

I



Belief and Action

1] *Global vs. Local Justification*

Philosophers and scientists have often arranged, either tacitly or explicitly, a rather delicate division of labor. In science, justification of belief is demanded only when the need for such justification arises in the context of specific inquiries. Philosophers have often been discontent with justification in this "local" sense. Like Descartes or some of the contemporary writers who worry about choosing between "conceptual schemes," many philosophers occupy themselves with efforts at the more "global" justification of the totality of beliefs held at a given time.

Consider an investigator who is puzzling over apparent cloud formations that have been recorded on pictures of the surface of Mars. He wants to know whether they are sandstorms, collections of moisture or mere illusions created by some malfunction of the camera. His specific question controls what is to count as a relevant answer. At the outset, he is in doubt as to which of the relevant answers to accept as true. He wishes primarily to remove this doubt; when he finally proposes an answer, however, he also feels obligated to justify his picking that answer rather than some alternative.

Supplying this justification requires an appeal to evidence, which will include observation reports and theoretical assumptions, as well as much of the apparatus of logic and mathematics. In short, evidence will consist of those of the investigator's findings and beliefs that are relevant to the problem at hand and are not likely to be questioned by any participant in the inquiry or by anyone who is qualified to evaluate its results. Evidence is not ruled out as illegitimate solely because of the possibility that in some future inquiry it may be shown to be false. Sleeping dogs are allowed to lie when there is no apparent reason, at the moment, for arousing them.

Global justification is more demanding. Following Descartes, the globalist wishes to show that all his beliefs (at least those he holds at a given time) are justified. Consequently, he seeks an evidential base in which all the evidence is evident; for the nature of evident beliefs is that they carry with them their own justifications, which render them impervious to legitimate question in further inquiry.

Evident beliefs are generally held to belong to one of two categories: (a) Belief in the truth of necessary propositions. To understand such a proposition is to recognize the justification of its truth. (b) Belief in the direct testimony of the senses. Here understanding conjoins with direct empirical confrontation to justify belief.

Efforts at global justification have historically foundered on three major difficulties. The domain of necessary truth, upon being subjected to close scrutiny, has been found to be restricted to the realm of logical truth. The incorrigible status of direct reports of experience, on the other hand, has been impugned. Finally, since Hume, even fairly accommodating conceptions of the scope of the evident have had trouble including within the confines of the evident presuppositions adequate to justify the multitude of nonevident beliefs that are supported by science and common sense. When two "conceptual schemes" both of which satisfy minimal requirements of logical consistency and empirical adequacy are brought face to face, the suspicion arises that no rational means is available for deciding between them on global terms.

Global skepticism does not, however, imply local skepticism. Evidence in local inquiry includes both necessary truth (what-

ever that may be) and observation reports. In short, it will contain whatever counts as evident from the global point of view. But the price of admission to evidential status is not global. The evident (if there be such) is evidence in local inquiry because all parties to the inquiry agree that such beliefs do not stand in need of justification. Beliefs that are nonevident can also meet this less rigorous standard: laws and theories can and generally will be taken as evidence even though they are not evident. To include H in the evidence requires that (a) at the time of the inquiry H be believed true, and (b) at that time, critical scrutiny of H in the light of new evidence be considered pointless. What cannot legitimately count as evidence consists of beliefs that are subject to question in the inquiry itself and beliefs which, while they are not actually questioned at the time of the inquiry, are recognized at that time as calling for further examination.

Satisfaction of the conditions for evidential status does not imply incorrigibility. Results of new experiments, new disagreements, and deeper curiosity can all provide the occasion for removing some item from the evidence. To regain evidential status, the truth of the hypothesis in question would have to be justified by an appeal to other evidence, which in turn would have to be shown to be sufficiently decisive to justify terminating further efforts at the collection of evidence.

Thus, the central problem of a theory of local justification or rational belief is the establishment of criteria for determining which of the relevant answers to a given question, on the evidence available, is the best. These criteria do not determine what questions are raised or what evidence is available. Rather they reveal features of legitimate inference that are invariant over broad categories of local inquiries, regardless of the questions and the evidence peculiar to each individual problem.

Admittedly some comments on questions and evidence will be in order. The presuppositions of questions and other evidential assumptions are often, during the course of scientific investigations, subject to critical review. But even here, local justification for accepting H as evidence requires local justification for taking H to be true on other evidence. A theory of evidence, insofar as one is needed, depends upon a theory of inference—not the other way round.

Interest in local problems does not imply disparagement of

global concerns. The sense in which justification is demanded by globalists is different from the sense in which justification is pertinent to local inquiries. The skeptical despair that threatens quests for global justification need not infect efforts at local justification. Whether that despair stands in need of alleviation in its own right, however, is an entirely separate matter.

This book is concerned with justification in the local sense. Some features of local justification will be considered that seem to remain invariant over a wide variety of investigations. This investigation, though broad in scope, is itself a local one. No attempt will be made to justify the criteria offered by deriving them in some globalistically impeccable manner from the incontrovertibly evident. Such justification as is possessed by the proposals to be made comes from their success in systematically accounting for more or less widely acknowledged features of local justification. These proposals are, it goes without saying, liable to error and subject to revision.

2] *Local Skepticism*

Skepticism appears in many forms. Although local justification appears to be immune to the variety that threatens global justification, a relatively recent strain has been bred in a culture brewed from contemporary reflections on the relations between rational belief and rational action. In reaction against certain oversimplifications that are to be found in classical models of this relation—especially when these are applied to risky situations—some writers have concluded that no beliefs can be justified, even locally, that are not deducible from the available evidence.

From the local point of view, the evidence available at a given time consists of assumptions that might and often will be subject to question at some later time. When they are so questioned, even local justification will require supporting belief by appeal to evidence via nondeductive inference. Consequently, once evidence has been questioned, it can never regain its status unless it is shown to be evident (or deducible from the evident). This new skepticism, which arises in the context of local justification, will lead an investigator who takes it seriously, over the long run, to restrict his stock of beliefs to the evident, to what is deducible

from the evident and to those nonevident beliefs that to date he has not questioned. Local skepticism thus converges on global skepticism. The remainder of this chapter will be given over to a consideration of those features of rational belief and action that seem to imply this new skepticism and to some suggestions for its elimination.

3] Naive Cognitivism

Most philosophers, in discussing the problem of selecting an optimal policy for realizing given ends, adopt a very simple model for dealing with this situation.¹ They view the decision-maker as confronted with a choice among several optional courses of action. If he is to make a rational choice among these options, he must form some judgment about the consequences of the alternative policies relative to his goals and compare these consequences in terms of the extent to which they constitute realizations of these objectives.

Let A_1, A_2, \dots, A_n be a set of alternative policies open to the decision-maker, o_1, o_2, \dots, o_n the set of relevant outcomes expected by the decision-maker to eventuate from adoption of these options, and $u(o_i)$ the "utility" or value attached to the i th outcome in the light of the decision-maker's goals.² The simple model just described may be represented as follows:

A_1	$u(o_1)$
A_2	$u(o_2)$
.	.
.	.
.	.
A_n	$u(o_n)$

¹ The view to be discussed here has apparently been considered too obvious to warrant detailed scrutiny by students of ethics. Writers like Dewey, Moore and Stevenson seem to share it. Those philosophers who, like Kant, attempt to minimize the relevance of consequences in typically ethical decision problems do, nonetheless, adopt this position with respect to questions of prudence and expediency. The subsequent discussion leaves open the question of the extent to which the problem of selecting an optimum policy for realizing given ends is typical of ethical decision problems.

² The notion of utility as used here may refer either to the position of a given outcome in a rank ordering of outcomes with respect to value relative

Given the information in this representation, the rational decision-maker is enjoined to adopt the A_i for which $u(o_i)$ is greater than any $u(o_j)$, where j is different from i . In case of a "tie," the acts involved are deemed equally optimal.

According to this simple model, two factors are central to determining a rational choice among a given set of options: the outcomes or consequences of these options and the utility or value attached to them. Most discussion by writers on ethics has been devoted to a consideration of the second factor. Determination of the consequences of action has been taken to be a matter for scientists to handle. Philosophical questions that pertain to the determination of consequences are, therefore, considered to be properly assigned to students of scientific method and inference and not to students of ethics.

The point of interest here has nothing to do with this division of labor. Rather what is involved is the assumption that science can help us ascertain the outcomes or consequences to which utilities or values are to be attached. This claim presupposes that part of the product of scientific investigation consists of predictions—i.e., statements about the future that are accepted as true. Such a view conforms well to the classical position that holds that at least one of the aims of science is to replace doubt by true belief. The result of efforts to attain this aim is a body of propositions divided into those accepted as true, those rejected as false and those still consigned to the limbo of doubt. Part of this output—to wit, that portion consisting of predictions of the outcomes of policies—is the scientific contribution to rational decision-making. Thus, the scientific quest for truth works hand in glove with the practical man's effort to attain his ends in the best way possible.

Many supporters of naive cognitivism (as we shall term the view just outlined) recognize that the conclusions reached in

to the goals of the decision-maker or, when the context calls for it, an interval measure of value—e.g., the interval measure based on postulates for rational preference of a Von Neumann-Morgenstern variety.

The utility assigned to an outcome can be understood to reflect all kinds of values, including conformity to or violation of moral laws, the goals of the decision-maker, prudential and aesthetic considerations, etc. The terms "goal," "objective," etc. could be interpreted as shorthand for the factors determining the decision-maker's utility function. The subsequent discussion attempts to remain neutral between this and narrower readings.

scientific investigations cannot be guaranteed to be true solely on the basis of the evidence that supports them. Hence, the scientist cannot assure the decision-maker that the predicted consequences of his actions will come true. The decision-maker has to realize that his policies may result in outcomes other than those predicted, and these outcomes may be far less desirable relative to his goals than the ones he anticipates. At the very least, therefore, the diagrammatic representation of the decision-maker's problem has to be modified in order to take into account these other outcomes.

	H_1	H_2	...	H_m
A_1	$u(o_{11})$	$u(o_{12})$...	$u(o_{1m})$
A_2	$u(o_{21})$	$u(o_{22})$...	$u(o_{2m})$
·	·	·	...	·
·	·	·	...	·
·	·	·	...	·
A_n	$u(o_{n1})$	$u(o_{n2})$...	$u(o_{nm})$

In this representation, the H_j 's are an exclusive and exhaustive set of hypotheses, which describe the possible "states of nature" under which the decision-maker may be acting. The outcome o_{ij} represents the result of adopting policy A_i when H_j is true.³

The naive cognitivist view may be restated in terms of this matrix representation as follows: The scientist aids the decision-maker by indicating which of the alternative hypotheses H_j we are entitled to accept as true on the basis of the available evidence. The selection of an H_j restricts the decision-maker's problem to considering the utilities of outcomes in the j th column of the matrix, according to the method previously described.

Although most philosophers who write in ethics seem to subscribe to some variant of naive cognitivism, this view has been

³ Real-life decision-makers would usually consider the possible outcomes of each act to be available to them separately, without making them a function of a single set of exclusive and exhaustive hypotheses. However, insofar as the decision-maker is able to specify alternative outcomes for each policy considered, simple formal devices can be used to convert the representation of his problem into this form. Since it is both customary and convenient to use this scheme, it will be adopted here. It must be remembered, however, that real-life decision-makers are often vague in their ideas about alternative outcomes of options available to them. Hence, this discussion is infected with a certain (hopefully tolerable) amount of idealization.

almost universally rejected by those statisticians, social scientists and philosophers who in recent years have concerned themselves with decision theory. Certain elementary, almost commonsensical, considerations support this rejection. First, the evidence available to the scientist may oblige him to suspend judgment. In that event, his conclusions are of no help to the decision-maker, who still has to decide what to do. Thus, a physician may prescribe a certain therapy even though he cannot honestly predict what the outcome of the therapy will be. He may argue that, while he cannot make the prediction, the evidence available to him indicates that the chances of successful cure are greater through this therapy than through its alternatives. Second, the evidence may entitle the scientist to accept one of the H_i 's as true, yet may not warrant the decision-maker's choosing the act that produces maximum utility when H_i is true. Recently certain medical groups temporarily suspended dispensing the birth control pill, Enovid, pending further examination of evidence regarding its safety. Several physicians endorsed this policy, even though they acknowledged that they believed the pill to be safe. The trouble here stems from the fact that no amount of evidence can provide an infallible guarantee of the safety of the pill. Conclusions reached regarding its safety run the risk of error. Similarly, the policies adopted concerning the use of the pill run the risk of leading to disastrous consequences. If moral considerations demand greater insurance against possible harmful side effects of use of the pill than our cognitive scruples demand against possible error, the available evidence could render it quite reasonable to predict the safety of the drug and still not to recommend its use.

These considerations suggest that naive cognitivists oversimplify the relevance of the conclusions of science to policy-making. If the conclusions reached by scientists about the consequences of policies are relevant to the choice among policies, they are relevant in a more indirect way than is indicated by naive cognitivists. For this reason, alternatives to naive cognitivism must be considered.

4] *Behavioralism*

The considerations just cited as reasons for abandoning naive cognitivism point to the conclusion that not belief but probability is the guide in life. This is not to say that decision-makers must rely on probabilities in a sense that conforms to the requirements of the calculus of probabilities. The point is that it is the degree to which evidence supports the alternative hypotheses, confirms them, renders them more likely, etc., that is relevant to selecting optimal policies. How and in what sense degrees of evidential support are to be measured remain open questions. Hence, if the scientist does contribute any guidance to the decision-maker, it is not through the prediction of consequences. Rather it is by indicating how likely various outcomes will be when given policies are undertaken, or how well the available evidence supports alternative hypotheses about the state of nature.

This thesis about the relevance of science to action has often been accompanied by a still stronger claim—namely, that scientists do not accept or reject propositions at all, unless such acceptance or rejection is reduced to action that is related to practical objectives. “Behavioralists,” as supporters of this position may be called, sometimes deny outright that scientists either accept or reject hypotheses. Thus, Rudolf Carnap contends that the “conclusions” of nondeductive or inductive inferences are the assignments of degrees of confirmation to hypotheses. According to Carnap, the only statements that can be accepted are those that are directly confirmed by observation, logical and analytic truths, and the deductive consequences of these.⁴ Other writers (e.g., Richard Rudner and C. W. Churchman) do assert that scientists accept hypotheses, but they seem to interpret “accepting a hypothesis as true” to mean the same as “acting or being disposed to act in the manner which would be best relative to a given objective if the hypothesis were true.”⁵ The significant dif-

⁴ R. Carnap, *Logical Foundations of Probability* (2nd ed.; Chicago: University of Chicago Press, 1962), pp. 205–206.

⁵ R. Rudner, “The Scientist *qua* Scientist Makes Value Judgments,” *Philosophy of Science*, 20 (1953), 3 f. and C. W. Churchman, “Science and Decision Making,” *Philosophy of Science*, 23 (1956), 248–249.

ference between these two versions of behavioralism is that the latter views the scientist as being actually a decision-maker, whereas the former relegates the role of the scientist in decision-making to that of a guidance counselor. Both positions agree, however, that insofar as scientific inquiry can be isolated from practical concerns, the output of such investigation consists of assignments to statements of degrees of probability, confirmation, likelihood, and so forth.

5] *Critical Cognitivism*

The considerations adduced in favor of rejecting naive cognitivism do not necessitate the adoption of a behaviorist viewpoint.⁶ At least some scientific inquiries may be concerned with prediction, postdiction, explanation, and other activities that lead to accepting propositions as true, rejecting them as false, or suspending judgment on them. The force of the objections raised previously was directed against the assumption that the outcomes of such activities contribute to rational decision-making. If what scientists do is attempt to replace doubt by true belief, then the results of their efforts are not directly relevant to practical affairs.

To be sure, those "critical cognitivists" who affirm the antecedent of this conditional acknowledge that scientific inquiry bears some relevance to decision-making. An investigator who is engaged in the quest for truth will have to weigh evidence for and against alternative conclusions; the degrees of probability, confirmation, evidential support reflecting such weighings are relevant to decision-making as much as to truth-seeking. But critical cognitivists will insist—contrary to the behaviorist view—that these weighings are no more the end-products of scientific inquiry than they are of decision-making. They are rather pro-

⁶ Rudolf Carnap has argued against the propriety of inductive rules of acceptance by way of considerations similar in all essential respects to the arguments described previously as telling against naive cognitivism. See R. Carnap, "Replies and Systematic Expositions," in *The Philosophy of Rudolf Carnap*, Paul A. Schilpp, ed. (LaSalle, Ill.: Open Court Publishing Co., 1964), pp. 972–973. He uses such arguments as a basis for defending his own behaviorist point of view.

cedures relevant to the attainment of two distinct types of objectives—practical and cognitive (theoretical) objectives.

Thus, critical cognitivism renders asunder, at least partially, what many philosophers have endeavored to join together—theoretical and practical wisdom. John Dewey, for example, found it possible to accommodate the view that scientific inquiry is disinterested inquiry with the thesis that the results of such inquiries are important as guides to action.⁷ This accommodation is predicated on the naive cognitivist assumption that propositions accepted as true are appropriate guides. If this assumption is true, then an investigator can engage in a quest for truth without concern for any specific practical objective. His conclusions will nevertheless—pending further evidence—provide the basis for rational action relative to any practical objective for which the answers he obtains are relevant. Similarly, writers like Braithwaite, who are eager to uphold an action analysis of belief, can maintain, with the tacit aid of naive cognitivism, that belief that *p* entails a disposition to act as if *p* were true, in relation to any practical objective.⁸

The objections previously raised against naive cognitivism suggest that positions such as Dewey's or Braithwaite's are in need of drastic revision. Situations can be specified in which a rational agent would not act as if a proposition *p* were true unless he had an infallible guarantee of the truth of the proposition.⁹ Evidence might warrant acting on the proposition relative to some objectives; owing to the seriousness of error, however, the same evidence might justify acting as if the proposition were false relative to other objectives. This situation cannot be avoided except by evidence that entails the proposition in question. Thus an action analysis of belief of the sort that Braithwaite at one time advocated entails the requirement that a rational agent believe

⁷ John Dewey, *Reconstruction in Philosophy* (New York: Beacon, 1948), pp. 121–123.

⁸ R. B. Braithwaite, "The Nature of Believing," *Proceedings of the Aristotelian Society*, 33 (1932–33), 129–146; and "Belief and Action," *Aristotelian Society Supplementary Volume*, 20 (1946), 1–19.

⁹ See I. Levi and S. Morgenbesser, "Disposition & Belief," *American Philosophical Quarterly*, 1 (1964), 222–223. R. C. Jeffrey has used substantially the same argument to support a behaviorist viewpoint. See R. C. Jeffrey, "Valuation and Acceptance of Scientific Hypotheses," *Philosophy of Science*, 33 (1956), 245.

only those propositions to be true that constitute evidence. This would imply, as has already been argued, that rational belief will tend to be restricted to the evident—a consequence that neither Dewey nor Braithwaite would be prepared to accept.

Advocates of action analyses of belief can avoid this difficulty by abandoning the thesis that accepting a proposition as true entails a disposition to act as if the proposition were true, relative to *every* objective to which the proposition is relevant. Restrictions have to be imposed on the objectives. The most obvious suggestions are the following:

a) Equate "believes that p" with "being disposed to act as if p were true relative to objective O," where O is a specific practical objective. This view (which is at least implicit in the position advanced by Richard Rudner¹) reduces attempting to replace doubt by true belief to practical decision-making. In short, it results in one version of behavioralism.

b) Equate "believes that p" with "believes that p to a certain degree." Since degrees of belief (at least in the sense in which they are understood to be subjective probabilities) are supposed to reflect the decision-maker's judgment with regard to how well evidence supports hypotheses, the obstacles cited above to an action analysis of belief do not apply. "Believes p to degree k" can be interpreted as characterizing a disposition to act as if p were true for those objectives relative to which evidential support for p to degree k rationally warrants such action. This analysis leads to that variant of behavioralism according to which the conclusions of science are degrees of probability, confirmation, evidential support, etc., which determine rational degrees of belief.

Thus, quite aside from various other considerations that have been adduced in favor of behavioralism, it is easy to see why writers who are committed to action analyses of belief, or adherents to the idea that knowledge is power, would opt for behavioralism rather than critical cognitivism. Critical cognitivism insists that both the scientist and the common man are frequently concerned to replace doubt by true belief in a sense that is not amenable to analyses either of type (a) or of type (b). It recognizes the quest for truth as a legitimate human activity whose

¹ Rudner, *op. cit.*, pp. 2-3.

aims and products are not directly relevant to practical concerns; it rejects action analyses of belief.

Yet, in spite of its inhospitability toward action analyses of belief, critical cognitivism deserves a hearing if for no other reason than the liabilities from which behavioralism suffers. Behavioralists must admit that in some instances scientists do accept propositions in a sense that is difficult to reduce to a disposition to act relative to practical objectives. Even the decision-maker must consider the range of possible outcomes of alternative policies; this requires that he accept as true statements that specify alternative outcomes. These disjunctive statements may in some instances be logical truths, such as a behavioralist might be prepared to admit are accepted as true. However, the decision-maker must also accept statements that describe the evidence on which assignments of probability, degrees of confirmation, etc. are based; as is usually acknowledged, such statements cannot be provided with an infallible guarantee. Finally, what counts as evidence is frequently a function of theoretical commitments.² Practical deliberation, like scientific inquiry, relies on evidence that is not evident.

Action analyses of belief are sometimes thought to be important to the clarification of obscurities that reside in the notions of belief, disbelief, and suspension of judgment. These notions are indeed in need of clarification; but then again, so are the notions of declaring war, taking a walk, making an omelet, writing a book, and so on. In particular, attempts to reconstruct such concepts in terms of language that describes publicly observable behavior seem to be no easier to carry through than analogous attempts in the case of belief. Critical cognitivists are free to regard belief predicates as theoretical, relative to language that describes overt behavior; they can also concede that such attributions are in need of clarification.

Such clarification must ultimately depend, however, upon advances in psychological theory. Proposals of criteria for rational belief need not wait for such advances. If it is reasonable (as indeed it is) to proceed with systematic studies of rational action without a fully adequate understanding of the notion of an act

² For further discussion, see C. G. Hempel, "Deductive-Nomological vs. Statistical Explanation," *Minnesota Studies in the Philosophy of Science*, 3 (1962), 161.

(rational or irrational), surely it cannot be unreasonable in itself to attempt studies of criteria for rational belief with only a vague understanding of the notion of belief.³

6] *Autonomy*

The critical cognitivist outlook is hostile not only to the more or less familiar varieties of action analyses of belief attributions. By insisting that scientific inquiry has objectives quite distinct from those of economic, political, moral, etc. deliberation, it rejects the point of view which holds that the scientific propriety of a man's beliefs is dependent, at least in part, on the moral, political, economic, etc. consequences of his having these beliefs.⁴

Beliefs, being inner (psychological) states, are accessible to public scrutiny only by way of their manifestations in overt behavior; these manifestations can on some occasions have psychological and social consequences of a morally, economically, etc. relevant nature. On such occasions, it might be argued, justification of belief should take into consideration these consequences.

Consider, for example, an economist who is acting as advisor to the government and is asked whether a tax cut will have sig-

³ The view regarding belief predicates advocated by the author follows lines similar in many respects to the position proposed with respect to want predicates by Brandt and Kim (R. Brandt and J. Kim, "Wants as Explanations of Actions," *Journal of Philosophy*, 60 [1963], 425-435). However, one important qualification ought to be mentioned here. Brandt and Kim contend that common sense is committed to rudimentary psychological theories which determine the conceptual content of want predicates in ordinary language. Developments in psychology might lead to the supplementation and revision of these theoretical assumptions and bridge laws, but certain conditions (Brandt and Kim cite illustrations) would be revised only at the expense of a radical alteration in the concept of "wants." In the case of many psychological predicates, it seems doubtful whether families of conditions of this sort can be furnished which are not extremely weak. What can sometimes be done is to provide a system of principles of rationality—e.g., of rational belief. Such principles are admittedly normative. The assumption that they are realizable or are realized to some degree, however, may be taken as a theoretical postulate characterizing a "core" meaning for the predicates involved—subject of course to modification through psychological inquiry. If this observation is correct, not only may an investigation of rational belief proceed without benefit of a full-bodied analysis of belief, it may contribute to the development of such an analysis.

⁴ The view under consideration has been taken by Richard Rudner in an oral reply to criticisms that I have levelled against him.

nificant inflationary effects. His beliefs will influence the answer he offers and thereby have an impact on the economy. Surely he ought to take the consequences of his beliefs into account in forming his opinion on this question.

Perhaps occasions do arise in which deciding what to believe brings forward serious moral, political, economic, etc. questions. And it is conceivable (but just barely) that on some occasions moral, political or economic considerations might carry sufficient weight to justify conclusions that scientific considerations would not by themselves justify. Perhaps, a person is justified in believing the earth to be flat if not to do so would do him great emotional harm. Cases such as this arise when there is a conflict between the interests of scientific inquiry and other human interests. How such conflicts are to be adjusted may indeed be an important and complex question, quite on a par with adjudicating between conflicting moral claims, moral and political claims, etc. Nevertheless, recognition of this fact does not rule out maintaining that scientific inquiry has its own objectives, in the light of which the results of such inquiry are to be judged qua scientific results. A person may be justified in believing the earth to be flat; but he cannot be justified in so believing according to scientific standards of fixing belief.

The main point to keep in mind here is that, when they are engaged in prediction, estimation, explanation, etc., the conclusions that scientists reach, insofar as they are put forth as conclusions justified by scientific standards, are to be judged in terms of the institutional objectives of scientific inquiry. The critical cognitivist point of view assumes that such standards are operative in the scientific evaluation of beliefs. It does not insist that the objectives of scientific inquiry are identifiable with the personal ends of scientific investigators but only that, insofar as the scientist (or average man) seeks to have his opinions appraised in a scientific or "rational" (in one of the many senses of that term) way, the appraisal is to be made with reference to these objectives.

One need not be dogmatic with regard to how the "cognitive" or "theoretical" aims of scientific inquiry are distinguished from the "practical" ends of moral, economic, political, etc. deliberation, in order to insist that they are different. Nor is it necessary to suppose that all scientific inquiry has a single type of cognitive

objective. It might very well prove to be true that the objectives involved in explanation are different from those involved in prediction and estimation. Questions such as these are best postponed until some effort has been made to isolate the objectives that are characteristic of various kinds of inquiry and to compare them in a systematic way. Perhaps certain features can then be selected that will characterize the aims of scientific inquiry as contrasted with the ends of practical deliberation.

A full defense of such a position requires elaboration of a theory of scientific inference which will relate the criteria for legitimate inference to scientific objectives; a partial defense can be offered at the outset, however, in the light of at least two considerations.

First, scientific inquiries are often conducted in situations where no practical interests are visibly at stake—at any rate, none that are seriously considered relevant to assessing the results. (One can always cite the investigator's ego involvement in his work; but is that relevant to appraising the scientific value of his contributions?) In such cases, truth, relief from agnosticism, explanatory power, etc. seem to be desiderata, but these are precisely the sorts of values that are characteristic of theoretical as contrasted with practical concerns.

Second, it should be noted that scientific interests are public interests, in the sense that conclusions reached by scientists in order to further scientific ends are of value to the scientific community only insofar as they are communicated and subjected to scrutiny by that community. This implies that along with the cognitive objectives of science account should be taken of procedural norms that pertain, among other things, to honesty in reporting results and are intended to facilitate the furtherance of the cognitive ends of science.

Now, regardless of whether serious conflicts ever do arise between scientific and other interests with respect to belief, it is sometimes true that conflicts of a morally serious nature are occasioned when scientists reflect on whether they should communicate the results that they have obtained. From the legendary anguish of the Pythagorean community to the concern of Einstein and others over their responsibilities with regard to the atomic bomb, the history of science is replete with moral problems that have been occasioned by *prima facie* obligation to scientific procedures.

It is difficult to understand, however, why such conflicts are serious—indeed, are conflicts at all—unless scientists are understood to be committed to certain objectives distinctive of the scientific enterprise. If science is indeed the handmaiden of practice, as the behavioralists would have it, then all conflicts involving scientists would appear to be conflicts between moral, economic, political, aesthetic, etc. values.

Consideration of implications such as these suggests, if it does not establish, that scientific inquiry is part of an autonomous enterprise, which is engaged in pursuing special objectives, distinct from those that are found in other sorts of deliberate human activity.

7] *Statistical Inference*

Many of the observations that have been made in outlining the critical cognitivist viewpoint might appear to be philosophically commonplace. To be sure, the factor of risk, which is so damaging to the naive cognitivist viewpoint, has not until recently received from philosophers a fraction of the attention that it deserves. But hopefully it has been shown that to abandon naive cognitivism need not necessarily mean adopting a behaviorist viewpoint.

However, the strongest weapon in the behaviorist arsenal has yet to be considered. Since the work of Neyman and Pearson on the testing of hypotheses and interval estimation, the point of view adopted by writers on the theory of statistical inference has become increasingly a behaviorist one. The problems of statistical inference are, according to what is by now the orthodox position, problems of decision-making under risk or uncertainty. Such decision-making is generally understood to be practical decision-making, in the sense in which a production manager, a politician, a general is engaged in practical decision-making. Inductive inference is, in the language of Jerzy Neyman, reduced to inductive behavior.⁵

The authority of statisticians is not necessarily decisive in matters as controversial as behaviorism. However, the con-

⁵ See J. Neyman, *Lectures and Conferences on Mathematical Statistics and Probability* (Washington: Graduate School, U.S. Department of Agriculture, 1952), p. 210.

tributions of mathematical statisticians are sometimes applied; some applications are made in psychology, the social sciences, and genetics, in situations where *prima facie* cognitive interests are at stake. Since these procedures have obtained whatever theoretical backing they possess from behaviorist assumptions, critical cognitivists have to acknowledge that in many cases procedures used in science for reaching conclusions have no rationale that they can find acceptable.

Two alternatives are open to the confirmed cognitivist: either he must scrap much scientific research, on the grounds that the procedures used were illegitimate, or else he must provide an alternative rationale for these procedures, which is compatible with cognitivist commitments. On the assumption that the first choice is a desperate measure, the proper course is to show that a decision-theoretic approach to inductive inference that conforms to cognitivist requirements is feasible.

The idea behind this alternative is that although scientific inquiry has its own objectives, they are, after all, objectives. As in the case of practical deliberation, the attainment of ends involves the selection of one option from among alternatives. And, as in much practical deliberation, there is no guarantee that the option chosen will not fail. Thus, in certain respects, justifying reaching a conclusion via nondeductive inference is comparable to making a practical decision under conditions of uncertainty or risk. If this is so, then the general criteria for rational decision-making might be operative both in practical deliberation and in scientific inference.⁶ Yet, the standards for legitimate scientific inference will not be reducible *in toto* to decision-making criteria; for the distinctive commitments of the scientific inquirer will impose constraints upon him in addition to those that have already been imposed by the criteria for rational decision-making.

⁶ Thus, critical cognitivism is capable of preserving many of the insights of the Peirce-Dewey tradition. The account of the occasion for development and termination of inquiry presented by that tradition can be understood as a characterization of certain features that are shared in common by inquiries into the truth and deliberations for the purpose of practical decision. What critical cognitivism rejects is the much stronger claim that the conclusions of scientific inquiry are either decisions to act relative to practical goals or aids to such decision-making, whose value is to be assessed in terms of their success as "guides in life."

The approach being proposed here has also been suggested by Hempel, *op. cit.*, pp. 149-159.

If it proved possible to develop such a decision-theoretic yet cognitivist approach to scientific inference, a rationale for current statistical practice would be forthcoming.

Note should be made in passing that adoption of a decision-theoretic approach to scientific inference commits no hostages to the view that men choose their beliefs. Whether they do so or not, their beliefs are appropriately subject to criticism, which evaluates beliefs in the light of alternative opinions that might have been held. Such opinions are evaluated in turn with reference to the ends of inquiry. In other words, there is a sufficient analogy between choosing how to act and reaching conclusions, to warrant exploring the possibility of subjecting both action and belief to similar conditions of rationality.

8] *The Aim of This Book*

These considerations, which are based on reflection on decision-making under uncertainty and risk as well as on recent developments in statistical theory, point to the desirability of initial efforts to construct an account of scientific inference that is frankly decision-theoretic while still remaining faithful to a cognitivist point of view. Removal of the threat of local skepticism that is implicit in behavioralism and better understanding of the relations between rational belief and rational action are not, however, the only dividends to be reaped from a successful attempt to achieve such a program. Scientific inquiry is often alleged to have ends whose generic traits control the criteria for fixing belief scientifically. This claim remains a barren commonplace, without philosophical fruit, unless a specification of these ends is accompanied by criteria for picking the best options by which to attain given ends on given evidence. To be informed that simplicity, informativeness, falsifiability, etc. are desiderata is of little help, even when these notions are precisely explicated, if a conception of the way in which these desiderata control the selection of a "best" conclusion is lacking. Similarly, the relevance of truth and probability (in its various statistical and inductive interpretations) to inductive inference stands in need of clarification. A decision-theoretic approach to inductive inference offers a promise of remedying these deficiencies.

Tentative and speculative first steps are taken in these directions in the following discussion. No pretense is made that the proposals to be introduced are immune against revision—even radical revision. They should be taken as constituting a first approximation to an adequate account of scientific inference sufficiently plausible to warrant further inquiry along the lines of the program being suggested.