

Against the Fact – Value Dichotomy

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"Is" and "Ought"

One of the singular aspects of the logical Positivist conception of scientific theory was the associated doctrine of the fact-value or empirical-normative dichotomy used in classifying non-analytic statements. Strictly speaking, the positivist account of scientific theory need not be interpreted in terms of commitment to such a dichotomy. The model of theory implies rather, a broader distinction between cognitively meaningful and meaningless statements. Cognitively meaningless statements are those without an interpretation in terms of the direct objects of sense experience; while within the category of meaningless statements are those of ethics, metaphysics, and aesthetics as well as any other statements without the required observational reference.

The specific fact-value distinction arises in connection with the above view because of the historical importance of conflicts between science and religion and because of the influence of earlier philosophers in distinguishing factual statements from ethical judgments. There are, in any event, three essential supports for the fact-value dichotomy itself: a) the "logical gulf between "is" and "ought" statements, b) the seemingly obvious syntactical differences between the two types of sentences, and c) the semantical distinction between the two types of sentences which

establishes according to the positivist view, the cognitive meaningfulness of value sentences.

Syntactics, and the "Logical 'Gulf'"

In philosophy (a) and (b) have come under a fierce attack, an attack which in our opinion is successful enough to deprive the fact-value dichotomy of these two means of support. In fact, the critique of these logical bases for the fact-value distinction implies that (b) can undoubtedly never be rehabilitated as a means of making the distinction, while (a), far from allowing a distinction between fact and value, can itself only be saved if we first have semantical criteria for distinguishing "is" statements from "ought" statements. This shifts the whole burden of the distinction to (c), which if it could be established might in conjunction with (a) save the fact-value dichotomy. As we shall see however, (c) is untenable in light of some philosophical arguments, which suggest, in turn, a greatly revised view of the role of description and evaluation in scientific theory.

I will consider first some of the problems involved in making a logical distinction between fact and value, and thereafter will take up the problems besetting attempts to make the distinction on the basis of semantics. Not surprisingly, I will find that some of the difficulties of a semantical account arise because of the shift in philosophy from logical positivism/empiricism and its characteristic view of theory as a set of statements ultimately reducible to terms of observation to a more complex conception of scientific theory as a linguistic network of interrelated knowledge claims that is thoroughly theory-impregnated.

The attempt to distinguish factual from value statements is perhaps on weakest ground in the area of unvarnished syntactics. It seems initially plausible to distinguish fact statements from value statements on the basis of the presence

of existence predicates such as "is", in the former and normative predicates such as "ought", "should", "right" etc. in the latter. But ordinary language provides no basis for this distinction (Cf. White, 1956, Ch. XIII). If we say "the fact is that prejudice is morally wrong," we are not offering a statement that can be classified as either descriptive or normative *merely on the basis of syntax*.

The situation is even more acute in the case of means-ends "ought" statements such as: "I ought to fight the Nazis." Statements of this type do not mention "is" at all. If analyzed in a more complete context of intentionality however they may prove to be prescriptions of action which are instrumentalities for some higher ends (for example, in the case of fighting the Nazis some set of humanitarian ideals), and such instrumental ought statements, of course, are clearly both descriptive of a means seen as effective for accomplishing the higher ends in question, and conditionally evaluative, given the acceptance of those higher ends.

So syntactics will not serve to make the required fact-value distinction. And since this is so, it should make us quite suspicious of the presumed logical "gulf" between "is" and "ought", since this doctrine is based on a syntactical observation of David Hume. Thus, we cannot, it is said, from the premise:

(1) Apathy is widespread among the citizens of mass democracy;

deduce

(2) Apathy ought to be widespread among the citizens of mass democracy.

The logical gulf between the two statements derives from the syntactical point that since ought does not occur in (1) we cannot transmit ought, as seems required from (1) to (2).

No doubt this argument from Hume is correct and in an instance such as the above (of what White, 1956, p. 241 calls an "ought" statement and its "corresponding" "is" statement) we cannot deduce an "ought" from an "is". However there are many other contexts in which the gulf doctrine simply breaks down. There is, for example, the case of an "ought" sentence and its associated" (White, 1956, p. 241) "is" sentence:

- (1) Apathy ought to be widespread among the citizens of mass democracy.
- (2) Apathy is something that ought to be widespread among the citizens of mass democracy,

Or, there are the two paradigms of inference from descriptive to normative given by Max Black (1964):

- (1) alienation causes loss of identity
- (2) Therefore if nothing that causes loss of identity ought exist, alienation ought not exist.

and

- (1) We want to achieve higher political integration in Italy
- (2) strengthening the public school system is the one and only way to achieve such integration;
- (3) Everybody should do anything which is the one and only way to achieve anything he wants to achieve; and therefore,
- (4) we should strengthen the Italian public school system.

Initially, this last seems not to be a counter-example to the gulf doctrine. But if we take (3) as an analytic statement, true by virtue of definition of the term 'want' (see von Wright's (1971) account of practical inference) then since analytic statements do not affect the truth of arguments, (3) can be deleted from the premises without invalidating the conclusion. Thus, in Black's example a "should" sentence, (4), does seem to follow from "is" sentences, (1) and (2).

There are still other examples of the bridging of the logical gulf. The well-known “ought-implies-can” argument may be put (CF. Morscher, 1972):

- (1) If p is an obligation then p must be possible to do; which implies
- (2) If p is not possible to do then p ought not be done.

While there is some question as to whether (2) is valid since some do not accept that the negation of a normative sentence is normative, it is clear that on the basis of syntactical considerations alone, this is a counterexample to the gulf doctrine since "ought" appears in the conclusion while only the descriptive 'obligation' appears in the premise.

A final example is (CF. Morscher, 1972):

- (1) X knows that p.
- (2) therefore p

But this has the same form as:

- (3) X knows that he ought to do p.
- (4) therefore, he ought to do p.

Again, it may at first seem that this does not bridge the gap between "is" and "ought". But if we consider that we can define “p” as equivalent to “he ought to do p,” then it is clear that the last two arguments have the same form and that the inference is indeed from a descriptive statement to a normative one.

Semantics and the Network Conception

Now an obvious objection to examples such as the above is that they make use

of "logical tricks" which confuse an issue that seems intuitively clear. That is, in all our counter-examples the inference from "is" to "ought" depends on the existence in the premises of "mixed" predicates that upon an analysis of their "meanings" do turn out to have "ought" components.

This, however, is just the point that the above examples are intended to illustrate. The doctrine of the logical gulf between "is" and "ought" statements appears to depend on ***prior clarification of the semantics*** of "is", and "ought" statements. If we had such a clarification we could unambiguously characterize the premises in the above examples as descriptive statements, normative statements, or mixed predicates. In the absence of semantic criteria for distinction the logical gulf doctrine is vague and is subject to an endless barrage of counter-examples, the validity of which are subject to our intuitive feelings about linguistic meaning.

As soon as we shift our attempt to find a "hard" basis for the distinction between descriptive and normative statements to the realm of semantics, however, we run head on into the difficulties of semantical analysis discovered in the course of development of the network conception of scientific theory.

Consider Quine's (1960, 1961 2nd edition, 1966, 1969) results on synonymy, analyticity, definition, and linguistic translation. To accept these is to exclude the possibility of semantical analysis of the meaning of terms or statements in isolation from their theoretical context. To decide whether a statement is descriptive or normative we must, at least, examine the criteria of reference for the terms in the statement as well as the linguistic relations the statement bears to others in the theory. Again, "meaning" in such a construal of theory is a dual function of the linguistic context of a term or statement and of the chosen reference categories of the theoretical language. This means that if we wish to make the distinction between "is and ought," "descriptive and normative", "fact and value", we have to relativize the distinction to our theoretical networks and

to say that we are engaging in descriptive or normative theorizing depending upon network, rather than sentence or term characteristics.

But before we conclude that the "fact - value" distinction can be made even at this abstract level we must examine some implications of an attempt to move to the network conception in order to save the dichotomy. First, consider Black's (1964) **second example** of the breakdown of the gulf doctrine. One of the implications of the network notion of theory is our freedom to decide which statements in our theories we will consider analytic and which synthetic. Clearly, in the context of Black's example, this means that our theory will deny the fact-value distinction if we decide with Black to make (3) analytic, while it will uphold it, if we choose to say that (3) is non-analytic. But this in turn, means that our decision to constitute a theory in a particular way can determine whether, in particular contexts, the fact-value distinction is to hold or not. In cases like this then, the distinction appears not to be a function of meaning or semantics in the ordinary sense. Rather, the network view implies that the tenability of the distinction is merely a consequence of our determination to maintain it.

Moreover, this point about the primarily decisional nature, apart from semantics, of the fact - value distinction does not emerge only from examples of the type just discussed. That the point is of much more general relevance can be made clear through reference to an exchange that took place some years ago between Carnap (1950), and Quine (1951).

It was Carnap's contention that the ontology of theories is specified by a set of pre-theoretical decisions to adopt certain existence statements, for example, "there are physical objects." Such statements, and the decisions to adopt them, are justified on pragmatic grounds (by implication including our values, since the effect of our interests on problem selection clearly affects ontology CF. Rudner, 1953, pp. 5-6). They are, further, to be distinguished from other

existence statements, for example, "there are black swans," which are not ontological, and are not established by pre-theoretical pragmatic decision.

The question of the pragmatic acceptance of ontological statements is a question external to the theoretical language being constructed. From a point of view internal to the language these statements are taken as necessarily true — as analytic. But the question of the truth of the class of non-ontological existence statements is a question internal to the theoretical language. And it is to be settled by observation. From the internal point of view these statements therefore are taken as contingent—or synthetic.

Quine attacked this formulation by noting its dependence on the analytic-synthetic distinction, his criticism of which (1961) is well-known. If we disallow the intrinsic analyticity of not only ontological statements but, as Quine suggests, of all propositions in our theoretical system, it is clear that all the statements of science whether "empirical," logical, mathematical or ontological are on an epistemological par in that they are all ultimately contingent. Without the analytic-synthetic distinction there can be no distinction between pre-theoretical, "external" questions of language, and "internal" questions of empirical truth. Rather, all statements of our scientific networks are open to pragmatic questioning about whether and in what manner they are to function in our theoretical system. Some may function as (more or less) "pinned down" (quasi-analytic) statements, and others as (more or less) expendable (synthetic) statements open to testing and refutation. But these differences among statements are differences in degree and not in kind. And the differences in degree, moreover, should be determined by how well our decisions about whether to treat a statement as "pinned down or as expendable, help us to organize experience.

This exchange between Carnap and Quine is relevant to the fact - value dichotomy in the following way. If we adhere to Carnap's position we can

exclude value considerations from an essential role in theoretical networks by claiming that the legitimacy of any value statements that may be assumed in the practical decision to select a problem as a subject for a scientific investigation is a pre-theoretical question, and that viewed from a standpoint internal to theory these statements are merely analytic givens, not relevant to scientific investigation. However, Quine's argument is, precisely, that there are no pre-theoretical questions in this sense, and no statements the legitimacy of which can simply be assumed without reference to the role of those statements in the network which organizes our experience.

The values that guide us to scientific problems therefore, are expressed in contingent statements that we can question. If we choose not to question such statements, or in more positivist terminology to consider them as analytic insofar as they underlie scientific endeavors, and as a special kind of cognitively meaningless synthetic statement distinct from factual statements, and therefore beyond the pale of questioning, insofar as they purport to assert knowledge; then we must recognize that this characterization is a result of a decision on our part to uphold the fact-value dichotomy.

Moreover, once again this decision is not one that can be made on some a priori ground that fact statements are somehow different from value statements. For our previous arguments have shown that, at best, the distinction is one that must be relativized to theoretical networks. Hence, if we are to uphold the dichotomy we must do so on the kind of systematic grounds (the "goodness of fit" of the network to experience) to which Quine refers. That is, we must show that the fact-value distinction performs a useful role in our efforts to organize experience through systematic theory, or to put this in more Popperian terms, we must show that the distinction performs a useful role in contributing to the growth of knowledge.

The Fact-Value Dichotomy as Decision

What then is the perceived role of the fact-value distinction in science? And if we can state its role adequately will this statement provide a basis for making the clear distinctions between descriptive and normative theoretical networks that we seem to require?

The sort of answer to these questions given by positivists seems familiar enough. In giving an explication of scientific theory we want, above all, to make clear the intersubjective nature of scientific investigation. We want to distinguish science as an inter-subjective investigative method from other methods which do not employ norms whose purpose it is to guarantee intersubjectivity. The modern temper, on the other hand, views the area of value as a subjective realm, and views value theory as non-testable relative to experience. It follows that normative statements must be considered as established by pre-theoretical pragmatic considerations if, on this view, science is to function at all. And it follows also that pre-scientific synthetic value statements must be considered as entirely distinct from synthetic factual statements.

But why is the realm of value a subjective realm? And to again state our original problem how are we to distinguish the evaluational and subjective from the factual and objective? We have seen that we cannot make this distinction on syntactic grounds, nor in view of the network conception of theory on the grounds of an intensional semantics which provides us with a criterion for distinguishing factual statements from value statements.

In the network context, the positivist answer to this question must therefore be something like the following. It is not really important for us to make syntactical or intensional semantic distinctions between fact and value, and we grant that these distinctions are impossible to make within the network perspective

anyway. Further it is not even necessary or important for us to exclude from science sentences with terms like "ought" and "should"¹ in them. What is necessary is for us to ignore the linguistic components of sentences that assert or imply the "goodness" or the "oughtness" or the "shouldness" of states of the world in the intrinsic value sense of these terms. For the reference of these components of language is not to states of the world or to relationships among the components of a language but rather to the subjective states of our minds, to the way we want the world to be; indeed to our emotional response to the world.

The fact - value distinction thus, is, in this view, not, strictly, a distinction between kinds of sentences, or kinds of theoretical networks in that it points to two sets of markings on a page and asserts that one set of markings is factual or descriptive or scientific theory, and the other value or ethical theory. Rather, the distinction is one between the linguistic functions of description and evaluation. It says of a particular language thus, that it has two kinds of significance: a) cognitive significance in that it describes the way the world is and b) emotive significance in that it states or implies what we would like the world to be like. To exclude the subjective realm of value from science, therefore (and this is the answer to the question of how we distinguish descriptive from normative), all we need to do is consider the emotive significance of our scientific networks as given, or not a subject for scientific investigation, or not, finally, entering into the determination of the truth or falsity of the descriptions of the world which the network also provides us.

And so, a partisan of the fact - value dichotomy might conclude, in spite of the difficulty of making syntactic and intensional semantic distinctions between fact and value, there is really no problem, from the point of view of the network conception of theory, in making the distinction between fact and value. In sum, we implement the distinction by merely ignoring the emotive aspects of our theory; or if you prefer another way of putting this, by taking these aspects as

given. Moreover, the distinction is justified by the systematic consideration that, given the subjective character of emotive significance, to allow an interpretation of scientific theory in terms of it would simply render intersubjective scientific investigation, that is, science itself, impossible. To organize experience at all, or, if you like, to provide an opportunity for the growth of knowledge, we must exclude, therefore, the emotive significance of theory from the realm of scientific investigation.

Have we, finally found a basis for the fact - value distinction which will work within the context of the network view of theory? We must note, first, the dependence of this justification upon the identification of emotive significance, or the emotive aspects of our judgments about values, with subjectivity in investigation and hence with the impossibility of an intersubjective science of values. We do not think however, that this identification is tenable and, further, we think that it occurs primarily because those who wish to maintain the fact-value dichotomy believe one or more of the following three things:

- a) emotive judgments about the legitimacy or illegitimacy of "ought" statements, or more simply, about "right" and "wrong" are an unreliable basis for intersubjective investigation;
- b) An acceptable theory (analogous to a theory of truth) of legitimate as opposed to illegitimate value claims is unavailable for value theory; and
- c) An acceptable theory of evaluation and refutation of value claims as legitimate or illegitimate is also unavailable.

I will show elsewhere that these objections to the possibility of value science are mistaken. Here, however, I would like to begin my reply to the proponents of the fact - value distinction by pointing to the damage the distinction does relative to the scientific aims of organizing our experience, and providing for the growth of knowledge.

Against the Fact – Value Dichotomy: Relativism

The first argument against the fact - value distinction and its attribution of subjectivity to the valuational aspects of statements is simply that it deprives us of the possibility of organizing our non-cognitive emotive experience along the dimension of valuational validity—invalidity. It forces us to view the categories of our theories from the perspective of what "is" or 'what is possible or probable'⁵ alone and excludes the perspective of "what ought to be brought about."

This is not a trivial argument even though it merely states the fact - value dichotomy itself. For on the practical level it means that intrinsic moral judgments are beyond scientific criticism, that we are free to consider such intrinsic judgments as personal matters that are no one's business but our own, and, in general to espouse that moral relativism which so paralyzed the intellectuals of Europe in the aftermath of World War I, and during the rise of Hitler.

It is a fool's game to attribute all the problems of modern society to the relativism inherent in the positivist dichotomy of fact and value. But I have noted elsewhere the directionless character of modern society, and the potential role of social science interpreted as Ideology in solving the problem of social intelligence. A clear disadvantage of a conception of science that recognizes the fact - value dichotomy and its related implication of subjectivity is that the Ideological role of social science I sketched there would be sharply limited. Nor can we deny the possibility that the exclusion of values from science may have contributed to the societal drift that is the characteristic condition of many modern societies.

The standard reply to this and the countless similar complaints leveled at the logical positivist conception of values is to say that the relativistic implications of science for ethics are unfortunate, but they are not something that one can do anything about because they are simply a consequence of the inherent subjectivity of value investigation. The choice is either one of allowing subjectivity in science or of isolating the valuational cancer in the quarantine of cognitive meaninglessness.

But this reply only holds if the standard objections to the possibility of value science stand. If we can develop a strong argument against these objections later, then the all too plain social consequences of the decision to "pin down" value statements in our scientific networks constitutes a strong argument for stopping this practice and hence for abandoning the fact-value dichotomy.

Against the Fact-Value Dichotomy; Choosing Problems, Categories and Hypotheses

Another way in which the decision to maintain the fact - value dichotomy affects science is that it allows essentially subjective influences upon the growth of knowledge. The argument here may be put quite simply. We take it that scientific investigation is initially motivated by valuational considerations and that there is wide agreement on this point. Proponents of the fact - value distinction think that they can insulate science against these values by considering them pre-scientific and by taking them as the given background of scientific investigation. But this background clearly affects the choice of scientific problems for investigation. In turn, the conceptual categories in terms of which the scientific problem is couched as well as some sub-set of the full range of possible hypotheses utilizing such categories must find their way into the scientific theory used in investigation, for otherwise the theory would simply be irrelevant to the problem which initially motivated it.

But if some of the hypotheses and categories utilized by theory to provide a description of the world are derived from scientific problems and the problems from valuational backgrounds, then clearly these backgrounds affect the subject matter of the description of the world a theory provides even if we cannot say, in a radically subjectivist vein, that they affect the specific form of the description given by theory. And further, if the subject matter of theory is affected, in part, by our values, then it also follows that the growth of knowledge is so effected. Knowledge grows through a process of competition among scientific theories. If the subject

matter of these theories is affected by our values, then the growth of knowledge will also be affected by these values. More specifically values will indirectly determine the direction in which our knowledge grows in that they will select those subject matters (problem areas, categories, hypotheses) in terms of which our knowledge is expanding.

That our values affect the growth of knowledge is, on this view, unavoidable. Why have I said then, that it is the fact - value dichotomy that allows essentially subjective influences on such growth when it seems that I am claiming that regardless of the dichotomy, values inevitably affect it? My answer is that values are not necessarily subjective influences on problem selection and theory construction. Whether they are or not depends upon how we decide to treat the valuational background. If we take the logical positivist view that this background is given to us, that it is non-questionable from the standpoint of objectivist methods for the acquisition of knowledge, then the consequence of our decision to handle values in this way is that we will be allowing putative value knowledge, uncontrolled by an objectivist method, to affect the way in which scientific knowledge is to grow. This is the sense in which it is true to say that we are allowing an essentially subjectivist influence to affect science.

If on the other hand we decide to consider the valuational background as contingent, and if we can formulate a method of test the valuational legitimacy of our scientific theories, then we will escape from value subjectivity and from the arbitrary influences of it upon our scientific knowledge. In sum, I am suggesting here that values, through the mediation of scientific problems, affect the formulation of conceptual categories and hence hypotheses in theories. This influence is unavoidable because it depends on the relevance connections which must obtain among values, problems, concepts, hypotheses and scientific theories. Given the necessity of these connections the decision to maintain the fact - value dichotomy only results in importing subjectivity into our scientific theories. The only way to remove this subjectivity is to face squarely the need for developing the science of values and for eliminating the fact - value dichotomy.

Against the Fact-Value Dichotomy: Testing Hypotheses

My third argument against the fact - value distinction relates to the problem of selecting among hypotheses. I claim the distinction leads to subjectivity in hypothesis testing and selection. My argument develops out of the position taken by Rudner (1953), Churchman (1948) and Braithewaite (1953), and defended and, in my opinion, strengthened by Leach (1968). A summary of Rudner's argument as offered by Leach is (p. 96):

- "(1) 'the scientist as scientist accepts or rejects hypotheses,'
- (2) 'No scientific hypothesis is ever completely verified.'
- (3) Therefore, 'the scientist must make the decision that the evidence is sufficiently strong or that the probability is sufficiently high to warrant the acceptance of the hypothesis.'
- (4) The 'decision regarding the evidence and respecting how strong is "strong enough" is going to be a function of the importance in the typically ethical sense, of making a mistake in accepting or rejecting the hypothesis.'
- (5) Therefore 'the scientist as scientist does make value judgments.'"

In explicating this argument, Leach distinguishes three versions of the value neutrality (V-N) thesis in science. First, the proponent of V-N may deny (1) and claim that in his role as a scientist, the scientist makes no decisions about the truth or falsity of beliefs. He merely "assigns degrees of confirmation or probability to hypotheses so that others, pragmatic decision makers, can decide the truth or falsity of the beliefs" (p. 94).

This is the "odds-maker" view of the scientist. It is immediately untenable on most classical versions of empiricism, on Popper's view of philosophy of science, and most especially on any view which at least sees the scientist as offering scientific explanations, or as engaging in future tests of retained theories. To engage in

either or both of these activities tentative acceptance of hypotheses is necessary and (1) therefore seems clearly valid.

A second version of the V-N thesis is the "truth-seeker". On this account the role of the scientist is to appraise hypotheses or theories in terms of the probability of their truth. (3) which is entailed by (1) and (2) (the principle of fallibilism) denies the truth-seeker view. For, as Leach puts it (p. 98):

"Even in the pursuit of truth one must assess or attach utilities to the degree true answers are desirable and mistakes undesirable. To weigh all mistakes equally comes to refusing to accept an hypothesis as warranted until all the evidence is both in and affirmative."

Of course, the last is an impossible condition, and the characterization of the scientific role of "truth-seeker" is therefore also invalidated.

So the question becomes: what kind of utilities enter into the role of scientific decision-maker? A third, and least demanding, but also strongest version of the V-N thesis (CF. Hempel, 1962, 1966), answers that the values underlying scientific decision-making are epistemic utilities relating to pure scientific research. These have nothing to do with the utilities which might attach to research applications. Such epistemic utilities, which supplement the weight of the evidence as decision criteria of hypothesis acceptance or refutation are derivative of the methodological norms of science, and specifically of the "logic" of selection. They are norms such as empirical fit, simplicity, systematic fruitfulness, projectibility, and others.

Rudner's argument, however, purports to show that even this, cognitive decision-maker, version of V-N is untenable. And since (1) - (3) above seem acceptable and (5) follows from (1) - (4), it seems that the success of the argument against V-N hinges on the acceptability of (4).

Isaac Levi, in two articles (1961, 1962), attacks Rudner's position by claiming that the move from (3) to (4) is enthymematic, and by further claiming that the entailment of (4) requires (Leach 1968, p. 100, Levi, 1961, p. 348):

“(6) 'To choose to accept a hypothesis H as true (or to believe that H is true) is equivalent to choosing to act on the basis of H relative to some specific objective p.'

(7) 'The degree of confirmation a hypothesis H must have before one is warranted in choosing to act on the basis of H relative to an objective P is a function of the seriousness of the error relative to P resulting from basing the action on the wrong hypothesis.'”

Levi accepts (7) because it appears to follow from research in statistical decision theory, and indeed such research was also the source of Rudner's (4). Levi, however, does not accept (6) because it expresses a behaviorist view equating belief and action, and he therefore concludes that since (4) is not entailed by (3) and (7) alone, but (3), (6) and (7), the cognitive decision-maker view of the scientific role, and with it the V-N thesis, is saved.

Leach replies to Levi's criticisms of the Rudner-Churchman-Braithewaite position by countering (6). In particular he considers (6) as an unnecessarily strong assumption that need not be made for the entailment of (4), and he therefore rejects Levi's attribution of (6) to Rudner's argument. In place of (6) he suggests (p. 101):

"(6') to choose to accept a hypothesis H as true implies as a contingently necessary condition the disposition to act on the basis of H relative to some specific objective P.

(6'') To be disposed to act on the basis of H relative to some specific objective P implies as a contingently necessary condition acting on the

basis of H relative to P in the appropriate circumstances C.”

Now, Leach makes clear that he intends (6') and (6'') as relations of material implication which would hold for a cognitivist, but not non-behaviorist, psychology, rather than as expressing "identity, deductive entailment or even material equivalence." He also makes clear (p. 102) that the adequacy of (6') and (6'') turns on his view of the "conceptual meanings of 'belief' and 'action'." It follows therefore that he presents (6') and (6'') as analytic statements.

In this context then it is instructive to note the following. First (6') is clearly accepted as analytic by those philosophies such as Empiricism, Pragmatism and Hermeneutics that emphasize the interconnection of belief and action, that see man as purposive, that, specifically, see rational man, which certainly the scientist must be, as normatively, and therefore methodologically bound to take decisions that are rational at least in terms of his own goals and definition of the situation. It seems to me to have nothing to do with the present argument that it is certainly empirically possible for a scientist to accept a hypothesis H and yet have a disposition to act on a contradictory hypothesis H_1 that he knew to be false relative to some specific objective P. The point here is that no rational man, and therefore, no scientist, as this role is interpreted by any of the major schools of philosophy of science, should have a disposition contrary to (6'). We think this last is an important point, missed by Leach, which strengthens his argument.

A second point, by way of strengthening Leach's argument, relates to (6''). One way of interpreting that proposition is to say that it is an abbreviated statement of a practical inference syllogism as reconstructed by von Wright (1971). This means that the Hermeneutic view of the relationship of belief to action is committed to (6''). Since all other propositions in the Rudner argument seem consistent with Hermeneutics, it seems clear that this school of philosophy, at least, is mandated to accept the conclusion that the role of the scientist includes the making of value judgments. More generally, it also seems clear that all philosophies, whether they include themselves in the hermeneutic movement, or not, would, if they accept the

practical inference syllogism as a valid logical argument, be committed to (6''), and, since (6') expresses the same general outlook on rational man as purposive, to this proposition as well. But this means, since the other propositions in the argument do not seem problematic, that the conclusion on the entrance of value judgments into science would be mandated for these philosophers as well.

Thus, the Leach reconstruction of Rudner's argument proceeding from: (1) \rightarrow (2) \rightarrow (3) \rightarrow (6') \rightarrow (6'') \rightarrow (7) \rightarrow (4) \rightarrow (5); does seem to commit those philosophers, including empiricists, who would mandate rationality in the scientific role, to the abandonment of the fact - value dichotomy. But there is still a counter-argument which empiricist proponents of the dichotomy can make which remains to be answered.

The empiricist can accept the whole of the argument Leach outlined and still offer the following rebuttal designed to save the cognitive decision-maker version of V-N. The scientist in his role as a scientist is not, after all, equivalent to rational man. He is, indeed, an abstraction from rational man, a type of rational man from which the dimension of pragmatic action has been conceptually excluded. What remains to scientific man as a normative logical possibility is scientific action, conceived of as those actions which relate to the maximization of epistemic utilities, and it is epistemic utilities that shape the specific objectives P to which all scientific actions and all scientific dispositions to act are relative.

But even this final (we hope) move will not save the fact - value dichotomy. For, there are two objections which we can immediately make to this argument. The first, is, that it saves the dichotomy by completely divorcing hypothesis selection in "pure" and in applied science and that this has an unacceptable consequence.

Thus if our purpose in accepting a theory is to continue testing it, or to use it in a scientific explanation, or to further develop its conceptual apparatus, then the position implies that our acceptance of the theory will depend only upon epistemic utilities. But if our purpose in accepting it is to apply the theory in curing illness, or

in reconstructing constitutional governments, then our acceptance of it will, on this view depend on pragmatic as well as epistemic utilities.

So, the consequence of this view is that there are two forms of hypothesis selection: value-free pure science hypothesis selection, and value "impregnated" applied science hypothesis selection. But from a societal point of view which sort of hypothesis selection is more important to bring under the control of methodological rules? Which sort of philosophy of science is it more important to develop? And over time which sort of hypothesis selection methodology and related philosophy of science is likely to survive as an important endeavor?

The point is, of course, that the cognitive decision-maker response to Leach's argument in distinguishing pure from applied science induction restricts the task of devising methodological rules for inductive inference to pure science, and, by implication, abandons the problem of devising such rules for applied science. To accept such a philosophy is to threaten, in the long term, the very existence of science, because it denies the social responsibility of science and leaves it to chance as to whether the development of pure science will be helpful, harmful, or merely irrelevant to society.

And this brings us to the second objection to this possible counter-move to Leach's argument. It is, in fact, as the above external critique of the counter-move suggests, impossible to entirely divorce, even in a normative logical context, epistemic from pragmatic utility. Let us assume that (with Maxwell's, 1972 well-taken reconstruction of Popper's account, 1959, 1962) the goals of science are to:

- (1) get closer and closer to the truth; and
- (2) continually increase the explanatory power of our theories.

On this assumption, or we think, on any other assumption giving content to the epistemic utility view of science, a necessary means to fulfillment of these goals will be a choice among alternative theories based on continual tests of those theories.

Now, in terms of the above goals, (or any other set of purely epistemic goals), we will be mandated by the cognitive decision-maker view to select those empirical tests of our theories which will be most instrumental in distinguishing among them relative to truth (or in a broader view than the present example any other epistemic utility which can be affected by our tests), regardless of the consequences for society of our performing these tests. Now I trust that there will be certain areas of science in which the tests most instrumental for testing a particular theory will be very harmful to Society. And normally, scientists take a practical view with respect to situations like this (or perhaps society does) and forego (or Society refuses to allow) dangerous tests. But in doing so, they violate the epistemic utility conception of science. If they really acted in accordance with this view, the result would be (or it often is now when they take value judgments which set problems for granted) that their tests would greatly harm, and in certain areas could conceivably, destroy society. But, clearly, to destroy, or seriously harm society, is to act inconsistently with the continuing search for truth and explanatory power, and, more generally, any other epistemic goals we may have. For to maximize such goals we must maximize scientific activity in accordance with them, and scientific activity is clearly empirically, if not logically, dependent on the support society gives it.

I am saying, in other terms, that one cannot define the scientific role, or any other social role for that matter, in isolation from a broader system of social roles. The role concept itself, while it implies some autonomy for the cluster of activities which define a role, also implies some interdependence with other roles which constitute a social system. The question of whether a normative explication of the scientific decision-making role is valid, therefore, depends not merely on traditional philosophic considerations, but depends also on the sociological one of how the role concept is to be defined. A normative explication of the scientific role can be invalidated if it contradicts sociological considerations about what roles are possible for scientists to perform. This is not to say that philosophers need be committed to any particular current representative of sociological role theory. It is to say however, that, once again, the sociological concept of role does imply some degree of interdependence among the roles constituting a society. And this, in turn, means

that a normative explication of the scientific role which considered it logically possible for the scientist qua scientist to divorce herself entirely from her other roles is a contradiction in terms.

It might be replied at this point that philosophers need not accept even the highly generalized requirements of the sociological role concept just advanced and therefore that the philosopher's conception of role is simply entirely distinct from the sociologist's and also consistent with the assumption of complete role independence. But clearly, this reply is simply mistaken. Either the philosopher means (very roughly) by role a cluster of socially defined activities systemically related to other such clusters of activity, in which case he must give up the fact-value distinction on logical grounds or he means by role a cluster of socially defined activities unrelated to other such clusters.

If he means the second, then waiving the conceptual contradiction engendered by "socially defined" and yet "unrelated," he must still justify the fact - value distinction on empirical grounds by showing that not only is the scientific role entirely logically independent of other roles, but also that its independence is empirically possible to attain. For if he could not show such empirical independence, then the fact - value dichotomy is still open to the objection that, as far as we know, this distinction is impossible of attainment and therefore a methodological norm which commends it to us can have no binding force, since "ought" implies "can."

Of course, our argument on the interdependence of science and society vis-a-vis the possible harmful consequences of theory testing on society and the possible feedback effect from society to the conditions for theory-testing which must shape the growth of scientific knowledge, is an argument about the empirical impossibility of fulfillment of a scientific role defined without reference to pragmatic utilities. A consequence of that position is that we take present-day science as hiding behind the fact - value distinction and abdicating its responsibility to rationally evaluate theories in terms of pragmatic utilities. But, again, it is not possible on the above arguments to decide questions of theory acceptance or refutation without reference

to pragmatic utilities. Thus, the key questions are a) how are those utilities introduced into the scientific investigation process now, and b) what is the impact on scientific goals like the pursuit of truth (the growth of knowledge) and explanatory power (organizing our experience) of the present manner of introducing pragmatic utilities into the scientific decision process.

Our answer to a) is that such utilities are introduced by scientists sub-rosa and are determined by their personalities and by their interaction with the institutional context in which they work. It would be beside the point to illustrate this here with some obvious examples of the relation between scientific judgments and pragmatic utilities as empiricists have never denied the presence but only the necessity, and normative desirability, of values in science.

The question of impact on scientific goals of the sub-rosa introduction of values into science deserves a study in the sociology of science which is beyond us here. But we need only refer to the general practice in the social sciences of accepting hypotheses as theoretically significant on the basis of tests of their statistical significance in order to document the general point that the sub-rosa introduction of values, which, on the above account is a necessary consequence of the fact-value distinction, results in a) the biased evaluation of scientific theory or b) the introduction of uncontrolled subjectivity into Science. The way to handle bias is to develop a decision rule for evaluation of theories in accordance with the rational reconstruction of scientific decision making just given. The way to handle subjectivity is to break the fact - value dichotomy and to develop a value science which would render pragmatic utilities systematically determinate.

We must still make a final point about this third argument against the fact - value distinction. On first blush the argument may seem to imply the doctrine that values or pragmatic utilities ought to determine whether or not we accept a hypothesis. Such a doctrine would be rightly rejected as one which reduces existential science to either normative theory or to subjectivity (depending upon how one feels about the possibility of value science). ***We cannot emphasize strongly enough that***

the Rudner - Churchman - Braithewaite - position does not imply such a reduction.

Classical Empiricism insisted that the acceptance of a scientific hypothesis depend only on the weight of the empirical evidence in its favor. The classical empiricist thus adhered to the truth-seeker version of V-N. Contemporary empiricists in adopting the cognitive decision-maker view of V-N add to weight of the evidence, other criteria for theory choice such as simplicity, systematic fruitfulness, projectibility and so on. In doing this they are not asserting that the original empiricist criterion of weight of the empirical evidence is completely or even substantially discounted in the decision to accept a theory. Instead the claims of other criteria are also recognized.

In rejecting the fact - value dichotomy the Rudner argument is not that criteria of epistemic utility are not important in scientific decision-making or that pragmatic utilities should be controlling. It is merely that pragmatic utilities should be taken into account in a system of independent weights, no one of which is necessary or sufficient in a decision to accept or reject a theory. In most instances of theory or hypothesis selection, the pragmatic utilities in such a normative decision-making scheme would not weigh heavily enough to outweigh epistemic utility. But we must recognize there are propositions where the pragmatic utilities will be controlling. Is this bias or reduction to normative theory?

If the pragmatic weights are determined by scientifically tested value theory, bias will not be involved. As for reduction of descriptive to normative or value theory, we must recognize that this is what occurs in the case of our acceptance of methodological propositions as analytic, for example, in defining the goals of science. That is, for example, if we accept truth-seeking as normative we must also accept that to find the truth (even if we cannot be certain, as fallibilism assumes, that we have found it) is empirically possible. But since, unless we make this assumption, we cannot even begin systematic inquiry, its acceptance is an instance of pragmatic decision criteria outweighing all others.

Conclusion

The fact - value distinction, in sum, is normatively unacceptable because it:

- a) deprives us of the possibility of organizing our non-cognitive emotive experience through value-science;
- b) leads to the introduction of unsystematic and therefore subjective value judgments in the process of formulating the conceptual categories of theories which in turn distorts the growth of scientific knowledge; and
- c) leads to the introduction of subjective value judgments in the hypothesis selection process which, in turn, also distorts the growth of knowledge.

We have no alternative then, if the above arguments hold, to an attempt to systematize our evaluative knowledge. The problem of scientific methodology for the study of values is therefore a critical one. But it is one I must take up on another occasion.

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Press, 1956.

Note:

This paper was extracted from an unpublished book-length ms. entitled *The Adaptive Crisis and the Foundations of Social Science*. Most of it was drafted in 1973, and it undoubtedly needs considerable updating in light of work done in this area since. Recently, Hilary Putnam has had a good deal to say about the collapse of the fact - value dichotomy in a book of essays whose title essay is on that very subject. I'll probably be analyzing his book carefully in the coming months to see if his perspectives on the issues are new. His book leaves the impression that the dichotomy has only recently "collapsed." But I think the dichotomy has been under severe attack for 50 years, and its durability in the face of criticism signifies how important it is to the ideas of scientific objectivity and methodological naturalism and how reluctant people are to give up the idea. Even though the development in this paper is more than 30 years old, I think it still may provide a good framework for evaluating the issues related to the dichotomy.