IN THIS VOLUME REGULAR PARENTHESES ARE USED INSTEAD OF ANGLE BRACKETS AS NOTATION FOR ORDERED SETS

HERBERT FEIGL

The "Orthodox" View of Theories: Remarks in Defense as well as Critique

The purpose of the following remarks is to present in outline some of the more important features of scientific theories. I shall discuss the "standard" or "orthodox" view, mainly in order to set up a target for criticisms, some of which I shall briefly sketch by way of anticipation. The standard account of the structure of scientific theories was given quite explicitly by Norman R. Campbell [7], as well as independently in a little-known article by R. Carnap [12]. A large part of the voluminous literature in the philosophy of science of the logical empiricists and related thinkers contains, though with a great many variations, developments, modifications, and terminological diversities, essentially similar analyses of the logical structure and the empirical foundations of the theories of physics, biology, psychology, and some of the social sciences. Anticipating to some extent Campbell and Carnap, Moritz Schlick, in his epoch-making *Allgemeine Erkenntnislehre* [38], championed the doctrine of "implicit definition." In this he was influenced by David Hilbert's axiomatization of geometry, as well as by Henri Poincaré's and Albert Einstein's conceptions of theoretical physics and the role of geometry in physics. These matters were then developed more fully and precisely in the work of H. Reichenbach, R. Carnap, C. G. Hempel, R. B. Braithwaite, E. Nagel, and many other logicians and methodologists of science.

In order to understand the aim of this important approach in the philosophy of science it is essential to distinguish it from historical, sociological, or psychological studies of scientific theories. Since a good deal of regrettable misunderstanding has occurred about this, I shall try to defend the legitimacy and the fruitfulness of the distinction before I discuss what, even in my own opinion, are the more problematic points in the "orthodox" logico-analytic account.

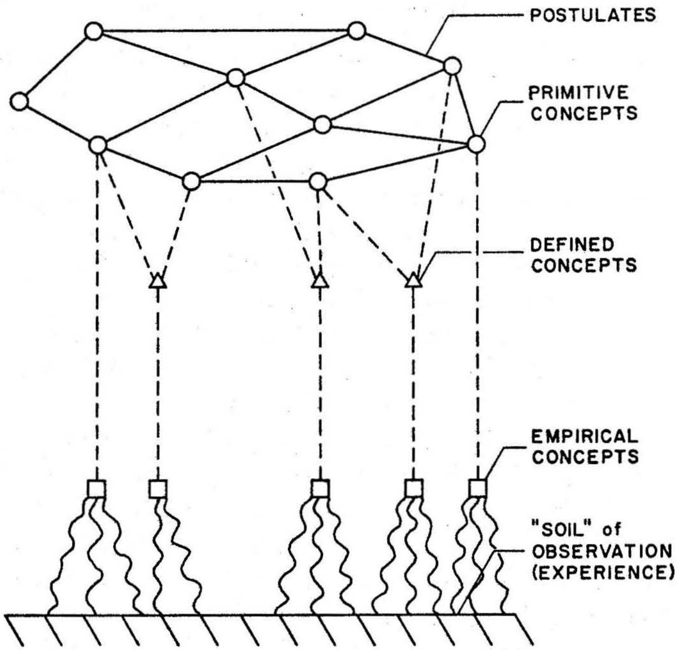
It was Hans Reichenbach [36] who coined the labels for the important distinction between "analyses in the context of discovery" and "analyses in the context of justification." Even if this widely used terminology is perhaps not the most felicitous, its intent is quite clear: It is one thing to retrace the historical origins, the psychological genesis and development, the social-political-economic conditions for the acceptance or rejection of scientific theories; and it is quite another thing to provide a logical reconstruction of the conceptual structures and of the testing of scientific theories.

I confess I am dismayed by the amount of—it seems almost deliberate—misunderstanding and opposition to which this distinction has been subjected in recent years. The distinction and, along with it, the related idea of a rational reconstruction are quite simple, and are as old as Aristotle and Euclid. In Aristotle's account of deductive logic, mainly in his syllogistics, we have an early attempt to make explicit the rules of validity of necessary inference. For this purpose it was indispensable for Aristotle to disregard psychological factors such as plausibility and to formulate explicitly some of the forms of the propositions involved in deductive reasoning. This also required transforming the locutions of ordinary language into standard formal expressions. For an extremely simple example, remember that "Only adults are admitted" has to be rendered as "All those admitted are adults." Only after the standard forms have replaced the expressions of common discourse can the validity of deductive inferences be checked "automatically," e.g., nowadays by electronic computers.

Furthermore, Euclid already had a fairly clear notion of the difference between purely logical or "formal" truths and extralogical truths. This is explicit in his distinction between the axioms and the postulates of geometry. From our modern point of view it is still imperative to distinguish between the correctness (validity) of a derivation, be it in the proof of a theorem in pure mathematics or a corresponding proof in applied mathematics (such as in theoretical physics), and the empirical adequacy (confirmation or corroboration) of a scientific theory. In fairly close accordance with the paradigm of Euclid's geometry, theories in the factual sciences have for a long time been viewed as hypothetico-deductive systems. That is to say that theories are sets of assumptions, containing "primitive," i.e., undefined terms. The most important of these assumptions are lawlike, i.e., universal, propositions in their logi-

cal form. And, just as in geometry, definitions are needed in order to derive theorems of a more specific character. These definitions may be of a variety of kinds: explicit, contextual, coordinative, etc. They are indispensable for the derivation of empirical laws from the more general and usually more abstract assumptions (postulates). The "primitive" concepts serve as the definientia of the "derived" ones. The primitives themselves remain undefined (by explicit definition). They may be regarded as only "implicitly" defined by the total set of axioms (postulates). But it is important to realize that implicit definition thus understood is of a purely syntactical character. Concepts thus defined are devoid of empirical content. One may well hesitate to speak of "concepts" here, since strictly speaking even "logical" meaning as understood by Frege and Russell is absent. Any postulate system if taken as (erstwhile) *empirically uninterpreted* merely establishes a network of symbols. The symbols are to be manipulated according to preassigned formation and transformation rules and their "meanings" are, if one can speak of meanings here at all, purely formal. From the point of view of classical logic implicit definitions are circular. But as C. I. Lewis once so nicely put it, a circle is the less vicious the larger it is. I take this to mean that a "fruitful" or "fertile" postulate set is one from which a great (possibly unlimited) number of theorems can be (nontrivially) derived, and this desirable feature is clearly due to the manner in which the primitive terms are connected with one another in the network formed by the postulates, and also by aptness of the definitions of the derived (defined) terms.

In the picturesque but illuminating elucidations used, e.g., by Schlick, Carnap, Hempel, and Margenau, the "pure calculus," i.e., the uninterpreted postulate system, "floats" or "hovers" freely above the plane of empirical facts. It is only through the "connecting links," i.e., the "coordinative definitions" (Reichenbach's terms, roughly synonymous with the "correspondence rules" of Margenau and Carnap, or the "epistemic correlations" of Northrop, and only related to but not strictly identical with Bridgman's "operational definitions"), that the postulate system acquires empirical meaning. A simple diagram (actually greatly oversimplified!) will illustrate the logical situation. As the diagram indicates, the basic theoretical concepts (primitives) are implicitly defined by the postulates in which they occur. These primitives (○), or more usually derived concepts (△) explicitly defined in terms of them, are then



linked ("coordinated") by correspondence rules to concepts (□) referring to items of observation, e.g., in the physical sciences usually fairly directly measurable quantities like mass, temperature, and light intensity. These empirical concepts are in turn "operationally defined," i.e., by a specification of the rules of observation, measurement, experimentation, or statistical design which determine and delimit their applicability and application.

Bridgman distinguished between "physical" and "mental" operations. What he had in mind is perhaps more clearly but also more cumbersomely expressed by distinguishing observational (cum mensurational-experimental) from logico-mathematical, e.g., computational, procedures. Conceived broadly enough, these two types of "operations" cover the entire variety of specifications of meaning of any sort of scientific concept. But Bridgman's examples indicate that he focused his attention primarily upon concepts that are fairly close to the "plane of observation." One very elementary case is the concept of (average) velocity of a moving body for a given distance in space, and a corresponding interval of time: Determine, with yardstick or tape measure, etc., the distance, and with the help of a stopwatch or other chronometric devices the duration in question; these are examples of Bridgman's "physi-

cal" operations. Then divide the numerical result of the first by the numerical result of the second ("mental" operation of arithmetic division), and you have arrived at your result: the (average) velocity.

Clearly, highly theoretical concepts such as, for example, that of the "spin" in quantum mechanics, involve much more complex operations—of *both* types. Hence, I think it is advisable to speak of operational definitions only for "empirical" concepts. The meaning of theoretical concepts can be specified only by their place in the entire theoretical system involving the postulates, definitions, correspondence rules, and finally the operational definitions. These last are indicated by the "rootlets" that "anchor" the empirical concepts in the "soil" of experience, i.e., mensurational-experimental observations.

In view of the "orthodox" logical analysis of scientific theories it is generally held that the concepts ("primitives") in the postulates, as well as the postulates themselves, can be given no more than a partial interpretation. This presupposes a sharp distinction between the language of observation (observational language; O.L.) and the language of theories (theoretical language; T.L.). It is asserted that the O.L. is fully understood. Indeed, in the view of Carnap, for example, the O.L. is not in any way theory-laden or "contaminated" with theoretical assumptions or presuppositions. In an earlier phase of positivism, for example in Carnap's [8], something like a language of sense data (actually a language of momentary total immediate experience) was proposed as the testing ground of all interpretive, inferential, or theoretical propositions. This was clearly the Humean doctrine of "impressions" brought up to date with the help of modern logic. Carnap, very likely influenced by Otto Neurath's and Karl R. Popper's criticisms, later proposed an intersubjective "physicalistic" O.L. as preferable to an essentially subjectivistic ("methodologically solipsistic") O.L. Hence, pointer readings and other similarly objective or intersubjectively concordant "data" would serve as an observation basis. Sharply in contrast to terms thus referring to intersubjectively observable qualities and relations are the theoretical concepts. Terms like "electromagnetic field," "neutron," "neutrino," and "spin" are understood only partially, i.e., with the help of postulates, explicit definitions, correspondence rules, and operational definitions. In the picturesque description of our diagram, it was said that there is an "upward seepage" of meaning from the observational terms to the theoretical concepts.

This, in brief outline, is the "orthodox" account of theories in the factual sciences. It has provided the guidelines for numerous axiomatizations of empirical theories. Various branches of theoretical physics [35], biology [40], especially genetics, psychology, especially learning theory [23], and the more recent voluminous output of P. Suppes and his collaborators at Stanford University in a wide range of subjects—all furnish examples of the many ways in which such reconstructions can be pursued. It is a matter of controversy just how fruitful or helpful strict axiomatizations are for the ongoing creative work of the theoretical scientists. If we disregard such relatively informal and "halfway" axiomatizations as can be found in the work of the great scientific innovators, such as Newton, Maxwell, and Einstein, it may well be said that the logicians of science work primarily by way of hindsight. That is, they *analyze* a given theory in regard to its logical structure and its empirical basis, but do not in any way add to the content of the theory in question. It seems to me that even this relatively modest endeavor can be useful in the following ways: (1) It enables us to understand a given theory more clearly; this is important at least in the teaching and learning procedures. (2) It provides a more precise tool for assessing the correctness of the logico-mathematical derivations on the one hand and the degree of evidential support (or else of disconfirmation) on the other. (3) Since no genuinely fruitful and important theory is "monolithic," but rather consists of a number of logically independent postulates, an exact reconstruction may well show which postulates rest on what empirical evidence.

It must be said immediately that all three of these contentions are being disputed nowadays. Regarding point (1), some criticisms concern the "partial interpretation" view. It is maintained, first of all, that the difference between observational and theoretical concepts is not all that sharp or fundamental; secondly, in this connection, it is urged that there are no observation statements that are free of theoretical presuppositions. Feyerabend even goes farther: He thinks that no neutral observation base exists and that none is needed for the testing of theories. He maintains that theories are tested against each other. If this were so, which I do not concede, then even the most liberal empiricism would have to be abandoned in favor of a, to me, highly questionable form of rationalism. But Feyerabend's construal of the history of scientific theories seems to me rather extravagant!

Furthermore, it is contended that we can understand scientific theories quite fully and that therefore the doctrine of "upward seepage" is all wrong. One reason why this criticism may seem justified is that the understanding of theoretical concepts and postulates rests on the use of analogies or analogical models. I would immediately admit the enormous importance of analogical conception and inference in heuristic and didactic matters. But it is a moot question whether analogical conception is part of the actual cognitive content of theories.

In regard to point (2), i.e., the separation of the assessment of the validity of derivations from the appraisal of the empirical adequacy of theories, I can hardly see any good grounds for criticism. To be sure, it is conceivable that the use of alternative, e.g., many-valued, logics might raise some questions here. But ever since the analyses of scientific explanation given on the basis of *statistical* postulates, especially by C. G. Hempel [20, 21], we have known how to explicate nondeductive derivations, which are actually the *rule* rather than the exception in recent science. Much more weighty are the questions regarding the precise analysis of the notion of evidential support or the "substantiation" of theories by observations (implemented wherever feasible by measurement, experimentation, or statistical design). I shall only mention here the radically different points of view of Carnap and Popper. Carnap has proposed a "logical" concept of probability, or of degree of confirmation of a hypothesis on the basis of a given body of evidence. Popper believes that the growth of scientific knowledge occurs through the severe testing of proposed hypotheses and that those hypotheses which survive such tests are "corroborated." Popper's "degree of corroboration," unlike Carnap's "degree of confirmation," is not a probability; it does not conform to the principles of the calculus of probabilities. The dispute between these two schools of thought still continues, but it is fairly clear that they are really reconstructing different concepts, each holding some promise of genuine illumination. There are also basic disagreements among the various schools of thought in statistical method. The controversies between the "Bayesians" or "subjectivists" and the "objectivists," e.g., those taking the Neyman-Pearson approach, might be mentioned here. It would lead us too far afield even to sketch in outline the various important points at issue.

Finally, in regard to point (3) we face the issues raised originally by Pierre Duhem, and more recently by W. V. O. Quine. Their contention is that theories can be tested only globally, in that it is (usually) the conjunction of all the postulates of a theory from which a conclusion is derived which is then either verified or refuted by observation. This contention is not to be confused with the (rather incredible) sort of assertion made on occasion by Sigmund Freud or his disciples that, as far as they are concerned, psychoanalytic theory is "monolithic," i.e., to be accepted or else rejected in its entirety. Duhem and Quine do not deny that the theories in the empirical sciences consist of *logically independent* postulates, or that they can at least be so reconstructed. What they deny is that the postulates can be independently *tested*. Prima facie this seems plausible, for in testing one postulate others are presupposed. The very use of instruments of observation and experimentation involves assumptions about the functioning of those instruments. In the formal reconstruction of theory testing there are then always assumptions, or auxiliary hypotheses, or parts of general background knowledge that are, in the given context, taken for granted. A closer look at the actual history and procedures of scientific research, however, indicates that the auxiliary hypotheses, etc., have usually been "secured" by previous confirmation (or corroboration). And while, of course, even the best established hypotheses are in principle kept open for revision, it would be foolish to call them into doubt when some other more "risky" hypotheses are under critical scrutiny. Thus, for example, the astronomer relies on the optics of his telescopes, spectroscopes, cameras, and so on in testing a given ("far-out") astrophysical hypothesis. Similarly, the functioning of the instruments of experimental atomic and subatomic physics (cloud or bubble chambers, Geiger counters, accelerators, etc.) is taken for granted in the scrutiny of a given hypothesis in quantum mechanics or nuclear theory. All this is simply the practical wisdom that enjoins us not to doubt everything equally strongly at the same time. Even more to the point: It seems that the "pinpointing of the culprit," i.e., the spotting of the false assumptions, is one of the primary aims as well as virtues of the experimental or statistical techniques. Thus the hypothesis of the stationary ether was refuted by the Michelson-Morley and analogous experiments. It was definitively refuted provided the theoretical physicists did not resort to special ad hoc hypotheses. Ritz's "ballistic" hypothesis regarding the propagation of light, and electromagnetic radiation generally, was refuted by the observations of de Sitter on double stars. *Both* of these pieces of evidence are needed for a justification of

Einstein's postulates in the special theory of relativity. Einstein's genius characteristically manifested itself when he guessed correctly in 1905 what de Sitter demonstrated only six years later. And there is some reason to believe that he did not explicitly utilize even the outcome of the Michelson-Morley experiment. Nevertheless, an objective confirmation of Einstein's theory does depend on these types of evidence.

Along with the "orthodox" view of the structure of scientific theories there goes an account of the levels of scientific explanation which, though often implicit, I formulated explicitly in one of my early articles [14]. This account has been, perhaps somewhat sarcastically, referred to by Feyerabend as the "layer-cake" view of theories. I still think that this account is illuminating even though it needs some emendations. As a first crude approximation the account in question maintains that the ground level consists of descriptions; whether they are based on observation or inference does not matter in this context. On this first level we place the explanandum, i.e., the individual fact or event to be explained, or rather its linguistic or mathematical formulation. Logically speaking only singular sentences or conjunctions thereof should appear on this level. Immediately above this level are the empirical laws (deterministic or statistical, as the case may be). We can utilize these empirical (or experimental) laws in the explanation of the facts or events described on the ground level. These explanations usually strike us as rather trivial because they amount to simply subsuming the individual fact or event under a class specified in the empirical law. For example, the fact that a lens functions as a magnifying glass can be explained by Snell's laws of the refraction of light rays. Snell's law specifies the relation of the angle of incidence to the angle of refraction in terms of a simple mathematical function. Snell's law in turn can be derived from the wave theory of light. This theory already enables us to derive not only the laws of refraction but also those of propagation, reflection, diffraction, interference, and polarization. A still higher level of explanation is attained in Maxwell's principles of electrodynamics (electromagnetics). Here the phenomena of light are explained as a small subclass of electromagnetic waves, along with radio waves, infrared, ultraviolet, X rays, gamma radiations, etc. But in order to understand such optical phenomena as reflection and refraction, a theory of the interaction of the electromagnetic waves with various types of material substances is called for. In order to achieve that, the atomic and electron theories were introduced toward the end of the last century. But for a fuller and more precise explanation we can ascend to the next, and thus far "highest," level, viz. the theories of quantum physics.

This level-structure analysis makes clear, I think, the progress from empirical laws to theories of greater and greater explanatory power. To speak very informally, it is the fact-postulate ratio that represents the explanatory power of theories. The aim of scientific explanation throughout the ages has been *unification*, i.e., the comprehending of a maximum of facts and regularities in terms of a minimum of theoretical concepts and assumptions. The remarkable success achieved, especially in the theories of physics, chemistry, and to some extent recent biology, has encouraged pursuit of a unitary system of explanatory premises. Whether this aim is attainable depends, of course, both on the nature of the world and on the ingenuity of the scientists. I think this is what Einstein had in mind in his famous sayings: "God is subtle but He is not malicious"; "The only thing that is incomprehensible, is that the world is comprehensible." (There is serious doubt about the contention of a third well-known bon mot of Einstein's, "God does not play with dice.") Einstein's deep conviction of the basic determinism—at "rock bottom"—of nature is shared by very few theoretical physicists today. There may be no rock bottom; moreover there is no criterion that would tell us that we have reached rock bottom (if indeed we had!).

The plausibility of the level-structure model has been, however, drastically affected by Feyerabend's criticisms. He pointed out quite some years ago that there is hardly an example which illustrates strict deducibility of the lower from the higher levels, even in theories with 100 percent deterministic lawlike postulates. The simple reason is that in straightforward deductive inference there can be no concepts in the conclusion that are not present in the premises and definitions. Most of us thought that definitions, or else bridge laws, would accomplish the job. In fact, however, the lower levels which (historically) usually precede, in their formulation, the construction of the higher levels are, as a rule, incisively revised in the light of the higher level theory. This certainly was the case in the relations of Newtonian to Einsteinian physics, of Maxwellian to quantum electrodynamics, etc. When presenting the level scheme in my philosophy of science courses I have, for more than thirty years, spoken of "corrections from above" accruing to the lower level lawlike

assertions. It is also to be admitted that while some of those corrections, within a certain range of the relevant variables, are so minute as to be practically negligible, they become quite significant and even indefinitely large outside that range. Moreover, and this is important, the conceptual frameworks of the theories of different levels are so radically different as to exclude any deductive relationships. Only if bridge laws help in defining the lower level concepts can the derivations be rendered deductive.

In disagreement with Feyerabend, I remain convinced that in the testing of a new theory, the relevant observation language must not be contaminated by that theory; nor need there be a competing alternative theory. If he contends that in most concerns of empirical testing there are presuppositions of a pervasive theoretical character, I would argue that those pervasive presuppositions, for example, regarding the relative permanence of the laboratory instruments, of the experimental records, are "theoretical" only from a deep epistemological point of view and are not called into question when, for example, we try to decide experimentally between rival theories in the physical, biological, or social sciences.

In conclusion I wish to say that the "orthodox" view of scientific theories can help in clarifying their logico-mathematical structure, as well as their empirical confirmation (or disconfirmation). It should be stressed, and not merely bashfully admitted, that the rational reconstruction of theories is a highly artificial hindsight operation which has little to do with the work of the creative scientist. No philosopher of science in his right mind considers this sort of analysis as a recipe for the construction of theories. Yet even the creative scientist employs, at least informally and implicitly, some of the criteria of logico-empirical analysis and appraisal which the logician of science endeavors to make fully explicit. Perhaps there is here an analogy with the difference between a creative composer of music and a specialist in musical theory (counterpoint, harmony, etc.). *Psychologically* the creation of a work of art and the creation of a scientific theory may have much in common. But logically, the standards and criteria of appraisal are radically different, if for no other reason than that the *aims* of art and science are so different.

According to the standard view correspondence rules are semantic designation rules. They merely provide an empirical interpretation of an erstwhile completely uninterpreted postulate system (pure calculus). Let me emphasize once more that this manner of regarding theories is a matter of highly artificial reconstruction. It does not in the least reflect the way in which theories originate. Correspondence rules thus understood differ from bridge laws in that the latter make empirical assertions. For example, if a bridge law states the relation between the mean kinetic energy of gas molecules and the thermometrically determined gas temperature, then this is, logically speaking, a matter of contingent empirical regularity. Nevertheless, in a complete theory of heat, i.e., statistical and quantum mechanical, the behavior of thermometric substances, e.g., alcohol, mercury, and gases, should in principle be derivable. Hence the bridge laws are to be regarded as *theorems* of the respective theories. This can also be formulated by saying that a logically contingent identification of empirical with theoretical concepts is thus achieved. This is surely part of what occurs in the *reduction* of empirical laws to theories, or of theories of lower level to a theory of higher level. Thus the theory of light rays (optics) is reduced to the theory of electromagnetic waves. Or light rays are identified with electromagnetic waves of certain wavelengths and frequencies. Similarly, ordinary (crystalline) table salt is identified with a three-dimensional lattice of sodium and chlorine atoms, etc., etc. The reduction of (parts of) psychology to neurophysiology is still scientifically and philosophically problematic and controversial, but if it were to succeed, it would involve the identification of the qualities of immediate experience with certain patterns of neural processes. In a unitary theory of perception the data of observation could then be characterizable as the direct-acquaintance aspect of brain states.

REFERENCES

1. Achinstein, Peter. *Concepts of Science*. Baltimore: Johns Hopkins Press, 1968.
2. Achinstein, Peter, and Stephen F. Barker, eds. *The Legacy of Logical Positivism*. Baltimore: Johns Hopkins Press, 1969.
3. Braithwaite, R. B. *Scientific Explanation*. Cambridge: Cambridge University Press, 1968.
4. Bridgman, P. W. *The Logic of Modern Physics*. New York: Macmillan, 1927.
5. Bridgman, P. W. *The Nature of Physical Theory*. Princeton, N.J.: Princeton University Press, 1936.
6. Bunge, Mario. *The Foundations of Physics*. Berlin, Heidelberg, and New York: Springer, 1967.
7. Campbell, Norman Robert. *Physics: The Elements*. Cambridge: Cambridge University Press, 1920.
8. Carnap, Rudolf. *Der Logische Aufbau der Welt*. Berlin-Schlachtensee: Weltkreis-Verlag, 1928.

9. Carnap, Rudolf. *Foundations of Logic and Mathematics*, vol. I, no. 3 of the *International Encyclopedia of Unified Science*. Chicago: University of Chicago Press, 1939.
10. Carnap, Rudolf. "The Methodological Character of Theoretical Concepts," in H. Feigl and M. Scriven, eds., *Minnesota Studies in the Philosophy of Science*, vol. I. Minneapolis: University of Minnesota Press, 1956.
11. Carnap, Rudolf. *Philosophical Foundations of Physics*. New York: Basic Books, 1966.
12. Carnap, Rudolf. "Ueber die Aufgabe der Physik und die Anwendung des Grundsatzes der Einfachstheit," *Kant-Studien*, 28 (1923), 90–107.
13. Colodny, Robert G., ed. *Beyond the Edge of Certainty*. Englewood Cliffs, N.J.: Prentice-Hall, 1965.
14. Feigl, Herbert. "Some Remarks on the Meaning of Scientific Explanation," in H. Feigl and W. Sellars, eds., *Readings in Philosophical Analysis*. New York: Appleton-Century-Crofts, 1949.
15. Feigl, Herbert. "Confirmability and Confirmation," in P. P. Wiener, ed., *Readings in Philosophy of Science*. New York: Scribner's, 1953.
16. Feigl, Herbert. *The "Mental" and the "Physical": The Essay and a Postscript*. Minneapolis: University of Minnesota Press, 1967.
17. Feyerabend, Paul K. "Problems of Empiricism," in R. G. Colodny, ed., *Beyond the Edge of Certainty*. Englewood Cliffs, N.J.: Prentice-Hall, 1965.
18. Feyerabend, Paul K. "How to Be a Good Empiricist—A Plea for Tolerance in Matters Epistemological," in B. Baumrin, ed., *Philosophy of Science: The Delaware Seminar*, vol. 2. New York: Wiley, 1963.
19. Grünbaum, Adolf. *Philosophical Problems of Space and Time*. New York: Knopf, 1963.
20. Hempel, Carl G. *Aspects of Scientific Explanation*. New York: Free Press, 1965.
21. Hempel, Carl G. "Deductive-Nomological vs. Statistical Explanation," in H. Feigl and G. Maxwell, eds., *Minnesota Studies in the Philosophy of Science*, vol. III. Minneapolis: University of Minnesota Press, 1962.
22. Hempel, Carl G. *Fundamentals of Concept Formation in Empirical Science*. Chicago: University of Chicago Press, 1952.
23. Hull, Clark L., *et al. Mathematico-Deductive Theory of Rote Learning*. New Haven, Conn.: Yale University Press, 1940.
24. Körner, Stephan. *Experience and Theory*. New York: Humanities, 1966.
25. Lenzen, V. F. *The Nature of Physical Theory*. New York: Wiley, 1931.
26. Margenau, Henry. *The Nature of Physical Reality*. New York: McGraw-Hill, 1950.
27. Mehlberg, Henryk. *The Reach of Science*. Toronto: University of Toronto Press, 1958.
28. Nagel, Ernest. *The Structure of Science*. New York: Harcourt, Brace and World, 1961.
29. Northrop, F. S. C. *The Logic of the Sciences and the Humanities*. New York: Macmillan, 1947.
30. Pap, Arthur. *An Introduction to the Philosophy of Science*. New York: Free Press, 1962.
31. Poincaré, Henri. *Science and Hypothesis*. New York: Dover, 1952.
32. Poincaré, Henri. *Science and Method*. New York: Dover, 1952.
33. Popper, Karl R. *Conjectures and Refutations*. New York: Basic Books, 1962.
34. Popper, Karl R. *The Logic of Scientific Discovery*. New York: Harper, 1959.
35. Reichenbach, Hans. *Axiomatik der relativistischen Raum-Zeit-Lehre*. Braunschweig: Vieweg, 1924.

36. Reichenbach, Hans. *Experience and Prediction*. Chicago: University of Chicago Press, 1938.
37. Scheffler, Israel. *The Anatomy of Inquiry*. New York: Knopf, 1963.
38. Schlick, Moritz. *Allgemeine Erkenntnislehre*. 2nd ed. Berlin: Springer, 1925 (1st ed. 1918).
39. Smart, J. J. C. *Between Science and Philosophy*. New York: Random House, 1968.
40. Woodger, Joseph Henry. *The Techniques of Theory Construction*. Chicago: University of Chicago Press, 1939.

PAUL K. FEYERABEND

Against Method: Outline of an Anarchistic Theory of Knowledge

What is all this commotion good for? The most it can achieve is to ruin one's peace of mind. There one has one's little rooms. Everything in them is known, has been added, one item after another, has become loved, and well esteemed. Need I fear that the clock will breathe fire into my face or that the bird will emerge from its cage and greedily attack the dog? No. The clock strikes six when it is six like it has been six for three thousand years. This is what I call *order*. This is what one loves, this is what one can identify with. CARL STERNHEIM, *Die Hose*

Preface

The following essay has been written in the conviction that *anarchism*, while perhaps not the most attractive *political* philosophy, is certainly an excellent foundation for *epistemology*, and for the *philosophy of science*.

The reason is not difficult to find.

"History generally, and the history of revolutions in particular, is always richer in content, more varied, more manysided, more lively and 'subtle' than even" the best historian and the best methodologist can imagine.[1] * "Accidents and conjunctures, and curious juxtapositions of events" [2] are the very substance of history, and the "complexity of human change and the unpredictable character of the ultimate consequences of any given act or decision of men" [3] its most conspicuous feature. Are we really to believe that a bunch of rather naive and simple-minded rules will be capable of explaining such a "maze of interactions"? [4] And is it not clear that a person who *participates* in a complex process of this kind will succeed only if he is a ruthless *opportunist*, and capable of quickly changing from one method to another?

This is indeed the lesson that has been drawn by intelligent and

AUTHOR'S NOTE: For support of research I am indebted to the National Science Foundation.

* The notes for this essay begin on p. 94.