong with my essay: it was pretentious and in places was unclear, masking confusion, uncertainty, or ignorance with a brilliant, or at least eye-catching, style. I was, he told me, more interested in the effect I was producing than in reaching toward the truth. I do not know why; but from that moment I loved him and knew that I could learn from him: that it would be worth any difficulty that might arise.

3. A week later I first attended his seminar. The practice at graduate seminars in England and America is for the student to read a paper, which is then followed by questions and comments and general discussion from the other participants. The professor may or may not enter much into the discussion: he or she chairs and steers the meeting, serving as a kind of final authority designated in advance. Popper's seminars were different: they were intense confrontations between Popper and the person reading the paper—whether student or visiting scholar. At this particular meeting, the student managed to read about two paragraphs. Popper interrupted every sentence; nothing passed unchallenged: every word was important. He asked a question; the student dodged it. Popper asked the same question again. Again the student dodged it. Popper repeated the question once again. And then the student answered at last. "Were you then wrong in what you said first?" Popper inquired. The student evaded this unwelcome conclusion with a flow of words. Popper listened, then said, "Yes. But were you then wrong in what you said first?" The student was learning, and admitted his error. "Do you apologize?" Popper asked. The student did so, and Popper smiled broadly: "Good," he said. "Then we can be friends."

Another student did not fare so well. He dodged Popper's questions, shifted his position without acknowledging it, avoided all criticism, appealed to the audience, tried to deliver a speech, became more and more incoherent . . . and belligerent. Popper finally asked him to leave. He refused. Popper then took him by the collar and threw him out of the seminar.

I later heard Popper explain his procedure. There was nothing easier, he remarked, than to nod sagely at a student and say that what the student wrote or said was "interesting." But that is not teaching, and does not involve learning. That is, rather, only the genteel ritual of academe. Certainly philosophical discussion was for Popper no gentlemanly pastime; it was a battle on the frontiers of human understanding and values. This caused many difficulties, but it also gave him an extraordinary capacity to stir intelligence into active life in others. "I take my students seriously," he would say. And he took their intellectual well-being as a personal responsibility, motivated by the same kind of concern that led the American philosopher Charles Sanders Peirce to write: "It is terrible to see how a single unclear idea, a single formula without meaning, lurking in a young man's head, will sometimes act like an obstruction of inert matter in an artery, hindering the nutrition of the brain, and condemning its victim to pine away in the fullness of his intellectual vigor and in the midst of intellectual plenty."[3]

Popper had already as a student in Vienna dreamt of one day founding his own school, and writes in *Unended Quest*, his autobiography, of "a school in which young people could learn without boredom, and would be stimulated to pose problems and discuss them; a school in which no unwanted answers to unasked questions would have to be listened to; in which one did not study for the sake of passing examinations."[4]

Some members of Popper's seminar were, however, not ready for such a school—and certainly not ready for the intense experience of being taken seriously—and soon dropped out. Of the dozen or so who remained, at least six did produce "very good books" within the next several years. These included my *The Retreat to Commitment* (1962), Joseph Agassi's *Towards an Historiography of Science* (1963), Ian C. Jarvie's *The Revolution in Anthropology* (1964), Imre Lakatos's *Proofs and Refutations* (1964), J. W. N. Watkins's *Hobbes's System of Ideas* (1965), and A. I. Sabra's *Theories of Light from Descartes to Newton* (1967). The main parts of all these works were, I believe, read for the first time in Popper's seminar during this period. Few seminars anywhere could match such a record.

Preston King, later the author of *The Ideology of Order* (1974) and *Toleration* (1976), was also a regular member of the seminar. Another senior member of the seminar was J. O. Wisdom, the author of *The Foundations of Inference in Natural Science* (1952). The philosopher Ernest Gellner, author of *Words and Things* (1959), was not a member of the seminar, but was on the LSE staff and in close touch with many of the rest of us. Sir Ernst Gombrich, the author of *Art and Illusion* (1960), should also be mentioned. Although he was not a member

of the seminar, he was one of Popper's closest friends, and we saw him frequently.

Each of the works mentioned reflects Popper's philosophical ideas; each also, more deeply, shows his personal influence. His philosophical ideas—on falsifiability, propensity theory of probability, indeterminism, theory of natural deduction, the mind–body problem, objective knowledge, metaphysical research programs, piecemeal social engineering, and so on, as well as his critiques of Marxism, various branches of psychology, Platonism, Hegelianism, analytical philosophy, and positivism—these he taught us in his formal lectures and in his writings. In his seminars he taught us:

- You must have a problem, *not* a topic.
- Do not *try* to be original. Find a problem that excites you. Work on it and take what you get.
- You must *want* to communicate to your reader; you must be clear, never use big words or anything needlessly complicated. ("Write it for Tirzah," he would say—referring to Agassi's eight-year-old daughter.) Do not use logical symbols or mathematical formulae, for instance, if you can possibly avoid it. *Know* logic, but do not parade it.
- It is immoral to be pretentious, or to try to impress the reader or listener with your knowledge. For you are ignorant. Although we may differ in the little things we know, in our infinite ignorance we are all equal.
- Do not be attached to your ideas. You must expose yourself, put yourself to risk. Do not be cautious in your ideas. Ideas are not scarce: there are more where they came from. Let your ideas come forth: *any* idea is better than no idea. But once the idea is stated, you must try *not* to defend it, not to *believe* it, but to criticize it and to learn from discovering its defects. Ideas are only conjectures. What is important is not the defense of any particular conjecture but the growth of knowledge.
- So be scrupulous in admitting your mistakes: you cannot learn from them if you never admit that you make them.

Although Popper fortunately has a sense of humor, I can recall a few moments when such unrelieved, intense dedication to uncovering the truth became somehow out of balance, even comical. There was, for instance, the medical doctor whom Popper was consulting for the first time. After Popper had explained his symptoms, the unlucky doctor, preparing to announce a series of tests that he wanted to make, commented: "Now of course I don't believe in conjectural medicine."

"What do you mean?" Popper demanded. "Don't you realize that *all*

medicine is conjectural?" There ensued a twenty-minute lecture on the methodology of conjecture and refutation—no doubt the only time Popper was charged for giving a lecture.

4. When I first met Popper, it was hard to have a conversation with him. For he would often interrupt what one was saying and begin a long and flowing discourse; and there was no hope of interrupting once that had started. This led some people to complain that they could not get a word in edgewise. The fact is that he was already quite deaf. Hence, taking up on a few cues, he conjectured what was being said or asked, and replied to that. Often he was on target; but it remained difficult to present one's own point of view in detail.

I found a way around this obstacle. I would write him letters, setting out the issues that I wanted to discuss at our next meeting. This way he had an understanding of my views and problems beforehand, and directed his discussion accordingly when we met.

Modest in those areas where he had a right to be vain, and vain in those where he had a right to be modest, Popper was reluctant to admit his deafness to himself, and for years refused to wear a hearing aid. Finally, when he gave the Sherman Lectures at University College London, an incident occurred that changed this. He failed to hear the question raised by a visiting American professor, and his reply was not to the point. Afterward the professor went about complaining that Popper had "deliberately pretended to mishear" in order "to dodge my question." When Popper heard this story he was upset that anyone should think so of him. The next day he purchased a hearing aid, and the problem diminished radically.

5. Popper made a personal impact in another way—as a person of boundless vitality. I do not mean just the bubbling, overflowing genius of his mind, which seemed to know of and to have thought deeply about everything. This has, to be sure, always been extraordinary, and was remarkable even in Popper's most obscure years—those nine years that he spent in New Zealand during the 1930s and 1940s. His New Zealand student Peter Munz, now professor of history at the University of Wellington, reports Popper's unselfconscious vitality and exuberance, throwing the chalk into the air and catching it as he lectured. The geologist R. S. Allan, Popper's colleague in New Zealand, recalls how Popper "strode up and down the room gesticulating wildly and poured forth ideas."[5] The historians of the University of Canterbury report that "Popper's impact on the academic life of the College was greater than that of any other person, before or since." They write that he "acted as a kind of intellectual champagne after the dry depression years. . . . Staff and students alike crowded his open lectures not for instruction or information, but for enlightenment and the sheer intellectual joy of exploring the unknown with him."[6]

This energy was still astounding when I was his student in the late 1950s and early 1960s. At age sixty, Popper used to *run* up the escalators on the deep

tube lines in London, two steps at a time, while I, a young man of twenty-seven, struggled, panting, breathless, behind him. I do not know whether this was owing to, or in spite of, his strict personal habits: he neither smoked nor drank, and never ate more than a bar of Swiss chocolate for lunch. He did not impose such habits on his students (although they were strictly forbidden to smoke in his presence). Yet it is interesting that most of them do not smoke, and use alcohol with great restraint, if at all.

The simplicity of his personal habits was reflected also in his home. Since 1950, he and Lady Popper have lived in the village of Penn, in the Buckinghamshire countryside, an hour's train ride from London—secluded from the interruptions of London life; and the peace of this retreat has given him the opportunity to write.

I once asked him how they had selected Penn, but did not expect the answer that was immediately forthcoming. "Oh, we decided on it when we were still in New Zealand," he told me. In 1945, shortly after receiving his invitation to the LSE, he got hold of the University of London regulations, which required him to live within thirty miles of Senate House. He found a map of the area around London, and drew with a compass a circle enclosing the permitted area. On the western perimeter of this circle lay Penn, a spot that he and Lady Popper already knew from before the war. And they decided upon it then and there.

The house that they eventually acquired, on two acres, is tucked away on a private road. Its living room is long, opening out onto the garden, where Popper often takes visitors to walk out his ideas. An Austrian peasant rug runs from one end of it to the other. The room contains only five pieces of furniture: a grand piano, which Popper plays (he also composes and was at one time a member of Schönberg's Verein für musikalische Privataufführungen); a small desk and chair; a bookcase filled with stories of the Arctic explorations; and two large wooden easy chairs, which sit at the end of the room next to the fireplace. These chairs Popper had made to his own design (he was a carpenter's apprentice in the 1920s), and he and his wife have been sitting in them for decades. At the end of this room a door opens into a small study, where Popper does his writing. It contains a large writing table and chair, a tall cabinet where he keeps his manuscripts, and some loosely stacked bookshelves, also designed by him, where he keeps those books that he needs at hand. There is no television in the house.

6. As an undergraduate, writing for and editing *The Harvard Crimson*, I had a sense that the thinking and writing I did were connected with my culture.[7] I had a sense of audience, community, and task. My education of course came in part from reading and hearing lectures; but it was acquired mainly by growing four years older in extremely good company. As a graduate student there, I began to feel boundaries where previously there had been freedom. The boldness and vitality were gone—or, rather, a sense of sin overcast them. What

was left was narrowness, an absurd neurotic stuttering caution and wariness of criticism—and irrelevance. Community was gone: what was left was "the professional community"—what amounted to a bunch of rather insignificant and conventional professors.

Working with Popper enabled me to regain my earlier sense of community and high purpose. Association with him meant for me a permission—one of course that ought not to have been needed—an irrevocable permission and freedom to throw myself into the world of ideas, to think unfetteredly, to experiment, to tackle problems self-critically yet boldly, to adventure with—and to infect others with—ideas. It set at nought the academic cult of caution and fashion: the cult of those terrified to make a mistake. At some level I already knew that important truism of psychology: that those who are afraid to hurt other people will inevitably hurt them, that those who are afraid to err will most certainly do so.

I do not think I ever discussed this with Popper, but he obviously knew it. One could see this in the sense of sadness that seemed to overcome him—sadness mixed with anger—whenever the question of "placing" one of his students amongst the professional philosophers came up. He always discouraged his own undergraduates from doing graduate work in philosophy or preparing to enter academic life. As he later wrote: "It is very necessary these days to apologize for being concerned with philosophy in any form whatever. Apart perhaps from some Marxists, most professional philosophers seem to have lost touch with reality. And as for the Marxists—'The Marxists have merely *interpreted* Marxism in various ways; the point, however, it to *change* it.' "[8]

When it did come time to find us jobs, he did his best to discourage that aspect of our ambition which was concerned with finding fashionable and comfortable (and corrupting) niches for ourselves. The only point of having a professorship, he would say, was to have a job in which one got paid for doing what one liked. One of his students came in forlornly one day to report that he had been offered a job at a small and undistinguished college with a high teaching load. That did not matter at all, Popper advised him. Since the students would not be very bright, one would just teach them at half the ordinary pace; so the teaching load would not matter. What mattered was whether one would have the conditions to learn, to grow, and—if one had the talent—to write. Popper himself had thought through his basic philosophical ideas when he was a carpenter's apprentice in Vienna. And until his mid-thirties he had been a high school teacher.[9] His first two books, *Die beiden Grundprobleme der Erkenntnistheorie* and *Logik der Forschung*, had been written at night, after a hard day's teaching.

Popper was eager to put across his ideas, and disappointed and sometimes offended that he was not more successful in doing so, for he was passionately convinced of their importance to Western culture, and to the defense and

enhancement of the open society. But he had not the slightest interest in academic politics. And he took it for granted that his students would have similar priorities.

7. Karl Popper is a difficult man. My own relationship with him, to be sure, seemed almost idyllic for seven years. We did not quarrel once during this time, and my fellow students and colleagues wondered at and teased me about this. Finally, in 1965, we did have our quarrel: afterwards we did not speak for twelve years. And now we are friends again. This is a long story, for another time and place. I mention it here only to avoid giving a distorted picture of our relationship. For this essay I have another purpose.

II. His Contribution to the Philosophy of Science

8. In this essay I want to consider not the man Karl Popper but the real harvest of his thought. I have already indicated that a problem arises, both for Popper and for myself, with regard to the philosophical profession. This problem will emerge as a kind of theme for this essay, in an attempt to illuminate the discussion of intellectual revolution.

There is a widespread impression—created in part by a superficial reading of Popper's own writings[10]—that the process of scientific revolution is very simple: that a fact is found that conflicts with a theory; and that the theory is at once dropped and a search undertaken for a new theory. By extension, this view may be applied to philosophy: that when an argument on which a philosophy rests is refuted, that philosophy is immediately dropped and gives way to a new philosophy—or at least to the search for a new philosophy. And it is supposed that it is the professional community of scientists—or philosophers—who do the deciding: who determine when one viewpoint has been refuted and another is to be put in its place. However the facts may be, many would maintain that the scientific and philosophical professional communities—and the elite of these communities—*should* decide when such a shift should take place (see Section 20, below).

Yet if we look at the reception of Popper's own revolutionary new ideas, we find that no such process has taken place, and that the process that *has* taken place is more complicated and interesting. For although Popper's philosophical ideas have been widely acknowledged by the *scientific* elite—including numerous Nobel prizewinners and other scientists who stand at the very peak of scientific achievement—they are widely dismissed or ignored by the bulk of the professional philosophical community.

In this essay I want to begin to consider both the scientific acceptance of and the professional philosophical resistance to the Popperian revolution. This essay will touch only the surface of this problem, making use of an example

that seems to provide an illuminating case-study for the philosopher or historian of science—and one with wider cultural ramifications as well. It is indeed curious that, during the past two decades, in all the discussion, Popperian and otherwise, of the sociology of paradigms and paradigm shifts, no one has examined the paradigm shift associated with the rise, development, and assimilation of Popperianism itself, or the social and institutional questions connected to that.

In the present, second, part of this essay, I shall first sketch the development of the profession of philosophy of science and its chief problems, showing how, just when the profession of philosophy of science was first organizing, the fundamental theory of the philosophy of science was in chaos. I then will turn to the main outlines of Popper's own solutions to these problems, and his resolution of the crisis in philosophy of science. Finally, I shall sketch the very different response to crisis taken by the bulk of the philosophical profession.

9. Professional philosophy of science was born in Austria and Germany, and is, as I write this, being vigorously revived in those countries. It has become so prominent, and so influential, in the English-speaking countries during the last four decades, that it is easily forgotten that in the 1930s philosophy of science, as an independent discipline or profession, barely existed. There were, of course, classically important English-language works in the field—such as those of Mill and Whewell—and all the main problems of the subject were part of the philosophical curriculum. There were also some philosophers who *did* philosophy of science professionally; but the greater part of these were newly arrived, of Austrian or German origin, émigrés to the English-speaking countries from the prewar positivist centers of Vienna, Prague, and Berlin. During the preceding six decades, most of the best-known philosophical treatments of issues in the sciences, in English, were not even written by professional philosophers. Among these authors, W. K. Clifford was a mathematician; Karl Pearson, a biologist and statistician; John Maynard Keynes, a polymath, an insurance executive, an economist; Joseph Needham, a biologist; Sir Arthur Eddington and Sir James Jeans were physicists, as was P. W. Bridgman. Charles Sanders Peirce could not keep an academic post, and spent most of his career working for the United States Coastal and Geodesic Survey.

It would be easy to find exceptions to this list—C. D. Broad and Bertrand Russell most conspicuously—but the point has been made.

This situation began to change in the 1930s, as a result of the crusading positivism of the Vienna Circle, and of the emigration of its members to England and America. In the chief doctrines of the Vienna Circle, there was already a strong basis for communication with English-speaking philosophers. The public organization formed by members of the Circle had been called the "Ernst Mach Verein," after the Austrian physicist, psychologist, and

philosopher Ernst Mach (1838–1916).[11] The Viennese championing of Mach's sensationalist ideas as the basis for the understanding of physics reminded English-speaking philosophers of a part of their own heritage that had been in eclipse during the nineteenth century. Several eighteenth-century British philosophers had deeply influenced Mach—David Hume and, especially, Bishop Berkeley.[12]

One result of this shared background and newly focused interest in the philosophy of science was the journal *Philosophy of Science*, which began to publish in the mid-1930s. But only in the late 1940s and early 1950s—with such events as the organization of the Philosophy of Science Group (later the British Society for the Philosophy of Science), the founding of *The British Journal for the Philosophy of Science*, and the creation of strong graduate programs in philosophy of science at Minnesota, Iowa, Berkeley, London, and elsewhere—did the profession really come into being.

I put some importance on this point because it is easy, in surveying the publications and problems that have shaped the past four decades of debate in philosophy of science, to forget that these same years mark the creation of a profession—an event that would in almost any field be bound to have a distorting effect on the subject itself. One such effect is that particular emphases present in the late 1940s and 1950s, at the time of the organization of the profession, have tended to be institutionalized and perpetuated.

I shall return to this profession later. First, I want to review very briefly—for they are treated at length elsewhere in this volume—some of the chief issues of the philosophy of science as perceived by this profession.

10. The first two issues to be considered are the problems of induction and demarcation. Both of these, and particularly the first, concern the relationship between evidence and what is evidenced. On the inductivist and empiricist approach that is associated with the Humean doctrine that nothing is in the mind that is not previously in the senses, sense observation reports are seen as the only legitimate source, justification, and evidence for other contentions.

The problem is that there is no way *logically* to justify or prove the universal laws of nature by appealing to singular observation reports, however many. Statements of law are more than combinations of observations; they are of stronger content than the evidenciary statements used in defending them.[13]

This problem had been stated in antiquity by Sextus Empiricus, and little progress on it had been made since then.[14] The positivists of the 1930s and 1940s had no way to solve it, but supposed that it could be circumvented through the application of probability and confirmation theory; that observation reports might not logically entail law statements, but nonetheless would make them more probable, and thus "confirm" them.

The probabilistic approach, developed by John Maynard Keynes, Rudolf Carnap, and others in the 1920s and 1930s, was in serious trouble by the late

1940s, as shown by the publication of the "paradoxes of confirmation" by C. G. Hempel and Nelson Goodman, who argued, among other things, that—within such an approach—every instance of non-A "confirms" a hypothesis of the form "All A are B." Thus an observation of a red herring confirms the hypothesis that all swans are white; the observation of a green emerald today confirms that tomorrow all emeralds will be blue.[15] The positivists had tied up nature in colored tape—red and otherwise.

The problem of demarcation is closely related. In the 1930s, 1940s, and 1950s, under the influence of the positivists and of Wittgenstein, philosophers of science tended to conceive it as one of demarcating meaningful utterances—that is, those that are logical or scientific—from those that are meaningless. The aim of such demarcation was, as expressed in Carnap's famous essay, "Die Ueberwindung der Metaphysik durch logische Analyse der Sprache," the elimination of metaphysics, including all philosophy of value and normative theory.[16] Alleged statements in this domain were taken to be, as Carnap put it, "entirely meaningless" pseudo-statements. One avoided these—and here was the demarcation—by making no utterances not reducible to or probabilistically confirmable by sense observation reports. This position is imperialistic (see Part IV, below) in the sense that all legitimate statements are required to conform to what are supposed to be the standards of logical and scientific discourse.[17]

All this is a truncated version of an older and wider problem. Consider the following chart:

good traits	bad traits
true	false
clear and distinct	unclear and indistinct
probable	improbable
empirical	unempirical
scientific	nonscientific
verifiable	unverifiable
meaningful	meaningless

Where ideas compete, it is obviously useful to be able to demarcate good ones from bad ones. But what makes an idea good? The left-hand column of my chart suggests how philosophers have answered this question. The right-hand column indicates traits to be avoided. During modern times, attempts to find a criterion of truth have in general been abandoned; and the deficiencies of Descartes's criterion of clarity and distinctness were shown by Kant. Yet most

of the remaining "good traits" held sway. It was widely assumed that any good theory would combine the remaining left-hand traits: it would be more probable than its rivals, and also be empirical, scientific, verifiable, and meaningful. Whereas if one of these characteristics were lacking, the others would be lacking too: thus a nonempirical, unverifiable, and nonscientific statement would be meaningless.

The difficulties of induction and the "paradoxes" of confirmation and probability theory took their toll here too. The proposed criteria of demarcation simply did not work—and were prevented from working by the problem of induction. Scientific laws turned out to be improbable, unverifiable, and meaningless. Whereas some metaphysical statements were both probable and verifiable.[18]

In sum, in failing to solve these two problems, professional philosophers of science demonstrated their inability to give a coherent account of the relationship obtaining between scientific theories and evidenciary observational reports. Worse, any such account seemed in principle unattainable.

11. One aspect of the demarcation problem deserves emphasis. It is interesting that the problem was expressed as one of demarcating science from nonscience. A characterization of science is obviously important in general cultural terms. Two of the most strident motifs in our intellectual life are the effort of science and nonscience to come to grips with each other, and the effort of science to find out just what it is that makes it scientific. Many fields—religion, philosophy, history, social science, psychoanalysis, psychology, sociology—characterize themselves, understand themselves, by contrast to and in comparison with "the sciences." An understanding of the nature of science is hence—literally—a prerequisite of self-knowledge for other disciplines. Since the most widely accepted notions of science—based on the unworkable positivist demarcation—are incorrect and even incoherent; and since most discussions in other disciplines, including philosophy, are nonetheless framed in terms of the supposition that the positivist account of science is correct *for science*, most disciplines are now mischaracterized by their own proponents—and are, as it were, methodologically "neurotic" as a result. It is often and correctly remarked that academic ideology in the Marxist countries is distorted, particularly in the social sciences, because of its reliance on a false Marxist characterization of science. But in the West, the most widely accepted characterizations of science are *also* false.

12. The problems were not all methodological. The impact on the emerging profession of philosophy of science of Einstein's theories of relativity, particularly in their bearing on the understanding of space and time, can hardly be overestimated. Let me mention briefly just two examples.

The first is Kant's problem of the infinity or finitude of the universe with regard to space and time. Kant argued that these issues produce contradictions

that are in principle unresolvable, the so-called "antinomies of pure reason." He appeared to be able to prove, both of space and of time, that they were both finite and infinite. Einstein hoped to resolve the problem regarding space with the idea of a universe that is both finite and without limits, but his efforts have now been abandoned. With regard to time, the issue is even more complicated and also remains unresolved.

A second problem, raised by the logician Kurt Gödel (1906–1978) in 1949, has to do with the question of whether relativity theory has idealistic implications.[19] The relativity of simultaneity implies, for the most part, the relativity of succession. Thus the assertions that A and B are simultaneous, and that A happened before B, lose objective meaning, and depend on the observer. This provides a proof, Gödel concludes, that supports the views of Kant and of modern subjectivists and idealists to the effect that change is an illusion arising from one's mode of perception. It can be shown that there are some worlds—*possibly* including our own—wherein one can, by making a round trip in a rocket ship in a sufficiently wide curve, travel into any region of the past, present, or future, and back, just as one familiarly travels to other parts of space.

Notwithstanding the consensus one finds in science fiction, there is as yet no agreement about the implications of Gödel's argument; but the possibility he demonstrates of anomalies of temporal order in general relativity gives rise to the question, as Howard Stein puts it nicely, "to what extent the existence or non-existence of a univocal time-ordering along all time-like world-lines is susceptible of manipulation (by the physical rearrangement of matter) within a given cosmic model."[20]

13. If relativity theory seemed to give some support to idealistic and subjectivist positions, quantum mechanics gave very much more. This was especially so in its Copenhagen Interpretation (usually credited to Niels Bohr, Werner Heisenberg, and Wolfgang Pauli). For the behavior of electrons and photons seemed to breach both the laws of nature and the laws of logic. The behavior of waves is *essentially* different from that of particles. Yet nature appeared to manifest itself, on the microscopic level, in these two contradictory ways: sometimes as waves, sometimes as particles; and the transitions from one manifestation to another took place, impossibly, at superluminal velocities. Moreover, the role of the observer was argued to be crucial, thus dissolving the distinction between subject and object: when electrons are looked at, their distribution is different from the way it is when they are not. As the physicist Eugene P. Wigner put it in his *Symmetries and Reflections*: it was not possible to formulate the laws of quantum mechanics in a fully consistent way "without reference to the consciousness."[21]

14. There has also been a problem about the status of scientific theories. For most of the history of science, it was agreed that scientific theories were

attempts to *represent* and hence *explain* the physical universe.[22] During this century, however, the philosophical doctrine of instrumentalism—according to which laws of nature are not descriptive but *only* instruments or tools for the organization of phenomena—has been widely accepted. An instrumentalist approach had already been part of the Machian philosophy that lay at the basis of logical positivism; another important influence came from a French source: the great work on *The Aim and Structure of Physical Theory* by the French theoretical physicist Pierre Duhem (1861–1916).[23] Instrumentalism got its greatest boost, however, from quantum mechanics, in the form of Niels Bohr's principle of complementarity. A way of handling the contradictions and conflicts in quantum theory, it amounted to forsaking the attempt to represent the world in a consistent coherent model, and settled for using the formalism of the theory, in application to single experiments, for instrumental or pragmatic purposes of prediction, organization, and control.

Instrumentalism also provided a way to deal with the difficulties of induction and demarcation; and by the same token, the difficulties of induction and demarcation helped pave the way for the acceptance of instrumentalism. To accept induction—even to *insist* upon it—despite its own logical incoherency, and despite the paradoxes that had arisen in attempts to state it coherently, amounts to treating the theory of induction instrumentally too. For it was the aim of representation and explanation that had required consistency in the first place. Instruments, tools, do not *need* to be consistent, for they do not pretend to explain.

15. I have indicated briefly some of the main problems and issues of the philosophy of science as they were often stated some thirty to forty years ago, and as they are still widely understood today. By putting them in a neutral way, it is easy to give the impression that they were ordinary, and relatively minor, problems. *Nothing could be further from the truth.* At the very moment when the profession of philosophy of science was organizing, the fundamental theory of the philosophy of science was in a state of collapse. Yet this was not acknowledged. Rather, there was a glorification of contradiction, and even paradox, almost as if it were a sign of profundity. As a way of dealing—or not dealing—with the crisis, the importance of real anomalies was played down. And at the same time pseudo-paradoxes flourished.[24] Sponsored by instrumentalism, this attitude expressed itself most revealingly in the simultaneous prizing of formalism (including logical formalism) and practical contempt for logic. Complementarity, for instance, rescued the Copenhagen Interpretation from contradiction on an *ad hoc* basis. The same sorts of moves were taken in positivism, in confirmation theory (and also in certain other areas that I cannot probe here, such as behaviorism in psychology, and linguistic analysis[25]), thus rendering the wider ideology that included all of these into a reinforced dogmatism—a dogmatism strengthened by "good reasons" why the strongest

criticisms should not be taken seriously. Criticism was thus sealed off, diminished, explained away in advance. Evidence of this appears in hundreds of writers. Few, however, attained the eloquence of Hilary Putnam, of Harvard University, who wrote:

> In this sense "induction is circular." But of course it is! Induction has no deductive justification; induction is not deduction. . . . The fact that a justification is circular only means that that justification has no power to serve as a *reason*, unless the person to whom it is given as a reason already has some propensity to accept the conclusion. We do have a propensity—an a priori propensity, if you like—to reason "inductively," and the past success of "induction" increases that propensity. . . . Practice is primary.[26]

To argue in such a way is to legitimate dogmatism. For practice is *not* "primary"; it is just practice. To have a propensity—inclination or predisposition—to accept something that would, *apart from* such a predisposition, be unacceptable is precisely what is meant by "to be prejudiced" or "to act as judge in one's own cause." One aim of rationality and of traditional scientific civilization has been to review prejudices, attachments, propensities (a priori, "natural," or otherwise), and practices, and to subject them to examination, restraint, and criticism. A *good* reason—as opposed to a rationalization—is one that works independently of a priori propensity.

16. It was into this crisis situation that the revolutionary ideas of Karl Popper were broadcast in January 1959, with the publication of *The Logic of Scientific Discovery*.

Popper's ideas had, of course, been published long before, in 1934, in his native Vienna, as *Logik der Forschung*. But as the storms of war gathered, the philosophers of science at the great centers of Vienna, Prague, Berlin, and Warsaw were already dispersing, many of them, like Popper, being of Jewish descent. The first edition of *Logik der Forschung* thus had a limited circulation. And during the war itself, Popper was removed from the scene, teaching in the obscurity of New Zealand. He did not return to Europe—to the London School of Economics—until early 1946; and at that time he became known first as a social and political philosopher, a philosopher of history, and a historian of philosophy, as a response to his *The Open Society and Its Enemies* (1945), and *The Poverty of Historicism* (1944 and 1957).

The Logic of Scientific Discovery is, however, the theoretical work on which his historical, political, and other studies depend.[27] In it Popper was able, simply and straightforwardly, to resolve many of the outstanding issues in the philosophy of science. *I mean this literally.* In what Popper would call World 3—the world of abstract argumentation—the problem of induction *is*

solved, and the verificationism and inductivism that lay behind it are *refuted*.

Popper shows that induction does not exist.[28] Rejecting the empiricist theory of learning as primitive and in conflict with biological knowledge, Popper sees the mind as no passive "bucket" into which experience simply rains and which can, at most, recombine that experience in various ways. On the contrary, the mind actively anticipates the future with hypotheses that, of necessity, go far beyond experience: hypotheses precede observations psychologically, logically, even genetically: all experience is theory impregnated. Every animal is born with expectations—that is, with something closely parallel to hypotheses, which, if verbalized, express hypotheses or theories. The role of experience is to *break* expectations: to criticize and to challenge hypotheses. The ability of an animal to learn will depend on the extent to which it can modify expectations contradicted by experience, on the extent to which it is able to invent new expectations or theories to deal with unanticipated situations.[29]

In terms of this argument, Popper shows that the relationship between theory and observation is *deductively logical* after all. There is a basic asymmetry between the verification and the falsification of a theory: although no amount of observations could ever verify a theory logically, one single observation may falsify it, serving logically as a counter-example to it.[30]

Here there is limited use of probability to evaluate statements and no confirmation theory whatever. The paradoxes associated with probability and confirmation, and stressed by Hempel and Goodman, simply do not arise for Popper (see note 15).

How then are hypotheses or theories to be confirmed? *They are not to be confirmed*. There is no way to confirm—that is, to prove, verify, make firmer, make more probable—any theory of any interest. They are and remain forever conjectural. There is no certain knowledge. What *is* done—and what has been mistaken for confirmation—Popper calls "corroboration." For a theory to be corroborated is simply *to have been tested* severely and to have passed the test. Such a theory is not made more probable thereby: it may fail a yet more severe test tomorrow.[31]

17. Popper's approach to demarcation flows immediately from his treatment of the problem of induction. To demarcate science from nonscience Popper proposes the testability or falsifiability criterion. For what would science have to be? Not the systematic body of confirmed, probable, verified theories envisioned by positivists.[32] Rather, it consists of a nexus of problems, theories put forward to solve them, and tests made of them. A scientific theory is one that is testable or falsifiable: that is, in potential conflict with possible results of observation.

Whereas Humean verifiability excluded not only wild speculation but also most of the highest achievements of science, falsifiability makes a more effective cut: most acknowledged scientific theories are in *potential* conflict

with sense experience (and thus empirically criticizable); whereas most religious and many speculative statements are untestable. No empirical observation, for instance, stands in potential conflict with "God exists," "There are angels," "There is an afterlife," or "There is a fountain of youth." Such unrestricted statements of existence are often untestable as a result of their form alone. Other theories—Popper cites as examples those of Freud,[33] Adler, and Marx—are untestable (and unscientific) because they contain stratagems for deflecting empirical criticism.[34]

The relative untestability of Marxism and psychoanalysis indicates low content and relative weakness—"the more a theory forbids, the more it says" is one of Popper's slogans. In this respect they contrast with the more developed sciences: "What impressed me most," Popper writes, "was Einstein's own clear statement that he would regard his theory as untenable if it should fail in certain tests."[35]

Untestability has, however, nothing to do with meaning: untestable statements may stand in logical relations with testable statements and thus cannot differ from them with regard to meaningfulness. (Moreover, Popper explains how the positivist preoccupation with meaning stems from an interesting error: the classical logical paradoxes had indeed been able to be resolved—and had dissolved—through meaning analysis; and it was mistakenly assumed that the traditional problems of metaphysics would similarly disappear under meaning analysis. But there is a decisive difference between the traditional logical paradoxes and the traditional problems: the paradoxes are produced by self-reference, whereas self-reference is absent in traditional philosophical problems. Hence the parallel fails.)

This does not imply that scientific theories are the only "good" ideas. Many valuable theories, particularly in early stages of development, will not be testable. They may, however, be criticizable in other ways: they may be shown to fail to solve those problems that they purport to solve; or to solve other problems; or to contain self-immunizing stratagems for deflecting criticism; or to be internally incoherent.[36] Many ideas important in the history of science, although not testable, acted as "metaphysical research programmes" and principles of interpretation, guiding and sponsoring scientific research. These have included atomism and Darwin's theory of natural selection in their early stages of development.[37] Two other untestable viewpoints, determinism and indeterminism, mark an issue of great importance in the interpretation of contemporary quantum theory.[38]

18. Although Popper does not use probability to evaluate scientific theories, he sees the question of the interpretation of the probability calculus as the key to the solution of the chief problems of quantum theory. The interpretation of quantum theory, he argues, hinges on whether one attributes subjective or objective status to probability statements in physics. What does it

mean, for example, to say that the probability of a photon passing through a half-silvered mirror is one-half? According to the subjective interpretation, this means that, *due to our ignorance* of the relevant initial conditions, we have no more reason to expect the photon to pass through than to be reflected. Whereas, on the objective interpretation, this means that *nature is indifferent* between letting it through and reflecting it back.

The objective interpretation seems, on the face of it, more reasonable. It seems preposterous to suppose that pennies fall or molecules collide randomly because we are unaware of initial conditions—and that they would do otherwise if these conditions were known to us.

Nonetheless, the majority of physicists adopt a subjective interpretation. Their reason for this is twofold. First, existing objective accounts of probability—the so-called "frequency interpretation," for instance—have been importantly inadequate, being unable to handle probabilities for single events. Second, most physicists presuppose what Popper calls *metaphysical determinism*, and thus cannot possibly accept an objective account of probability. For a metaphysical determinist, nothing is really, objectively, only probable: everything is exactly fixed. Relative to a *complete* knowledge of all the laws of nature and all initial conditions, the probability of any possible event is either one or zero. Hence probabilities with intermediate values, as in quantum mechanics, can refer only to the state of information available. The upshot is that quantum mechanics is ordinarily presented as a report of *our knowledge* about particles, not as a report of objective reality.

To combat this, Popper provided his *propensity* interpretation of probability, which is not subject to the defects of the frequency interpretation. And he developed a series of arguments on behalf of metaphysical *in*determinism.[39] Popper's account of quantum theory requires no move to instrumentalism to protect it from paradox or anomaly.[40] In estimating the status of scientific theories he himself opts for a realist, representationalist[41] point of view.

After beginning his work with philosophy of physics, Popper went on to develop new foundations for logic and probability theory, and then established another reputation as a social and political philosopher. In Popper's hands these are not separate areas, but are closely connected: indeterminism in history is integrated with indeterminism in physics; and criticism of theories, with rational reform of social institutions. In his later years, he has turned his attention to another area barely hinted at in his earlier work: biology and evolutionary theory. The new work is not simply incremental: it unifies all his thought. In *The Logic of Scientific Discovery*, Popper had urged epistemologists to approach their task through the study of the most advanced forms of knowledge: scientific theories. Now he turns also to primitive and prehuman forms of knowledge, and to evolution, for examples of the growth of knowledge, maintaining that "The main task of the theory of knowledge is to understand it as continuous with animal knowledge; and to understand also its

discontinuities—if any—from animal knowledge." Epistemology becomes a science of comparative cognitive apparatuses.[42]

Both scientific theories and the biologically based cognitive structures of animals interest Popper as objective structures of knowledge achievement. Both are produced by the same Darwinian mechanism: the highest creative thought, like animal adaptation, is a product of blind variation and selective retention.[43]

In his most recent work, *The Self and Its Brain* (1977), written in collaboration with the Nobel prizewinning neurophysiologist Sir John Eccles, Popper turns to the mind–body problem, where he develops an interactionist theory that is also anti-behaviorist and anti-materialist.

19. Popper's life's work thus poses a striking challenge to conventional theories of science. At a time when most philosophers of science are inductivist, subjectivist, positivist, instrumentalist, behaviorist, materialist, monistic, Popper is deductivist, realist, anti-positivist, anti-instrumentalist, anti-behaviorist, anti-materialist, interactionist. The significance is not simply in the attack: theories of science have been challenged before, often in connection with ideological defenses of religion, or for political purposes. No such motive lies behind Popper's work. Rather, it is a fundamental reexamination of the philosophy of science from *within* science and on behalf of a more adequate scientific world view. A methodology deeply dependent on historical studies, it is applicable to all fields and disciplines, to the extent to which they aim to achieve more adequate explanations and descriptions.

Throughout, Popper's work contrasts sharply with that of the positivists and his other predecessors and rivals. Although Popper, like the positivists, made important contributions to logic and physics, his work is not minute. There is a largeness and boldness about his ideas—a flowing ruminative expansiveness that denies the "two cultures." And they have an amazing scope—ranging from physics through biology to sociology, political theory, theory of education, Greek history and philosophy, and even the history of polyphonic music.[44] As if electrically charged, he drew an original spark from any subject he touched.

It would be absurd to suggest that Popper's theories resolve all legitimate controversies in the philosophy of science. For instance, although he has written on the theory of time, he has not contributed in a major way to relativity theory; nor do his ideas resolve the problems in relativity theory mentioned above.[45] Moreover, his work in quantum theory, which is major, is in part hostage to the theories of Alfred Landé, which have never been rigorously examined. Various aspects of Popper's methodology are also defective: his theory of demarcation is sometimes clumsy in application, and is defective in various other ways; his theories of verisimilitude and corroboration are also inadequate.[46]

Nonetheless, his work leaves philosophy of science in a state utterly

different from that in which professional philosophy of science was born in the late 1940s.

III. His Reception by the Profession: Con Lamento

20. In the preceding part, I have indicated briefly the nature of the crisis in philosophy and philosophy of science, and of Popper's resolution of it.

How then has his work been received? Has his resolution been accepted and incorporated into the framework of professional philosophy?

Not at all.

Had Popper's work been widely known when it was first published, in 1934, it would perhaps have had a greater impact on the philosophical profession. But it was subjected to a long delay: published in a limited edition in German immediately prior to the interruptions in communication caused by World War II, *The Logic of Scientific Discovery* was, during the war, repeatedly plagiarized and garbled. When it was finally published in English in 1959, it was greeted warmly in Britain. Richard Wollheim called it "one of the most important philosophical works of our century," and Sir Peter Medawar called Popper "incomparably the greatest philosopher of science that has ever been." The anonymous reviewer in the *Times Literary Supplement* remarked that the book had "that quality of greatness that, once seen, it appears simple and almost obvious," and went on to speculate: "One cannot help feeling that, if it had been translated as soon as it was originally published, philosophy in this country might have been saved some detours."[47]

The impact of all this on professional philosophy was, however, marginal. Between 1934 and 1959, two separate professional ideologies had become entrenched in the philosophy departments of the English-speaking universities. One of these was professional philosophy of science, the outgrowth of the crusades of logical positivism; the second, curiously encompassing the first, was the cult of ordinary language and of the later Wittgenstein.

Institutionally and professionally, these still dominate. They have captured graduate departments of philosophy and characterize, even define, "professional philosophy." By the 1950s, logical positivism had, on the whole, been modified by and incorporated into the general framework of the philosophy of the later Wittgenstein (see Part IV, below). A relatively easy shift, it was exemplified not only in Wittgenstein's own development but also, say, in the contrast between Sir A. J. Ayer's *Language, Truth and Logic* (1935) and his *The Problem of Knowledge* (1956); or in Morton White's brilliant attempt to reconcile positivism, pragmatism, and language analysis in *Toward Reunion in Philosophy* (1956).[48] Positivism and Wittgensteinian philosophy sit well together; for Wittgensteinian philosophy can tolerate positivism as an account

of the "language of science," even where it rejects its wider claims. Neither, however, can tolerate Popperian philosophy, which attacks the fundamental presuppositions of both, and cannot be assimilated to or digested by either. Thus in the World 3 of abstract argumentation, verificationism and inductivism are refuted—and objectively so. But in the World 3 of professional institutional arrangements and in the World 2 of philosophers' minds and loyalties, this abstract World 3 event has had comparatively little effect, apart from raising defenses.

In consequence, Popper and his followers are not true participants in the contemporary professional philosophical dialogue woven by these two schools. Rather, Popper has ruined that dialogue. For if he is on the right track, then the majority of professional philosophers the world over have wasted or are wasting their intellectual careers. The gulf between Popper's way of doing philosophy and that of the bulk of contemporary professional philosophers is as great as that between astronomy and astrology.

The dialogue may have been ruined; but it continues. It is one in which Popper's work is either ignored or radically misunderstood. By reading professional journals, one would not gather that the problems discussed above indeed have been solved. Inductivism continues as before; and a philosophy that *presupposes* the insolubility of the problem of induction dominates all professional discussion.

A good example of the ignoring of Popper's work is *Justification and Knowledge* (1979), a book of essays by some of America's best-known epistemologists, including Keith Lehrer, Roderick M. Chisholm, and Wilfrid Sellars. As George Pappas, its editor, proudly reports in his Introduction: "The literature on epistemic justification . . . in English-language journals and books is vast and growing all the time."[49] The entire discussion of this book was, however, rendered completely obsolete by Popper's work in the 1930s, and by some extensions to it that I made in the early 1960s.[50] Yet Popper's name is mentioned in the work only once, in passing and inaccurately, in a list of names.

An example of the misunderstanding—or garbling—of Popper's views is N. M. L. Nathan's *Evidence and Assurance*, published by the Cambridge University Press in 1980.[51] Nathan states that "Critical Rationalism" (the name given by Popper to his position, and widely used to refer to it) assumes that the rigorous examination of a theory ensures that it is probably true, and that Critical Rationalists want to "guarantee the truth or probable truth of what is rationally believed". On the basis of these assumptions, he accuses the position of vicious infinite regression and other difficulties. Yet Popper, Hans Albert, and I (the three Critical Rationalists under discussion) have explicitly denied these assumptions, and Popper has denied them, literally, *ad nauseam*. Cambridge University Press would be unlikely to publish a work attributing to Einstein a belief in the luminiferous aether, but it publishes a work—no doubt

refereed and approved by "experts" from the philosophical profession—attributing to Popper and his followers *precisely those ideas that they have spent their lives denying.*

Yet such experts are the "professional elite" that this same kind of professional philosophy—and the sociological philosophy that it has spawned—wishes to set up as *authoritative.* The authoritarian, elitist tenor of the dominant community of contemporary philosophers is reflected everywhere in its publications. As one example, there is "Justification and the Psychology of Human Reasoning," published in *Philosophy of Science,* the organ of American professional philosophy of science. Its authors counsel that there is a higher court of appeal than an individual's own "reflective equilibrium." Namely, it is that of his "cognitive betters." "There are," these authors write,

> people in our subject's society who are recognized as *authorities* on one or another sort of inference. . . . He need only seek out the experts and ask them. The role of experts and authorities in our cognitive lives has been all but ignored by modern epistemologists. Yet it is a hallmark of an educated and reflective person that he recognizes, consults and defers to authority on a wide range of topics. . . . One of the principle effects of education is to socialize people to defer to cognitive authorities. . . . Deference to authority is not merely the habitual practice of educated people, it is, generally, the right thing to do, from a normative point of view.[52]

These authors appear to be the type of "right-thinking men" to whom Sartre refers when he writes: "To the right-thinking man, to be alone and to be wrong are one and the same."[53]

Harold I. Brown's *Perception, Theory and Commitment: The New Philosophy of Science* conforms to this authoritarian, expert-oriented, deferential mood. Thus Brown argues for a relativistic conception of scientific truth according to which the truth of a scientific theory reflects or is a projection of the consensus of the scientific community, and a theory is false when it is rejected by that community; and if the scientific community has made no commitment, then the theory is neither true nor false.[54]

How seriously can one take such self-interested professional proclamations of professional authority?

Not very. In fact, far too much attention is paid to these professional "elites." It is of course useful to know how they operate—and even to see that they are to a certain extent indispensable in intellectual communities in contributing what Campbell refers to as the "stability requirement."[55] Yet from many of these discussions of elites, and from the work of Thomas Kuhn and various sociologists of knowledge, one might gather the absurd autistic idea that intellectual issues are settled in university departments. Most of this writing is

done by American academics, department-oriented "team men" who have only a glimmering awareness of any wider cultural terms of reference. Thus what we hear of is the sociology of growth within schools of professionals, and the sociology of strife between schools of professionals—as if philosophical ideas mattered only to professionals, and were to be arbitrated by their own self-determined "elites."

In determining Popper's real influence, one has to make a sharp distinction between the general culture, including science, and the profession of philosophy. The impact of his ideas on science and on cultural and intellectual discourse has been immense—particularly in Germany and in England.[56] And he has won as disciples and working colleagues independent people of intellectual standing comparable to his own. Among the most distinguished of these are Sir Ernst Gombrich, the art historian; Donald T. Campbell, the psychologist; F. A. von Hayek, the economist; Sir John Eccles, the neurophysiologist; Sir Peter Medawar, the biologist; and Jacques Monod, the biochemist. Hayek, Eccles, Medawar, and Monod are all Nobel prizewinners—just as Popper himself has joined Winston Churchill, Albert Schweitzer, Bertrand Russell, Arthur Koestler, and Niels Bohr as the winner of Denmark's Sonning Prize.[57] Another Nobel laureate, Konrad Lorenz, although never an associate of Popper's, has also acknowledged the close bearing of Popper's ideas on his own.[58] Some evidence of the way in which Popper has reached the general public became apparent in 1978, when *The Times Higher Education Supplement* printed a critique of Popper by an Oxford philosopher. There were dozens of letters of protest from the general public, some of them printed in subsequent issues. Most of the responses printed, although clearly competent, were by members of the public unknown to the professional community—including Popper's group.[59]

It is indeed in that country where intellectual life is most professionalized (i.e., segregated from the general culture)—the United States—that Popper's work is least widely recognized. Campbell is the only American in the list of Popper's associates given above. And if we turn to the next generation, to Popper's own chief students—Joseph Agassi, Hans Albert, I. C. Jarvie, Imre Lakatos, A. I. Sabra, Paul K. Feyerabend, J. O. Wisdom, J.W.N. Watkins, and myself—one finds that I am the only American among them. As Ernest Gellner has so aptly expressed the matter: "if the several thousands or more of professional philosophers in America were all assembled in one place, and a small nuclear device were detonated over it, American society would remain totally unaffected."[60]

In Britain, where there exists a strong intellectual culture independent of the universities, the professional situation is different. Even thoroughly academic and professional philosophical thought plays a role in British culture through the BBC, the *Times Literary Supplement* and *Times Higher Education Supplement,*

through public lecture programs, and distinguished journals for the educated public such as *Encounter, The Economist,* and *The New Scientist.* In that wider culture Popper is an important literary and scientific figure, a best-selling author, a major influence.[61] In Britain, despite the self-isolating effects of linguistic philosophy, it is still recognized that some of the main issues in physics, psychology, and biology are philosophical in character *and* of deep cultural relevance. The professional British philosophers may on the whole be intellectuals without ideas—like the characters in Angus Wilson's novels or the author of A. J. Ayer's autobiography.[62] Yet this does not matter so much, since the culture remains sufficiently integrated to prevent the self-isolating tendencies of professional philosophy from being fully put into effect. Similar remarks could be made about Germany, and about Popper's reception there during the past decade.[63] Whereas in America the self-isolating tendencies are virtually unchecked.

21. The situation just discussed is bizarre: the scientific, literary, and learned communities of Europe are acclaiming Popper; and his own profession disowns him. In a remark to me, he himself perhaps put it best: "Here I am being showered with honours as no professional philosopher before me; yet three generations of professional philosophers know nothing about my work."

However one may judge this, and explain it, one thing is clear: *the widespread current discussion among professionals about the role of professional elites in determining the acceptance and influence of ideas throws virtually no light on a case like this.* For here several overlapping yet distinct communities (or econiches) make very different judgments and decisions. The professional discussion of such matters has had no clear point of reference; no clear econiche is specified; yet evolutionary survival is always relative to some specific econiche that the organism (or group) itself in part selects and creates. Like *self,* and closely related to it, *community* is a World 3 creation in Popper's sense.[64]

Such confusion is hardly surprising. Nor does it appear only in contemporary philosophy. It appears also in contemporary psychology, and even in physics. For it is not as easy as it was (see Section 6, above) to identify one's audience and the community for which one is writing and thinking. With the decline in standards of education, in university and cultural budgets, in the graduate schools, in the standards of literacy throughout the Western world, and in the book publishing industry, those who inhabit the world of ideas face an ecological crisis—a crisis whose resolution demands a sharp confrontation with and a deep rethinking of the entire question of community. Separated as it is from the wider culture, the "intellectual community" threatens to dissolve into a loosely federated band of "disciplinary" craftsmen—*federated* less by a common tradition or shared values as by the need to exert concerted pressure to gain financial support.

Like any econiche, a community is characterized by the possibilities or

potentialities that are open to it—whether as yet discovered or not. Its future survival will depend on whether, and the way in which, it exploits these potentialities. But professional groups often function not to open up possibilities but to close them out. Here a narrow conception of culture and community operates, in which these are characterized by current presuppositions and practices, rather than by as yet undiscovered potentialities.

22. *Does the situation matter?* What are the chances of the survival of the Popperian approach?

To answer this question, one must bear in mind that all evolution, intellectual and otherwise, occurs in three distinct and indispensable phases or rhythms. To consider the question of the survival and propagation of a Popperian approach to philosophy, one must fix *which* econiche (or community) one is discussing, and must look at how it is doing on each of these three levels within the relevant econiche.

These three phases or rhythms are:

1. Unjustified variations on existing forms (sometimes called "random variation").[65]
2. Systematic elimination and selection.
3. Retention: duplication, transmission, and preservation of selected variations.

On the first level, that of unjustified variation, Popper and the group around him are doing splendidly, both in terms of the professional communities and the wider culture. There have been some remarkable developments of his position, including Ernst Gombrich's application of it to art, and D. T. Campbell's application to biology and evolutionary epistemology. And a series of important mutations—monsters, hopeful and otherwise—have sprung from the midst of the Popperians. One is the philosophy of Paul K. Feyerabend, which wildly overemphasizes the role of unjustified variation (i.e., theoretical pluralism) in the growth of knowledge.[66] Another example would be my own early work, an unjustified variation that emphasizes the *unjustified* character of both variation and selection, and which perhaps overemphasizes the role of systematic elimination (i.e., unjustified criticism) in the growth of knowledge.[67] Then there is the mutation that is the philosophy of Imre Lakatos—a cumbersome mutation built on misreadings and of poor construction.[68] But it has a beautiful tail and is widely attractive to professionals.

On the second level, systematic elimination and selection, the position of the Popperians is ambiguous. They are under attack not only from the professionals, but from one another: within the group there has been self-destructive internal dissent. One Popperian became so alarmed by this that he wrote, in some exaggeration, that "instead of a coherent philosophical position, one finds a lightly disguised squabble of alley cats."[69]

On the third level, that of retention—propagation, transmission, duplication—the Popperians are, as we have already seen, doing poorly within the

professional econiche.[70] If the professional econiche were all there were, they would be doomed to extinction; for as D. L. Krantz has written: "Ideas without recruits become like Bishop Berkeley's unheard fallen tree."[71] Within the profession, Popperians have been minimally effective in duplicating themselves, transmitting their approach and tradition, propagating. They hold no positions of influence; even in their own departments they are "marginal men";[72] they are widely dispersed and have no regular meetings and no official journals; they are not awarded research grants; their publications are sometimes blackballed;[73] there is no bibliography of their publications, and these publications are scattered among many different journals throughout the world; many of their findings are not being utilized and developed; they have no graduate school (with the possible exception of the LSE) from which to generate new professional philosophers of their own persuasion, or to inculcate that "tacit knowledge" whose importance Polanyi emphasizes. And even if they did, there are no longer many jobs available for any professional philosophers of *any* persuasion, due to the decline in university enrollments in Britain and America. Thus, in terms of the profession, the Popperians clearly do not meet the "structural requirements" that Campbell has specified[74] for a "self-perpetuating social system" that is a vehicle for scientific knowledge. Rather, within the profession they form an "encapsulated and self-isolating school of thought" of the sort that Krantz sees as most likely to appear when fundamental assumptions are in question.[75] Thus, if one were to restrict one's attention to the profession, it would be difficult to be optimistic about the survival of the Popperian approach. For in the professional econiche that is conventionally thought to provide the route for the study, development, application, dissemination of ideas—that is, the system of graduate education—the gates are guarded and secured by an ideologically hostile professionalism.

If one looks at the question of transmission more broadly, however, the situation looks different. The philosophical profession, as it is at present constituted, seems—at least to me—to be an evolutionary dead end: like the arthropods whose brains are built around their gullets. Many professionals—especially in philosophy, but also to a certain extent in physics, medicine, and psychology—are the products of isolation and inbreeding, and the departments that they inhabit provide econiches unfavorable for the evolution or propagation of revolutionary new ideas. Such ideas—and civilization—emerge through contact and communication, not through specialization in isolation.

On this latter ground, the Popperians have been more adaptive, and have outflanked the profession. Like Popper himself, they address the wider community (although their plentiful "academic" publications also attest to their professional competence). Unlike the professional philosophers, most Popperians live intellectually in several diverse cultural groups: the profession and the wider scientific and literary culture; also unlike the professional philosophers, they live much more internationally, and are less confined to their national professional associations. Thus their apparent weakness may in fact be a great strength.

The Popperians are then marginal men in the sociological sense. Although marginality can freeze people with anxiety and resentment,[76] it can also—when successfully managed—be the source of creativity.[77] The marginality of the Popperians has provided them with the opportunity, in negotiating among conflicting communities, values, and ideologies—as well as the incentive—to develop objectivity and perspective, as well as that creativity that consists in rendering what is given problematic.

In terms of survival, what matters to the Popperians is, first, that they communicate to the public and disseminate their ideas thereby. Here there is evidence of their success in dealing with their marginality. Second, they must create a nurturing intellectual environment for themselves. Here there is the evidence of their having failed to deal fully with their marginality.

But does it matter whether the Popperian approach to philosophy survives? No: not at all. That is, *unless it is correct.*

IV. The Intellectual Position of His Opponents: The Wittgensteinian Problematic

23. There are many reasons for this deep divide between Popperian philosophy and that of the philosophical profession. Some are historical, psychological, sociological, and personal. But there is also an intellectual obstacle. In this part, I would like to reconstruct what Popper would call "the logic of the situation"—in terms of what I shall call the "Wittgensteinian problematic"—the very different route that the bulk of the philosophical profession has taken in response to the crisis that I outlined in Part II of this essay. It is easy to see this opposition as simply hostile, and to ignore the problematic that forces the position. By becoming clearer about this, we can identify where vigorous rational argument would be most relevantly and effectively applied—to the extent that the opposition is amenable to it.

In examining philosophical viewpoints, one has to attend carefully to at least three separate aspects: (*a*) their tenets and the problems they claim to have solved; (*b*) their problematic; and (*c*) their research programs.[78] These different aspects may not be equally well known or equally influential. Thus one philosopher's influence may come chiefly from the problems he or she has solved, whereas the influence of another philosopher—who may indeed not even have solved any problem—may come chiefly from the research program that he or she has sponsored.

The thrust of Popperian philosophy is clearly theory and problem oriented. Thus I can, in a preliminary way, define Popper's position in the history of

thought, and indicate my own relationship to him, in a sentence: with his theory of falsifiability, Popper solved the problem of induction and made an ingenious, but somewhat defective, solution of the problem of demarcation; moreover, by generalizing and somewhat correcting Popper's theory of criticism, one can solve the problems of skepticism, fideism, and rationality.[79] All this renders traditional epistemology—and much of the rest of traditional philosophy—obsolete.

Popper's *problematic*, the nexus of background, influences, and problems that he exploited in building his philosophical outlook, cannot so readily be summarized, depending as it does on a historical account. But it is readily available, in his own work and in that of others.[80]

With Wittgenstein, the situation is different. It is not easy to identify any philosophical problem that he can be said to have solved, or any new philosophical theory that he propounded. If one turns to the work of the young Wittgenstein, the *Tractatus Logico-Philosophicus* (1921), one must qualify this judgment a bit, for in that work Wittgenstein did attempt to dispense with Russell's theory of types through arguing that to know the sense of a symbol definitely and completely is to know all its possible combinations; and that one thus need not in addition state its range of applicability. This view impressed Russell, and was, as Russell put it in his Preface to the *Tractatus* in 1922, "not at any point obviously wrong." It was, however, refuted by the work of Church and Gödel in the 1930s.[81]

Wittgenstein's influence stems, rather, chiefly from the research program inspired by his later philosophy. I shall discuss this below.

It is, however, the relatively unappreciated Wittgensteinian problematic that I want to discuss first; for this is rarely articulated. And here Popper and Wittgenstein clash most dramatically. By reconstructing the logic of the situation that leads people into Wittgensteinian philosophy in the first place, and then traps them there, we may better appreciate what the key issues must be. For Popper's significance here lies in having undermined the agreed problematic that sustains Wittgensteinian thinking.

In my view, one problem—and only one problem—lies at the root of the Wittgensteinian problematic and at the root of the split between Wittgensteinians and Popperians. This is the old problem of induction. If the problem of induction remains insoluble, then philosophy may take the path the professionals have staked out. If Popper has, as he claims, solved the problem of induction, then professional Wittgensteinian philosophy is a mistake, and continued work in that vein is wasted.

In other words, the problem is not merely one of fashion. I do not want to follow Bertrand Russell when he wrote of his own displacement by the Wittgensteinians: "It is not an altogether pleasant experience to find oneself regarded as antiquated after having been, for a time, in the fashion. It is difficult

to accept this experience gracefully."[82] Bertrand Russell's experience both caters to and lies outside the Wittgensteinian problematic. For Russell could not solve, and did not claim to have solved, the problem of induction, even though he was preoccupied with it throughout his life and found its solution of overriding importance.[83] Wittgensteinian philosophy, on the other hand, begins with the conviction—the correct conviction—that the problem is insoluble in Russellian terms, and proceeds from there: from its viewpoint, Russell's work *is* antiquated.

In the following, therefore, I propose to offer a historical reconstruction of the problem situation that leads to the development of contemporary professional philosophy; and to show how this development hinges on the assumption that the problem of induction cannot be solved. The entire matter looks very different indeed from a perspective, such as the Popperian one, within which the problem of induction *has been solved.*

24. I shall proceed in this way because most professionals come in—and settle in—in the middle of the story, as it were, and never have the opportunity to look at the development from a perspective in which it is anything but necessary or desirable.

(1.) To generate our problem situation, we need, as a start, a *scientific imperialism* of the sort available in logical positivism. I refer of course to the claim that all legitimate utterances are to be judged in terms of the canons and criteria of science, conformity to which is assumed as a hallmark of progress and intellectual advance.

The positivist idea that sense observation is the foundation of all knowledge works together with elementary logic to create a universal *theory of criticism* and *explanation of error.* Thus, if pure sense observation is the one and only true source of knowledge, and if such reports of sense observation serve as the only premises in argument, their truth will be transmitted to the conclusion of that argument. On this account, any legitimate (i.e., properly sourced or justified) theory would be one derived logically from, and justified in terms of, such premises, and an unacceptable theory would be one that cannot be so derived. Error stems from the acceptance of a theory (or pseudo-statement) that cannot be derived logically from sense observation reports.

(2.) Next, we need to generate an epistemological crisis. It is noticed—and it is about time that it was noticed!—that many *perfectly legitimate scientific claims* cannot be justified in the way demanded. As explained earlier, every universal law of nature is too strong to function as the conclusion of a valid argument whose only premises are sense observation reports. There is no way logically to reach from a finite set of such reports as premises to a universal law of nature as conclusion. And the problem is of course larger: not only are scientific laws not derivable from sense observation reports; various principles supposed to be indispensable to science—including principles of induction,

verification, and causality—also cannot be derived from sense observation reports. In short, the principle of criticism that was advanced simply does not work. And it *appears* that the relationship between evidence and conclusion must be *illogical*. Illogic is at the heart of science.

(3.) The next, crucial, step in our developing problem situation is to attempt to resolve the crisis. It is asserted, often as if triumphantly, and even as if profoundly, that the relationship between evidence and conclusion is *not* illogical, only *non*-logical. There are two kinds of inference: there is *de*duction, which defines logic; and there is *in*duction, which defines science. Induction is indeed not deductive; but there is no need for it to be so. "Everything is what it is and not another thing." A pseudo-problem was artificially created by the unwarranted imperialistic assumption that the canons of science have to conform to the canons of logic; whereas, in fact, induction is not a *faulty* sort of deduction. Rather, induction is ultimate, defining science—just as deduction is ultimate, defining logic. As Wittgenstein put it: "Here grounds are not propositions which logically imply what is believed. . . . For the question here is not one of an approximation to logical inference . . . . That is an inference; but not one belonging to logic."[84] Thus the problem of induction is "dissolved" by learning not to apply the standards of logic to inductive inference.[85]

The philosopher should then, so it is contended, not *judge* between deduction and induction, not judge induction by deductive standards. Rather, his or her job is to describe and attempt to make clearer the standards and principles of deductive *and* of inductive reasoning, as they are embedded in actual practice.

(4.) A question is now raised. Why not extend the whole process a step further? For there exist other disciplines and "forms of life" that are neither logical nor scientific. There are, for instance, history and jurisprudence and religion and politics. In the past, such disciplines have frequently been criticized, even by their own practitioners, by reference to logical and scientific standards. Yet if logic cannot be permitted to judge science, can science be permitted to judge other disciplines?

(5.) A negative answer is quickly provided. Each discipline or field or "language game" or "form of life," it is said, has its own ungrounded ultimate standards or principles or "logic," *embedded in action*,[86] which need not conform to any other standards and which it is the job of the philosopher to describe. As Wittgenstein says: "As if giving grounds did not come to an end sometime. But the end is not an ungrounded presupposition; it is an ungrounded way of acting."[87]

(6.) But this means that there is no arguing or judging among disciplines—or different activities—any more. Logic cannot judge science; or science, history; or history, religion. There is a spangled diversity. Scientific imperialism makes way for disciplinary independence—some might say anarchy. The fragmentation

of the university and of the community is given a theoretical justification; and in this theoretical justification itself resides all that remains of unity and community. Whereas the positivist or empiricist explanation of error provided a universal theory of criticism, the new explanation of error that arises here does away with such criticism: philosophical error is now thought to arise from the imposition of standards in usage in one area on other different areas.[88] Philosophical error is no longer (as in positivism) attributed to the use of utterances that are not logically reducible to reports of sense observations. Yet the concern to eliminate philosophical error perversely remains: and its chief source—that is, the chief source of "the bewitchment of our intelligence by means of language"[89]—is to apply the rules of one activity, of one language game, to another. Language will trespass its limits, or "go on holiday," when particular sorts of expression are used outside their proper domain or range of application. Philosophical critique becomes critique not of content but of criteria application: the activity of showing how language may stray from its proper place and then bringing it back to its correct context. Taken as a whole, empiricist philosophy may itself be regarded as one grand "category mistake," that of supposing that different language games must satisfy the criteria of one supremely authoritative language game: that of science. Yet, there is nothing wrong, so it is contended, with an empiricism within its proper limits: *empiricism is all right for science:* it expresses the principles behind the shared practice of the community of scientists.

(7.) In the course of this argument, the nature of philosophy has been recharacterized. Contemplating their discovery that the empiricist theory of criticism could not work, philosophers have reached the conviction that *any* general philosophical theory of criticism is impossible. To criticize, to evaluate, to explain, these are no longer proper philosophical aims: what remains to the philosopher is to describe the logics or grammars of various kinds of discourse and activity, the many different sorts of language games and the forms of life in which they are embedded.

A new explanation of error—and later Wittgensteinian thought certainly ranks as such—has often, in the history of philosophy, led to a program of reform whose aim is to create conditions under which such errors will no longer arise. So it is here. Wittgenstein himself never claimed that *all* identifiable disciplines and activities in which people engage are separate language games each with its own sets of rules. Many of his followers, however, did just this, supposing that each individual activity—law, history, science, logic, ethics, politics, religion—has its own special grammar or logic; that mixing the grammar of one of these with that of another leads to philosophical error; and that it is the *new* job of the philosopher—his new research program[90] under the Wittgensteinian dispensation—to describe in detail these separate logics or grammars. In this spirit two generations of British and American professional

philosophers came to write books with titles such as *The Vocabulary of Politics, The Language of Morals, The Logic of Moral Discourse, The Logic of Historical Explanation, The Language of (Literary) Criticism, The Language of Fiction, The Uses of Argument, The Logic of the Social Sciences, The Logic of the Sciences, The Province of Logic, The Language of Education, The Logic of Religious Language, Faith and Logic, Christian Discourse, The Language of Christian Belief, The Logic of Colour Words,* and so on *ad nauseam*.

Any philosopher, whether well-seasoned in his or her subject or a budding Ph.D., was thus provided with a simple "research formula" whereby a book or learned paper could be produced: "Take one of the phrases 'The Logic of *x*,' 'The Language of *x*,' or 'The Grammar of *x*,' substitute for *x* some activity or discipline such as just named; write a treatise on the topic so created." The easiness with which such programs could be carried out goes far to explain the immense success of such philosophizing—as witness to which I should mention that each of the titles cited has decorated a book or monograph actually published.[91]

And yet, latent in all this is a new imperialism, generally unconscious, according to which disciplines or forms of life must conform as follows: *true* forms of life (*a*) must not judge one another; and (*b*) must not try to describe some *common* world in collaboration with another discipline since *each* form of life creates its own world. In this new imperialism the nature of philosophy is recharacterized—and the opportunity of community is lost.

(8.) The essentials of our problematic have now been set down; but some important matters have not yet been mentioned. There is, for example, the way in which this whole line of thinking has been reinforced by positive reactions on the part of other disciplines. Colonized subjects may of course be expected to rejoice in the difficulties of their masters; and so it is here.

Take as an example the case of religion. Quite interestingly, one can find in much philosophy of religion of the past fifty years a development almost exactly parallel to (although largely independent of) that in professional philosophy. Traditional *apologetic* philosophy of religion had also been imperialistic, insisting that findings in other areas of human life at least conform to those of religion. With the great theologian Karl Barth, however, one finds the rejection of apologetic theology and the substitution for it of *kerygmatic* theology, wherein the job of the theologian becomes simply to describe the basic—and ultimate—presuppositions of Christianity. Consequently, it comes as no real surprise that philosophy of religion and philosophical theology have been given a new lease on life by the Wittgensteinian problematic.[92] *For the self-conception of such disciplines now matches the professional characterization of the way all disciplines and ways of life must be.*

(9.) One final element in the problem situation lying behind contemporary

professional philosophy should be noted. Though it is psychological and sociological in character, there is no doubting its importance. The development discussed provides what amounts to a *recipe* for generating a team-style department of philosophy, in which one professional does logic, another does science, and so on—where "does" means to describe the logical structure, the "grammar" of various established fields. The *activity* of old-fashioned positivism may even remain here: that is, formalism and the descriptive analysis of the methods and presuppositions of the natural sciences and logic. What must be sacrificed is not the activity but the judgmentalism of early positivism. There is a "live and let live" attitude—subsumed under a common paradigm wherein it is assumed that the problem of induction is a pseudo-problem, insoluble on its own terms because those terms are misconceived.

Thus it is that Ludwig Wittgenstein, for all his trials and tribulations, never had to battle for recognition—*for he told the professionals what they wanted to hear.* It is consoling for all those isolated from the wider culture to be reassured that it is all right to "do their thing," and—ironically, through "team work" with colleagues—to continue to destroy rather than to create community: that their usage and activity, whatever it may be, is indeed authoritative.

25. This entire chain of argumentation, and the problematic that it constitutes, depend on the first steps: the claims that sense experience is the foundation and justification of all knowledge; that there exists induction and that the problem of induction cannot be solved nor the scientific method charted while deductive logic is retained. When these claims are shown to be invalid, as Popper has done, the entire argument unravels, and a whole generation of philosophizing is intellectually undone.

Within the Popperian perspective an alternative program of criticism is put forward that develops critical methods consistent with, although not restricted to or limited to, science; methods that are positively applicable to and to be integrated with the examination of all fields to the extent at least to which they are devoted to uncovering and discovering the truth, to the extent to which such fields aim at explanations, representations, descriptions of the universe in which we live, and to the extent to which they aim at increased "fit" in Campbell's sense.[93] The growth of science is assimilated into a critical evolutionary perspective that provides a theory of growth generally.

Hence when one considers the question of how ideas improve with regard to their fit to the environment—the question of their correspondence to reality—any question of their *justification* is as irrelevant as any question about whether a particular mutation is justified (or foresighted, or suitable in advance of natural selection, in the Lamarckian sense). Nor does survival justify or guarantee the survivor: a species that survives for thousands of years may nonetheless become extinct. And a theory that survives for generations may eventually be refuted—as was Newton's.

All disciplines are seen as evolutionary products that, as far as their

intellectual viability is concerned, are to be subjected to critical examination—an examination that includes the critical review of their fundamental principles. There are no longer any principles that are fundamental in the sense of being beyond criticism and examination. There is no method *peculiar* to philosophy or to science or to logic (see epigraphs to this essay). The same general critical method is universal. Moreover, it is only now that the question can arise as *to what extent* the methods of the sciences are applicable in other areas.

A false understanding of the methodology of science led to a "research" program that characterizes contemporary professional philosophy; a new and correct understanding of the methodology of science leads, however, to a very different research program for philosophy and undercuts the assumptions on which the previous "research" was based. The recharacterization of science leads to a recharacterization of virtually every other discipline (see Section 11, above).

V. The Popperian Harvest

26. In this section I should like to state very briefly, without elaboration, the main achievements of the Popperian work and perspective.

(1.) Popper solved the problem of induction, in all its classic manifestations.[94]

(2.) His solution to the problem of induction proved to be *exemplary*, in the first sense that Thomas Kuhn gives to the term "paradigmatic."[95] Exactly comparable approaches, using the same strategies and ideas, could immediately be applied to all the main problems of epistemology and methodology: the is/ought problem; the problem of other minds, of the external world, of the uniformity of nature, of the existence of the past, of the existence of matter, of the existence of physical space, and of time independent of perception. This is no arbitrary listing of epistemological problems. These are the problems treated by Bertrand Russell in his classic work *The Problems of Philosophy* (1912), and by A. J. Ayer in his *The Problem of Knowledge* (1956) and *The Central Questions of Philosophy* (1973). They are Hume's epistemological problems.[96]

(3.) Thereby classical epistemology—and also most of meta-ethics—is rendered obsolete. All classical epistemology can be shown to depend on a mistaken fusion of justification and criticism.[97]

(4.) In place of classical epistemology, a new *evolutionary epistemology*, at once descriptive and normative (or methodological), is introduced. This epistemology is embedded in a *metaphysical outlook* rooted both in biology and in physics and in a *characteristic morality* and posting of goals oriented to the advancement of human self-knowledge and knowledge of the universe. This approach specifies the metaphysical, philosophical, and scientific presuppositions of the open society; and it argues that these presuppositions in fact obtain in our universe—*the open universe in which something comes from nothing.*[98] This aspect of the Popperian perspective is paradigmatic in Kuhn's second main sense: it identifies a constellation of views and values: a viewpoint about one's own self-nature and the nature of consciousness, about the nature of society and of the universe in which one lives.

(5.) Popper's solution to the problem of induction proved to be exemplary not only for epistemology, but also for a variety of other subject matters. Thus Gombrich applied it to the theory of learning and to the history of art; Campbell applied it to biology and evolutionary theory; and Watkins applied it to ethics. Tyrrell Burgess and H. J. Perkinson have more recently applied it to educational theory—which of course is appropriate in that educational theory is one of the chief sources of the entire Popperian perspective.[99]

(6.) Although exemplary and paradigm-shifting from the very start, Popper's approach originally contained no explanation of its own power, no identification of the source of its originality. I provided such an explanation and identification, and thereby generalized the approach, in the course of my own resolution of the problems of skepticism and presuppositions. The source of the power of the Popperian approach lay in its implicit unfusing of justification and criticism.[100] Involved here is not merely a paradigm shift in either of Kuhn's main senses, but something farther reaching that I call a "metacontextual shift."[101]

(7.) A need for the rewriting of the history of science is immediately created—a problem-oriented historical program conforming to principles of evolution and learning theory rather than to the principles, presuppositions, and style of inductivism. Joseph Agassi charted the outlines of such a program in his *Towards an Historiography of Science* (1963).[102]

(8.) A need for rewriting and restructuring the history of philosophy is created. Seen anew from the perspective of a separation between justification and criticism, the chief crises and turning points of the history of philosophy undergo metamorphosis.

NOTES

0. Sections of Parts I and II of this essay have appeared, in preliminary draft, in "Haphilosophia shell Hamada Bemabath LeHachor," in *Machshavot*, Tel Aviv, 1979; and "Ein schwieriger Mensch: Eine Porträtskizze von Sir Karl Popper," in *Physiognomien: Philosophen des 20. Jahrhunderts in Portraits*, ed. Eckhard Nordhofen (Königstein: Athenäum Verlag, 1980), pp. 43–69. I am grateful for helpful comments on earlier drafts of this essay from Avner Cohen, Brian Gomes da Costa, Ian C. Jarvie, the late Walter Kaufmann, Paul Levinson, Peter Munz, Ben-Ami Scharfstein, and J.W.N. Watkins.

1. A. J. Ayer, *The Problem of Knowledge* (Edinburgh: Penguin Books, 1956), p. 7.

2. Karl R. Popper, "Preface to the English Edition, 1958," *The Logic of Scientific Discovery* (London: Hutchinson, 1959), pp. 15–16.

3. Charles S. Peirce, "How to Make Our Ideas Clear," in *Collected Papers* V (Cambridge, Mass.: Harvard University Press, 1934), p. 252. The essay was first published in *Popular Science Monthly* in 1878.

4. Popper, *Unended Quest* (London: Fontana, 1976), p. 40.

5. W. J. Gardner, E. T. Beardsley, and T. E. Carter, *A History of the University of Canterbury, 1873–1973* (Christchurch: University of Canterbury, 1973), p. 308.

6. *Ibid.*, pp. 262–263.

7. See Greg Lawless, ed., *The Harvard Crimson Anthology* (New York: Houghton Mifflin, 1980).

8. Popper, *Objective Knowledge* (London: Oxford University Press, 1972), p. 32. (Popper attributes the last line to R. Hochhuth.) See also Bryan Magee, *Modern British Philosophy* (New York: St. Martin's Press, 1971), pp. 66ff.

9. See W. W. Bartley, III, "Theory of Language and Philosophy of Science as Instruments of Educational Reform: Wittgenstein and Popper as Austrian Schoolteachers," in *Methodological and Historical Essays in the Natural and Social Sciences*, ed. R. S. Cohen and M. W. Wartofsky (Boston: D. Reidel, 1974); Bartley, *Ludwig Wittgenstein e Karl R. Popper: Maestri di scuola elementare* (Rome: Armando Armando, 1976); and Bartley, *Wittgenstein* (New York: J. B. Lippincott, 1973).

10. See Popper's correction to this mistake in the "Preface 1980" to his *Realism and the Aim of Science*, vol. 1 of the *Postscript to the Logic of Scientific Discovery*, ed. W. W. Bartley, III (London: Hutchinson, in press).

11. See John T. Blackmore, *Ernst Mach: His Work, Life, and Influence* (Berkeley: University of California Press, 1972); and Bartley, "Philosophy of Biology *versus* Philosophy of Physics," in *Fundamenta Scientiae* 3 (1982) (forthcoming).

12. See Popper, "A Note on Berkeley as Precursor of Mach and Einstein," in his *Conjectures and Refutations* (London: Routledge & Kegan Paul, 1962), pp. 166–174; John Myhill, "Berkeley's 'De Motu': An Anticipation of Mach," *University of California Publications in Philosophy* 29 (1957): 141–157; and Gerard Hinrichs, "The Logical Positivism of *De Motu*," *Review of Metaphysics* 3 (1950): 491–505.

13. See Bartley, "Logical Strength and Demarcation," in *Rationality in Science and Politics*, ed. Gunnar Andersson (forthcoming).

14. On the anticipation of Hume by Sextus Empiricus, see R. H. Popkin, *The History of Scepticism from Erasmus to Spinoza* (Berkeley: University of California Press, 1979), and *The High Road to Pyrrhonism* (San Diego: Austin Hill Press, 1980).

15. See Bartley, "Goodman's Paradox: A Simple-Minded Solution," in *Philosophical Studies* (December 1968): 85–88; "Theories of Demarcation between Science and Metaphysics," in *Problems in the Philosophy of Science*, ed. I. Lakatos and A. Musgrave (Amsterdam: North-Holland, 1968), pp. 40–119; "Critical Study: The Philosophy of Karl Popper: Part III: Rationality, Criticism, and Logic," *Philosophia* (1982); and "Eine Lösung des Goodman-Paradoxons," in *Voraussetzungen und Grenzen der Wissenschaft*, ed. G. Radnitzky and G. Andersson (Tübingen: J. C. B. Mohr [Paul Siebeck] Verlag, 1981). See also Nelson Goodman, *Fact, Fiction and Forecast* (Cambridge, Mass.: Harvard University Press, 1955; new edition, 1973), esp. pp. 74ff. See also W. V. Quine, "On Popper's Negative Methodology," in *The Philosophy of Karl Popper*, ed. P. A. Schilpp (LaSalle, Ill.: Open Court, 1974), p. 220.

16. Reprinted in A. J. Ayer, ed., *Logical Positivism* (London: George Allen & Unwin, 1959), pp. 60–81.

17. See Rudolf Carnap, "Preface to the First Edition," of *The Logical Structure of the World* (Berkeley: University of California Press, 1967), p. xvii.

18. See Popper, *The Logic of Scientific Discovery*; and C. G. Hempel, "Problems and Changes in the Empiricist Criterion of Meaning," *Revue Internationale de Philosophie* (1950), reprinted in Ayer, ed., *Logical Positivism*.

19. Kurt Gödel, "A Remark about the Relationship between Relativity Theory and Idealistic Philosophy," in *Albert Einstein: Philosopher-Scientist*, ed. P. A. Schilpp (New York: Harper Torchbook, 1959).

20. Howard Stein, "On the Paradoxical Time-Structures of Gödel," *Philosophy of Science* (December 1970): 589–601.

21. Eugene P. Wigner, *Symmetries and Reflections* (Bloomington, Ind.: Indiana University Press, 1967), p. 186. See also Bartley, "Critical Study: The Philosophy of Karl Popper: Part II: Consciousness and Physics: Quantum Mechanics, Probability, Indeterminism, the Body-Mind Problem," *Philosophia* 7 (1978): 676–716. See also Popper, *Quantum Theory and the Schism in Physics*, vol. 3 of the *Postscript to the Logic of Scientific Discovery*, ed. Bartley.

22. See my "Philosophy of Biology *versus* Philosophy of Physics."

23. Pierre Duhem, *The Aim and Structure of Physical Theory* (Princeton, N.J.: Princeton University Press, 1914/1954).

24. Two such pseudo-paradoxes are Goodman's puzzle about the "grue" emeralds and Lewis Carroll's Barber-Shop Paradox. See my discussions of Goodman, as cited in note 15, above; and my discussion of Carroll in Bartley, *Lewis Carroll's Symbolic Logic* (New York: Clarkson N. Potter, 1977), pp. 444–449.

25. See Ernest Gellner, *Words and Things* (London: Gollancz, 1959).

26. Hilary Putnam, "The 'Corroboration' of Theories," in *The Philosophy of Karl Popper*, ed. Schilpp, pp. 238–239. See my discussion of a similar approach by Ayer in my *The Retreat to Commitment* (New York: Alfred A. Knopf, 1962), and in my "Rationality versus the Theory of Rationality," in *The Critical Approach*, ed. M. Bunge (New York: Free Press, 1964).

27. See also Popper's first book, finally published in 1979: *Die beiden Grundprobleme der Erkenntnistheorie* (Tübingen: J. C. B. Mohr [Paul Siebeck] Verlag, 1979), in which many of the basic ideas of *Logik der Forschung* are given their first statement.

28. See Popper's extended discussions of induction in *Objective Knowledge*, chaps. 1 and 2; and in *Realism and the Aim of Science*, chap. 1.

29. For a discussion of the psychological ramifications, and of work in psychology that refutes the inductivist approach, see Walter B. Weimer's discussion of Pribram's work on habituation, in Walter B. Weimer, "The Psychology of Inference and Expectation," in *Induction, Probability, and Confirmation*, ed. Grover Maxwell and R. M. Anderson, Jr. (Minneapolis: University of Minnesota Press, 1975), p. 459. See also Weimer's *Notes on the Methodology of Scientific Research* (Hillsdale, N.J.: Erlbaum [John Wiley], 1979), and his "The History of Psychology and Its Retrieval from Historiography," Parts I and II, in *Science Studies* 4 (1974): 235–258 and 367–396.

30. Falsifying observations are also theory-impregnated and thus *also* falsifiable. See Popper, *Logic of Scientific Discovery*, sec. 27, and Gunnar Andersson's essay in the present volume.

31. See my criticisms of Popper on corroboration in "Rationality, Criticism, and Logic."

32. See Carnap, "Preface," p. xvii.

33. Although Popper's criticisms of Freudian theory are on the whole sound, they are too extreme. Adolf Grünbaum has gone to the other extreme in his "Is Freudian Psychoanalytic Theory Pseudo-Scientific by Karl Popper's Criterion of Demarcation?" *American Philosophical Quarterly* 16 (1979): 131–141. See Popper's extended discussion of Freud in *Realism and the Aim of Science*.

34. See J.W.N. Watkins, "Confirmable and Influential Metaphysics," *Mind* (1958): 345–347; and Popper, *Logic of Scientific Discovery*, sec. 15, and *Conjectures and Refutations*, chap. 1.

35. See Popper, *Unended Quest*, p. 38.

36. See my *The Retreat to Commitment*, pp. 156–175; and "Rationality, Criticism, and Logic."

37. See my "Critical Study: The Philosophy of Karl Popper: Part I: Biology and Evolutionary Epistemology," *Philosophia* 6 (1976): 463–494.

38. See Popper, *The Open Universe: An Argument for Indeterminism*, vol. 2 of the *Postscript to the Logic of Scientific Discovery*, ed. Bartley.

39. *Ibid.*

40. See Popper's *Quantum Theory and the Schism in Physics*; see also Bartley, "Consciousness and Physics"; and Jim Edwards, "Hidden Variables and the Propensity Theory of Probability," *British Journal for the Philosophy of Science* 30 (1979): 315–328.

41. See Blackmore, *Ernst Mach*; and Bartley, "Philosophy of Biology *versus* Philosophy of Physics."

42. Popper, in *The Philosophy of Karl Popper*, ed. Schilpp, p. 1061; see also Bartley, "Biology and Evolutionary Epistemology."

43. See Donald T. Campbell, "Blind Variation and Selective Retention in Creative Thought as in Other Knowledge Processes," *Psychological Review* 67 (1960): 380–400.

44. See his *Unended Quest*.

45. Popper, "On the Possibility of an Infinite Past: A Reply to Whitrow," *British Journal for the Philosophy of Science* 29 (1978): 47–48; Popper, *Unended Quest*, secs. 28, 30, 35–36.

46. See Bartley, "Rationality, Criticism, and Logic." See also Gunnar Andersson, "The Problem of Verisimilitude," in *Progress and Rationality in Science*, ed. Gerard Radnitzky and Gunnar Andersson (Boston: Reidel, 1978), pp. 291–310, and the bibliographical references cited there. See also Joseph Agassi, "The Role of Corroboration," *Australasian Journal of Philosophy* 39 (1961): 82–91; and Popper's *Objective Knowledge*, rev. ed., app. 2, on Miller's paradox.

47. *Times Literary Supplement*, February 27, 1959, p. H 177.

48. See my discussion of Ayer and White in *The Retreat to Commitment*.

49. George S. Pappas, ed., *Justification and Knowledge* (Boston: Reidel, 1979), p. xv.

50. See my *The Retreat to Commitment*; "Rationality versus the Theory of Rationality"; and "Rationality, Criticism, and Logic."

51. N.M.L. Nathan, *Evidence and Assurance* (London: Cambridge University Press, 1980).

52. Stephen P. Stich and Richard E. Nisbett, "Justification and Psychology of Human Reasoning," *Philosophy of Science* 47 (1980): 188–202.

53. Jean-Paul Sartre, *Saint Genet: Actor and Martyr* (New York: George Braziller, 1963), p. 24.

54. Harold I. Brown, *Perception, Theory and Commitment: The New Philosophy of Science* (Chicago: Precedent, 1977).

55. See Campbell, "Descriptive Epistemology: Psychological, Sociological, and Evolutionary," mimeographed (William James Lectures, Harvard University, 1977). See also Campbell, "A Tribal Model of the Social System Vehicle Carrying Scientific Knowledge," in *Knowledge: Creation, Diffusion, Utilization* 1 (1979): 181–201. Incidentally, one unremarked unintended consequence of the current sociology of knowledge is almost certain to retard the growth of knowledge. Namely, some professionals will take advantage of their knowledge of the sociology and psychology of academic communities in order to manipulate them to their own advantage and thereby to perpetuate themselves at the expense of the growth of knowledge.

56. See, for example, Georg Lührs, Thilo Sarrazin, Frithjof Spreer, and Manfred Tietzel, eds., *Kritischer Rationalismus und Sozialdemokratie*, with Foreword by Helmut Schmidt (Berlin: J. H. W. Dietz Verlag, 1975); or T. W. Adorno, H. Albert, R. Dahrendorf, J. Habermas, H. Pilot, and K. R. Popper, *The Positivist Dispute in German Sociology* (London: Heinemann, 1976).

57. The winners of the Sonning Prize are: Winston Churchill, 1950; Albert Schweitzer, 1959; Bertrand Russell, 1960; Niels Bohr, 1961; Alvar Aalto, 1962; Karl Barth, 1963; Dominique Pire, 1964; Richard von Coudenhove-Kalergi, 1965; Sir Laurence Olivier, 1966; W. A. Visser't Hooft, 1967; Arthur Koestler, 1968; Halldor Laxness, 1969; Max Tau, 1970; Danilo Dolci, 1971; Sir Karl Popper, 1973; Hannah Arendt, 1975; Arne Naess, 1977; Hermann Gmeiner, 1979. A Sonning Music Prize is also awarded annually, whose recipients include Leonard Bernstein, Andrés Segovia, and Mstislav Rostropovitch.

58. See Konrad Lorenz, *Behind the Mirror: A Search for a Natural History of Human Knowledge* (New York: Harcourt Brace Jovanovich, 1977); and Alec Nisbett, *Konrad Lorenz: A Biography* (New York: Harcourt Brace Jovanovich, 1976). See also Campbell: "Reintroducing Konrad Lorenz to Psychology," in Richard I. Evans, *Konrad Lorenz: The Man and His Ideas* (New York: Harcourt Brace Jovanovich, 1975).

59. *Times Higher Education Supplement*, July 14, 1978, p. 11; August 4, 1978, p. 10; September 1, 1978, p. 11.

60. Gellner, *The Devil in Modern Philosophy* (London: Routledge & Kegan Paul, 1974), pp. 37–38.

61. See Gellner, "The LSE—a Contested Academy," in *Times Higher Education Supplement*, November 7, 1980, pp. 12–13; and Peter Scott, in *Times Higher Education Supplement*, November 16, 1979, p. 18.

62. See Ayer, *Part of My Life* (London: Oxford University Press, 1977); and Ayer, "The Currents of an Independent Ayer," *Times Higher Education Supplement*, January 23, 1981, pp. 9–10.

63. See Lührs et al., *Kritischer Rationalismus*; Adorno et al., *Positivist Dispute*; and the works of Hans Albert.

64. See Popper, in K. R. Popper and J. C. Eccles, *The Self and Its Brain* (New York: Springer Verlag, 1977), sec. 42: "The Self Anchored in World 3."

65. See Campbell, "Evolutionary Epistemology," in *The Philosophy of Karl Popper*, ed. Schilpp, pp. 413–463.

66. See Paul K. Feyerabend, *Against Method* (London: NLB, 1975); *Science in a Free Society* (London: NLB, 1978); *Knowledge without Foundations* (Oberlin, Ohio: Oberlin College, 1961); and *Realism, Rationalism and Scientific Method* and *Problems of Empiricism*, vols. 1 and 2 of his *Philosophical Papers* (Cambridge: Cambridge University Press, 1981).

67. See my *The Retreat to Commitment*; "Rationality, Criticism, and Logic"; and "Rationality versus the Theory of Rationality."

68. See Imre Lakatos, *Proofs and Refutations* (Cambridge: Cambridge University Press, 1976); *The Methodology of Scientific Research Programmes* (Cambridge: Cambridge University Press, 1978); and *Mathematics, Science and Epistemology* (Cambridge: Cambridge University Press, 1978). See also my "On Imre Lakatos," in *Essays in Memory of Imre Lakatos*, ed. R. S. Cohen, P. K. Feyerabend, and M. W. Wartofsky (Boston: D. Reidel, 1976), pp. 37–38.

69. See Weimer, *Notes on the Methodology of Scientific Research*, p. xi.

70. This is not surprising. Phases one and three are always at war with one another: to the extent that the Popperians succeed in phase one, they are likely to have trouble with phase three. See Campbell, "Descriptive Epistemology," and "A Tribal Model."

71. See D. L. Krantz and Lynda Wiggins, "Personal and Impersonal Channels of Recruitment in the Growth of Theory," *Human Development* 16 (1973): 133.

72. See Melvin Seeman, "Intellectual Perspective and Adjustment to Minority Status," *Social Problems* 3 (1956): 142–153.

73. Just a few examples from Popper himself: *The Open Society* was rejected for publication by twenty different publishers and their philosophical advisers before Ernst Gombrich, the art historian, and F. A. von Hayek, the Nobel prizewinning economist, convinced Routledge & Kegan Paul to publish it. Twenty years after its publication, Sir Isaiah Berlin still called it "the most scrupulous and formidable criticism of Marx by any living writer." *The Poverty of Historicism*, Popper's other critique of Marxism and the social sciences, which Arthur Koestler described as "one of the few books that would outlive this century," was turned down for publication in *Mind* by G. E. Moore.

74. Perhaps this may work to the advantage of Popper's ideas. Compare Allen Wheelis, *The Quest for Identity* (New York: Norton, 1958), pp. 153–154:

> Freud's work was quickly institutionalized, and thereafter was defended and promoted as a vested interest. . . . Marx, like Freud, used his work as the capital endowment of a social movement. . . . Darwin, in contrast, did not retain possession of his work. Battles were fought over the issues he raised, but his defenders were volunteers. No one was enlisted, and no one was compelled to follow a party line. There were no membership cards, no International Evolutionary Association. No one was prevented from furthering Darwin's work because of altering his theory. The discovery was attacked by institutions, but did not become institutionalized in defense. It was besieged by vested interests, but did not itself become a vested interest. Yet it survived, and has since participated widely in the ongoing process of science.

75. See David L. Krantz, "The Separate Worlds of Operant and Non-Operant Psychology," *Journal of Applied Behavior Analysis* 4 (1971): 61–70.

76. See David Riesman, *Individualism Reconsidered* (Glencoe, Ill.: Free Press, 1954), p. 163.

77. See Robert E. Park, "Human Migration and the Marginal Man," *The American Journal of Sociology* (May 1928): 881–893.

78. I use the term "research program" in the sense given by Popper in the "Metaphysical Epilogue" to his *Postscript to the Logic of Scientific Discovery*, vol. 3, *Quantum Theory and the Schism in Physics*. This idea was popularized, and given a somewhat different sense, by the late Imre Lakatos, for which see his *The Methodology of Scientific Research Programmes*, and my "On Imre Lakatos."

79. See my "Rationality, Criticism, and Logic"; "Rationality versus the Theory of Rationality"; and *The Retreat to Commitment*.

80. See Popper, *Unended Quest*; Bartley, "Theory of Language and Philosophy of Science as Instruments of Educational Reform"; Bartley, *Wittgenstein*; and Bartley, "Ein schwieriger Mensch."

81. Ludwig Wittgenstein, *Tractatus Logico-Philosophicus* (London: Routledge & Kegan Paul, 1922). See my *Wittgenstein*, pp. 48–51; and James Griffin, *Wittgenstein's Logical Atomism* (Seattle: University of Washington Press, 1964).

82. Bertrand Russell, *My Philosophical Development* (London: George Allen & Unwin, 1959), p. 159.

83. See Russell, *The Problems of Philosophy* (London: Williams and Norgate, 1912); and Bartley, "Logical Strength and Demarcation."

84. Ludwig Wittgenstein, *Philosophical Investigations* (Oxford: Blackwell, 1953), paras. 481, 486.

85. See Hilary Putnam's remark, quoted in Section 15, above.

86. See Putnam, "The 'Corroboration' of Theories."

87. Wittgenstein, *On Certainty* (Oxford: Blackwell, 1969), para. 110.

88. Many of the debates of the past four decades can be understood only in terms of this step. Take the "covering law debate," for instance. This debate—one of the most intense in professional philosophy during the 1950s and early 1960s—concerned the question whether Popper's model of scientific explanation (widely referred to as the "Popper–Hempel model" or as the "covering law model") could be applied, as both Popper and Hempel had maintained, to explanation in history writing, just as it was applied to explanation in physics and other sciences. This debate was peculiar for several reasons: (*a*) because of the large number of philosophers independently attracted to it; (*b*) because it was a pseudo-debate, depending almost entirely on misreadings of what Popper and Hempel had actually said, and on a string of non-sequiturs (see my "Achilles, the Tortoise, and Explanation in Science and in History," *British Journal for the Philosophy of Science* 13 [1962]: 15–33; and app. C of my *Lewis Carroll's Symbolic Logic*, pp. 466–470); and (*c*) because the issues were intrinsically unimportant: what Popper says is trivially correct—and rather unenlightening with regard to historical explanation. This debate can be understood only within the wider context of the Wittgensteinian problematic. If it is assumed that standards of inference must be field dependent and not universal, then any important standard-setting feature of investigative activity in any area—and certainly so important a feature as explanation—that purported to be universal, applying to all fields, and not field dependent, would pose a threat and a challenge. Hence the debate over the covering law model is in reality an attempt to show (an attempt contemptuous of the facts and of what was in fact written) that explanation *must* be field dependent too. What is really involved is the uncritical rejection of the contention that a model for scientific explanation could apply in an area that was nonscientific and that thus "of course" had a different "logic." Consequently, this debate produced no serious investigations of historical narrative, only disguised and misplaced polemics against what was conceived as scientific imperialism.

89. Wittgenstein, *Philosophical Investigations*, para. 109.

90. This is "research" only in a quixotic sense. True research, advancing knowledge, does more than grindingly apply a central theme to various different areas.

91. See my *Wittgenstein*, pp. 167–170.

92. See my *The Retreat to Commitment*, pp. 124–133.

93. See Campbell, "Descriptive Epistemology," and "A Tribal Model."

94. See *The Logic of Scientific Discovery*, for extended treatments by Popper of the problem of induction in its various aspects; see also his *Objective Knowledge*, chap. 1, and *Realism and the Aim of Science*, chap. 1, esp. secs. 4–7.

95. Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2d ed. (Chicago: University of Chicago Press, 1970), "Postscript—1969," pp. 175ff.

96. See my "Logical Strength and Demarcation."

97. On the fusion and possibility of separating justification and criticism, see my *The Retreat to Commitment*; "Rationality versus the Theory of Rationality"; and "Rationality, Criticism, and Logic."

98. See Popper, *The Open Universe*.

99. See my "Theory of Language and Philosophy of Science as Instruments of Educational Reform." [See also Perkinson's essay in the present volume, and his note 2, for references to his recent work in this area—Ed.]

100. See my *The Retreat to Commitment*; "Rationality versus the Theory of Rationality"; and "Rationality, Criticism, and Logic."

101. See my "Rationality, Criticism, and Logic," sec. 4.

102. Agassi, *Towards an Historiography of Science* (The Hague: Mouton & Co., 1963).