

III
THE CRITICAL APPROACH
TO SCIENCE

The Nature of Scientific Problems and Their Roots in Metaphysics*

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ACCORDING TO POPPER'S PHILOSOPHY the perfect division of labor in research would soon stop scientific progress. His view explains why in the history of science many investigators have concentrated on a handful of problems. The problem arises: How did investigators coordinate their choice of scientific problems? By what criteria did the bulk of investigators of a given period decide which problem was fundamental or important?

There exist a variety of such criteria, but one criterion stands out as the most important. Those scientific problems were chosen which were related to metaphysical problems of the period; those scientific results were sought which could throw light on topical metaphysical issues.

My aim is to present this as a historical thesis. I do not contend that scientific interest devoid of metaphysical interest is in any sense illegitimate or inferior. Investigators may wish to study a small part of the universe without bothering to study the universe as a whole, without even bothering to ask how their partial picture integrates with man's picture of the universe as a whole. Yet I contend, firstly, that very frequently problems, theories, and experiments which are traditionally regarded as important are highly relevant to the metaphysics of their time; and secondly, that my first contention provides a solution to the question of how the choice of scientific problems is coordinated.

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This is all I wish to assert in the present essay. I shall discuss problems of demarcation of science, of pseudo-science, and of metaphysics, mainly to dispel some vulgar errors concerning metaphysics (namely the identification of it with pseudo-science) and its role in the scientific tradition. I shall argue that metaphysics can progress—not so much in order to defend metaphysics as to expound my view of metaphysics as a coordinating agent in the field of scientific research.

I. Scientific Research Centers Around a Few Problems

Since there are more scientific problems to be studied than researchers to study them, a complete avoidance of overlap between projects is quite possible. The more the number of existing problems exceeds the number of researchers, the more one would expect the actual case to tend naturally toward the ideal of complete absence of duplication. But the facts are quite otherwise. Here are two historical examples where numerous obvious problems have been ignored. Diffusion is a phenomenon with instances widespread in physical nature: river water rapidly mixes with the oceans' waters, smoke with the atmosphere, salt with soup. Until the late eighteenth century no one paid any attention to this phenomenon and the scores of problems it raises. Priestley seems to be the first who studied it; Dalton concentrated on it for a while. Yet though Dalton's study received great publicity, only a handful of thinkers worked on diffusion before the celebrated studies of Maxwell rendered it an integral part of physics. My second example is elasticity, which was left almost entirely unstudied between the days of Hooke and of Young but was studied more and more seriously in the nineteenth century, only to be relegated in the twentieth century to the borders of applied mathematics and technology.

Whether concentration of intellectual power on a few problems is advantageous or a waste has hardly been studied because of misconceptions about science. Popper's theory of science answers this question unambiguously: perfect division of scientific research work will quickly bring scientific progress to an end. This theory makes the "friendly-hostile cooperation" between individuals crucial for progress. Some offer new ideas, some offer criticisms of these ideas, some offer alternatives to these ideas; if they all worked on different problems there could be no cooperation. Robinson Crusoe would be unable to sustain the development of science, because of his limited capacity to criticize himself and thus to get out of the routine of his way of thinking.

The existence of a variety of problems to be solved, and the fact that newcomers to science have a great variety of reasons which draw them to

science, would by itself render science almost Crusonian. But by some process which has not yet been studied or even noticed, the more a person's interest develops, the nearer it approaches the interest of other students of the same field. Somehow interests coordinate themselves. And my problem now is what is this means of coordination (though I shall not discuss here the way by which individuals learn to apply it).

Undoubtedly, there exists a variety of coordinating factors. New economic and political needs, new mathematical or experimental techniques, offer new avenues which are sometimes explored. Yet, by and large, there are minor and often secondary factors—secondary, because developments of techniques and of their fields of application often follow interests. By and large, widespread scientific interests may be shown to be connected with some metaphysical problem of the day. It is my contention that whatever the starting point of a person's interest in a science, the more that person's interest develops the closer it approaches the general interest, the interest which dominates the tradition in that science, and that this general interest springs from, and flows back to, metaphysics.

Most philosophers and historians of science would vehemently oppose this view. Descartes, as is well known, developed a philosophical theory in which metaphysics provides the framework for science. His ideas were greatly improved by Kant, but this was the last significant effort in this direction; for good reasons or bad Kant's idea has been universally rejected. In this essay I wish to rehabilitate metaphysics as a framework for science, but within the framework of Popper's critical philosophy.¹

My view is this. Metaphysical theories are views about the nature of things (such as Faraday's theory of the universe as a field of forces). Scientific theories and facts can be interpreted from different metaphysical viewpoints. For example, Newton's theory of gravitation as action at a distance was interpreted by Faraday as an approximation to a (future) gravitational field theory. An interpretation may develop into a scientific theory (such as Einstein's gravitational field theory) and the new scientific theory may be difficult to interpret from a competing metaphysical viewpoint. Metaphysical doctrines are not normally as criticizeable as are scientific theories; there is usually no refutation, and hence no crucial experiment, in metaphysics. But something like a crucial experiment may occur in the following process. Two different metaphysical views offer two different interpretations of a body of known fact. Each of these inter-

¹ This essay contains deviations from Popper's own views—as expressed in his classical *Logik der Forschung*. These deviations permit Popper's philosophy to accommodate the view that metaphysics is a framework for science. I do not think, however, that Popper himself will widely disagree with the content of this essay; indeed, I am happy to acknowledge much of it to his guidance, in lectures and in frequent and lengthy private discussions over a period of seven years, including the period of my graduate studies under his supervision.

pretations is developed into a scientific theory, and one of the two scientific theories is defeated in a crucial experiment. The metaphysics behind the defeated scientific theory loses its interpretative power and is then abandoned. This is how some scientific problems are relevant to metaphysics; and as a rule it is the class of scientific problems that exhibit this relevance which is chosen to be studied.

II. The Anti-metaphysical Tradition Is Outdated

My own interest in physics originates from a very early interest in metaphysics; the present essay may be no more than a projection of my own case history into the history of science at large. In my undergraduate days I used to resent the hostility toward metaphysics displayed by my physics teachers; my present view is in a sense an inversion of theirs. They derided all metaphysics as the physics of the past; I extol some metaphysics as the physics of the future. But I wish to be fair to their view, and perhaps the best means to arrive at a fair attitude to a doctrine is to try to see it in its historical perspective.

Francis Bacon's anti-Aristotelian-metaphysics, which was the first fanfare of the modern positivists, was very valuable. In launching an attack on Aristotelian metaphysics, he overenthusiastically took it to be an attack on all metaphysics. This was an exaggeration, and a very understandable and effective one at a time when Aristotelian metaphysics reigned supreme. Then came the victory of Copernicanism and of the Galilean-Cartesian metaphysics. This development admittedly altered the situation. From then onward Bacon's exaggerated idea might have been profitably cut down to size by studying the difference between Aristotle's bad metaphysics and Descartes's good metaphysics. Yet this is debatable, since at that time there was still a need to encourage experimentation rather than speculation. Moreover, throughout the seventeenth and eighteenth centuries metaphysics was closely linked with religion; and religion had to be banned from scientific discussions for very obvious social and political reasons. Since the early nineteenth century both of these factors have become negligible, but other factors have taken their place; fortunately for the positivist knight-errants, there was the task of slaying such awful metaphysical dragons as the Hegelians and the existentialists. Unlike Aristotelianism, positivism has not been useless during its period of obsolescence. It is still fighting bad metaphysics, under the somewhat absurd guise of fighting metaphysics as such.

In addition to being an overzealous criticism of irrationalist metaphysics, positivism has also served the rationalist metaphysician. Meta-

physics can easily degenerate into pseudo-science by providing a framework for *ad hoc* explanations instead of scientific ones. The Baconian-positivist attack on metaphysics as *ad hoc* or pseudo-scientific helped the good metaphysician by putting him on guard against irrational practices.

It is unfortunate that the merits of positivism are so often exaggerated, since positivism is conducive to ignorance. I have met physicists who know about only one metaphysician—Hegel—and only one detail concerning him—that he said when a doctrine of his turned out not to accord with facts “so much the worse for the facts.” Rarely has anyone paid more dearly for a silly joke.

It is not my purpose here to disprove positivism but I feel I have to stress that in this essay I am speaking of good metaphysics while intentionally ignoring bad metaphysics, after having acknowledged the partial justice of the positivist attack on it. Every field of human activity ought to be judged by its very best, and it is time to notice that examples of bad metaphysics do not show that all metaphysics is bad. One can show that all metaphysics is bad, but only after abandoning the ordinary or traditional meaning of the word ‘metaphysics.’ This word is used by Hegelians and by positivists to signify the theory of the cosmos as a whole, of the very mystery or essence of the universe. In his *Tractatus* Wittgenstein accepted Newton’s metaphysics as a framework for physics, but he did not call it ‘metaphysics’; he considered ‘the mystical’ alone to be the subject matter of metaphysics. The positivists, the Hegelians, and the mystics, rightly claim that the mystical is unexpressible. This is a point which Russell rightly considered (in his *Mysticism and Logic*) trivially true. Metaphysics in the sense of a theory of the mystical is hence impossible. My own use of the word ‘metaphysics’ in the present essay is in its traditional and much narrower sense. Metaphysical doctrines are to be found, first and foremost, in Aristotle’s *Metaphysics*, especially in Book Alpha: all is water; atoms and the void; matter and form; etc. There are a variety of sets of first principles of physics. Do these belong to scientific physics? Are they entailed by scientific theories? Are they useful for scientific research? I think they do not belong to scientific physics (though in principle they might). Metaphysical ideas belong to scientific research as crucially important regulative ideas; and scientific physics belongs to the rational debate concerning metaphysical ideas. Some of the greatest single experiments in the history of modern physics are experiments related to metaphysics. I suggest that their relevance to metaphysics contributes to their uncontested high status. And yet, I contend, the metaphysical theories related to these experiments were not parts of science. This raises the problem of what kind of relation between a given theory and observable facts renders that theory scientific.

III. A Historical Note on Science and Metaphysics

The term 'speculative metaphysics' and the term 'speculations,' when used as synonyms for 'metaphysics' (by Boscovitch, Faraday, and others), indicate the view that metaphysical doctrines are products of the imagination, in contrast with scientific theories which are—allegedly—products of inductive inference from facts. It was indeed this view which led to the tradition of divorcing science from metaphysics. The first modern positivist, Francis Bacon, presented the two methods, of induction and of speculation, as irreconcilably opposed to one another. The proper inductive investigation, he proclaimed, can be conducted only in the absence of all preconceived notions. Those whose minds are full of speculations are entirely unfit for proper scientific experiment and observation, much less for theorizing inductively: they are biased in favor of their speculations, and this bias makes them ready to observe only those facts which verify their speculations and unwilling to observe those facts which refute them. Consequently, they achieve not the truth but the reinforcement of their own preconceived opinions, and their biases thus become prejudices and superstitions.

Bacon's violent opposition to metaphysics was less violent than the ultra-modern one. His opposition to metaphysics was merely an opposition to its method; it was not an opposition to the abstract character of metaphysics but to the leaping to metaphysical conclusions. By developing science properly, by starting with observation and then slowly developing theories by gradually increasing the abstractness of knowledge, by ascending the inductive ladder properly without skipping any step, Bacon held, we shall end up with the most fundamental theory, namely, with scientific metaphysics. This metaphysics will be scientific because it will have been achieved, not by the speculative method, but by the inductive method.

Scientific metaphysics was later defended by Descartes and by Kant, each of whom considered his own metaphysics to be a body of certain, and hence scientific, knowledge. Their idea of certitude differed from Bacon's; it was based on *a priori* reasoning rather than on inductive inference. Consequently they viewed metaphysics as the beginning, not the end, of scientific inquiry. But both in viewing science as certain, and in taking it for granted that metaphysics must be scientific or perish, they barely differed from Bacon. It was William Whewell, the disciple of both Bacon and Kant, who first defended unscientific metaphysics from a scientific point of view.

In Whewell's view scientific doctrines do not emerge inductively from facts; they are first imagined and then verified empirically. And he con-

sidered his own (Newtonian-Kantian) metaphysics *a priori* valid, namely, demonstrable independently of empirical evidence. In accepting Kant's *apriorism* he rejected Bacon's view that all preconceived ideas are verifiable by virtue of their being prejudices, contending that much as people had sought to verify Newton's optics, much as they were prejudiced in its favor, they ultimately rejected it. His problem was how to explain why assent to Newton's mechanics was justifiable and assent to Newton's optics unjustifiable. He wished to find out the proper canon of verification and show that Newton's theory of gravity, but not Newton's optics, had conformed to it.

In brief (and in a slightly improved version), Whewell's canon can be put thus: proper verification is the result of severe tests. The procedure of severe testing is this: First try to explain known facts and state your explanatory theory as explicitly as possible. Then try to deduce in a rigorous manner from the theory a new prediction of observable facts. Then, and only then, decide by observation whether this prediction is true or false. If the prediction is false then the theory is obviously false too; if the prediction is true then the theory obviously explains the new facts without adjustment ("adjustment" being a suitable alteration or addition). In the latter case, Whewell declares, the theory is verified. Newton's theory of gravitation had been severely tested, and consequently the result of the tests could either refute it or be explained by it without any adjustment. In contradistinction, Newton's optics never stood the risk of a test and hence never explained a single new fact. Many new facts were alleged to be explicable by Newton's optics. Even Laplace had endorsed this allegation. Yet upon a simple and clear examination, which Whewell executed in a most masterly fashion, each of these new facts turned out to be explicable not by the original theory but by the adjusted theory.

Both Bacon and Whewell were interested in the problem of the demarcation of science. But their interests stemmed from different roots. Bacon considered Aristotelianism, which was then the academic metaphysics, to be the chief impediment to the advancement of learning. Whewell viewed Newton's metaphysics, which was by then the academic metaphysics, as demonstrable. His problem was not metaphysics but the overthrow of the allegedly verified Newtonian optics. Thus, while Bacon demarcated science mainly from metaphysics, Whewell demarcated science mainly from pseudo-science.

Since, according to Whewell, science begins by the invention of explanatory hypotheses, he was all for every possible source of inspiration. And he viewed all (reasonable) metaphysics as such a possible source. He gave a striking example for this. Kepler had developed his scientific hypotheses, Whewell maintained, in an attempt to carry out Plato's metaphysical program as outlined in his *Timeus*. This idea of Whewell's was so revolutionary that this great philosopher is now almost entirely forgotten

because Mill and his followers condemned him as an intuitionist. (This charge is, of course, quite untrue. Whewell relied not only on intuition but also on Kantian transcendental arguments and on empirical tests.)

Initially, Popper's interest in the problem of demarcation was similar to Whewell's, though his examples were different; it was Marxism and Freudianism which he viewed as pseudo-scientific. His demarcation of science may be contrasted with Whewell's thus: Whewell demands that a scientific theory be testable and emerge triumphant from the tests, while Popper merely demands testability. Neither of them is hostile to metaphysics, and both contend that metaphysics is sometimes important as a source of scientific inspiration. A remnant of positivist prejudice may perhaps be detected in Popper's lumping together (like Bacon and unlike Whewell) of a few kinds of nonscientific theories, including metaphysics, pseudo-science, and superstition, under the one label 'metaphysical.' Though I dislike this label, I do not think it matters beyond leaving some ambiguity concerning the difference between metaphysics and pseudo-science.

IV. Pseudo-science Is Not the Same as Non-science

Popper's idea (pseudo-science is untestable) is a marvel of simplicity. It explains why no matter how bad a pseudo-scientific doctrine is, its proponent may regularly win debates. It resolves the conflict involved when we feel obliged, against our own better judgment, to take a theory seriously because its proponents seem to be entirely undefeatable. It amounts to a proposal not to embark on the game before fixing its rules, before deciding in advance what kind of argument, if any, would be capable of defeating the proponent of a theory, and determining not to try to defeat him if he turns out too evasive to be vincible. As Whewell has pointed out, no kind of argument will defeat the proponent of any theory if he is allowed to adjust even minor details of his theory in an *ad hoc* fashion. On this Whewell and Popper are agreed. Yet wonderful as Whewell's ideas about pseudo-science are, by demanding too much from science he threw out the baby with the bath-water.

According to Whewell, scientific theories must also have withstood test. Consequently, he viewed as pseudo-scientific those theories which falsely claim to have withstood test. This leaves unclassified those theories which are testable but have been obviously refuted. As Whewell considered these to be neither scientific nor metaphysical, he confusedly implied that they are pseudo-scientific, especially when they are submitted to recurrent readjustment and retest. According to Popper such theories are

scientific, for he only demands testability; according to Whewell they could not be considered scientific, and so he held them in contempt. He knew that Newton's optics had been falsely held to have been verified. Yet he did not see that as long as verification was considered a hallmark of respectability, the immense respect for Newton gave these false claims an immense appeal. But if the requirement of Whewell and his predecessors of a respectable scientific theory is too stringent, is not Popper's requirement of a respectable scientific theory, namely, a high degree of refutability, a trifle too lax?

Traditionally, a variety of characteristics have been attributed to science. Popper accepts some of these attributes, such as high explanatory power, high informative content, abstractness, generality, precision, and simplicity; he rejects others, such as obviousness and verifiability. He seems to have claimed in his *Logic of Scientific Discovery* that the characteristics in the first group are all reducible to one, to testability. This is his justification for requiring only this one characteristic of a theory before labelling it 'scientific.' I have little doubt that Popper will fully agree that the spurious simplicity of some monistic doctrines (such as Marxism or mechanism) rather than their spurious explanatory power has deluded some people into regarding them as scientific. Simplicity, however, is traditionally viewed (since Leibniz) as the paucity of assumptions relative to the amount of factual information they explain, so that there is no need to differentiate between simplicity and high explanatory power for the purpose of demarcation. And Popper would say the same concerning explanatory power, which, in his opinion, increases with refutability. For my part, I consider that the various characteristics of science are less often dependent on each other than Popper suggests. But I still side with Popper in viewing spurious refutability, rather than, say, spurious simplicity, as the chief characteristic of pseudo-science, and for two reasons. First, whatever else may characterize a scientific theory, the very acceptance of the proposal that scientific theories are agenda to be tested renders Popper's proposal to check whether a doctrine in question is testable or only spuriously testable a matter of supreme practical importance. Second, the claim of pseudo-science is the claim for empirical character. And empirical character is nothing else but empirical refutability, as I shall soon explain. Thus, Popper's demarcation between science and pseudo-science does not require any amendment even on the assumption that he has erred in correlating the various characteristics of science. As to his characterization of science as such, it requires a reformulation if, as I think, his way of correlating the various characteristics of science is in error. I think we have to characterize scientific theories not only by their refutability, but also by their simplicity, high explanatory power, etc. This has an immediate bearing on the problem of selection of scientific problems and of scientific theories which is the topic of the present essay. According to Popper we

always look for the most easily refutable theory. In my opinion this is not the case.

V. Popper's Theory of Science

Popper's arguments for his claim that empirical character is empirical refutability are very compelling. Logically, observation reports can contradict theories but not entail them in any way. Philosophically, Popper's view is the doctrine of learning from experience as a special case of learning from mistakes, of the critical method. Socially, it presents students of nature as human rather than as unerring supermen. Historically, it opens wide vistas of new studies of the history of science uncharted by the modern science textbook. Popper's greatest contribution to the philosophy of science seems to me to be rooted in the simple idea that since empirical character is empirical refutability, scientific research is a special case of Socratic dialogue. But I deny that the empirical character of science is all that makes science what it is.

It is not difficult to find empirical developments, i.e., empirical refutations, outside the field of science. Thales's metaphysical doctrine ("all is water") was refuted empirically when water was first decomposed; Moebius (as I. Lakatos would say) may have refuted empirically the mathematical theory "all surfaces have two sides"; Faraday refuted empirically some spiritualistic superstitions; Marx's prophecy about the geographical location of the socialist revolution has been refuted by his Russian followers; and this amounts to the refutation of his materialism since it entails the valuelessness of imaginative ideas; the very important philosophical doctrine about the universality of common sense (which even Duhem still advocated) is empirically refutable by comparative studies. Necessarily, either such cases should be viewed as scientific or Popper's proposal should be considered inadequate. My choice is the latter: I propose to use Popper's convention as a convention concerning the empirical character of science, not concerning empirical science as such. There is no difficulty in admitting that daily experience, as well as some developments of mathematics (or metaphysics, or any other field of intellectual or practical development), manifest a certain empirical character, even though they do not belong to empirical science. Empirical science manifests its empirical character more systematically than mathematics, and it manifests other characteristics as well, which are lacking in mathematics.

But what about the claim that theories manifesting empirical character, i.e., refutable theories, also necessarily manifest the other characteristics of science, i.e., they have informative content, explanatory power, simplicity, abstractness, generality, and precision? I simply reject this claim.

As I have said earlier, I interpret a great deal of Popper's discussion in his classical work to be an attempt to support this claim. I consider the value of that part of his discussion as a valid criticism of his opponents and as stimulating heuristic material, but as very far from being a finished product.

To maintain my thesis I must contradict Popper here. He would say that research is conducted toward the finding and the testing of highly testable hypotheses, whereas I say that it is very often conducted toward the finding and the testing of metaphysically relevant hypotheses. And as a rule, I shall later show, research tends to begin with which hypotheses have a low degree of testability or are not testable at all. Consequently investigators often have to use great ingenuity to test a barely testable hypothesis, and even first improve a hypothesis to the point of rendering it testable to some degree. If the aim of science were merely producing testable hypotheses and then testing them such procedures would be irrational. But the aim of science, or rather the aims of science, are different.

The aim of science is to attempt to comprehend the world rationally, as we all agree (including the positivists who should disagree). But this is too vague. What is the rational method and what is comprehension? Rationality, said Popper, is manifest in empirical tests. He later generalized this: the rational method is the critical method. Is metaphysics rationally debatable? Yes. I shall argue that the study of a hypothesis of a low degree of testability is often conducted with a view to criticizing some metaphysical theory upon which it may have some bearing. So much for rationality. As to comprehension, Popper views it as deductive explanation, and he has suggested that explanatory power goes with refutability. I deny that explanation is the only method of comprehension. As I shall show later, the attempt to coordinate our various explanations within one metaphysical framework is not explanation, yet it is, in some weaker sense, an attempt at comprehension. Moreover, I deny that explanatory power is always dependent on refutability. Already in the last section of his great book Popper has noted that some theoretical systems may have some explanatory power and yet be untestable. I have already mentioned examples of refuted theories of little or no explanatory power.

Degrees of testability are, I think, of little practical importance. All that matters is that we may test in at least one way an interesting theory. According to Popper, there are two factors contributing to the degree of testability of a theory, the number of possible events which may refute that theory, and the probability of each potential refutation. To my mind the possibility of observing the next refuting event is all that matters, not the number of possible refutations. As it is the number of all excluded possibilities which is the content of the hypothesis, content is not the same as practical testability. *Ad hoc* explanations have some empirical content yet are untestable. Explanatory power is not content, and not even truth-

content (i.e., that part of a theory's content which is true), but I should say (in agreement with Leibniz's idea as I understand it), known-truth-content (i.e., the overlap of a theory's content with the class of true observation-reports). And high explanatory power is not the sole characteristic of a satisfactory explanation. As I have learned from Popper himself, a satisfactory explanation must be independently testable. Thus, Weyl's theory which unifies Maxwell's and Einstein's has a high explanatory power and a high degree of testability, but no known independent testability, and thus it is not considered scientific. Simplicity depends not only on explanatory power and the paucity of parameters, as Popper mentions in his early work, but also on depth, as he now says. Nor does abstractness go together with universality: Boyle's law is more general but less abstract than the theory of consumers' demand, and the Heitler-London theory is more abstract but less general than Schroedinger's theory.

The result is pluralism: we may admire one theory for its boldness, another for its explanatory power, another for its elegance; and yet another, I suggest, for the light it throws on some topical metaphysical issues.

There seem to be very good reasons for Popper's correlation of a higher degree of testability with a higher degree of explanatory power, etc., and these reasons are of heuristic value. One reason of Popper's is this: If one theory explains another theory, it is obviously not less refutable than the other. If one theory explains another theory as a first approximation, then it is more precise, and a higher degree of precision goes together with a higher degree of testability. This is so because a more precise theory excludes more (logically) possible states of affairs, thereby possessing both a higher informative content and a better (*a priori*) chance of being refuted, or a lesser *a priori* probability. These arguments are valuable but insufficient and partly incorrect.

In his classical paper "The Nature of Philosophical Problems and Their Roots in Science" Popper has given an admirable account of Pythagoras's metaphysics and the history of its refutation. When I read this excellent essay I decided to study under Popper; so the title of the present essay adverts to his, partly for sentimental reasons. Yet, perhaps because my prejudice in favor of metaphysics came first, I was unhappy about his taking Pythagorean metaphysics to be scientific. Since his reason was that this metaphysics was refuted, I was bound to examine his refutability criterion for the demarcation of science. I now propose his empirical refutability criterion to be the criterion of empirical character, not of empirical science as such. Empirical science is the set of highly informative and simple explanations which exhibit independent empirical character—satisfactory explanations, for short. I owe this idea to Popper himself: in his lecture courses Popper presents science rather in this way than in the way he does in his classical *Logic of Scientific Discovery*.

VI. Superstition, Pseudo-science, and Metaphysics Use Instances in Different Ways

Bacon justified his lumping together metaphysics with superstition and pseudo-science by saying that the method of them all is that of marshalling verifying or confirming examples or instances and persistently ignoring counter-examples or refuting instances. This is much too coarse a characterization; to refine it we must first notice a few of the different roles that instances may play in intellectual activity.

The role of an instance may be solely presentational: we understand an abstract idea better when we are told how to apply it to concrete cases. So long as the purpose of an instance is elucidatory, an author is at liberty to choose his instances so as to avoid a discussion concerning their truth or falsity, and the more obvious the instance the better. One should either take a presentational instance for granted or use another in its stead. The moment an instance is sufficiently significant to be not easily dispensable, it has additional roles.

The most important role of instances is their role as refuting instances. This is the crux of Popper's solution of the problem of induction: learning from experience is learning from a refuting instance. The refuting instance then becomes a problematic instance, namely, an instance which ought to be explained by a new theory. The last important role of instances is that of showing how high is the explanatory power of a proposed theory. Perhaps one may consider the instances explicable by a theory as problematic for those who wish to propose an equally good alternative to it. This would explain why usually previously refuting and/or problematic instances are presented as explained instances of a theory though that theory explains many other instances as well. So much for instances in science.

A common, though by now highly suspect, role is played by instances which Bacon has called 'clandestine.' A clandestine instance hints at a possible truth. For instance, a miraculous recovery may be due to unknown causes or due to the excellence of the doctor in whose charge the patient was at the time. If we accept an instance as clandestine, we need not at once accept the theory it points to (in our example, that Dr. X is excellent), but we are well advised to investigate the matter seriously. And the more clandestine instances there are that suggest a particular theory, the more seriously we should take the theory.

The most obvious characteristic of the superstitious is their serious approach to clandestine instances; the root of this lies in their want of a critical attitude. Not all errors are superstitions, only those concerning which we cannot conceive that we may be critical towards them.

In this sense of 'superstitious,' medieval empirical research was largely superstitious. The taking up of clandestine instances, hints which Mother Nature has mercifully thrown in our way, was quite routine procedure then. In modern times, mainly under Galileo's and Boyle's impact, this has been outlawed.

This immediately raises the question of the difference between a problematic instance, which requires explanation, and a clandestine instance, which should be ignored. The chief difference between them, I think, is that of attitude both toward theory and toward fact. When we have a problematic instance we first try to explain it and leave the question of the truth or falsity of our explanation to be discussed in a critical fashion afterward; whereas following a clandestine instance we hope to find the truth even though we may not fully understand it or fully formulate it to begin with—even though, that is to say, we are not capable of subjecting it to rational discussion straight away. And the same applies to facts. The fact constituting a clandestine instance, being a wondrous hint, should be taken seriously at once; whereas a problematic one should be capable of critical examination, and hence it must be repeatable.

Yet a critical attitude is but a necessary condition. While it is true that unrepeatable facts are useless, too many repeatable ones are left unstudied. Footprints in the sand are as repeatable as one could wish, yet science says precious little about them. In my view the ignored phenomena are those which our metaphysical frameworks are too poor to interpret (in the sense discussed below). They are too problematic. The same applies to theories, like elasticity theory, which are too difficult to incorporate within the existing metaphysical frameworks, and hence are not scientifically interesting.

Next comes the confirming or verifying instance. Whenever someone marshals instance after instance, challenging you to examine their truth, it is on the tacit assumption that if his instances are true his theory is also true. If you admit his instances and yet reject his theory he will marshal more instances. If you prove impervious to all his instances he will proclaim you unreasonable.

Confirming instances play the same role today as clandestine instances played in the Middle Ages. They play the role of clandestine instances for the uncritical audience and explained instances for the less uncritical audience. They are usually unsatisfactorily explained instances, yet the poor explanations are overlooked by audiences who are impressed because they are striking clandestine instances. For my own part I prefer to view all confirming instances as explained ones. For presenting an unsatisfactory explanation is still an attempt to explain, an attempt at a rational procedure; marshalling clandestine instances is plainly irrational.

To take an example. If someone throws a child into the river, Adler would interpret this act as one of self-assertion. And he would say the same if someone else rescues the child from the river. Thus, says Popper,

opposite modes of behavior toward others are both somehow covered by Adler's doctrine. Hence it is no explanation. Adler's doctrine plus one of a given set of additional hypotheses, selected to suit each of the different cases, will indeed explain each action. But then all these explanations are *ad hoc*. The feeling is conveyed that many cases have been strikingly explained by one single hypothesis because Adler has claimed, in effect, that these instances are indications of self-assertion, clandestine instances for his theory.

An example to this effect which has greatly impressed me is Freud's story of a married woman who unthinkingly signed her maiden name. Freud interpreted this as an unconscious expression of suppressed discontent with her husband and, indeed, he triumphantly added, a few months after her pen slipped in that ominous fashion the poor lady was divorced. This is pseudo-science at its worst; it is a glaring case of a clandestine instance thinly masked as explained instance. Since some married women divorce their husbands without having accidentally used their maiden names, and since other married women use their maiden names by mistake without ever asking for a divorce, clearly in this special case Freud erroneously claimed that the error and the divorce were explained by his theory of slips of the pen. Yet it does appear as if this theory spectacularly explains the unexpected relation between a slip of pen and a divorce.

The mark of pseudo-science is the use of confirming instances. The practitioner of pseudo-science, unlike the superstitious, is not surprised by criticism. On the contrary, he is often painfully aware of the existence of critics; he is only too ready to meet his critics and argue with them. He will claim in the argument that every relevant case is an instance of his theory, that his critics' challenge can easily be met, that the critics do not see the immense explanatory power of his view simply through being so hostile toward it. When his explanations are scrutinized, however, it will be seen that the critic's facts are explicable not by the theory itself, but by the theory plus some additional hypothesis. Usually the additional hypothesis is so trite and plausible that one hardly notices its having been added, and those who make a fuss about it are prone to be successfully dismissed as mere pedants. Yet the great ease with which the pseudo-scientist so impressively explains all phenomena rests on these trivial (and usually acceptable) additions, not on the original theory.

Popper has accepted the claim of the pseudo-scientist that he can interpret all phenomena. He has stressed (in his "Personal Report") that since pseudo-science can interpret any conceivable (relevant) phenomenon it is not refutable by any conceivable phenomena, and hence it is untestable or unscientific. This is very neat, and quite important, yet perhaps it ought to be more explicitly stressed that though pseudo-scientific doctrines have high interpretative power, they have low explanatory power. This characteristic pseudo-science shares with metaphysics.

When Thales said that all was water, he provides a few instances for his doctrine, instances which led Aristotle to hint, and Bacon to assert, that he had based his metaphysical doctrine on facts by using the inductive method (to wit, that his metaphysical doctrine was to some degree scientific). Thales used the freezing and the evaporation of water as examples of his doctrines. He also claimed that solid deposits left in kettles by boiled water, and solid deposits in river-mouths, were instances of water turning into solids.

It is difficult for me to say what would be Thales's answer to such questions as why can we not turn a whole bulk of water into a piece of chalk. Quite possibly Thales, being the first metaphysician, was partly superstitious and partly (in some sense) a pseudo-scientist, and also (as Aristotle states) partly a mythologist; I do not know. Yet I suspect he was really none of these. I imagine he was asked such questions and in reply simply confessed his ignorance. Descartes's answer, and Newton's and Faraday's (whose doctrines I shall soon discuss), however, are clear and straightforward: we are not unaware of the lacunae in our doctrines, they would say, and we shall try and find some scientific theories to deal with your question in due course.

There is a similarity and a difference between pseudo-science and metaphysics. Freud's theory of the slips of the pen, like Descartes's and Newton's and Faraday's (if not also Thales's) metaphysics, sketch possible explanations. Metaphysics may be viewed as a research program, and the false claims of pseudo-science as the result of confusing a program with the finished product.

One corollary of this is that metaphysics can degenerate into pseudo-science. This corollary seems to me to be true, and exemplified by Aristotle's metaphysics, which becomes appallingly *ad hoc* when applied to phenomena, as in his *De Caelo*. I find the following corollary more interesting: it may be possible to elevate a pseudo-scientific theory to the rank of metaphysics. The first step in this direction is to strip it of its pretentiousness by making its logic clear. Expurgated, Freud's theory may be viewed as an interesting metaphysics of psychology.

As instances of a metaphysical doctrine are not clandestine or even confirming, what kind of instances are they? Thales's instances, I think, served two purposes: one presentational, and one to show that his doctrine, be it true or false, is not as fantastic as it sounds. Newton's metaphysics, which asserts that the universe consists of atoms with their associated conservative central forces, was instantiated by his theory of gravity. This instance served a more significant role than a merely presentational one. It illustrated the potentiality of his metaphysics and thus constitutes a challenge to construct instances of that metaphysics which are satisfactory explanations of all known physical phenomena. I shall call such instances 'conforming instances.'

Since Newton's metaphysics does not specify what central force causes gravitation, Newton's theory of gravity does not follow from Newton's metaphysics; it is not an explained instance. Otherwise Newton's metaphysics would be refuted by the refutation of his theory of gravity, which it was not. Newton's metaphysics does not follow from his theory of gravity: the one asserts that all phenomena are governed by central forces, whereas the other is confined to fewer phenomena. Generally, a metaphysical doctrine neither entails nor follows from any of its conforming instances. Nor does it follow from the set of all its conforming instances unless it may be assumed that the set is exhaustive. Since such assumptions are testable, the metaphysical doctrines in question would follow from scientific theories, and thus they could legitimately claim scientific status.

This is the ideal case. To my knowledge it has never been achieved. The doctrine that arrived closest to this ideal was Newton's metaphysics as it appeared around 1800. Yet the ideal had an immense driving force. The debate about metaphysical doctrines often concerns their status, and this often leads to the development of scientific instances conforming to them, or to the discussion of whether such developments are possible. Thus the desire to render a metaphysics scientific leads to viewing it as a scientific research program whose satisfactoriness is open to critical discussion. To illustrate this I shall discuss in the next section the possible unsatisfactoriness of such research programs, and in the following section their possible satisfactoriness.

VII. Metaphysical Doctrines Are Often Insufficient Frameworks for Science

The methodology of this and the next section is a generalized Cartesian methodology, and the generalization I am offering is possible only within Popper's framework. Descartes's metaphysics (which was an improvement on Galileo's), was a clockwork view of the universe. It explained almost nothing; it was not intended to explain anything. Descartes claimed that any scientific hypothesis which he could endorse must be one which conformed to his metaphysics. He added that explanatory hypotheses conforming to his metaphysics could always be found. Boyle made the same claim concerning his own semi-Cartesian metaphysics, and so did Newton concerning his own metaphysics (in his preface and the *Scholium Generale* to *Principia*). But this repeated claim of the metaphysician is often false. It may be argued that his doctrine allows insufficient room for explanation, that it provides too narrow a framework. When this is felt to be the case, the demand for a new metaphysical framework arises. Metaphysics

which stagnates in scienceless (or uncritical) cultures, is progressive in scientific ones. It progresses then because existing metaphysical doctrines are felt to be constricting frameworks, and thus unsatisfactory.

Thales's doctrine aimed at explaining (physical and chemical) diversity and change by assuming an underlying and unchanging unity. Any such approach runs the danger of being too successful and thus self-defeating. For the assumption of an unchanging unity leads to regarding observable facts as illusory. This was the magnificent discovery of Parmenides, and it was this discovery that made him deny the existence of diversity. We have no right to despise him for having preferred his own logic to common sense, for having proclaimed appearances to be illusory; rather we should admire his dazzling logical acumen. But for him we might not have had Leucippus and Democritus. The greatest novelty of their atomic doctrine is that it expressly allowed for both unity—of the atomic character of matter—and diversity—of the atoms' shapes, sizes, and spatial order. To put it in quasi-ancient idiom, atomist metaphysics is a program to explain the many not by the one but by the few; it is thus more accommodating than the metaphysics of Thales.

(Of Parmenides's other great logical discovery, of the nonexistence of the void, I cannot speak here beyond saying that it was the cornerstone of the theory of space developed by Leibniz, Faraday, and Einstein. Further details of the story of Parmenides and Democritus, as well as the story of the downfall of the Pythagorean program, the demonstration of its narrowness, and its rectification by Plato, as well as the relation between Plato's program and Euclid's geometry, have been admirably presented by Popper in the paper already mentioned. The great role played by both Democritus's and Plato's programs in the seventeenth century have been beautifully told by Koyré. The relation between Leibniz's program and Einstein's scientific theory of space is discussed in Einstein's exciting preface to Jammer's *Concepts of Space*.)

My next example of an unsatisfactory metaphysics is Cartesian metaphysics, which contained the thesis that all (non-inertial) motion was due to push. The example for this was the suction pump whose (pull) action had been scientifically explained as due to atmospheric pressure (push). Lifting a jar seems to be pulling it upwards, but in fact it is pushing the jar upwards by the handles. Now this last example was implicitly criticized by Newton. If the jar is strong, or contains light material, it can be lifted by its handles; otherwise, pushing its handles upward hard enough will only constitute lifting the handles while leaving the jar itself on the floor. We must admit, then, that lifting it by its handles not only involves pushing the handles upward but also pulling the jar itself upward with the aid of the attractive forces which keep the jar and its handles connected—by the forces of cohesion. This example justifies Newton's claim (preface to *Principia*) that his program was in the first place more accom-

modating than Descartes's, and so could be more fruitfully adopted even if ultimately we should return to Descartes's program.

But Newton's metaphysical program, too, was so naive, that one may wonder how it was accepted for so long. Assuming, with Coulomb, that electric forces act solely between electric charges and that gross or ordinary matter is subject to the forces of gravity and cohesion only, why then does the charge remain on the charged body and pull it along when moving toward, or away from, another charge? This question (which was raised in 1800 with the discovery of electrochemistry) clearly indicates that gross matter is in some sense electrical. Yet so strongly impressed were people with Newtonianism that twenty years after Faraday had produced wonderful scientific theories which incorporate the supposition that gross matter has some electric characteristics, these theories were almost unanimously ignored (the exceptions were Kelvin and two other, rather minor, physicists). Those statements in the *Encyclopaedia Britannica* of the eighteen forties and fifties which appear to allude to Faraday's theories are certainly contemptuous and derisive.

It is not accidental that Boltzmann explained in 1885 (in a letter to *Nature*, p. 413) the general opposition to Maxwell by the general adherence to Boscovitch's, not to Newton's, program. Boscovitch had modified Newton's program to permit one material particle to dispose a variety of forces. This he did because he had discovered that otherwise the program would not accommodate any explanation of the phenomena of elastic collisions. But his program became popular only after Faraday imposed his view that gross matter had electric properties.

Incidentally, the indifference to Maxwell which worried Boltzmann shows that even Boscovitchian metaphysics may be highly dangerous; but it is a truism that any idea may become dogma.

So far I have only spoken of the requirement that metaphysical doctrines be sufficiently wide frameworks to accommodate possible future scientific theories. In the next section I shall speak of the requirement that metaphysics be inspiring and lead to the development of scientific theories.

VIII. The Role of Interpretations in Physics

A statement of fact or a scientific hypothesis restated in terms of a new metaphysical doctrine is a new interpretation. New interpretations are only too often unsatisfactory explanations of the original statements—for example, interpreting a motive as a sex motive, or survival as due to a high degree of fitness, or change of a person's pattern of behavior is due to physical change in the brain. But the logic of interpretations is made clearer, and so is their possible usefulness, when we take examples from physics.

The handles of a jar stick to the jar. To repeat this in Newtonian terms, the particles of the jar are attracted (by some central forces, that is) to those of the handles. Is this restatement a circular or a satisfactory explanation? We do not know. How small are these particles? What is the magnitude of these forces? One may try an estimate on the basis of known facts—perhaps the force needed to tear the handles off. Or perhaps it is easier to measure the force of cohesion by observing a drop of water hanging on to a solid surface, where cohesion counteracts gravity. Or perhaps it is still easier to observe a drop of water in a tube, where the balance of cohesion and gravity is perfect, and where the weight of the drop and the area of the contact surface are more easily calculable. It is easy to develop the first step of Laplace's theory of capillarity by thinking along this line: restate the connection between the inner diameter of the glass tube and the height of the water-column (or mercury surface) in it in Newtonian terms. As Newtonian forces are central, it would follow at once that the narrower the glass tube the higher the water column (and the lower the mercury level) in it. And the relative curvature of the fluid surfaces will be equally easily explicable. This is a particularly fortunate interpretation.

Another example: Newtonianism forces us to view lightly as either particles or waves in an elastic medium. Each of these interpretations leads to obvious questions which may be given testable answers. Faraday's metaphysics, to take another example, which views the universe as one field of forces, invites the view that light consists of vibrations of the lines of force in empty space. Faraday himself considered light to be waves of the magnetic field of force. For decades he tried to test this hypothesis and failed. And Faraday's interpretation of the electric current as the collapse of an electric field is another example of his failure. Tyndall rightly declared that Faraday's theory of the current was unsatisfactory. But he was too eager to reject it offhand (being a dogmatic Boscovitchian); by further specification Poynting soon rendered it highly satisfactory.

Interpretations apply not only to facts but also to theories. Faraday accepted Coulomb's Newtonian theory of electrostatic forces, but reinterpreted it in his field conception. His interpretation seemed unsatisfactory, and he was painfully aware of this. He succeeded in rendering it satisfactory by looking for curved lines of electric forces, which his interpretation of Coulomb's theory, though not Coulomb's theory itself, allowed for. He thus found that electric lines of force curve in the presence of dielectrics, i.e., materials like glass or sulphur. It is no accident that Coulomb denied the possibility of dielectricity: he was a Newtonian. Nor is it accidental, I think, that Cavendish failed to publish his own discovery of dielectricity: he wished to work on it further and reincorporate it within Newton's metaphysics, and he died before accomplishing this formidable task. That this task could be performed with but a slight deviation from Newton's program Faraday knew, and he outlined ways of doing it, with-

out however being able to do so himself for want of mathematical technique. The technique had been provided by Poisson, and Faraday said as much, but he was too neurotic about mathematical symbols to write them down on paper. Shortly afterwards Liouville was in a quandary because Poisson had, on his death-bed, asked him to make Poisson's own work the topic of a prize essay, and Liouville felt understandably apprehensive in view of Faraday's discovery which seemed to him not to fit Poisson's Newtonianism too comfortably. Kelvin, who was a young lad then, related all this in a letter which he wrote to his father from Paris, and he added a description of how relieved Liouville was to hear that Kelvin could interpret Faraday's discovery in an almost Newtonian fashion by using Poisson's own method. This was Kelvin's first published paper.

But there was no escape from Faraday's inspirations. Kelvin's theory of the dielectric assumed gross matter to possess electrical properties; his theory was not Newtonian but Boscovitchian. It soon transpired that Boscovitch's program needed modification. Gauss and Weber tried, and the attempt continued until 1905. By then it was clear that the program had to be given up; it looked as if Faraday's program had won out at last. Yet this program too was abandoned very soon after. It was deterministic and determinism had to be abandoned.

IX. The History of Science as the History of Its Metaphysical Frameworks

The world is full of well known yet unstudied phenomena, of often heard but seldom debated theories. Historians of science all agree that some theories—Copernicus's, Maxwell's—and some experiments—Oersted's, Michelson's—are of supreme scientific importance.

That Oersted's experiment was of metaphysical significance is obvious in view of the supreme prestige Newton's metaphysics enjoyed at the time. The greatest problem in physics between 1820 and 1905 was, could there be a (satisfactory) Newtonian (or semi-Newtonian) explanation of Oersted's experiment? Study of this problem led to Newtonianism losing its interpretative power. It soon transpired that the only unrefuted satisfactory explanation of Oersted's experiment was Maxwell's, and it became an urgent task for Newtonians to interpret fields in accord with Newton's metaphysics, which means—since for Newton forces are attached to matter and since Maxwell's equations are not invariant to Galileo's transformations—with the aid of the assumption that space is full. A scientific version of this assumption was refuted by Michelson and Morley. In 1904 Kelvin still hoped that another Newtonian or Boscovitchian interpretation of electrodynamics could be found; but though a few shared his hope no one

did anything about it, especially since his misgivings about Maxwell's theory were not shared by others. Undoubtedly, Maxwell's theory was so significant because it was a satisfactory explanation which conformed to Faraday's metaphysics. Undoubtedly Planck's theory became so important in 1905 when Einstein showed its conflict with Maxwell's theory because it seemed a major breakaway from Faraday's program.

I do not know why the significant events in the history of science should be metaphysically significant, but I have so far found it almost always to be the case. I suggest the theory that significance with respect to (pure) science is usually significance with respect to science's metaphysical frameworks. It is understandable that if metaphysical frameworks are research projects they should be taken very seriously, but why should all (pure) research projects be geared to a few metaphysical doctrines? Indeed, I think most research projects are not intended, at least not consciously intended, to be relevant to the dispute between the few competing metaphysical doctrines of the day. Yet those projects viewed later as significant show a capacity to throw light on current metaphysical issue. I can see no other explanation of the situation but that it is essentially metaphysical interest which gives (purely scientific) significance to this part of science rather than to that; hence, most (pure) scientists are more interested in metaphysics than they seem to be.

There are many studies which are not directly related to metaphysics. Take the continuum theory; it is the study of properties of matter, especially elasticity, on the assumption that matter is continuous. This study belongs to applied mathematics or technology rather than to pure science because it is based on a metaphysically unacceptable assumption. Its value for pure physics becomes apparent only when it is shown to throw light on an important scientific problem related to metaphysics. Indeed, since the Newtonian interpretation of the wave theory of light is the theory of the elastic ether, the rise of the wave theory caused immense efforts to be made to create any theory of elasticity whatever which might be used as a tool to render the ether theory scientific. Prior to that, the effort to develop a theory of elasticity were strictly in the Newtonian mode. We see how a significant plan of scientific research was first directly and then indirectly metaphysically relevant, and later it lost all relevance and with it all significance. Present day aerodynamics interests only few non-aeronauts, but it will interest more of them if it will reveal some bearing on existing metaphysical issues.

But what about scientific work unrelated to metaphysics? Let us take two examples. Jenner's study, his attempt to refute some village superstition, was highly idiosyncratic. Possibly it was connected with Bacon's idea that superstitions are dangerous to science, and yet as hardly anyone except Jenner undertook such researches, his work may well be viewed as idiosyncratic. The device of vaccination, which resulted from his study, was

for long chiefly of practical value. The mechanistic interpretation of vaccination is identical with the theory of antibodies. It is thus a metaphysical theory. In the popular literature it is often presented pseudo-scientifically. Biochemists have used it as a program and found scientific instances which conform to it; they are still searching for others. This story shows how one idea entered into the mainstream of science because it fitted a metaphysical framework.

My second example is the discovery of the asteroids. It is insignificant. It refuted Hegel's doctoral dissertation, but this was of no value in any case. It refuted Kepler's metaphysics, but nobody had ever taken notice of this metaphysics. It agrees with Bode's law, but this law is related to no metaphysics. The discovery is insignificant because it has no direct or indirect relevance to topical metaphysics. It may, however, become significant, if asteroids are going to play some role in a future cosmogony.

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CRITICAL APPROACHES TO SCIENCE AND PHILOSOPHY

Edited by Mario Bunge

With a new introduction by the editor

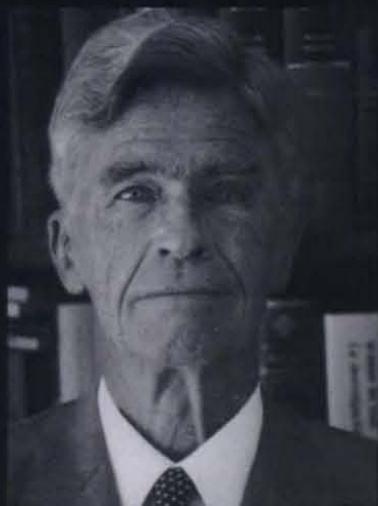
This collection of essays, written on four continents by scientists, philosophers, and humanists, was initially presented to Karl R. Popper on his sixtieth birthday as a token of critical admiration and in recognition of his work. But the volume also stands on its own as a remarkable series of statements utilizing Popper's critical vision in the study of philosophy proper, logic, mathematics, science as method and theory, and finally in the study of society and history. What is remarkable is that Popper worked in all of these areas, not in a cursory or discursive way, but with the utmost clarity and rigor.

The core position of this volume and its contributors is that the progress of knowledge is not a linear accumulation of definitive acquisitions but a zig-zagging process in which counterexamples and unfavorable evidence ruin generalizations and prompt the invention of more comprehensive and sometimes deeper generalizations, to be criticized in their turn. A critical approach to problems, procedures, and results in every field of inquiry is therefore a necessary condition for the continuance of progress.

The title of this volume then is, in a sense, an homage to Popper's critical rationalism and critical empiricism. The essays are a tribute to his unceasing and uncompromising quest, not for final certainty, but for closer truth and increased clarity. Among the contributors are outstanding figures in philosophy and the exact sciences in their own right, including Herbert Feigl, R. M. Hare, J. O. Wisdom, Nicholas Rescher, David Bohm, Paul K. Feyerabend, F. A. Hayek, and Adolf Grünbaum. Social science contributions include Hans Albert on social science and moral philosophy, W. B. Gallie on the critical philosophy of history, Pieter Geyl on *The Open Society and Its Enemies*, and George H. Nadel on the philosophy of history.

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