The Conduct of Inquiry

Methodology for Behavioral Science

Abraham Kaplan

With a new introduction by
Charles Wolf, Jr.

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To

I. J. K.

My Women of Valor

CONTENTS

II. CONCEPTS

III. LAWS

IV. EXPERIMENT

V. MEASUREMENT

VI. STATISTICS

VII. MODELS

VIII. THEORIES

IX. EXPLANATION

X. VALUES

INTRODUCTION TO THE TRANSACTION EDITION

ABE KAPLAN was a humanistic polymath. Born in Odessa, USSR, in 1918, he came to the United States when he was five, became a citizen at twelve, and subsequently acquired and retained dual citizenship in the United States and Israel. With an undergraduate degree in chemistry from the College of St. Thomas in Minnesota, and a Ph.D. in philosophy from UCLA, Abe's interests, knowledge, and sensibilities included the physical as well as the social sciences, the "hard" as well as the "soft" sciences.

The scope and enduring value of *The Conduct of Inquiry* attests to Kaplan's polymathy as he lucidly tours the domains of logic and psychology, experimentation, measurement, statistics, modeling, values, and policy applications. Some, and perhaps much, of the terrain that he covers reflects the many years of his distinguished teaching career in the United States as a professor of philosophy at UCLA, Michigan, and Cal Tech, in Israel both as dean of the faculty of social sciences and as professor of philosophy, and still later in the United States as a member of the faculty of The RAND Graduate School in Santa Monica.

Abe's association with The RAND Graduate School began in 1972 when he accepted an invitation from Harold Lasswell and me to be a member of the School's inaugural Academic Advisory Board that held its first meeting in April of 1972.[1] The board's other members initially

included Kenneth Arrow, Murray Gell-Mann, Ralph Tyler, Albert Wohlstetter, James Coleman, Bill Libby, and Paul Samuelson. Harold Lasswell was chairman of the board's nine members, four of whom were or soon became Nobel Laureates (in physics, in chemistry, and two in economics). The five non-Nobelists (Coleman, Lasswell, Kaplan, Tyler, and Wohlstetter) most assuredly did not depress the intellectual level of the group.

Abe was the only member of the board who subsequently became an active faculty member of the RAND Graduate School (about which more later). He left the board in 1976 after four years as a member, because his bi-continental schedule and commitments—at Haifa during the academic year, and at RGS and Cal Tech during the summer—made it infeasible for him to attend the board's regularly scheduled meetings in April and November.

One incident that arose during his tenure on the board is worth recounting because it bears, if only indirectly and obliquely, on some of the ground covered in the *Conduct of Inquiry.* During the school's initial application for accreditation by the Western Association of Schools and Colleges, a question arose about whether the name "RAND" should be deleted from the name of the institution before final accreditation was awarded. The proposal to do this was made in a motion by one of the board members on the grounds that the RAND name was strongly associated with the military establishment, that opposition to the war in Vietnam and to the military more generally had become a source of turmoil on American college campuses, and that removal of the RAND name might spare the embryonic RAND Graduate Institute from this turmoil. Abe expressed a strongly negative reaction to this proposal based on the view that the school was in fact part of RAND, that the RAND name did have "some magic" associated with it, and that his own experience was that when he mentioned his own connections to RAND "even the peaceniks" respected that connection. The motion was defeated.

Between 1977 and 1984, usually during the summer, Abe Kaplan taught seminars in The RAND Graduate School on the philosophical basis for policy formation and analysis, values in policy analysis, ethical issues in the policy sciences, policy analysis, and the behavioral sciences, and logic and policy analysis. Notwithstanding this variety,

the different seminars generally had a common core which included the "great books" (in the University of Chicago sense), selections from other literature in anthropology, economics, behavioral psychology, history, political science, psychoanalysis, sociology, and *The Conduct of Inquiry,* with a "spin" provided by Abe to meet the varying interests and concerns of the particular students in each class.

The decision by Transaction Publishers and its editorial chairman, Irving Louis Horowitz, to issue this posthumous edition of *The Conduct of Inquiry* is welcome and noteworthy: welcome, because of its continued relevance to the perennial controversies among social scientists, including quantifiers and qualifiers, theorists and empiricists, academicians and policy wonks; and noteworthy, as a testament to a remarkable humanist and polymath.

CHARLES WOLF, JR.
Dean, The Rand Graduate School

September 1997

Note

1. The RAND Graduate School of Policy Studies, initially established as The RAND Graduate Institute for Policy Studies in 1970, changed its name in 1986.

PREFACE

IN THIS BOOK I have emphasized what unites the several behavioral sciences more than what distinguishes them from one another. The special problems of the various disciplines are dealt with only so far as may be helpful in clarifying the general method of inquiry.

With regard to the widely differing and often mutually hostile schools and approaches in behavioral science my position can be regarded, I suppose, as neutralist, and will therefore be condemned, no doubt, by both sides—not rigorous enough for one, and too demanding for the other. My aim has not been compromise, however, nor my ideal a golden mean. What seems to me important is yielding, not to demands externally imposed, but rather to those intrinsic to our own aspirations.

In methodology itself I have a corresponding range of sympathies—with logical construction and linguistic analysis, as well as with the more substantive concerns of the older methodologists, of whom I think more highly than is now fashionable. In particular, those who are acquainted with pragmatism will be aware of how much greater my indebtedness is to Peirce, James, and Dewey than is made explicit by citations. But I have sought the comfort of like-mindedness wherever I could find it, without regard to broader philosophical commitments. References are to whatever editions and printings among those at hand I judged to be most readily accessible to the reader.

The leisure to read and write—and what is more, the stimulus to think about both activities—was afforded me by the Center for Advanced Study in Behavioral Science at Palo Alto, and by the Center for Advanced

Studies at Wesleyan University, in Middletown, Connecticut; to both I am deeply grateful. I want also to thank Alexander Sesonske and Clyde Coombs for reading portions of the manuscript.

ABRAHAM KAPLAN

Ann Arbor, Michigan
1963

INTRODUCTION

IN THIS BOOK for the first time a philosopher makes a systematic, rounded, and wide-ranging inquiry into behavioral science. In doing so he has been guided by the experience of sciences with longer histories, but he has been bound neither to their problems nor to their solutions. Instead, he has addressed himself to the methodology of behavioral science in the broad sense of both science and methodology. The tasks, achievements, limitations, and dilemmas of the newer disciplines are the focus of his attention. The work is not a formal exercise in the philosophy of science but rather a critical and constructive assessment of the developing standards and strategies of contemporary social inquiry. Professor Kaplan is familiar with the fields he discusses; he is not a visiting philosopher recounting a sightseeing trip.

Philosophers of science usually choose to write about the most fully developed sciences because the problems are clearer there. But the result is ordinarily of little benefit to the behavioral scientist. His most difficult task is the clarification of method where the precedents and analogies of physical science are inappropriate or obscure. What he needs most is a direct confrontation of methodological problems immediately relevant to his own discipline. He needs to read from the strengths of his own understanding, insights, expertness, and subject matter and not from the insecurity of a limited familiarity with a remote discipline. *The Conduct of Inquiry* goes a long way toward filling those needs because it is appropriate to the present state of the art and to the stages just ahead.

Professor Kaplan draws upon the whole scientific enterprise but always

with a purpose—to guide the behavioral scientist, to post warnings on pitfalls that may lie in his path, to remind him from time to time that he is not a nuclear physicist, to remind him that he is nevertheless a scientist in the somber and in the exciting significance of the term, and to place behavioral science in the context of an ongoing endeavor, particular as well as general.

This most useful philosopher is always lucid but he has not always made things easy. He is a hard taskmaster who holds high aspirations and high standards for behavioral science. He has no philosopher's stone that will turn empirical dross into theoretical gold or even empirical mud into theoretical pots. He does make it easier to distinguish the dross from the gold and the mud from the clay. It is clear that Professor Kaplan thinks behavioral science both feasible and worth the effort—worth his effort as a commentator and worth ours as practitioners.

The book, being an orientation in methodology for behavioral scientists, is indispensable for behavioral scientists and for aspiring ones irrespective of their orthodoxy or heterodoxy, but perhaps it will prove more palatable to the heterodox. It will be durable in its usefulness, and it will be useful to a wide range of readers many of whom will discover unsuspected strengths and weaknesses in their disciplines. The book proves by the fact of its existence that there is a community of scholarship between the humanities and behavioral science and that the validity of one does not depend upon alienation from the other.

There is a consistent emphasis on the common concerns of all the social sciences rather than on their parochial problems and their perhaps temporary points of isolation, an emphasis on major issues rather than side arguments, a concern with problems that are intrinsically important and recurrent rather than with transitory considerations or matters of technique. The author's intellectual tolerance does not extend to fads and fashions whose claims for attention rest on currency alone.

Professor Kaplan departs from the stylistic presumptions that anything interesting must look as if it is being said for the first time, that anything important must be said sententiously, that anything difficult must be made to appear more difficult—and therefore more important—and that anything that bears upon error must begin with a destructive attack on someone—as if human fallibility needed to be proved all over

again. In the pages that follow, the simple is said simply and the difficult is said clearly. A genuine erudition is opened to the student and the advanced scholar. The book fills a gap and does it with admirable clarity and often with engaging wit. It lacks pomposity, pedantry, and pretension, and it is bound to make an impact on the teaching of and, with luck, on research in the behavioral sciences.

LEONARD BROOM

Austin, Texas
1963

THE CONDUCT OF INQUIRY

Methodology for Behavioral Science

Chapter I

METHODOLOGY

§1. *Logic-in-Use and Reconstructed Logic*

It is one of the themes of this book that the various sciences, taken together, are not colonies subject to the governance of logic, methodology, philosophy of science, or any other discipline whatever, but are, and of right ought to be, free and independent. Following John Dewey, I shall refer to this declaration of scientific independence as the principle of *autonomy of inquiry*. It is the principle that the pursuit of truth is accountable to nothing and to no one not a part of that pursuit itself.

SCIENTIFIC AUTONOMY

Note that the declaration applies not to the several sciences but only to all of them taken together. There is no doctrine of states' rights, as it were, in accord with which the sovereignty of a particular discipline can be interposed between a parochial creed and the stern judgment of the scientific community as a whole. It will not do to say, "You people simply don't understand our problems!" or, "We have our own logic, our own sources and standards of truth." There are indeed differences among the

various disciplines, and these differences are important to the conduct of inquiry in each case. But they do not serve to cut the sciences off from one another.

For the domain of truth has no fixed boundaries within it. In the one world of ideas there are no barriers to trade or to travel. Each discipline may take from others techniques, concepts, laws, data, models, theories, or explanations—in short, whatever it finds useful in its own inquiries. And it is a measure of its success in these inquiries that it is asked in turn to give of its riches to other disciplines. Even more, it may find itself unexpectedly in an area conventionally identified as "belonging to" another science. Some of the most exciting encounters in the history of science are those between workers in what appear to be quite distinct fields who are suddenly brought face to face as a result of their independent investigations. The autonomy of inquiry is in no way incompatible with the mature dependency of the several sciences on one another.

Nor does this autonomy imply that the individual scientist is accountable only to himself. Every discipline develops standards of professional competence to which its workers are subject. There are certain acceptable ways of interpreting a projective test, of carrying out a dig, of surveying public opinion. Case studies, experiments, hypotheses, theories—all must meet certain conditions if they are to be taken seriously by the profession. These conditions are seldom made wholly explicit, and they differ for different disciplines and at different times; but in any case, their demands are likely to be firm and unyielding.

Every scientific community is a society in the small, so to speak, with its own agencies of social control. Officers of the professional associations, honored elders, editors of journals, reviewers, faculties, committees on grants, fellowships, and prizes—all exert a steady pressure for conformity to professional standards, as their counterparts in the larger society provide sanctions for the more general norms. In certain respects scientific training functions to produce not only competence but also a kind of respectability, essential to membership in the professional community. Doctoral examinations, most candidates agree, have much in common with the tortures of initiation rites—and with the added tribulation of fear of failure: no one has ever had to repeat his Bar Mitzvah.

The innate conservatism, or at least inertia, of professional standards has from time to time stood in the way of scientific progress. The martyrs

of science have sometimes been victims of the faithful rather than of the infidels. Ignaz Semmelweis and Georg Cantor (to go back no more than a scant century) were hounded by their respectable colleagues for their revolutionary ideas about puerperal fever and transfinite arithmetic. More recent, if less dramatic examples, are not hard to find. Yet for every resisted scientific genius there are numberless crackpots, for every martyr to the truth there are countless victims only of their own paranoid delusions. Standards of scientific excellence, though they may occasionally be self-defeating, on the whole and in the long run make for success. Adherence to the Law is the surest, and perhaps the only, safeguard against being misled by false Prophets.

The principle of autonomy does not deny authority to norms of scientific practice but rather derives their authority from the sovereignty of science itself. Standards governing the conduct of inquiry in any of its phases emerge from inquiry and are themselves subject to further inquiry. Both historically and on the present scene, the chief importance of an in sistence on the principle of autonomy lies in its defense of the integrity of science against encroachment by other social enterprises.

Theology, politics, morals, and metaphysics have all exerted a dubious and often repressive authority. The history of the warfare between science and theology (a more accurate expression here than "science and religion") has often been documented. Though the physical and biological sciences have largely won their independence, behavioral science in many parts of the world—including our own—is still subject to the influence, if not the authority, of theological doctrine. (Consider, for example, inquiry into such problems as the population explosion, capital punishment, drug addiction, and deterrents to war.) Political pressures on science are best known to us from the Nazi and Stalinist eras, with their dogmas not only in history, political science, and economics, but also in such fields as anthropology, genetics, linguistics, and mathematics. (In a public debate, Sidney Hook led off with a brilliantly effective question put to the professor of chemistry who had accepted the chancellorship of the East German University of Berlin: "What do you think of Stalin as a chemist?") But in our own country, too, the autonomy of science must recurrently be reaffirmed: psychoanalytic psychiatry is as suspect among the Radical Right as it is in the land of Pavlov. And legal restrictions affect not only atomic research, but also some inquiries into the workings of,

say, the jury system, as an American sociologist found when his study was denounced on the floor of Congress. Considerations of a moral kind (in the root sense as relating to the mores) make themselves felt equally in studies of the relations between the sexes and the races.

The part played by the influence and authority of metaphysics over science is more equivocal. The values of its contributions to scientific progress have been traced by E. A. Burtt, Philipp Frank, Karl Popper, and many others. Yet there is no doubt that it has also had a pernicious effect. Hegel was not unwilling to supply an *ipse dixit* on a variety of scientific questions, and many other metaphysicians since his day have sought to shape a priori the course of scientific inquiry.

It is no part of my purpose to argue that the scientific enterprise either is or ought to be dissociated from the larger world of men and affairs; quite the contrary (§§43 and 45). What I am insisting on is that the standards of scientific practice derive from science itself, even though the science of any period is intimately involved with every other human concern. My position is not that enterprises outside science have no authority over science; it is rather that, where such an enterprise does govern, it has the right to govern only by the consent of the governed.

AUTONOMY AND LOGIC

So far, scientists and philosophers of science are likely to agree, among themselves and with one another. But there *is* an issue concerning the normative force of logic in relation to science. Is not logic, and its practical elaboration as "methodology", the ultimate source and ground of the norms of scientific inquiry? Even God cannot violate the laws of logic; can the scientist? Of course, every scientist is "logical" or aspires to be so; the issue is whether logic is to be conceived as validating the process of scientific inquiry, or as ultimately validated by that process.

Some years ago a deservedly famous textbook was published with the title *An Introduction to Logic and Scientific Method*. To the word "and" in this title John Dewey took exception, for logic, in his view, is nothing other than the theory of inquiry. It is as though literary criticism were to be "applied" to literature: without such application it is nothing. In perspective it might be said that Dewey did not sufficiently appreciate the self-contained richness and vigor of the mathematical logic being de-

veloped in his time. Yet this logic is purely formal, and in a strict sense of the term, empty. Even where it deals with induction, as in Carnap's recent work, it remains in fact deductive, a logic of consistency rather than of truth. An inductive principle like "Use all the evidence available" is extralogical, not a tautology validated by the rules for the calculus in whose use it plays a part. The truths of formal logic are indeed ineluctable, as are the truths of pure mathematics, from which, in the current view, they are ultimately indistinguishable. But it would beg the question to conclude, on the basis of a formal analysis, that this formal logic *is* the logic of science. "Prove to me," Epictetus was challenged, "that I should study logic." "How will you know that it is a good proof?" was the reply.

A great deal hinges on whether science is viewed as a body of propositions or as the enterprise in which they are generated, as product or as process. An account of the norms bearing on the finished report of an investigation might well be expected to differ from one concerned with the conduct of the investigation itself. (The former is sometimes called "logic" and the latter "methodology", but this is not a happy usage.) In recent times the emphasis characteristically put on the process of science by pragmatists like Dewey and his great predecessor Charles Peirce has come to be more widely shared by philosophers than it was a few decades ago. I shall return shortly to the question whether there is such a thing as a "logic of discovery".

There is, then, something not quite so old-fashioned as we used to feel about John Stuart Mill's definition of logic: "the science [sic] which treats of the operations of the human understanding in the pursuit of truth." We might say, not only the pursuit of "truth", but perhaps also the pursuit of explanation, prediction, or control. In short, logic treats of "the operations of the human understanding" (it *does* have an old-fashioned ring after all!) in solving problems; we need not commit ourselves beforehand to any single characterization of what constitutes "solutions".

It is important to point out that logic treats these operations evaluatively. What is central is not the fact that they are performed by certain people under certain circumstances, but whether or not under those circumstances they succeed in producing the solutions to which they are directed. There is a nonevaluative sense of the term "logic", which occurs

in phrases like "the logic of the Unconscious" or "the logic of the Radical Right". I prefer in these cases to speak of the *cognitive style;* we can then say of a cognitive style that it is logical or illogical, in varying degrees, or in one respect or another. (There is also a transposed sense of the term "logic" in expressions like "the logic of events", which refers rather to what logic would disclose about the events—their necessary connections or outcomes, their explanation.) Logic, in short, deals with what scientists do when they are doing well as scientists.

Now the word "logic" is one of those, like "physiology" and "history", which is used both for a certain discipline and for its subject-matter. We all have physiologies and histories, and some of us also think and write about these things. Similarly, scientists and philosophers use a logic—they have a cognitive style which is more or less logical—and some of them also formulate it explicitly. I call the former the *logic-in-use*, and the latter the *reconstructed logic*. We can no more take them to be identical or even assume an exact correspondence between them, than we can in the case of the decline of Rome and Gibbon's account of it, a patient's fever and his physician's explanation of it.

Though I have said that a field of inquiry—say, psychoanalysis or parapsychology—cannot claim to have its "own logic" as a defense against scientific criticism, it is true that there are many logics-in-use. What is objectionable is only the claim to proprietorship, the implication that criticism is inadmissible unless the soundness of the criticized method is first granted. The policy is objectionable, equally in science and in politics, that what's mine is mine and what's yours is negotiable. The logic-in-use in any inquiry must prove itself; such proof consists in the success of the inquiry and is discernible to any other sound logic-in-use. (The condition that the other must be "sound" generates a circle, but it is not a vicious one.) That the world of ideas has no barriers, within or without, does not call for one true "Logic" to govern it. The conviction that there is such a logic—as it happens, ours—is a parochialism like those of which comparative ethnology made us painfully aware in the course of the last century. The myth of a "natural logic", defining a universal rationality, has been penetratingly analyzed by Benjamin Lee Whorf and his successors among linguists and anthropologists. Not only language and culture affect the logic-in-use, but also the state of knowledge, the stage of inquiry, and the special conditions of the particular

problem. There is more than one way to skin a cat, and the family *Felidae* has some remarkable specimens.

Whatever our position regarding logics-in-use, there can be no doubt that there are many reconstructed logics. John Locke, in discussing the syllogism, remarked that "God has not been so sparing to men to make them barely two-legged creatures and left it to Aristotle to make them rational." There were logics-in-use, that is to say, before Aristotle's reconstruction. And other reconstructions came after him. Augustus De Morgan pioneered the modern movement in logic with the observation that Aristotle's logic (that is, his reconstruction) could not prove that, because a horse is an animal, the head of a horse is the head of an animal; this proof waited for the development of a logic of relations. In the present century, Russell, Quine, and others have created reconstructions incomparably richer than Aristotle's, of which Kant had the misfortune to proclaim, just as its reign was ending, that it left no room for any further advances.

Locke and De Morgan between them remind us that a logic-in-use may precede and be superior to its own reconstruction. This reminder is true not only of the logic-in-use in everyday life but of the logic-in-use in science as well. Newton and his followers made excellent use of the calculus in physics, despite the astute criticisms of its foundations made by Bishop Berkeley, criticisms which were not satisfactorily met till the reconstruction by Weierstrass some two hundred years later. Conversely, a reconstruction may become, or at any rate influence, the logic-in-use. Aristotle's logic certainly played a part in the biology of fixed species, as well as in the logic-in-use in a wide variety of other disciplines. Russell's reconstruction of logic, together with those of other contemporary logicians, significantly affected the logic-in-use in mathematics and related sciences.

What Carnap and Reichenbach have called the "rational reconstruction" of science is the application to the scientific product, not of "Logical" analysis, but of the contemporary reconstructed logic. For some time, the most widely accepted reconstruction of science has been in terms of the so-called "hypothetico-deductive method", especially in a postulational form. According to this reconstruction, the scientist, by a combination of careful observation, shrewd guesses, and scientific intuition arrives at a set of postulates governing the phenomena in which he is

interested; from these he deduces observable consequences; he then tests these consequences by experiment, and so confirms or disconfirms the postulates, replacing them, where necessary, by others, and so continuing.

This reconstruction has been serviceable for some time, chiefly in application to the more advanced parts of physics, though in a few instances also to biological and behavioral science. But a reconstructed logic is itself, in effect, a hypothesis. As with other hypotheses, as time goes on it may become more and more awkward to "fit" the hypothesis to the facts—here, the facts constituted by the logic-in-use. It is not a question of whether the facts *can* be so construed, but rather whether it is still worthwhile to do so, whether the reconstruction in question continues to throw light on the sound operations actually being used. The "hypothetico-deductive" reconstruction fails to do justice to some of the logic-in-use, and conversely, some of the reconstructed logic has no counterpart in what is actually in use. The formation of hypotheses is treated as though it were largely an extralogical matter. On the other hand, formal deductions in postulational systems are so seldom found in science that the logician is called upon to construct such systems himself, so as to provide his reconstructions with a subject-matter.

To be sure, a reconstructed logic is not meant to be merely a description of what is actually being done by scientists, and for two reasons.

First, because logic is concerned with evaluations, it may be less interested in what is being done than in what is being left undone. But the formation of hypotheses in science and their replacement by more satisfactory ones is, on the whole, a matter of sound operations, and not something illogical or even extralogical. The criticism I am making is that in the "hypothetico-deductive" reconstruction the most important incidents in the drama of science are enacted somewhere behind the scenes. The growth of knowledge is surely basic to the scientific enterprise, even from a logical point of view. The conventional reconstruction presents the denouement, but we remain ignorant of the plot.

Second, a reconstructed logic is not a description but rather an idealization of scientific practice. Not even the greatest of scientists has a cognitive style which is wholly and perfectly logical, and the most brilliant piece of research still betrays its all-too-human divagations. The logic-in-use is embedded in a matrix of an *a*logic-in-use, even an *il*logic-in-use.

The reconstruction idealizes the logic of science only in showing us what it *would* be if it were extracted and refined to utmost purity.

This defense is important and, I think, sound—but only up to a point. The idealization may be carried so far that it is useful only for the further development of logic itself, and not for the understanding and evaluation of scientific practice. Reconstructions have been so idealized that, as Max Weber (*135*:114) wryly observed, "it is often difficult for the specialized disciplines to recognize themselves with the naked eye." At worst, the logician becomes so absorbed with enhancing the power and beauty of his instrument that he loses sight of the material with which it must work. At best, he commits himself to a questionable Platonism: that the proper way to analyze and understand something is to refer it to its most ideal form, that is, its form abstracted from any concrete embodiment. This is *a* way but it is not the only way; and I am far from convinced that it is always the best way.

The great danger in confusing the logic-in-use with a particular reconstructed logic, and especially a highly idealized one, is that thereby the autonomy of science is subtly subverted. The normative force of the logic has the effect, not necessarily of improving the logic-in-use, but only of bringing it into closer conformity with the imposed reconstruction. It is often said that behavioral science should stop trying to imitate physics. I believe that this recommendation is a mistake: the presumption is certainly in favor of those operations of the understanding which have already shown themselves to be so preeminently successful in the pursuit of truth. What *is* important, I believe, is that behavioral science should stop trying to imitate only what a particular reconstruction claims physics to be.

When the reconstruction is mathematically elegant, precise, and powerful—as is true of the "hypothetico-deductive" logic—its attractions are nearly irresistible. But the crucial question concerns, not the intrinsic virtues of the reconstructed logic taken in itself, but rather its usefulness in illuminating the logic-in-use. There is a story of a drunkard searching under a street lamp for his house key, which he had dropped some distance away. Asked why he didn't look where he had dropped it, he replied, "It's lighter here!" Much effort, not only in the logic of behavioral science, but also in behavioral science itself, is vitiated, in my opinion, by the principle of *the drunkard's search.*

§2. *Logic and Psychology*

A rational reconstruction, after all, may have something in common with rationalization in the psychological sense. (It is not to be forgotten that a proposition serving as a rationalization may nevertheless be true.) The new version satisfies standards of rationality which the old one may have been indifferent to or even violated. What is at work in rational reconstruction is rather like what psychoanalysts call the "secondary elaboration" of the primary dream process. The dream we report is not what we dreamt but something more sensible, more intelligible on the face of it. In the case of dreams, such "elaboration" does not wholly conceal the latent content, and may even facilitate a sound interpretation. The patient who says, "I fool my analyst: I don't really tell him my dreams, I just make them up!" is fooling only himself. The analyst of science is less fortunately situated.

JUSTIFICATION

The difficulty is that often in conventional logic much of what actually goes on in science is dismissed as belonging to psychology or sociology, until it has been transformed to suit an antecedently chosen reconstruction. Note that it is not just matters of cognitive style that are in question here. The mere fact that something is or is not done by a scientist is indeed irrelevant to the logic of science; not so the fact that the thing done has or does not have a certain outcome for inquiry. Logicians have properly dreaded "psychologism" if this term means a confusion between how we do think and how we ought to think. But the question of how we ought to think surely depends on what happens when we *do* think in a certain way, or what *would* happen if we did. Logic is normative in contrast to psychology as descriptive, but the norms are grounded in what is or can be described. The avoidance of psychologism does not commit us to a reconstructed logic in which a disembodied intellect moves in a realm of pure ideas. Such a reconstruction cannot yield an adequate account even of what goes on in pure mathematics or in logic itself, to say nothing of what goes on in the empirical sciences.

It has been urged that to admit into logic the actualities of scientific practice is to court the danger of succumbing to the genetic fallacy. This

is the fallacy of judging the truth of an assertion on the basis of its source rather than by the evidence or argument available for it. The position is that to say that a certain operation of the understanding is logical because scientists perform it would be comparable to accepting a proposition because it is put forward by a "pure Aryan" or by a "good Communist" I believe, however, that the cases are not comparable. What is fundamental is not the mere fact that the scientist uses the operation but the further fact that it shows itself to be useful in his work. Not the origin of the proposition but its outcome is decisive. Genetic considerations are no longer fallacious when they can be reconstructed as inductive inferences based on past successes. (Some problems concerning criteria of success will be considered in §36.)

In this perspective the study of the history of science becomes of central importance. I believe that the timeless truths of an abstract logic contribute toward an understanding of scientific method just to the degree that what they have been abstracted from is the historical reality. Fortunately, there is a growing recognition—even among philosophers—of the importance for methodology of a study of the history of science, as well as a study of the timeless structure of an idealized "language of science". To be sure, logical norms grounded in scientific practice can no longer lay claim to the certainty and universality which for Plato were the only marks of genuine knowledge as distinguished from mere "opinion". But the surrender of certainty is a price which I believe that logic must be prepared to pay. Science itself manages quite well even though its own most basic principles are something less than necessarily and unconditionally true. That there is a sharp and absolute difference between logical and empirical truths has come to be questioned by many distinguished contemporary logicians. The heresy of what C. I. Lewis a generation ago called the "pragmatic a priori" is today well on the way to becoming an orthodoxy. It is at least a reconstruction of logic-in-use which deserves careful consideration.

DISCOVERY

Now what in fact goes on in scientific inquiry is usually marked off as belonging to the "context of discovery", while logic, it is held, deals only with the "context of justification". On this view, logic is indifferent

to how the scientist arrives at his conclusions, but asks only whether he is justified in reaching them. The distinction between discovery and justification, and between their respective contexts, is valid and important. I suggest, however, that the limitation of logic to the context of justification stems from confusing *this* distinction with the one I have drawn above between logic-in-use and reconstructed logic. Because our reconstructions have occupied themselves with justifications, we have concluded that there is no logic-in-use in making discoveries. If logic is what we methodologists do, not what the scientist does, then it is indeed limited to the context of justification, for this context marks the limit of *our* involvement with the pursuit of truth.

That imagination, inspiration, and the like are of enormous importance in science is recognized by everyone. It can be granted that they belong to the context of discovery (including, of course, the discovery of justifications). But does it follow that they are therefore extraneous to logic, that they are a proper subject-matter only for the psychology of science? Let me follow the popular though loose usage and refer to all such faculties as "intuition". Then the point I am making is that intuition also has its logic-in-use, and so must find a place in any adequate reconstructed logic. There is surely a basic difference between intuition and guesswork —between the intuition of the great creative genius or even of the ordinary experienced scientific worker, and the complete novice's blind, blundering guesswork or mechanical trial and error.

The difference seems to me to lie in this: What we call "intuition" is any logic-in-use which is (1) preconscious, and (2) outside the inference schema for which we have readily available reconstructions. We speak of intuition, in short, when neither we nor the discoverer himself knows quite how he arrives at his discoveries, while the frequency or pattern of their occurrence makes us reluctant to ascribe them merely to chance. Countless scientists, like the mathematician Henri Poincaré, have aspired to some self-awareness of their intuitive skills. From a purely psychological standpoint, psychoanalysis has begun to throw some light on the matter. But study of such processes by no means need be limited to a psychological orientation. We can interest ourselves also in the effectiveness of the seeker's operations in achieving their end, and it is this effectiveness that is the concern of logic. From this point of view, for instance, G. Polya has examined the context of mathematical dis-

covery. Similarly, Herbert Simon and Alan Newell have studied the programming of a computer to solve problems in the propositional calculus, not by a mechanically determinate procedure, but in a way that simulates the efforts of the logician himself to discover a proof. Intuition has "a logic of its own" only if we let it be "its own", have no interest in making that logic ours.

The logic of intuition does not in the least minimize the importance to science of serendipity, the chance discovery. But in science—and usually elsewhere, too—luck has to be deserved, as Lagrange once said of Newton. The lucky find must be appreciated to be a "find" at all: noticed, explored, and interpreted. Six months after Roentgen's chance discovery of X-rays the following interview with him was published (quoted from *113*:77): " . . . I had been passing a current through the tube, and I noticed a peculiar black line across the paper." "What of that?" "The effect was one which could only be produced, in ordinary parlance, by the passage of light. No light could come from the tube because the shield which covered it was impervious to any light known, even that of the electric arc." "And what did you think?" "I did not think: I investigated. . . ."

There is a traditional question whether there is a "logic of discovery" as well as a "logic of proof." What is it that is being asked? Clearly, we cannot have reason to predict today what the substance of tomorrow's discoveries will be, for in that case, we would be making the discoveries today. On the other hand, we may well have reason to predict in a general way what discoveries will be made, and even how, in a general way, they will come about. We cannot in 1964 say what our moon rockets will disclose in 1965 or 1970 concerning the volcanic or meteoric origin of the lunar craters, but we might be willing to predict that they will resolve the issue in favor of one alternative or the other. The question then might be, Can we formulate logical norms operative beforehand in making such inquiries, or will the norms only tell us afterwards whether a particular inquiry was successful? Can logic tell us how to do science, or only certify that we have in fact done it?

To ask for a systematic procedure which guarantees the making of discoveries as a corresponding procedure guarantees the validity of a proof is surely expecting too much. "Invention," says Mill, "though it can be cultivated, cannot be reduced to rule; there is no science which

will enable a man to bethink himself of that which will suit his purpose." The point is, however, that "invention" *can* be cultivated. Though the scientific enterprise has a significant element of luck in it, it is not wholly a game of chance, and scientific training surely enhances in some degree the skill of the players. The "logic of discovery" is, so to say, the strategy of playing the game.

Here, I do not mean "strategy" in the strict sense of the contemporary theory of games: a determination of exactly what move will be made for every possible eventuality that may arise in the play of the game. Such a determination would correspond to a complete set of rules for making discoveries (unless, to be sure, the game is one that we are bound to lose even with optimal play). I mean, rather, strategy in the sense in which it is known to a good chess player. Norms like "Seize the open file!" and "Rook behind the pawn!" cannot be demonstrated to produce a win, but the novice is well advised to follow them.

The question we have been considering can then be phrased, Do such strategies of doing science have only psychological interest, or do they also have import for the logic of science? So put, the question answers itself, it seems to me. Of course they also have import for the logic—provided we do not restrict logic to what is available to us at any given time in our reconstructions.

The play of chess illustrates nicely the bearing of the distinction between logic-in-use and reconstructed logic on the matter of "psychologism". Available reconstructions are far from adequate to the subtleties of actual play, and masters like Tarrasch and Nimzowitch who excelled in the formulation of principles nevertheless failed to achieve world championship. But there is no need to invoke a mystique of "positional sense." Intuition in chess, as elsewhere, is a logic-in-use different from available reconstructions but *not* therefore incapable of being more and more closely approximated by better reconstructions. It is entirely possible that before long computers will be programmed so as to play chess with superhuman success. Such a program is essentially a reconstructed logic of the game, and the more successful it is as compared with chess masters the better reconstruction of their logics-in-use it is likely to be.

Now what is crucial is that the considerations that make for good play lend themselves to logical as well as psychological analysis. The great Lasker was famous for "playing his opponent" rather than "the board":

making objectively inferior moves, even known by him to be such, in order to exploit temperamental and other weaknesses in his adversary's personality. But what makes such play extraordinary is precisely that we *can* conceive of a move as being "good" without regard to psychological considerations. No doubt Lasker's style is a reflection of his own temperament, and his approach to the game embodies, as he himself tells us, his "philosophy of struggle". Every *use* of a logic is a matter of psychological fact, and being so, is subject to the factors that determine all such facts. But the logic-in-use itself is occupied, not with *those* factors, but with the factors of the problem at hand. Thinking, whether logical or illogical, is always a psychological process; but when it is logical, it reaches out to the larger world where the problem that occasioned thought has its locus. The question how we ought to think and not merely how we do think does not make psychology irrelevant but instead gives relevance to a good deal else besides.

That logic can and should concern itself with the process of scientific discovery, with the process of reaching conclusions as well as with the proof of the conclusions reached, is a position taken by many philosophers, from Aristotle to Peirce and Popper. Recently it has been suggested (N. Hanson in *41*) that the "logic of discovery" can be construed as a study of the reasons for entertaining a hypothesis, in contrast with the logic of proof, which deals with the reasons for accepting a hypothesis. To some extent, unquestionably, these reasons coincide. We entertain hypotheses with high antecedent probability, and this antecedent probability enters into the assessment of its confirmation by the evidence. The maxim "Look for the woman!" guides the detective, and the hypothesized motive it provides is an important element in the case against the suspect. But not all reasons for entertaining a hypothesis are of a kind which necessarily play a corresponding part in its acceptance.

The drunkard's search is relevant here; the pattern of search, we feel, should be closely related to the probability of the thing sought being in the place where the seeker is looking. But the joke may be on us. It may be sensible to look first in an unlikely place just *because* "it's light there". We might reasonably entertain one hypothesis rather than another because it is easier to refute if false, or because it will eliminate a greater number of possibilities, or because it will show us more clearly what steps to take next. We may conduct the search of an area by going up

one side and down the other, or by moving outward from the center in a spiral, or in a variety of other ways, according to circumstances. The optimal pattern of search does not simply mirror the pattern of probability density of what we seek. We accept the hypothesis that a thing sought is in a certain place because we remember having seen it there, or because it is usually in places of that kind, or for like reasons. But we entertain the hypothesis, that is, we look in a certain place, for additional reasons: we happen to be in the place already, others are looking elsewhere, and so on. I do not see that the difference between these kinds of reasons is one only of "refinement, degree, and intensity".

It might be said that these other sorts of reasons are to be distinguished from purely logical considerations as being of a merely practical kind. I agree to the adjective, but not to the adverb; there is nothing "mere" about it. If logic relates to scientific practice, it is inescapably concerned with what is "practical"—that is, with what works in science. I believe that the tendency to exclude such concerns from the domain of our reconstructed logics stems from the demand, of which I spoke earlier, for universality and necessity. What is practical in one set of circumstances may not be so in another, and in any case, we cannot establish that it *must* work. To be sure, logic is interested in the greatest possible range of application of its norms, and in the firmest possible grounding of their claims. But may it not stop short of the ultimate in both respects? Must logicality be, like the Stoic conception of virtue, a perfection which does not admit of degrees? What a scientist does in a particular case may be more or less reasonable, sensible, intelligent. What makes it such is not something in his psychology nor in ours who are appraising what he does, but something in his problem and in the appropriateness to it of the operations of his understanding. This latter something is the subject-matter of what I am calling logic.

§3. *The Task of Methodology*

The word "methodology", like the words "physiology", "history", and "logic" mentioned earlier, is also one which is used both for a certain discipline and for its subject-matter. I mean by *methodology* the study—the description, the explanation, and the justification—of methods, and not the methods themselves. Often when we speak of the

"methodology" of, say, economics, we refer to the method or methods used by economists (more likely, some particular school of economists). Though the ordinary usage is ambiguous in this respect, it does not, I think, lend itself to any serious equivocations. The uncertainty in the meaning of the term lies elsewhere. I distinguish four usages.

TECHNIQUES

Let me call "techniques" the specific procedures used in a given science, or in particular contexts of inquiry in that science. For example, there are certain techniques associated with the use of the Rorschach test, or with a mass opinion survey; there are statistical techniques, like those involved in factor analysis; techniques for conducting an interview or for running a rat through a maze; techniques of carbon dating and of deciphering unknown inscriptions; and so on endlessly. There is a right way and a wrong way to do everything, in science as in any other work; or at any rate, there are better and worse ways of doing it. The techniques of a science are the ways of doing the work of that science which are regarded, for more or less compelling reasons, as being acceptable. Scientific training is to a significant extent the mastery of techniques.

Now, what is often called "methodology" is a concern with techniques in this sense (if only the word "technology" did not already have another meaning!). In particular, "methodological studies", so-called, are usually inquiries into the potentialities and limitations of some technique or other, or explorations of some of its variants. Sometimes, no doubt, the concern with the technique is spurious, and the presumed "methodological" interest is in fact a retreat to a previously prepared line of defense against criticisms of the substantive outcome of the inquiry. Yet genuine "methodology" in this sense has a secure place in scientific literature, and indeed, again and again, has been connected with important scientific advances, as in the case of Freud's technique of free association and Pavlov's technique of conditioning.

HONORIFICS

There are other cases in which the term "methodology" is used as though it concerned, not the particularities of specific techniques, but rather what is thought of as "the" scientific method. In this usage,

a "methodological" prologue is something of a ritual, an invocation to the presiding deities of scientific method, serving to ensure an appropriately "scientific" status for what follows, and avowing the proper concern with meeting standards of scientific acceptability. This honorific use of "methodology" expresses that concern without any clear indication of how the concern was embodied in the inquiry itself. It is my impression that the honorific usage was more frequent two or three decades ago than it is today; it may well have been a response to the truly spectacular developments in logic in that period. Logic today, being more familiar, is perhaps not quite so awesome as it once was. The principle of autonomy does not imply, be it noted, that there are no norms of scientific inquiry but only that they are not derived from something outside science.

EPISTEMOLOGY

As used by philosophers, "methodology" is often indistinguishable from epistemology (theory of knowledge) or philosophy of science. In this sense, the subject-matter of methodology consists—very roughly speaking—of the most basic questions that can be raised concerning the pursuit of truth. Characteristically, this "methodology" deals with what can be said about science, or particular sciences, "in principle" or "in the last analysis". Its problems arise either from its own reconstructed logic or from various philosophical positions, rather than from recurrent difficulties encountered in the course of scientific inquiry itself.

For instance, the problem of the justification of induction, as it is known, has been of enormous importance in philosophy since Hume. The proposition that the patterns we have so far experienced will persist in what is yet to come, Hume pointed out, is neither a tautology nor can it be grounded in experience without begging the question. How we can know that the future will resemble the past, and whether, indeed, some principle of "uniformity of nature" is even presupposed by science—such questions have exercised many philosophers of science. Yet scientists themselves—and surely behavioral scientists—would be quite content to have only as much justification for their predictions as we have for expecting the sun to rise tomorrow.

Again, determinism-indeterminism is an issue widely debated, with reference not only to quantum mechanics but also to human behavior and

so-called "free will". Yet dialectical demonstrations that everything *is* "causally necessitated" do not assist inquiries into any particular causes, nor do demonstrations of an "ultimate indeterminacy" prevent us from trying to learn more than we now know about such determinants as there are. And as for the "free will" which is philosophically affirmed or denied, it appears to be quite remote from the freedom which is the concern, say, of the political scientist, economist, or child psychologist.

A less clear-cut example is provided by the problem of the so-called "counterfactual conditional" ("If X *had* happened, then Y *would* have followed"). In the last decade or so a voluminous literature on this problem has accumulated. The problem is that if the antecedent of a hypothetical (if—then) proposition is false, the proposition as a whole is true regardless of what the consequent is; yet we feel that it is reasonable to complete it with some consequents and not with others. The difficulty appears to be generated by a particular reconstruction of implication, the logic-in-use with "if—then". Here, however, the more narrowly philosophical problems shade over into other problems with direct bearing on scientific inquiry: the analysis of counterfactual conditionals seems intimately involved with the structure and function of scientific laws (§11).

There is no doubt that broad philosophical perspectives, and even specific metaphysical doctrines, have played a significant part in the history of science, exercising a considerable though indirect influence on the direction of inquiry and on the formation and acceptance of scientific hypotheses. The works of the mind are all of a piece; what happens in one science is affected, not only by what is going on in the others, but also by the thought of the period on matters of religion, politics, art and whatever.

Descartes' metaphysics, to select a simple illustration, was important in the history of medicine, for it encouraged viewing even the human body as a material thing. The anatomists who came after Descartes had fewer difficulties in getting cadavers for dissection than had afflicted Vesalius before him; and the concepts of physics and chemistry that were developed during the next two centuries found readier application to human biology. But the example illustrates also that the influence of a metaphysics on science can be a pernicious one. Cartesian dualism slowed the progress of psychosomatic medicine, and may still be operative in what Norman Reider has called the "demonology" of modern psychiatry.