ON FORMAL AIDS TO SCIENTIFIC DECISION MAKING

Thesis for the Degree of M. A.

MICHIGAN STATE UNIVERSITY

John M. Vickers

1959

ON FORMAL AIDS TO SCIENTIFIC DECISION MAKING

By

John M. Vickers

A THESIS

Submitted to the College of Science and Arts of Michigan State University of Agriculture and Applied Science in partial fulfillment of the requirements for the degree of

MASTER OF ARTS

Department of Philosophy

1959

ACKNOWLEDGEMENT

I should like to thank Professors Henry S. Leonard and Lewis K. Zerby who read a first draft and suggested many improvements. I must acknowledge the continued stimulation and aid of Professor Richard Rudner; any merit the thesis might have is surely a tribute to his skill and patience as a teacher. He is not, of course, to be held accountable for its shortcomings.

J. M. V.

CONTENTS

CHAPTER I

INTRODUCTION; THE TASK OF THE THESIS

1.1 This is not a thesis in the history of philosophy. That is to say, the conclusions of the thesis are not intended to be such as will trace the causal historical development of various philosophical problems and attempts at solutions of those problems. There is a sense, of course, in which any attempt at philosophical discourse involves itself with the problems of the history of philosophy. That sense, however, is so obvious as to merit no more than recognition here. Except for such involvement as this ineluctable sort the present work does not explicitly concern itself with questions usually treated by historians of philosophy.

The problems with which this thesis does concern itself are, perhaps, novel as regards philosophy. Many of these problems -- at least in their present form -- have not historically concerned philosophers, though they have been explicitly dealt with by other disciplines for various purposes and with various results. Because of this comparative novelty, it is to be expected that the thesis would be considered by some not to be a philosophical thesis at all, but rather a step-child properly belonging to any of several other disciplines. In the light of this possibility --

a real one, I fear -- it would seem that some argument should be advanced for the philosophical relevance of the thesis. I should hope that the work can argue for itself well enough, as regards the soundness of its conclusions; but I think it not to be expected that it argue for its being philosophical.

It would, however, seem reasonable to expect that the author of the thesis present some argument for this philosophical relevance. One should expect to find, somewhere in the finished work, a body of discourse which persuaded that what was being said was philosophical. No such discourse is included, unless the quite sketchy remarks of the last chapter along these lines be taken as persuasive, and I fear that the antipathetic reader will find little persuasion here. In short, if you do not think that the problem itself is philosophically relevant, there is probably nothing within the thesis which would incline you to change your attitude. You will go on thinking that my topic of concern is not a philosophical one.

Since the attempt at justification is abandoned a priori, perhaps some sort of a causal explanation of the author's attitudes will be of assistance to the reader who wonders just how anyone could consider such a thesis at all philosophical. Since, furthermore, we explicitly claim dispensation from any responsibility as regards historical worth, what we say of the history of philosophy may be taken as indicative of the author's philosophical concerns rather than as historical information.

With this in mind we refer to some developments in contemporary philosophical thought. We each -- to some extent -- choose our philosophical ancestry, and the author's choice of ancestry should become evident in the following brief historical comments.

Twentieth century philosophy has produced several seemingly disparate but actually intimately related endeavors. Some of these have enjoyed salutary success, others have fared less well. I should think that one endeavor which has been -- and continues to be -- largely successful is the formation of the pragmatic tradition in philosophy. The work of Peirce, James, Schiller and Dewey comes to full bloom in the pragmatic philosophy of C. I. Lewis. Lewis' value theory, in turn, lays the foundation for naturalist epistemology and value theory. The notion of value as *felt* value, of value as being inseparable from experience, and of knowledge as being functionally inseparable from purposes, relate themselves so as to form a coherent philosophical position; one which -- to the author -- is quite persuasive.

A second important and likewise largely successful endeavor of twentieth century philosophy centered about the publication of *Principia Mathematica*, by Russell and

Whitehead. The new logic -- first conceived in Boole's work of the nineteenth century -- was brought to maturity, exhibiting importance and relevance really heretofore undreamed of. Principia was a good indication of the genuine efficacy of the formal sciences; the unity of mathematics and logic was established, and the obligation of philosophy to attend to the new logic became obvious.

Closely allied to the advances of Principia was the work of Von Neumann in game theory.[1] With advances largely attendant to the publication of The Theory of Games in 1944, formal techniques found fertile new fields for application. Conceptually rigorous modes of conceiving conflict of interest in widely varied situations were established, and psychologists, economists, mathematicians, and even a few philosophers became excited about decision theory, utility theory, and the comprehensive schema of game theory. Interest has blossomed in this direction -- questions of decision and utility theories have shown themselves involved with topics in all areas of the social, physical and formal sciences.

---

[1]Although The Theory of Games and Economic Behavior (Princeton, 1944) is usually referred to as the definitive publication of von Neumann on Game Theory, his first publication, "Zur Theorie der Gesellschaftsspiele" (Mathematische Annalen, 100, 1928) contained the conceptual essentials of the later presentation.

A very important development of the century was the rise and -- if I may say so -- the fall of logical positivism. Related, no doubt, to the publication of Principia, the efforts of the Vienna Circle to clarify philosophical problems gradually culminated in a purge which threatened to legislate traditional philosophy into meaninglessness. Pragmatism learned from the adventures of positivism, being warned of pitfalls and encouraged to the exploration of new areas of relevance. Principia Mathematica provided the strong skeletal framework of an efficacious formal apparatus for the expression of positivist doctrine, and the remarkable advances of science provided a strong set of arguments for the positivistic bifurcation of knowledge -- the factual and the formal.

So many of the problems of this century have centered about the positivistic adventure -- its birth in the troubled reactions to Hegelian and British idealistic philosophy and the need to account for the importance of scientific knowledge, its rise with the appreciation of the importance and viability of a linguistic approach to philosophical questions; and its eventual demise at the hands of its own techniques and practitioners[2] -- that the drama of

---

[2]It is noteworthy that the most telling arguments against the positivistic theses were those of the positivists and their followers. I should think that one of the most admirable facets of the positivist movement is that it showed itself to be largely self-corrective.

positivism becomes, in large part, the drama of twentieth century philosophy. Because of this central position of positivism in the development of the philosophies of mid-century, and, therefore, in the formation of the author's heritage, we commence the thesis with a brief sketch of one tenet of positivistic thought. This tenet, it is felt, is a vital one; upon its success or failure hinged the success or failure of the positivistic constructive endeavor. In our examination of this theme we hope to show how positivism as it became more and more sophisticated finally faced the inadequacies in its own structure. In the resolution of these inadequacies the advances of the century become integrated, and analytic philosophy progresses toward maturity.

This thesis is intended to be a tentative step in the direction of that maturity. It is an attempt to utilize some of the formal tools which have become so excitingly effective and to make that utilization in self-conscious awareness of pragmatic developments in value theory and epistemology. The tenor of the thesis is pragmatic, insofar as pragmatism recognizes that human values depend upon human purposes. It is empirical where empiricism requires that philosophy be aware of human experience; and it is formal in that it makes free use of formal techniques.

2.1 Whitehead once remarked that every school of philosophy has two major exponents; the originator of the abstractive scheme which is the mark of the school, and a final exponent who universalizes the scheme. Philosophical positions are usually originated with the elucidation of specific problems in mind, it is at the hands of the consummator of the scheme that it is stretched to fit the parts of philosophy for which it was not intended: The result is a *reductio ad absurdum*, so to speak, which renders the scheme complete, makes its failings patent, and sometimes makes evident the total inapplicability of the scheme to problems of philosophy.

Rudolf Carnap has certainly performed the function of concluding exponent for positivism. It was at the hands of Carnap that the detailed linguistic bias of positivism was extended into the areas of value theory and ontology; the extension into value theory came with the emotivist movement in ethics of which Carnap -- if not the Sophists -- was the foremost early propounder; the extension into ontology will interest us in the next several sections where we shall discuss Carnap's efforts to preserve a united front to varying problems. The inadequacies of positivism became evident in these attempts at universalization of what had been a seemingly tenable epistemological thesis; we now praise positivism and the early positivists more for the tenor of their efforts than for their specific conclusions.

It is the ontological extension of positivism which first interests us here. The early positivists had maintained that all cognitively meaningful discourse was composed of factually (scientifically) determinate or logically determinate statements. Any expression, the truth value of which could not be ascertained by scientific or logico-mathematical means was declared to be at best emotively meaningful. Such statements conveyed information about no more than the speaker's 'attitudes', and said nothing 'objective' whatever. As a result of this legislation, all cognitive enterprises were either scientific or logico-mathematical. Science investigated the truth status of synthetic statements, and logic and mathematics investigated the truth claims of analytic statements.

2.2 This bifurcation was at the heart of the positivistic thesis. Given any statement, one was supposed to be able to assign it to the proper area for investigation. The business of philosophy was, for the most part, properly to assign statements to mathematics and the physical sciences for investigation. As positivism gained more momentum, dependence upon the tenability of the analytic/synthetic distinction became more and more evident. Attempts to formulate adequate meaning criteria all postulated the dichotomy, and the attacks on traditional metaphysics and value theory were largely implementations of it. Such an important axiom merits serious scrutiny, and analytic philosophers commenced an exhaustive examination. The efforts were largely directed towards making the distinction clear; attempts were made to define 'analytic' and 'synthetic' through extensional logical techniques; These attempts culminated in an article by Quine in 1951[3], which constituted an indictment of the distinction on the grounds that

> ...for all its a priori reasonableness, a boundary between analytic and synthetic statements simply has not been drawn. That there is such a distinction to be drawn at all is an unempirical dogma of empiricists, a metaphysical article of faith.[4]

Quine, in this article, examines the possibility of founding the distinction in a notion of synonymy which, he shows, is at least as unclear as the distinction to be justified. He then examines semantical rules as possibly justifying the distinction, and concludes that this foundation too is unsteady. It is Quine's thesis that the status of many statements as 'L-true' depends in large part upon quite arbitrary conventions. Statements which are L true in one language might very well not be so in another language. He maintains -- on this basis -- that the supposed dichotomy is more of a continuum and, as such, inadequate as a basis of division of knowledge.

---

[3]Williard V. Quine, "Two Dogmas of Empiricism", Philosophical Review, No. 60 (1951), 20-43.

[4]Ibid., p. 37.

2.3 Carnap's reply to this challenge was not an attempt to reinstate the dichotomy, it was rather the formulation of a new dichotomy which had its basis in another facet of language than the semantic dimension. The reply was phrased in an important article, Empiricism, Semantics and Ontology[5].

When Quine pointed out -- in Two Dogmas of Empiricism -- that statements which are L-true in one language might very well not be so in another language, and that the question of what was logically determinate depended in large part upon what language included the consideratum, Carnap was forced to consider at least some of the questions as cognitive which he had formerly relegated to the realm of the non-cognitive. In order to make this consideration possible and not relinquish all of his positivistic tenets he constructed a new realm of discourse. What had formerly been known as cognitively meaningful would henceforth be known as theoretical discourse. Those non-theoretical questions which were admissible -- under the new aegis -- to serious discourse, were to be known as practical questions. As specifically relevant to ontology, questions of existence which could be considered within a

---

[5]Rudolf Carnap, "Empiricism, Semantics and Ontology" (Revue Internationale de Philosophie, XI (1950), 20-40.

given language framework were to be considered internal theoretical questions; questions about the existence of entities, say, in the universe of discourse of a given language framework, or existential questions about the subject entities of the language which were not answerable within the language, were called external practical questions. To ask if unicorns exist is to ask a question which is answerable by empirical scientific means -- one looks about the world for evidence before replying. To ask, on the other hand, if physical objects exist is to ask a question which is not so answerable -- it is a practical external question. To ask if there are prime numbers greater than 100 is to ask a question, says Carnap, which is logically determinate as to reply, while to ask if numbers exist is to ask another practical external question.

This distinction has an initial intuitive plausibility to it, but upon further examination we see that the difference is not at all as clear as it would at first seem to be. To say that:

1. There are prime numbers greater than 100.

is L-true is to say that its truth is ascertainable by reference to the semantical rules of some language. In this case by reference to the semantical rules of formalized number theory. But the semantical rules of formalized number theory would never advise us of the truth of (1). To be sure, from the axioms of number theory (1) may be inferred, but then we have shown no more than that

2. If there are numbers (i.e. if the Peano axioms are satisfied) then there are prime numbers greater than 100.

is L-true, and the L-truth of (1) depends upon the L-truth of

3. There are numbers.

which is precisely the external practical statement the truth of which is not theoretically determinable.

This being the case, we can see another chance to make the distinction: One might say that 'Are there numbers?' is a <u>prior</u> question to 'Are there prime numbers greater than 100?' in the sense that the L-truth of (1) is inferrable from the L-truth of (3). That is, if (3) were L-true, then (1) would be L-true. To be sure, this is nothing like the iron clad distinction originally claimed, but let us nod provisionally to it and grant Carnap the priority (in the sense outlined) of (3) to (1); similarly the priority of

4. Physical objects exist.

to

5. Unicorns exist.

Carnap thus maintains that the question of the existence of numbers is not one which is asked or answered by mathematics or mathematicians. It is a question, he says, which has historically been a peculiarly philosophical question. It is a question which has been asked and answered differently by realists and idealists throughout the centuries. And, Carnap explains, the realists and idealists have been confused as to what sort of question they were asking. The question is not a factual question, it is a question of neither logic nor science; this is indicated by the lack of agreement as to what would constitute evidence in support of a reply. The question, according to Carnap, is actually a question of the utility, of the adequacy, of the aptitude of a given language framework relative to our purposes in communicating about a given universe of discourse. One might say that it is a question as to whether or not we decide to discuss the world in such a way that the universe of discourse in question becomes a category of our language. When Carnap says that numbers exist, he means that the language of number theory results in a categorization which is largely congruent with his purposes of discourse. When he says that physical objects exist, he is saying that the thing-language affords an adequate categorization of the mass of percepta which confront us in common experience.

After outlining this foundation of the external-internal dichotomy, Carnap goes on to alleviate the sting of the old positivist legislation: Although only internal questions can have direct cognitive meaning, external questions may be indirectly cognitively meaningful; they are pragmatic questions -- questions of the utility of an entire language framework. His conclusions from this are not unexpected:

> ...the decisive question is not the alleged ontological question of the existence of abstract entities, but rather the question whether the use of abstract linguistic forms, or, in technical terms, the use of variables beyond those of things or phenomenal data, is expedient and fruitful for the purposes for which semantical analyses are made, viz. the analysis, interpretation, clarification, or construction of languages of communication, particularly languages of science.[6]

and then in more comprehensive criticism of Quine's position;

> The (nominalistic) critics will have to show that it is possible to construct a semantical method which avoids all references to abstract entities and achieves by simpler means essentially the same results as other methods.[7]

A large part of what Carnap accomplished was to prescribe a different usage for 'exists'. He attempts, for perhaps the last time, to sidestep the central ontological question; "What exists?", not by directly denying the meaningfulness of the question, but by assigning a new meaning to 'exists' and requiring that the question be answered as if it were "What are the universal categories of languages?" We might agree with much of Carnap's thesis, but as he

---

[6] Ibid., p. 39.

[7] Ibid., p. 40.

evades ontology with his semantical footwork, we still want to ask the same old question, and we find that this is precisely the question that cannot even be asked in Carnap's new vocabulary.

2.4 In a reply to Carnap's article[8] Quine rephrases Carnap's external/internal distinction as a category-sub-class distinction. He then points out[9] that Carnap accepts his standard for judging whether a given theory accepts given alleged entities, i.e.

> The test is whether the variables of quantification have to include those entities in their range in order to make the theory true.[10]

Questions of category, says Quine, are questions of what alleged entities are included in the range of the category variables (i.e. the variables of the broadest range) of a language. Questions of sub-class are questions of what particular entities of a given species or sub-species are included in the range of restricted or 'limited' variables of a language. Assuming this to be congruent with Carnap's treatment, Quine points out that there remains no definite

---

[8]Willard V. Quine, "On Carnap's Views on Ontology", Philosophical Studies, II (1951), 65-72.

[9]Ibid., p. 67.

[10]Ibid., p. 69.

distinction between category and sub-class questions. 'Are there prime numbers greater than 100?' is a sub-class question when asked internally to the theory of natural numbers; when asked, in other words, in number theory where 'n' stands for 'number'. It becomes a category question, however, when asked in a language in which the variable of broadest range is 'p', where 'p', stands for 'prime number greater than 100'. If we cannot tell from examination of a question alone whether it is a category or a subclass question, (ergo whether it is an external or an internal question) then questions are only internal or external relative to a given language framework. But this tells us no more than we already know from the rules of an ordinary quantified logic. When we introduce a limited variable into our language, we do so usually <u>because</u> of its categoricity relative to a given universe of discourse. This saves us the trouble of additional notation and simplifies our deductive procedures considerably. The number theorist has no need for a variable which includes anything in its range other than numbers.

It seems that Carnap's attempt to ignore questions of ontology is an abortive one. Though we can sympathize with the positivist's desire to avoid the tangled confusions which frequently accompany metaphysical discourse, we still maintain that he is not exempt from considering such questions if we choose to ask them. We can fairly require that positivism, shown to be possessed of a demon, must either go into quarantine or suffer the rites of exorcism. The legislative act by which traditional metaphysics and value theory were ruled meaningless is now invalidated on the grounds that the legislation made a false and relevant assumption.

3.1 We have noticed that in an attempt to extend the scope of positivism so as adequately to answer ontological questions, a basic flaw became evident in the positivist scheme. The early positivistic legislation became invalidated when it was shown that the analytic/synthetic distinction was inadequate as an exhaustive and exclusive characterization of meaningful discourse. As a result of this the indifference to metaphysical problems which had marked linguistic philosophy from the time of the Vienna Circle became no longer conscionable. It is not beside the point here to consider the effects of this revelation in another part of the philosophical universe -- to consider its effects in particular on theories of value.

As the early positivists were indifferent to questions of ontology, so also were they indifferent to questions of value except insofar as predications of value were indicative only of the speaker's attitudes. Such a value theoretical position might be characterized as <u>emotivism</u>.[11] Positivism and emotivism complemented each other quite well: A positivist could not but be an emotivist, and most emotivists were of an otherwise positivistic persuasion. Emotivism provided a completely non-cognitive esthetics and ethics, and positivism provided a completely non-emotive logic and epistemology. Because of this congeniality, the troubles of emotivism are the troubles of positivism and vice versa.

If Carnap's external/internal dichotomy is assumed to hold universally, then any expression must be either external or internal. Expressions of value are obviously not L-true, and if they were synthetic-internal they would be confirmable through the procedures of some empirical science, and this -- the emotivists assumed -- was not the case.[12] Thus all questions of value were external questions, and, as pointed out above, all external questions were questions of value. Science, according to this

---

[11] See, e.g., C. L. Stevenson, <u>Ethics and Language</u> (Yale, 1944) passim, but esp. chap. iv.

[12] Stevenson, for example, though maintaining that scientific knowledge is in some way contributive to ethical decisions, says "...the task of selecting from the stores of knowledge and bringing together the information that bears on a specific moral issue, is one to which scientists do not address themselves." Ibid, p. 331.

viewpoint, patrols only the precincts of the internal, theoretical, non-value decision.

The destruction of Carnap's dichotomy disturbs this peaceful picture of delegated authority: If any question can become external or internal by an extension or restriction of the range of variables, then value questions need not be external in nature -- unless a question changes from a value to a non-value question as the styles of the involved variables change. It would seem fairly obvious that to make such a demand is to play havoc with the way in which we use logics: When we change the range of a variable, we do it for convenience and ease of deduction. The question, too, of the 'neutrality' of logic should be considered. Whether we can have metaphysically and ethically neutral logics or not, it is surely desirable to avoid the complete entanglement which would come about if valuings were considered incident upon the ranges of variables. It would be somewhat awkward if one were forced to pause whenever he changed a variable's range and survey which predications he had made evaluative and which non-evaluative. The only possible support from the value-theoretical side of the issue would include the establishment of a thorough and exhaustive means/ends distinction, thus building the required categoricity into the value realm instead of the fact realm -- questions of ultimate ends to be external while questions of means to be internal. But this is surely unsatisfactory, and it is evident that such a dichotomy would run into at least the same difficulties _qua_ value distinction as it does _qua_ fact distinction.

3.2 If emotivism as an ethical position proves inadequate, we question its suppositions. Is it feasible to conceive of predications of value as indicating no more than the speaker's attitudes? Evidently not, since the assumed dichotomy between the cognitive and the non-cognitive is not at all clearly established. We must look for other ways of interpreting evaluative discourse and hence for a different set of ethical suppositions.

If one cannot satisfactorily account for values as arising only from attitudes, then he must account for them as issuing from some portion of what is undergone by the organism without prejudging that they arise from this or that portion of what is undergone to the exclusion of other portions. We are requiring an extension, a broadening of ethical concepts: A factor in this broadening is the discarding of the attitude/belief distinction and the resulting attention to an unbifurcated experience in an attempt to discover those facets of human experience which have most to do with values and valuings. The value theorist cannot ignore human purposes and the goal seeking character of human endeavors. He might find more or less obvious ways of describing these purposes, but he cannot ignore them in founding a system of ethics or esthetics. Such vague remarks cannot characterize a position, they can serve at most as a partial criterion of theories of value in general: Such theories as would be congruent with the criterion, I should call naturalistic.

But to give a name to a class of ethical systems is not to solve problems of value theory. It is not a part of the task of this thesis to formulate a theory of value, so we are to some extent free of the difficulties of formulation: It is however a part of the task of this thesis to make specific remarks about values of a restricted sort; so we are required at least to comment on what sorts of theories of value would be needed in order that our remarks be coherent with respect to some more comprehensive position.

Our reference to naturalism is intended as an indication of such theories. We construe naturalism, broadly, as that ethical position which requires that all values be related to felt value. It might be noted that this requirement rules out transcendental schemes of value which assume the origination of values in some extra-experiential source. In this ruling out, however, we are anxious not to involve ourselves in the solipsitic predicament of emotivist or extreme relativist ethics. We should also require that naturalistic value theory make possible empirically meaningful predications of value.[13] In this requirement is presented the problem of reconciling 'subjective' human experience with 'objective' discourse. And this problem, we feel, is not far disparate from the epistemological problems encountered in explicating knowledge of a less valuative sort.

When the problem is phrased in this way, the relations of science and values become obviously important. Naturalistic value theory must make provision for the utilization of scientific techniques and conclusions. This should lead to an attempt to provide a science of value; which science is not restricted a priori from any meaningful investigation. If we can agree that the scientific method -- a method perhaps characterized as well by intent as by procedure -- constitutes the most adequate available mode of prediction and can bring about accurate control, then we should be eager to admit investigation of a scientific nature as the ultimate justification for value theoretical conclusions.

---

[13] I am aware that such a requirement invites accusations of falling into the so-called 'naturalistic fallacy'. I am not at all sure, however, that this purported fallacy has ever been explicated clearly enough that one could tell just what he would be doing were he committing it. It would seem that a careful characterization of the naturalistic position allays any danger of inherent fallacy. See C. I. Lewis, An Analysis of Knowledge and Valuation (Chicago, 1945) pp. 406 ff.

It would be erroneous to interpret the preceding remarks as implying that the task of value theory __is__ the task of a science of value; to identify the two would be analogous to identifying metaphysics and science in general. To provide a foundation for a science of value is -- as here intended -- to become concerned with the efficacy rather than the efficiency of the science. It is surely the case that concern with efficacy cannot be divorced from concern with efficiency, but this is not to say that efficacy cannot be distinguished from efficiency. One might have a very efficient science of prediction and control which was only trivially efficacious: If the theories of the science in terms of its results have little relevance to the course of experience, then the science is unimportant. An efficient science still has need of justification beyond its efficiency, and this justification can only come in indication that the efficiency of the endeavor is relevant to and involved with experience. To show this relevancy is to argue for the efficacy of the science, and to neglect it is to encourage the pursuit of nebulous and trivial goals. What is efficacious must be efficient to some extent, and it is a part of the task of this thesis to show that at least some of what is efficient must be efficacious in order that it be efficient. But an index of efficiency is not indicative of efficacy, nor does efficacy belie any certain degree of efficiency.

Let it be said, then, that naturalistic value theory involves itself with founding a science of value. Another involvement of such theories of value is with decision. The pragmatic requirement that values must be felt values immediately requires that a theory of value provide some methods for choosing to feel one rather than another value. The recognition of the impossibility of unwavering pursuit of one ultimate goal of activity should carry with it recommendation as to how to direct activities through the manifold of values that are encountered, formed, accepted or rejected. To call x good is not very meaningful unless one decides to pursue x or to prefer x to y. It would seem equally obvious that decisions, or the manifestations of such preferences, require references to values in their justification. Insofar as the activity of science is decision activity, scientific procedure requires valuation and appraisings as well as more directly describable cognitive processes. If the scientist decides to accept a hypothesis, he is deciding, e.g., that the risk of error is not as great as the cost involved in further investigation.[14] It is at this point that our thesis commences its investigations: We shall attempt partially to analyze this valuational involvement of science, making some recognitions

---

[14]The whole development leading to this conclusion is an elaboration of the views expressed by Richard Rudner, "The Scientist qua Scientist Makes Value Judgments" (Philosophy of Science, vol. 20, no. 1, 1953)

of purposes and the experiential bases of values.

3.3 If science and value theory involve each other as outlined above, then a part of the task of value theory is to explicate the valuings of science. Insofar as these valuings remain tacit, the value decision of the individual scientist need not be congruent with the purposes of science, e.g. as an institution or even with the purposes of the scientist himself. To the extent that the scientist identifies his goals with the goals of science in general, then to that extent will he need to recognize these goals so as to make their joint pursuit feasible in the scientific endeavor. Some attempt must be made to decide if and how the scientific decision-situation differs from the non-scientific decision-situation: we all make decisions constantly, of varying importance and accuracy. Are there any features of the scientific decision situation which incline it to result in more or less adequate decisions? And, right on the heels of this question, What is an adequate decision?

Such questions raise a plethora of related questions and invite a comprehensive characterization of values and decisions. Such a digression would be a luxury which we cannot afford. We should like to establish more firmly the direction of our inquiry by the introduction of a decision paradigm and a schematic arrangement. This paradigm is intended to set the tenor of the thesis; it should point to the problems and the methods with which the remainder of the work will be concerned.

4.1 A woman goes to buy thread to sew a dress. Upon reaching the store she discovers that she has neglected to bring along a sample of the material of the dress with which to match the thread. It is a long trip back home, and she needs the thread as soon as possible. Upon examining the various colors of thread in the store she finds that there are several which approximate to the color of the dress. She must depend upon her memory of the color of the dress to advise her of the proper hue to choose. If she is right, if the dress and the thread are of exactly the same color, she will be rewarded by having the proper materials for her task, with the eventuation that the finished dress will not be marred by a clash of colors. If she should be unsuccessful in her choice to the degree that the dress would be severely marred by utilization of the thread, she is 'punished' by being forced to make another trip to the store -- with the resultant delay in the completion of the dress or by an unhappy eventuation of her labors. If she buys several spools of thread, her chances of obtaining a matching color are increased with every additional spool she purchases. If she buys a great quantity of spools, she can be 'practically certain' of obtaining a matching color.

Before our distraught shopper selects which and how many spools of thread to buy she weighs the consequences of the alternatives open to her: Consider that there is a quantitative index for any alternative purchase (of a number of spools of thread) which is formed as the product of (1) the probability that she will be successful with the given selection,[15] and (2) the 'rewards' of successful choice. Call this index the <u>utility</u> of the choice. We might then evaluate the utility for every possible selection. Some simplifications are in order: Let the woman specify one spool of thread which she feels most strongly is the proper spool, then as she increases the cardinality of selection sets, the 'proper spool' occupies the approximate midpoint of the color set represented by each selection set. As the cardinality of the selection set increases, the probability of mistake decreases, and the negative utility of the spending of the price of selection increases.[16] The increased certainty obtained by adding one spool to the selection set is smaller as the selection set is larger,

---

[15]For the purposes of this illustration 'probability' is to be taken to mean simply 'probability of occurrence with respect to available evidence'. It is felt that comments upon interpretative procedures with respect to the calculus of probability would be confusing and unnecessary at this early and explicative stage. These questions will be treated in some detail in chap. II, <u>infra.</u>

[16]For the purposes of the present illustration 'negative utility' is to be taken in its most obvious intuitive sense. Once again we refrain from refinements for reasons of clarity. The reader who is troubled by such assumptions may look ahead <u>infra.</u>, chap. III, pp. 94-96.

while the increment in the negative utility of loss of money which is brought about by the addition of one unit is constant.[17] The utility of the money loss may be represented by the number of spools purchased multipled by minus one. We assume that the probability of money loss is always 1. In this representation the probability of a mistake times the value of a mistake added to the probability of cost (always one) times the value of cost gives the utility of the selection. Or, where 'Pm_i' abbreviates 'Probability of mistake when set i is chosen'; 'Vm_i' abbreviates 'value of mistake'. And where 'Pc_i' is 'probability of cost' and 'Vc_i' is 'value of cost of set i', the utility of a set, i, is represented by

$$U(i) = (Pm_i \times Vm_i) + (Pc_i \times V\rho_i)$$

Since we have specified that the probability of cost is always 1, we may simplify as follows:

$$U(i) = (Pm_i)(Vm_i) - (N'i)$$

where "N'i" represents the number of items in i. Then the

---

[17] It is assumed here that the utility of money is linear with respect to money, i.e. that the numerical quantity of an amount of money is indexical of its value. We are aware that this is not the case, but recent investigations show it to be sufficiently approximate to truth to make the assumption innocuous in illustrations. See Donald Davidson and Patrick Suppes with Sidney Siegel, Decision Making (Stanford, 1957).

choice situation might be viewed as follows, with appropriate probabilities invented for the example.[18]

Let the value of mistake be -20, in units corresponding to the cost of a spool of thread. Then utility is maximized[19] by maximizing the function U (i), where

$$U\ (i) = -(Pm_i)\ (20)\ -(N'i)$$

The following table then represents the choice situation:

number of spools purchased	probability of mistake	utility of mistake	utility of selection
1	.64	- 12.8	- 13.8
2	.32	- 6.4	- 8.4
3	.16	- 3.2	- 6.2
4	.08	- 1.6	- 5.6
5	.04	- 0.8	- 5.8
6	.02	- 0.4	- 6.4
7	.01	- 0.2	- 7.2

5.1 The above schematized choice situation makes available information about several quite relevant facets of any situation in which a choice or decision is to be made. We see that there is a state of affairs which dictates what might be called the 'inherent logic' of the

---

[18]The probabilities are invented solely for illustrative purposes.

[19]Cf. note 16, *supra.*

situation:[20] The color of the dress is just what it is and it is going to be matched by the color of one of the spools of thread or it is not going to be so matched. The decision that the woman makes awaits its ultimate evaluation until the color of the dress is compared with the color of the thread selected. Notice, however, that the decision might be capable of appraisal in other respects than this ultimate one of weighing its consequences. We might very well make an evaluation which took into account (1) the purposes of the subject, (2) the information available to the subject, and (3) the probability on the basis of (2) that the chosen activity will bring about (1). If we were to order alternative decisions in any given situation using the value of (3) associated with each decision as its index (this would be analogous to evaluating selection sets in the paradigm according to the 'utility of mistake' associated with each selection) then for any pair of members of the array we could specify which of the pair was 'more likely'[21] to eventuate in the desideratum. This evaluation would be independent of the actual eventuation, and it would make

---

[20]This phrase is introduced by R. B. Braithwaite, The Theory of Games as a Tool for the Moral Philosopher (Cambridge, 1951).

[21]Actually the relation which would be established as ordering the field would be 'is not less likely to eventuate in the desideratum than'. For obvious gain in clarity we use a locution which suggests the stronger and, I think, not establishable relation.

perfectly good sense to say that, although the decision eventuated unhappily, it was still the best decision in the sense that it was most likely to bring about the desideratum.

The attempt to evaluate decisions in this manner, though, would surely end in confusion. Many of the arrays would be either incomplete or infinite, since, for almost any desideratum act and information set, there is some probability that the act will eventuate in the desideratum. Further, we can almost always specify some act such that it is more likely to bring about the desideratum than any act included in the array. Notice, in the paradigm, that as we increased the cardinality, we decreased the probability of mistake. It is obvious that the probability would continue to decrease with increases in the cardinality, and it could so be constructed that the probability would approach zero as a limit when the cardinality grew very large. In point of fact this is just the situation, and we are forced to consider alternatives which are less likely to bring about the desideratum than other alternatives as being more worthy than the latter alternatives.

The difficulty is alleviated when we consider probable effects of an act other than the desideratum and assign probabilities and values to these acts as well. Then, if for each effect there is assignable some function which indicates the desirability or value of that effect to the subject, we might conceive of categorizing eventuations in terms of the net values of their results. This, generally, is the program of theories of utility -- to design some format whereby eventuations can be assigned relevant and comparable indices of value. It is obvious that any attempt to evaluate decisions as regards their likelihood of achieving desiderata is going to have to consider some assignment of indices to contemplated eventuations of these decisions, where the contemplated eventuations will include some eventuations other than the desiderata. It is this recognition of the necessity of considering the undesirable effects of acts which bring about desirable effects that renders utility theory feasible; increase in information increases the probability of acting to bring about the most advantageous results.

One might be tempted, at this juncture, to say that a subject ought to do that act which will so eventuate: that, to refer to the paradigm, the woman ought to choose four spools of thread because by so doing she would maximize her utility. Some reflection, however, convinces us that use of evaluative language at such an early stage is prejudging the issue as regards the relevance of such formal schemata generally: Why ought she? She ought to only if she accepts that systematic notions of value, utility, probability, etc. are sufficiently isomorphic with her presystematic notions of decision situations to convince her that such systematic conclusions reveal her real wants. It is at this point that the persuasion tends to become viciously circular. "Why", we should say, "If you will grant that our definitions are justified, then you must accept our conclusions." And, of course, our argument for acceptance of the definitions is that by utilization of them we arrive at conclusions which are presystematically desired. What we are obviously talking around is the distinction between normative and descriptive discourse.

5.2 To offer a normative theory of decision making is to involve oneself in the circularity indicated above unless such a theory is accompanied with empirical evidence which indicates the relevance of the decisions and conclusions to actual choice situations. An adequate descriptive theory would doubtless form the best normative theory; it would provide evidence that certain courses of decision activity resulted in specifiable eventuations, it would supply us with data to enable us to forecase utility yields and other types of rewards, and in so doing would instruct us as to maximal utility choices. The instructions of such a theory would be proper to the extent that its axioms and definitions were indicated to be applicable. The point here is that any normative theory of decision making would be descriptive to the extent that its advice was well founded.[22]

To offer a descriptive theory of decision making would be to offer -- among other things -- a means for predicting how subjects would make decisions when faced with various choice situations. The argument for acceptance of such a theory would have to include empirical evidence that, at least, the axioms were satisfied in some cases. One interesting feature of such a theory is that it would not be drastically different from other theories as regards the sorts of observations which would be admissible as confirmatory of its hypotheses. Assumedly what psychology will and what it will not admit as evidential behavior on the part of subjects is not going to undergo important modification for the phrasing of a new theory. What will be different, however, is the set of theoretical higher level terms which would characterize such a theory. Such theoretical terms would function in Campbellian hypotheses, in statements having to do with the measurement techniques of the theory, and would generally perform the task of knitting the lower level observations into a coherent conceptual whole. If such a descriptive theory is to become possible, it will be possible insofar as there are meaningful theoretical terms

---

[22] A very interesting and largely successful attempt to demonstrate the applicability of decision theories is characterized by Davidson et. al., <u>op. cit</u>.

available which can perform this novel relational function among the established types of relational procedures. In the present thesis some argument is made for the selection of such terms as 'utility', 'importance', etc. as adequate theoretical terms in a descriptive theory of decision making. The conclusions of this thesis will be directly relevant only to the program of a descriptive theory of scientific decision, but the indirect relevance to any theory of decision making should become evident in the phrasing of the proposals.

The current thesis, since it does not support its conclusions with direct empirical evidence, cannot purport to be a theory of decision making. The point of the preceding remarks is that if there is ever to be an adequate theory of scientific decision making it will require some preliminary explication of concepts and theoretical terms so as to permit the introduction of the novel framework into scientific discourse. There are available empirical theories which utilize such terms as mentioned above, but to my knowledge they are almost all in economics or allied disciplines and concern themselves with monetary indices of value and utility. Such empirical endeavors are, indeed, invaluable. The salutary success in prediction and explanation resulting from utilization of these theories is quite sufficient evidence of their worth. What is being contemplated here, however, is the extension of these concepts of utility and value so as to include other than monetary indices within their scope. Some of the ensuing discourse will be concerned with empirical work that has dealt with monetary concepts of utility and value, because most of the empirical work in this area has been so concerned.

5.3 The goals of individual scientists probably include not only such desiderata as fame, fortune, personal satisfaction and conformity, but also such altruistic goals as the making available of knowledge and the increased possibility of control over the environment. The altruistic goals are more social in nature; many people agree that a particular scientist should strive to make more knowledge available, while relatively few feel that he -- the particular scientist -- should strive to increase his fame. I should speculate that most scientists would prefer to make the personal goals subservient to the social goals, though this is at present an untestable hypothesis. If, however, there were made available to the scientist some method for incorporating social goals into his procedures while diminishing the importance of personal goals, then the hypothesis would become immediately testable.

6.1 The hope that such a method become available in the near future in indeed quixotic, but this does not at all change the desirability of possessing a method of 'value inculcation'. If we realize that the scientific decision is a decision of value, then we want our values influencing the decision to be at least analyzable. Once the scientist becomes aware that his value schema has an appreciable effect on the success or failure of his inquiry, then, if he desires success of his inquiry, he will be eager to make this effect beneficial rather than deterrent. What is contemplated here is not some means of forcing the scientist to investigate what society wants him to investigate, rather it would be some means of permitting examination of the implications of the scientist's valuings and the resultant adjustment of decisions so as to conform consistently with the scientist's more encompassing values. That some modification in value schemata will be a result of this adjustment is of course true, but whatever a value schema might be it cannot be conceived to be static; our values change at least as frequently and as strongly as do our beliefs. It is very infrequently that we modify a 'big' or central value -- most modifications of value schemata occur with minor and unimportant values and then so as to make minor values accord with more important values. And this is remarkably like the situation with beliefs: Most changes in minor beliefs come about so as to permit the reconciliation of more central beliefs with the world of 'brute fact'. We very infrequently change our central beliefs.[23] The methods of science have traditionally made every provision to

---

[23] cf. Quine, <u>op. cit</u>. and Quine, <u>Methods of Logic</u>, (New York, 1959).

incorporate changes of belief in the structure of inquiry, while there is almost no consideration of valuational changes. So long as it was felt possible to conduct science without reference to value, this situation was justified; but if we come to the realization that beliefs and values are inextricably related, then we can no longer condone such indifference to factors which wield so much influence in such a vital direction.

The goals of this thesis include, for the most part, a discussion of the relevance of recent work in value theory and utility theory to the philosophy of science. More explicitly, the thesis attempts to outline an approach to problems of scientific decision making which would explicitly recognize the involvement of valuations in such decisions and the efficacy of various formal and quasi-formal techniques for analyzing this involvement. An attempt of this nature is almost sure to raise more problems than it solves, and this heritage of puzzles is perhaps what marks the thesis as philosophical rather than scientific; when the philosopher answers a question we call him a scientist, when he persists in raising uncomfortable problems he is being philosophical.

In this first chapter we have attempted to show the involvement of decision with valuation in such a way that no adequate characterization of scientific decisions could omit mention of values and valuings. If the reader is not by now convinced of the ineluctable presence of value in scientific decisions, then the sequel will be not at all persuasive. If he is at least disposed to accept this involvement, then it is hoped that what follows will provoke him to productive thought. If one is looking for solutions rather than problems, then he will be disappointed.

The second chapter consists of an outline of some contemporary developments in confirmation theory. The outline involves itself with some problems of probability theory and a tentative position as regards interpretations of probability is accepted, some arguments for this acceptance are put forth. A method of evaluation of evidence is proposed which depends upon acceptance of the outlined position as regards probability. Some attempt is made to show the relations of various problems in the philosophy of science one to the other, and to the problems of characterizing the evaluative element in scientific decision. There is discussion throughout the chapter of difficulties of measurement in general and of real-valued measurement in particular.

The third chapter utilizes the evaluative method proposed in the second chapter as a tool for ordering hypotheses according to their values. Some basic vocabulary and concepts of utility theory are introduced and the discussion of value is conducted explicitly in terms of these concepts. In this chapter the goal of the thesis is phrased specifically as acheivement of an ordering of outcomes in a delineated outcome space: After some discussion, of orderings and metricization of functions over the outcome space, such an ordering is proposed and briefly commented upon.

The fourth and final chapter of the thesis discusses some of the more significant shortcomings of the ordering proposed in the third chapter. A cursory summary of other work in the thesis area is presented, and some effort is made to comment upon the efficacy of these other approaches. The discussion initiated in the first chapter on the relation of decision and value is resumed in the vocabulary of the second and third chapters, and the conclusions of the thesis are briefly summarized and discussed.

CHAPTER II

"THE EVALUATION OF EVIDENCE; A RULE OF REJECTION"

1.1 It is possible to take the view that an individual makes a decision when he crystallizes his attitudes in such a way that he assumes himself to desire some goal. This crystallization of attitudes -- I think we are aware -- comes about when we rule that the goal seeking activities under consideration eventuate in a net good for us. Such a view involves a sense of 'decision' which indicates introspective personal activity on the part of the subject. It is ineptly described, because it is such a prevalent and broadly relevant facet of human experience that it defies precise characterization. The point to be made is that because of this ineffability, to use 'decision' in this sense is to render a formalization of decision making at least unfeasible. It would be all but impossible to give any intersubjective empirical meaning to this sense of 'decision': One feels that he has decided, and he feels it so strongly that no amount of evidence will dissuade him from his feeling once he has crystallized the relevant attitudes in the manner indicated above.

Because of this difficulty with personal decision (a difficulty characteristic of such subjective states) theories of decision making must usually content themselves with a primitive observable term such as 'exhibits decision behavior'.[1] The psychological task then becomes one of attempting to make the systematic meaning of the primitive correspond as fruitfully as possible with the presystematic meaning of 'makes a decision'. The psychologist must test his definition, and if his subjects exhibit symptoms (e.g. telling the psychologist that they did not make decisions under various circumstances) which convince him that the systematic term is violently non-isomorphic with the presystematic term, then he so alters his criteria for application of the systematic term that the extensions become more nearly isomorphic. This is not dissimilar to many other tasks of the scientific endeavor, there is a gradual adjustment of the systematic and presystematic meanings in such a manner that the extensions become as nearly isomorphic as possible without doing undue violence to either term.

In the case of decision making by scientists we shall make an oversimplification, which is justified in the light of the appreciable amount of clarity it permits, in assuming the adjustment mentioned above to be an easy one. It is surely the case that the scientist makes many kinds of decisions; he decides to be a scientist, he

---

[1]Cf. Davidson et. al., Op. Cit.

decides to investigate a certain field of phenomena, and so on. For our purposes, however, we shall consider only one sort of scientific decision; the decision to accept or reject a hypothesis on the basis of specified evidence. We shall see, later, that we cannot consider this decision at all adequately without honoring at least some of the more peripheral sorts of decision, since the hypothesis' acceptance or rejection depends in large part upon attitudes which are diminished and strengthened by the making of other decisions. This initial simplification, nevertheless, has the decided advantage that clear-cut criteria may be specified for application of the systematic term 'exhibits decision behavior' which seems intuitively suitable for application of the presystematic correlate 'decides'. An index that a subject X has accepted a hypothesis H is then X's use of H in prediction.

If the scientist's activity is characterized as behavior in which he accepts or rejects hypotheses, then his behavior is characterized as involving decisions -- decisions which are in part decisions of value: For when the scientist decides to accept H, he decides that the cost of acceptance is lower than the cost of rejection, or that the rewards of acceptance are higher than the rewards of rejection. The scientist can never be certain that a given hypothesis is true or false, but he can and does decide that a probability assignment to the hypothesis is adequate or inadequate. The task of this chapter and the next is to make explicit the valuational considerations involved in this decision: We shall commence by mentioning some important facets of valuation as directed toward scientific hypotheses.

1.2 The positive evaluation of a true and accepted hypothesis may be broadly conceived to be a function of its predictive power and as a negative or inverse function of its adverse effect on other hypotheses. Generally <u>true</u> and <u>accepted</u> hypotheses tend to disconfirm false theories and confirm true ones, while <u>false</u> and <u>accepted</u> hypotheses confirm and disconfirm inversely. A similar case may be made for true and false rejected hypotheses. Much of the discussion throughout chapter I was intended to indicate the need for explicit recognition of evaluational factors unavoidably involved in the acceptance and rejection of hypotheses. Any scheme which aspired to such recognition and which attended only to the function of hypotheses in direct prediction would surely not meet the needs of science; indeed, where hypotheses refer directly to states of nature, we have little or no difficulty in making the decision to accept or reject -- we can make it so easily that naive characterizations of science which attend only to this direct relationship of theory to fact (such as that characterization which was typical of the early logical positivists)[2] can readily persuade us that no complex evaluation is entering into the decision to accept or reject hypotheses. What is of interest to the value theoretician is the non-direct case -- the case, for example, of a Campbellian hypothesis or a law of measurement within a particular theory. When these sorts of apparently non-confirmable statements show themselves necessary for adequate science, the philosopher of science has no real alternative to discovering some way of giving empirical meaning to them. There have, of course, been many attempts to stretch the criteria of naive empiricism to provide for the meaningfulness of theoretical statements of the sorts mentioned above.[3] Indeed, the history of the philosophy of science for the past several decades is quite well characterized by reference to the succession of meaning criteria which attempted to legitimize Campbellian hypotheses and other theoretical statements. Whatever the results of this quest, one fact has become increasingly evident: Science is not

---

[2]See, for example, Rudolf Carnap "Logical Foundations of the Unity of Science", International Encyclopedia of Unified Science (Chicago, 1938) I, No. 1.

[3]See, for example Carnap, "Testability and Meaning", Philosophy of Science, (1936,1937) 3 and 4. C. G. Hempel, "Problems and Changes in the Empiricist Criterion of Meaning", Revue Internationore de Philosophie (1950), 4. And Craig, op. cit.

going to adapt itself to a naive-empirical view of confirmation and meaningfulness, theorists of empirical meaning must adapt themselves and their techniques to the procedures of verification which are utilized in science. The word 'adapt' is used here advisedly: The scientist is quite sure that he cannot conduct his investigations without using at least some positivistically undesirable statements in his theories; there has been quite some evidence to the effect that no substitute is available for this procedure. The philosopher of science must either declare his investigations irrelevant to the progress of science, or he must be prepared to advise the scientist of ways of handling the troublesome statements, instead of merely telling the scientist that his higher level hypotheses are not empirically meaningful.

The distinction between higher and lower level hypotheses might be characterized in terms of the cost of changing hypotheses. If a very low level statement in a theory (say a direct report of physical phenomena) is varied so as to report differently, the requirements of modification higher in the theory are minimal. Frequently no change at all will be required, since the generalizations of the theory will be probabilistic and not prone to disverification by one particular contrary instance. Significant changes, however, in higher level hypotheses are not so innocuous; the best of scientific theories display a cohesiveness which prohibits significant change of one higher level hypothesis without significant change in other higher level hypotheses or significant change in lower level statements. Analogously, the acceptance or rejection of a lower level statement does not require complex and important valuations, since the cost of error in these cases is small. The difficulty becomes evident and vital, however, in just those higher level statements which display very little direct relevance to evidence: Here the costs of error are great and the rewards of correctness equally great. What is further to be noted is that how the scientist evaluates has an effect proportional to the level of the generalization upon what he declares to be the case, or, upon what truth claims he makes. If the scientist had some means of estimating the value of higher level hypotheses, he would have a tool which would greatly facilitate the proper resolution of the question. The value being considered claims to be generically ineffable, refusing to betray itself by any meaningful index.

2.1 The history of criteria of meaning[4] suggests that to search for value indices of hypotheses as relevant only to the statement in question is to search in vain. Contemporary attempts at prescribing methods of confirmation consider the unit of meaning to be the theory within

---

[4] Cf. Hempel, "Changes", <u>op. cit.</u>

which the hypothesis is found in evaluation of meaningfulness of an entire theory. R. B. Braithwaite[5] describes the process of theory interpretation and evaluation as a 'zipping up' of the theory -- a process in which the lowest level observation statements of the theory determine the interpretation to be placed on the higher level hypotheses. Braithwaite's view might be slightly amended to maintain that although this is the procedure, essentially some adjustment on the meanings of the lower level terms is made by the interrelations of the higher level terms. In such a mutually adjusting manner is the whole theoretical structure given an interpretation in terms of a broad context of human experience.

This attention to the more widespread conception of scientific theories has marked -- it would seem -- most of the successful work done in contemporary confirmation theory. Nelson Goodman, in his stimulating book <u>Fact, Fiction, and Forecast</u>,[6] has outlined an approach to theoretical evaluation which explicitly involves itself not only with whole theories, but -- in effect -- with the whole history of science. Goodman's results are of sufficient relevance and importance for our task to merit a summary of his theory of projection:

---

[5] R. B. Braithwaite, <u>Scientific Explanation</u> (Cambridge, 1955), Chap. iii.

[6] (Harvard, 1955)

Goodman's concern with the problem of projection of inductive logic commences with a puzzlement about the analysis and meaningfullness of counterfactual conditionals.[7] He examines several attempts to explicate counterfactuals in terms of truth functional languages, and comes to the conclusion that the problem is -- indeed -- unreasonably difficult. He then succeeds in showing that the problem of counterfactuals could be resolved only if a larger and even more puzzling problem could be explicated; namely, the establishment of a criterion for distinguishing lawlike statements from non-lawlike statements.

In the second part of <u>Fact, Fiction and Forecast</u> Goodman faces another very closely related problem, that of explicating modality, particularly modality expressed by use of the word 'possible'. He remarks that for those who are satisfied with the notion of possibility as clear, there is no problem. He finds himself, however, unable to condone such acceptance and is convinced that the notion is one which requires philosophic clarification. It appears that his doubts are well founded, if only for heuristic reasons, for he suceeds in showing that modality is another of the puzzles which would be resolved were a clear criterion of

---

[7] Ibid., chap. i., pp. 39-44.

lawlike statements made available.[8] What happens, of course, is that the problem of law gains much more importance than it might at first have had. We cannot fail to be impressed: Even if we feel that subjunctives are ineliminable and that modal concepts are meaningful without truth functional explication, we must still remark sympathetically the importance for Goodman's philosophical position of this quite central problem. This sympathy entails -- it would seem -- a perhaps grudging admission that a solution to the now pregnant problem of law would be a quite persuasive argument for the viability of Goodman's whole approach to matters philosophical, or, at least, to matters scientific-philosophical.

The third part of Goodman's programmatic work is an examination of the problem of induction with an eye to reformulation, if such be permitted. He commences by remarking on the remarkable persistence and difficulty of the problem of induction; indicates that his own difficulty with law is immediately relevant to the traditional problem, and sets about his task by inquiring

---

[8] "Predicates supposedly pertaining to (possible entities) are seen to apply to actual things, but to have extensions related in peculiar ways to, and usually broader than, the extensions of certain manifest predicates. ...The problem of dispositions looks suspiciously like one of the philosopher's oldest friends and enemies. The problem of induction." Ibid., pp. 50f.

> ...what precisely would constitute the justification we seek. If the problem is to explain how we know that certain predictions will turn out to be correct, the sufficient answer is that we don't know any such thing. If the problem is to find some way of distinguishing antecedently between true and false predictions, we are asking for prevision rather than for philosophical explanation. Nor does it help matters much to say that we are merely trying to show that or why certain predictions are probable. Often it is said that while we cannot tell in advance whether a prediction concerning a given throw of a die is true, we can decide whether the prediction is a probable one. But if this means determining how the prediction is related to actual frequency distributions of future throws of the die, surely there is no way of knowing or proving this in advance. On the other hand, if the judgment that the prediction is probable has nothing to do with subsequent occurrences, then the question remains in what sense a probable prediction is any better justified than an improbable one.[9]

Goodman goes on to remark that what is involved is not the wholesale justification of all inductions, but rather a quest for criteria of evaluation of specific inductions. One justifies deductions by reference to rules, and one should expect that the justification of inductions would be by similar reference. The rules themselves are justified by reference to accepted inferential practice. The circularity is virtuous; no specific induction justifies itself, nor does the class of all inductions justify the

---

[9] Ibid. pp. 65f.

process. "A rule is amended if it yields an inference we are unwilling to accept; an inference is rejected if it violates a rule we are unwilling to amend."[10]

This characterization sets the theme for the remainder of the work; Goodman's concern is to characterize what rules have historically operated effectively. The problem is narrowed when put in Goodman's terminology: To make an inductive inference is to utilize a predicate, which applies and has applied to a certain class of things, in such a way that a claim is made that the predicate also applies to another class of things. This stretching of predication Goodman calls '<u>projection</u>' of the predicate. A predicate P is projected when the claim is made that some object which in fact does not manifestly exhibit P is 'P-able', or that under given conditions the object would exhibit the symptoms associated with the possession of P. If specific inductive problems may be characterized as cases of,"Ought P to be projected in this case?" Then Goodman's problem -- to establish criteria for inductive evaluation -- is phrased by asking, "In what sorts of cases ought predicates to be projected?" This, obviously, is a case itself of projection; namely of projection of the predicate 'projected': What things, in short, are projectible?

---

[10]Ibid., p. 67.

The reply to this question is all but obvious: Those predicates are projectible which have been successfully projected most frequently. Such predicates, says Goodman, are 'well entrenched', and hence deserving of scientific attention.

The proposed theory of projection then defines 'H1 is a better entrenched hypotheses than H2' as a function of 'P is a well entrenched predicate'. Very generally, those hypotheses which contain better entrenched predicates are better entrenched.[11] In conclusion it is maintained that a hypothesis should be projected if and only if it does not disagree with a better entrenched hypothesis which could also be projected.

What Goodman accomplishes in his theory of projection is to lay quite solid groundwork for a criterion of scientific interest; Scientists ought to be interested in well entrenched hypotheses, and -- in this sense -- well entrenched hypotheses are scientifically interesting. The importance of this accomplishment to the present inquiry lies in the fact that it permits us to consider the values of only interesting hypothesis and to assume that a criterion is -- if not available -- feasible for the distinction of such interesting hypotheses.

---

[11]The establishment of the function which determines degree of entrenchment of a hypothesis from the entrenchment of contained predicates is, of course, the task of the theory which Goodman outlines. See Ibid., chap. iv.

One of the very central points of Goodman's development is the phrasing of the generalization; "A rule is amended if it yields an inference we are unwilling to accept; an inference is rejected if it violates a rule we are unwilling to amend." The benevolent circularity characterized here is the circularity involved in all systematization -- we systematize because we are concerned with what it is we systematize about. We have a set of beliefs about some matter, and we want to examine the set with an eye to determining, for example, whether the set is consistent, whether it is cotenable with other belief sets, what new beliefs are implied by the set, and so on. We modify the systematization when it tells us that two beliefs are not cotenable and we believe very strongly that they are cotenable, we modify a belief when it disagrees with a systematization in which we believe strongly. The process is one of mutual adjustment between -- to coin a phrase -- method and content, if such an artificial division may be appealed to. The point, of course, is that the division is not a division at all.

3.1 One might very well characterize the philosophy of science as that branch of philosophy which is concerned with the problem of induction in some one of its many forms. There is a very strong precedent for the attempts of philosophers of science to justify induction, but only recently has pause been taken to ask what would constitute the sought after justification. In asking for clarification, it becomes evident that we would be quite content with a suitable set of criteria for confirmation. We would consider induction generally justified if we had some means of knowing when every specific induction was properly made.[12] The problem parallels the problem of the justification of deduction: we consider that deduction is a justified procedure because we have a set of criteria by which we can evaluate specific deductive inferences. If a deductive inference conforms to the rules of some accepted system of logic, then it is a good deduction. Similarly with inductive inference, if an inductive inference conforms to the accepted criteria, then it is a good induction. One does not pretend, of course, that the historical problem of induction is thus solved by exchanging one unclear term ('justification') for a set of only slightly more clear terms, ('confirmation', 'properly made induction', 'accepted criteria') but the intuitive clarity thus gained is not entirely specious: The exchange of terms presents more opportunities for explication, permits us, perhaps, more definitely to point out where the lack of clarity is most invidious.

---

[12] It is interesting to note that decision procedures for deductive calculi cannot fulfill this requirement. Perhaps it is too stringent to require such complete decision procedures for inductive calculi.

3.2 To describe the task as one of seeking criteria of confirmation is to say that what is sought is a set of rules which would establish to what degree a given body of evidence confirms a given hypothesis. The decision procedure, however, need not be one which discovers one value among an infinite set of degrees of confirmation: That would be analogous to requiring that decision procedures for a deductive calculus discover all its theorems. What is required is that, given an assignment of a value to the degree to which a given body of evidence confirms a hypothesis, the decision procedure gives us a means for deciding whether the value is authorized.[13] It is in this qualified sense that we can say that a set of rules (constituting a decision procedure) establishes degree of confirmation.[14]

In the sequel 'probability' is to be taken to mean 'degree of confirmation'. The symbol 'p(H,E)' is read 'degree of confirmation of H by the evidence E'. Those who are uneasy with such an interpretation of the calculus of probability can be assured that no prohibition of other

---

[13]This decree of confirmation is abbreviated by Carnap as 'c(h,e)'. See Rudolf Carnap, Logical Foundations of Probability, (Chicago, 1950), passim. We shall use 'p(H,E)' throughout to mean 'degree of confirmation of the hypothesis H on the basis of the evidence E.'

[14]Ibid., pp. 198ff.

interpretations (e.g., limiting frequency probability) is intended. The interpretation as degree of confirmation is chosen herein for the following reasons:

1. Provision of an interpretation which permits the assignment of all real values, $0 \leq p(H,E) \leq 1$ to $p(H,E)$.

2. Provision of an interpretation which explicitly recognizes the relevance of available evidence to determination of a value for $p(H,E)$.

3. Provision of an interpretation which establishes (in the sense outlined above) a value of $p(H,E)$ for every H and E.[15]

The property described in (1), above, is sufficiently important in the sequel that some explicit comment is required on it: It is, to be sure, an assumption of the scheme of analysis proposed in this and the next chapter that degree of confirmation is a real valued function. One might object that this assumption is unwarranted, that no proof is available for adequate demonstration of (1). Whether this is or is not the case is not the concern of the present work. What is the concern of the present work regarding this real-valuedness may be summarized in two statements: (a) There is evidence that the requirement for proof

---

[15]This third requirement renders frequentist probability inept for our purposes. Frequentist assignments are to classes of hypotheses, and we require assignment to each pair, (H,E). Cf., e.g., Hans Reichenbach, "The Logical Foundations of the Concept of Probability" in Feigl and Brodbeck, <u>op.cit</u>., pp. 456-474.

needs clarification before an adequate proof is forthcoming, and (b) None of the transformations on degree of confirmation effected in the sequel impugn this supposed real valued feature of the function. In supposing degree of confirmation to be real valued we preserve whatever real valuedness might be possessed by functions of it.

As regards the need for proof indicated in (a): It has been shown that

> (a') If p(H,E), p(H',E') are two real numbers such that
> p(H,E) > p(H',E'), then if R is an adequate comparative notion of probability, R orders p(H,E), p(H',E') so that p(H,E) is more probable than p(H',E').[16]

This statement is necessarily vague, since it so much depends on what we accept as an 'adequate comparative notion' of probability. This vagueness points out just the difficulty involved in asking for the sort of proof seemingly required by (1). If we were confident of the efficacy of some comparative notion of probability, then we could evaluate degree of confirmation relative to that notion, by a procedure similar to that alluded to in (a'). If, on the other hand, we were confident of the efficacy of real valued degree of confirmation, then we could evaluate our more intuitive notions of comparative probability in a like manner. As it stands, the sorry shape of our convictions

---

[16] L. J. Savage, <u>Foundations of Statistics</u>, New York, chap. iii.

prohibits either procedure, and we must look for enlightenment on both fronts at once. The search is facilitated by some such requirement of comparison as (a') to at least some extent. Savage purports to show -- with a considerable degree of success -- that his <u>personal probability</u> is an adequate comparative notion,[17] and he further indicates that it can be the foundation for a metricized real valued probability.

The problems which surround interpretation of probability are fascinating, but -- perhaps regrettably -- they need not concern us any longer here. On the basis of our two statements, (a) and (b), we shall assume ourselves authorized to treat degree of confirmation as real valued, with the recognition that demonstration to the contrary would require corresponding modification of our thesis.

3.3 It was said above that the task of confirmation theory was in part to establish criteria which in turn establish to what degree a given body of evidence confirms a given hypothesis. Probability, under this interpretation, is a function which assigns a real value to each pair (H,E) where H is some hypothesis and E is a set of evidence statements.

---

[17] Ibid., p. 32.

Thus, suppose E is construed as a set of statements:

$$E = \{s_1, s_2, s_3, \ldots, s_n\}$$

then a genuine problem immediately arises. One must specify a criterion for inclusion in E by which evidential statements may be evaluated. Given a statement, how are we to decide if it should be included in E? Carnap's very detailed proposals are adequately defined only for very meager languages, and then with the somewhat dubitable device of state descriptions.[18] What about confirmation in richer languages, and for evidential statements not originating in a state description?

The problem may be viewed as one of deciding which evidence is relevant to a given hypothesis. The answer in Carnap's terms is that <u>all</u> evidence is relevant to any hypothesis.[19] We might agree with this only to point out that stubborn adherence to such a position would prohibit utilization of the calculus until an infinite evidence set was accumulated. In view of the difficulty of attaining such a set, we should like to declare Carnap's proposal

---

[18]My reasons for calling state descriptions "Somewhat dubitable" are based, in part, on the treatments in Nelson Goodman "On Infirmities of Confirmation Theory", <u>Philosophy and Phenomenological Research</u> (1947) No. 8. And J. G. Kemeny with Paul Oppenheim, "Degree of Factual Support", <u>Philosophy of Science</u>, (1952) 19; 307-324.

[19]See Carnap, <u>Foundations</u>, pp. 164f.

unusable, and to maintain -- in the same breath -- that some evidence was surely **more** relevant than other evidence. What is needed, granting the worth of our comment, is some criterion for establishing the degree of relevance of evidence. Our proposal is to rule to exclude evidence which does not make a significant difference in degree of confirmation. Evidence is to be excluded, in other words, if it does not change $p(H,E)$ by some amount greater than or equal to φp. Where φp is an 'interval of accuracy' so to speak, of the confirmation function.[20] The question is, to rephrase again, which s_i are to be utilized in confirmation of a given hypothesis?

The answer is, generally, that in important investigations s_i are to be utilized so that $p(H,E)$ is established to within very small limits, while in less important investigations, the establishment need not be so precise. In important investigations E will be a very large set,[21] while in cases where we are not so anxious for accuracy, E will be smaller. We can adjust E to suit the inquiry at hand by letting in statements above a certain

---

[20] What is phrased here is analogous to Braithwaite's 'Rule of Rejection' which is operative in class-ratio probability. See Braithwaite, Scientific Explanation, chap. vi.

[21] E need not, of course, be large in the sense of containing many statements. See below, chap. iv, pp. 111-113.

degree of relevance. And it would seem quite proper to call a statement relevant if and only if it changes p(H,E) by some specified amount. In short:

$$\text{If } E = \left\{s_1, s_2, \ldots, s_n\right\}$$

Then $s \varepsilon E$ if and only if the addition of s to E changes p(H,E) by some amount $\Delta p \geqslant \varphi p$.

It is evident that φp should be small as the inquiry is important, and large as the inquiry is unimportant. Thus evidence is relevant if and only if utilization of it changes the confirmation function to a degree above the limits of tolerance established by setting φp.

But now two additional questions present themselves:

(1) What methods should be used to compute p(H,E) from a given H and E?

(2) How are values to be set for φp?

As regards a reply to (1), we shall have something to say later in this chapter. Let us remark right now, however, that a reply to (1) ("The method M should be used.") is supported by reference to a structuring of belief (not to exclude so called structurings of 'attitudes') which M effects, where the beliefs so structured are relevant to human activity.

The reply to (2) is -- though difficult -- not so temptingly philosophical. It is evident that if φp is small, the investigation will be arduous, evidential statements will be included which refer to much evidence. If φp is quite large, the investigation will be more cursory, less evidence will be referred to. As we are more anxious to narrow the gap $k_2 - k_1$, $k_1 \leq p(H,E) \leq k_2$, so do we assign smaller values to φp. Being anxious to narrow the gap is being anxious that utilization of H result in well supported predictions, ergo (if our notions of what is well supported are what we want them to be) true predictions.

4.1 From our remarks in the preceding section, we should like to conclude that 'the' problem of induction is really several problems. A schema was presented which indicated that at least two questions could be considered relevant to fruitful solution of the problem. What is most important is that there are known methods to commence answering the pertinent questions raised by the two approaches mentioned. It is important to realize that the translation of the problem presented in section 3.2 is by no means the only adequate translation. Instead of searching for criteria of confirmation, one might -- as did Goodman -- ask for criteria for distinguishing <u>interesting</u> hypotheses from <u>uninteresting</u> hypotheses, and thus point the way to justifications which were relevant to the status of interesting hypotheses. It is the writer's opinion that any such attempt would involve itself ultimately with all the problems of confirmation criteria, but then it is readily evident that a confirmation criteria approach is very soon going to become involved with distinguishing interesting from uninteresting hypotheses.

We have characterized Goodman's 'distinction approach' to the problem of induction as one which attempted to distinguish interesting from uninteresting hypotheses. The importance of his criteria for the remainder of this thesis requires that we discuss these terms, 'interesting' and 'uninteresting': One might intend by the phrase 'interesting hypothesis' to indicate those hypotheses in which scientists show interest; but this would not indicate an intuitively acceptable set, for scientists have shown interest in some quite unfruitful hypotheses and wasted -- by their own admission -- much time in so doing. The old adage that a negative demonstration is just as valuable as a positive one applies only to negative demonstration relevant to interesting hypotheses, and not to those confused quests which result from inept phrasing or show themselves unrelated to other investigations. The sociologist of science -- a rare bird -- is of course quite justified in investigating what hypotheses actually interest scientists. But the philosopher of science would make insignificant contribution were he thus to limit his work. No, what must result from the philosophy of science is a set of recommendations as to what sorts of hypotheses scientists <u>ought</u> to show interest in. The temptation is strong to involve oneself in the sort of circularity mentioned in I.3.2 and I.3.3, above, and such circularity can only be avoided by indicating areas where empirical evidence is needed to show relevance of whatever recommendations are proposed. The net result of this thesis should be some such recommendations, and it is hoped that the indication of need for evidence is not herein neglected.

Much of the more interesting recent work in the philosophy of science has been concerned with what we have called the problem of distinction; how does one tell an interesting hypothesis from an uninteresting hypothesis? Or, to paraphrase, by what criterion may lawlike statements be distinguished? To search for such a criterion is to search for some property which might serve as an index of distinction, or perhaps for some property present in all hypotheses which is maximally (minimally) manifested in interesting hypotheses and minimally (maximally) in uninteresting ones. Goodman finds such a property as a function of the <u>histories</u> of the terms involved in hypotheses. Hypotheses which contain terms with interesting histories are worth bothering about, hypotheses which contain only terms with insignificant scientific histories are not worth detailed and exhaustive investigation. Goodman's recommendation appears at first to be ultra conservative -- since only terms which have historically interested scientists (or terms coextensive with them) can be called interesting. But this is not the case; what is required is that the investigation must commence in terms of extant theories, extending itself to virgin areas as the associated terms gain meaning through relation to the rest of science. Such a requirement implies no more than that what knowledge is available be made relevant to the investigation at hand; and this amounts to saying that science should be recognized in scientific investigation.

Goodman's criteria of interest grew out of several attempts to make the distinction of lawlike statements from non-lawlike. One of the sorts of attempts which will interest us here was made in terms of determining the effects of the hypothesis in question upon extant theories; not really far removed from determination of entrenchment. Any hypothesis, it was said, distinguishes two notable classes among extant hypotheses [22] those which agree with the hypothesis, and those which disagree with it. If the value of the first class was greater than the value of the second class, then -- went the recommendation -- the hypothesis should be accepted, if the comparison was weighted the other way, then the hypothesis should be rejected. There are, of course, many variations on this theme -- as the conscientious vagueness and ambiguity of the term 'value'

---

[22] The distinction is actually into three classes, the simplification is made possible by defining two exhaustive and exclusive classes, see <u>infra</u>. Chapt. III, pp. 76-79.

permits: One might say that the more evidence relevant, the more interesting the hypothesis, etc. Goodman's comments on this procedure are most enlightening, and -- to one who would try to formulate a criterion in terms of the procedure; maddening. He shows that by clever definition of predicates logically equivalent hypotheses can be evaluated quite disparately, and that as much evidence can be garnered for the acceptance of trivial hypotheses as for the acceptance of important ones.[23] In short, the condition is not sufficient to guarantee distinction.

To say, however, that the primitive distinction criterion is inadequate as a mode of distinction is not to say that it is completely useless. Indeed, the causes for its development *qua* criterion probably lie in the fact that it approaches the status of a necessary condition for interesting hypotheses. Most of the interesting hypotheses of science have schismatized scientific knowledge to some degree. Given a new hypothesis, if it be interesting, then there are interesting hypotheses which are in agreement with it and interesting hypotheses which are in disagreement with it. This follows from a definition of 'interesting' in terms of entrenchment. If the new candidate agrees with important scientific knowledge and disagrees only with unimportant scientific knowledge, then the cost of accepting

---

[23]See Goodman, *Forecast*, chap. iii.

it when it is false is not as large as the cost of rejecting it when it is true. Let it be emphasized that such a comparison is justified only in case the hypothesis in question has been established as interesting by some prior criterion. It is a simple task to invent uninteresting hypotheses which appear as interesting under the specified conditions; but given an interesting hypothesis we are prepared to consider the agreement condition as specifying part of the cost involved in acceptance or rejection.

4.2 In this section we shall propose a method of analysing evidence relevant to a given hypothesis. It is intended throughout that when evidence is determined relevant, it must be relevant according to some φp; Where φp is not specified, it does not enter into the immediate calculation and mention of it is omitted for the sake of simplification. Another simplification of the present section is the highly elliptical phrasing, 'H is interesting.' This is not intended to assume that a criterion of interest is available which absolutely delineates interesting from uninteresting hypotheses: It <u>is</u> intended to assume that some mode is available for deciding whether H is sufficiently interesting to merit consideration in the inquiry at hand.

Given, then, a hypothesis H which is being considered; assume that H is interesting. Consider $\{L_1 \ldots, L_n\}$ a set of laws such that $(i)p(L_i, H) > 0$ where all the L_i are accepted and interesting. The L_i, then, are all the accepted laws which H confirms, where this confirmation is established within limits set by some φp. Similarly define $\{L'_1, \ldots, L'_m\}$ so that $(i)p(L'_i, H) = 0 \pm \frac{\varphi p}{2}$, where all the L'_i are accepted and interesting. The L'_i are all the accepted laws which H disconfirms within limits established by some φp.

Similarly define $\{h_1, \ldots, h_r\}$ and $\{h'_1, \ldots, h'_s\}$ so that all h_i, h'_i are accepted and interesting and $(i)p(H, h_i) > 0$ and $(i)p(H, h'_i) = 0$. All the h_i confirm H and all the h'_i disconfirm H.

Then let $T = \{h_1, \ldots, h_r, H, L_1, \ldots, L_n\}$

and $T' = \{h'_1, \ldots, h'_s, L'_1, \ldots, L_m\}$

T will be called H's <u>including theory</u>, and T' will be called H's <u>rival theory</u>.

Let E be the set of evidence statements such that

$$p(T, E) > 0$$
$$p(T', E) > 0$$

That is to say, E is the set of evidence statements which confirms both T and T', both the including and the rival theory.

Let F be the set of evidence statements such that

$$p(T, F) > 0$$
$$p(T', F) = 0$$

F is the set of evidence statements which confirm T (the including theory) and do not confirm T' (the rival theory). Similarly define F' so that F' is the set of evidence statements where

$$p(T,F') = 0$$
$$p(T',F') > 0$$

Then $v_H(E) = \frac{p(T,E) + p(T',E)}{2}$

$$v_H(F) = p(T,E \cup F) - v_H(E)$$

$$v_H(F') = p(T',E \cup F') - v_H(E)$$

That is to say, the value of evidence supporting only the including (rival) theory is the difference in the degree of confirmation of the including (rival) theory and the value of evidence which supports both the rival and the including theory.

The three functions listed above are then defined over the same range as is degree of confirmation. (An implicit assumption has been that theories are weighed in accordance with the evidence which confirms them.) The definitions assume that degree of confirmation is real valued, that it is an extensive measure over the range of all pairs (H,E). Thus the proposed formulations constitute some attempt to weigh <u>the relative importance</u> of the rival and including theory.

We shall consider that the task of this chapter has been accomplished; some means has been proposed for the evalutation of evidence sets and theories which relate to a given interesting hypothesis. In the next chapter we shall attempt to utilize this evaluative method for weighing different hypotheses.

CHAPTER III

FOUNDATIONS OF A THEORY OF IMPORTANCE

1.1 In the previous chapter a method for appraisal of evidence relevant to a given hypothesis was outlined. Such an outline is certainly required if even a programmatic proposal for the evaluation[1] of hypotheses is to be made. The present chapter comprises an attempt to utilize the outline as a basis for some comments on the evaluation of hypotheses: These comments will explicitly presuppose that there has been antecedently provided, first, some efficacious method of evidential assessment, and second, some criterion for distinguishing scientifically <u>interesting</u> hypotheses. Without such criteria, the evaluation to be undertaken below of hypotheses in terms of importance becomes meaningless; for uninteresting hypotheses can show high importance values relative to similar interesting hypotheses. The method could thus -- in the absence of a criterion of interest -- be construed as indicating that uninteresting hypotheses ought to be accepted by scientists. In view of

---

[1] In the present and ensuing chapters we shall use 'value' and its cognates only where reference to value in the ethical or esthetical sense is intended. Locutions such as 'evaluation of evidence' will be avoided in favor of such expressions as 'appraisal of evidence'. This convention is adhered to not because of any misgivings about uses of 'value', but for clarity of exposition. There has been no attempt to use the word thus univocally in the preceding chapters since there is little danger of confusion.

this, any proposal for the weighing of hypotheses which makes use of the evidential criterion outlined in chapter II will be restricted by the presupposal of a criterion of interest.

The restriction to interesting hypotheses, however, is not as stringent as might at first appear. Goodman's theory of projection provides an informal scheme for the partial ordering of hypotheses as regards interest. If, to be sure, the criterion of evidential assessment were to be metricized, it would be necessary to metricize the theory of projection; but the lack of a metricized theory of projection should not prevent enlightened speculation as to what such a theory would be like were it available, and should not, in turn, prevent speculation as to what a theory of evidential assessment would be like if a metricized theory of projection were available. It is hoped that Goodman's theory of projection and the outline of evidential assessment procedures presented in chapter II are, in turn, adequate bases for speculation about a method of evaluation of hypotheses. In the sequel we shall attempt just such speculation, bearing in mind that such comments as eventuate are justified only in case both the relevant presupposed theories are shown to be empirically and formally sufficient.

Some comment should be made about the utilization of formal and quasi-formal modes of exposition in this and the preceding chapter. The exposition of evidential assessment on the basis of degree of confirmation assumed the additivity and multiplicativity of various functions. It is obvious that such assumptions are only to be justified by a quite thoroughgoing formal investigation accompanied by an interpreted theory dealing with the behavior of the values of the functions. It might be objected that what has been given is an irresponsible display of arithmetic sleight of hand, at best confusing and at worst erroneous. Such an objection is, I think, quite tenable if one takes the presented formalism to be a fully interpreted theory about the phenomena at hand. The formalism, however, is not intended to be such a theory: Speculation in formal language is no more noxious than speculation in any other language -- just as long as one clearly labels it speculation and does not purport to be offering mathematical 'proofs' for demonstrated' conclusions. The nature of the concepts dealt with and the author's ineptitude of expression make it advisable in the interests of simplicity and precision that what is said be said -- at least in part -- formally. This formalism may be taken as a heuristic device to aid in the explication attempted discursively if the reader's conscience balks at speculative formal discourse; but the intention of the writer is to make speculative comments in a quasi-formal idiom. Thus, the treatment of a concept as metricizable or linearly orderable or whatever is not intended to imply that the concept has been shown to be or is of such a nature;

when such implications are intended they will be made explicit. The primary goal of this chapter indeed, of this thesis -- is to show the feasibility of treating certain concepts as at least partially orderable. The <u>proof</u> that these concepts are so orderable would require that other concepts be metricizable or completely orderable, and further that such an ordering had empirical meaning of a quite specific nature. No such proof will be offered, but it should at least become evident where such proofs are needed and where empirical evidence is required to render a partial theory of scientific decision making. If such indication is clearly and correctly made, then the thesis has fulfilled one of its important functions.

2.1 The decision of a scientist to accept or reject a hypothesis is a product of many factors. Though it is true that science is the long arm of common sense, the arm is uncommonly long in that complex evidential rules and procedures of observation and inference are made an explicit part of scientific procedure, whereas in common sense judgments such components of decision making machinery are left more or less to function without explicit utilization or checks on their operation. This checking on the operation of decision making apparatus has become a carefully incorporated part of scientific procedure. The scientific method is replete with checks and counter checks -- observations are contrasted with other observations, inferential patterns are compared with other inferential patterns, hypotheses are checked against related hypotheses, and the whole machinery is made self-corrective insofar as that is possible. The methodologist must attend to this feature of science with scrupulous care. Advances in the efficacy of science's capacity for self-correction are significant improvements in scientific method.

Historically much attention has been directed at observational and inferential procedures. Science itself has invented observational equipment as the need for increasingly accurate observation became felt. Logicians and mathematicians have traditionally been concerned with the inferential patterns used by scientists. It is regrettable, however, that little attention has been directed at the influence of _valuings_ or _appraisings_ -- in Dewey's sense -- in science. It would seem pretty obvious that valuings influence the scientific decision, and that the interests of science would be best served by making valuative procedures explicitly self-corrective insofar as possible. In the ensuing sections some attempt is made to outline a method of such self correction. The method is quite narrow in scope, dealing only with decisions to accept or reject hypotheses, and at that only with decisions to accept or reject hypotheses previously judged interesting. But it affords some opportunity for ordering and is not -- it is hoped -- completely quixotic in its aspirations.

The relevant alternatives for scientists when faced with interesting hypotheses may be characterized as being in one of three general classes. After contemplating an interesting hypothesis the scientist either rejects it, accepts it, or holds it in abeyance. We shall assume that his initial attitude is one of abeyance -- that is to say that he neither accepts nor rejects the hypothesis, but decides to subject it to further testing. After a period of testing he assimilates his data and again chooses one of the three alternatives. If at some juncture he decides to accept the hypothesis, then he utilizes the hypothesis in prediction, assumes that it supports higher level generalizations and verifies lower level hypotheses,[2] and assumes that the probability associated with the hypothesis by the relevant theory is justified. If he decides to reject the hypothesis, then he assumes that higher level generalizations are disverified, that lower level hypotheses dependent upon it are disconfirmed. If at some juncture the scientist decides that the hypothesis in question is not as interesting as it at first appeared, or that it lacks some other quality necessary to legitimately testably hypotheses, then he may assign it to a Limbo, so to speak, of always-to-be-held-in-abeyance hypotheses. To hold a hypothesis in abeyance

---

[2] We say that a hypothesis verifies lower level statements and confirms higher level hypotheses. Reversing the implication, hypotheses disconfirm lower level statements and disverify higher level hypotheses. The barbarism 'disverify' seems condoned in view of its intuitive aptness.

is to refuse to accept or reject pending further evidence, the decision in no way requires that the scientist show any degree of diligence in accumulating such evidence. The decision to hold in abeyance might result in feverish investigative activity, or it might result in completely ignoring the hypothesis as uninteresting or otherwise unsuitable.

In characterizing this situation, one might be tempted to speak of a linear or quasi-linear ordering of preferences; we might ask scientists to specify under varying conditions in what order they prefer abeyance, rejection and acceptance for a given hypothesis. There are available ingenious and empirically tested methods for establishing such n-membered preference arrays[3] so the project is not at all chimerical. If however, we conducted such an investigation, in all probability we should find that certain kinds of arrays are never presented.[4] For instance, in any array where the scientist was indifferent between acceptance and rejection, he would prefer abeyance to both of them, and in

---

[3]Savage records one such device: "...(The subject) is instructed to rank the three acts in order, subject to the consideration that two of them will be drawn at random..., and that he is then to have whichever of these two acts he has assigned (more preference)." Savage, op.cit., p. 29.

[4]This, of course, is an empirical thesis. But in view of our stipulation the assumption seems safe. It is noteworthy that procedures such as that mentioned in note 3, above, are inapplicable to such situations.

any array where he greatly preferred acceptance to rejection or rejection to acceptance, he would also prefer that more preferred member to abeyance. In short, the preference value of abeyance is at least in part a function of the relative status of acceptance and rejection.

Because of this relationship among the three options, the construction of a scheme of appraisal which recognizes all three of them awaits the construction of an scheme of appraisal which recognizes two of them -- acceptance and rejection. If a fully articulated method of appraising acceptance and rejection were available, the problem of introducing the third option to explicit consideration would be quite manageable: There are techniques of sequential analysis in statistical inference and sampling theory which would be of great assistance here.[5] It is obvious that appraisal of acceptance and rejection is the prior and major task.

Since we are considering that the initial attitude toward a hypothesis is always abeyance, and that later decisions to accept or reject follow a decision not to hold in abeyance, we may informally characterize the entire decision situation as being a decision between two alternatives (abeyance or not abeyance) one of which (not abeyance) results in a two alternative decision itself, (accept or reject) and the other of which (abeyance) can lead back to a reposing of

---

[5]See for example, Savage, op. cit., pp. 142ff.

the original decision. We shall attempt a partial formalization of appraising procedures which rank the two alternatives of acceptance and rejection for a large though quite restricted class of hypotheses.

2.2 If the scientist is depicted as facing two alternatives, then we can appraise his choice in one of two ways -- as mentioned in chapter I. We can decide if he chose rightly on the basis of available evidence and goals, no matter what the outcome, or we can play 'Monday morning quarterback' and distinguish right from wrong choices in terms of their eventuations. Our task here is to prescribe a method of appraisal in the first sense -- on the basis of goals and evidence -- such that specific appraisings by the use of this method coincide, with greater regularity than any alternative method provides, with ultimate appraisals in terms of eventuations. In short, we wish to prescribe a method for appraising alternative courses of action by which a course of action is appraised independently of whether it is undertaken or not and, if undertaken, independently of its specific results.

In order to weigh alternatives and thus appraise decisions, we shall consider that the appraisal is to be made in terms of a probability distribution over the space of outcomes of the decision. We have described the scientist's decision as schematically representable by two alternatives (abstracting from the decision to not decide).

We shall consider that the hypothesis in question must be either true or false. The decision situation may then be schematized by reference to a 2X2 matrix:

	H is true	H is false
scientist accepts H	H is accepted and true	H is accepted and false
scientist rejects H	H is rejected and true	H is rejected and false

FIGURE 1[6]

It is generally agreed that we ought to accept true hypotheses and reject false ones,[7] so a generic assignment of possible evaluations may be made as follows:

	H is true	H is false
accept	good	bad
reject	bad	good

FIGURE 2

---

[6] This format, in an abbreviated form, will be used throughout the remainder of the work. Figure 1 is intended as a guide to reading the matrix format. The matrix presentation invites such interpretations as "2-person zero-sum game" in which the scientist is depicted as playing against nature. Such anthropomorphic references are inacceptable -- it would seem -- in that it is difficult to associate the notion of a <u>strategy</u> with the activities of nature, even if the universe is case in a purposive role. cf chap. IV pp. 113-117.

[7] This, of course, is an oversimplification. It frequently comes about that rejection of a true hypothesis or acceptance of a false one results in a utility maximisation. This shortcoming of the present scheme is noted and briefly discussed in chapter IV pp. 117-120.

The matrix depicts an evaluation of points in the outcome space, and not of procedures of appraisal.[8] We shall eventually make some comments about assessment of decisions according to methods, but our concern here is to make a general prediction as to what we shall judge about the outcomes of the decisions, regardless of the procedures used in arriving at those decisions. We are in truth playing 'Monday morning quarterback', but we are doing it on Friday night; we know that if we should decide to accept a hypothesis which turns out to be true it will be good for us,[9] that if we should decide to accept one which is false it will be bad, and so on. What methods we use to reach these decisions to accept or reject the hypothesis in question has no bearing whatsoever on the _values of the eventuations_. Good methods, it is true, usually result in happy eventuations, but good eventuations need not arise from good methods.

3.1 Before the assignment of values to points in the outcome space can be made more specific, some additional properties of hypotheses must be noted which will make such assignment possible. The first of these properties which we shall note is _importance_: A hypothesis is important insofar as its acceptance results in the rejection of formerly accepted hypotheses. Importance is distinct from _interest_ in

---

[8] cf. chap., I, pp. 24, f.

[9] Except as noted in note 7, above.

that very interesting hypotheses (i.e. well entrenched hypotheses) might be acceptable at very little expense of relinquishing other accepted hypotheses. In the extreme case of a highly interesting but not important hypothesis, the hypothesis may be considered to be a re-statement of extant theories. Such a hypothesis might offer a gain in systematic clarity and economy by way of reformulation. To say, then, that the only hypotheses considered for evaluation are <u>interesting</u> hypotheses is not to have filled the need for assessing the <u>importance</u> of considered hypotheses.

Importance is defined as follows: Let the <u>degree of importance</u> of H be ψH, where

$$\psi H = v_H(F') = p(T', E \cup F') - v_H(E)$$

and where

$$v_H(E) = \frac{p(T,E) + p(T',E)}{2}.$$

so that "$v_H(E)$" indicates the worth, or weight, so to speak, of evidence which confirms both T and T'; where "T" and "T'" indicate, respectively, the comprehensive theory which includes H and the comprehensive theory which rivals H. (We shall speak of these as "H's including theory" and "H's rival theory.") Thus, "ψH" may be interpreted as referring to <u>the increment in confirmation bestowed</u> on the rival theory by evidence which does not confirm the including theory.

The definition of "ψH" guarantees that for highly important H, the rival theory will in general be highly confirmed, and moreover that the rival theory and the including theory will have little supporting evidence in common. Notice the fact that highly important H might be included by theories which are highly confirmed or not. $v_H(F)$ is irrelevant to the determination of the importance function, ψH.

Perhaps it will now become a bit more clear what was intended by saying that H was important insofar as its acceptance tended to bring about the rejection of previously accepted theories. It is evident that the formal definition of "degree of importance" is an incomplete definition, but it is noteworthy that counter-examples to the definition (i.e. hypotheses which are formally important but intuitively unimportant) are -- in most cases -- either excluded by criteria of interest or are also counter exemplary to criteria of interest.[10] It is for this reason that such emphasis is placed on the priority of a criterion of interest to a criterion of appraisal of importance. Insofar as this development is taken to be a theory, it fails in the face of counter examples not excluded by criteria of interest: Insofar, however, as the development is explicative and speculative, it might be said that such counter examples show its success -- the notion must be understood before it can be counter exemplified.

In the preceding section we noted that a generic

[10] cf. p. 88, this chapter.

assignment of values to each of four points in the decision outcome space was at least intuitively acceptable. We also noted that insofar as such an assignment was innocuous, it was trivial: One doesn't need a 2X2 matrix to learn that it is good to accept true hypotheses. In order to utilize a system of evaluation by matrix inspection, some further refinement of value assignments must be accomplished. The first step in this refinement is the establishment of an 'importance index' to be determinable for any interesting hypothesis.

'Importance index' is so defined above as to be metricizable. This definition is, of course, quite optimistic. To render such a concept amenable to metrical measurement would require a much more thoroughgoing formalism than is feasible within the confines of the present work. What -- it is hoped -- is accomplished by such formal sketching as that presented here, is some idea of how important indices would (functionally) behave if the concept were made thoroughly metrical. An assumption which is essential to what theorizing does go on in the sequel is that degree of importance is at least partially orderable; it is assumed that given any two interesting hypotheses, it is ascertainable which if either brings about in the event of its acceptance more extensive modification of extant theories. It is for this reason that we introduce the notion of weighing theories in accordance with the support lent them by evidence statements, and then bring about comparison of evidence statement sets among themselves. If the explicating theory is rejected, then the evidence statements are left unexplained, so to speak, and one must re-theorize to bring them under the rubric of explicative theory. If the acceptance of a hypothesis brings about such rejection -- so that some evidence statements are left unexplicated -- then that hypothesis is important to an extent which agrees with the amount of observed phenomena left unexplicated. 'Amount of phenomena' is defined in terms of the degree of confirmation (or the increment to degree of confirmation) lent to some theory by the evidence statements in question. Some reflection will assure us that this measurement of evidence is a difficult matter;[11] to count the evidence statements won't do, since any amount of evidence can be expressed in any number of statements: One would like to speak of the <u>area</u> of phenomena 'covered' by the theory, but this would seem to require cardinal comparison of very large sets,[12] and where such comparison is not required, eventuate in drastically counter-intuitive conclusions.

For these reasons the notion of importance is introduced. Although, as mentioned above, we treat importance

---

[11]See, for example, the very interesting attempt in J. G. Kemeny with Paul Oppenheim, "Degree of Factual Support", <u>loc. cit</u>. The reader will note similarities between our notion of importance and the Kemeny-Oppenheim 'F'. It is hoped that the importance function does away with necessity of reference to state descriptions.

[12]Compare, for instance, the number of stars with the number of electrons.

as if it were extensively measurable -- i.e. metricizable -- for explicative purposes, our concern is primarily that we indicate it to be intensively measurable. We want to introduce a relative notion of importance ('H1 is more important than H2.') as a relation which orders the field of interesting hypotheses. We do this by supposing importance to be metricizable and then, to render this supposition innocuous, only utilize 'importance' in comparative locutions, e.g. 'The importance of H1 is greater than the importance of H2.' In the two culminating formulas of the development, T.III.1 and T.III.2, we compare importance with another evidential function of H, $v_H(F)$. This comparison is permissible even for partially ordered importance because $v_H(F) = \psi H'$. H' is the rival hypothesis of H, so to speak, and we need know no more about it than that

1. $v_H(F) = \psi H'$
2. $v_{H'}(F) = \psi H$.

3.2 Consider hypotheses of the highest generality -- axioms of articulated and developed theories. Such hypotheses have no corresponding set of laws $\{L_1, \ldots L_n\}$ which they confirm, and the including theory for such a hypothesis consists of H, the highest level hypothesis in question, and the set $\{h_1, \ldots, h_r\}$ of lower level hypotheses which it verifies. There might, of course, be more than one highest level hypothesis, but this in no way effects the absence of a set

$\{L_1, \ldots L_n\}$ for any highest level H.[13] Similarly, there will be few laws disverified by H, because of its high generality, but many lower level hypotheses which it disconfirms. In this extreme case, the evidence for the including theory will be the evidence for H and the evidence for the rival theory will be the evidence against H. E will be -- if not empty -- of very small weight,[14] and v(F') will be simply the degree of confirmation of the rival theory on the basis of available evidence evaluated with respect to some φp. In such a case the information afforded by the computation of importance functions is at best trivial, and, possibly, erroneous as regards intuitive evaluations. If the importance function of a highest level hypothesis is nothing more nor less than the degree of confirmation of the rival theory of the hypothesis, then it is of no more use than a straightforward examination of relevant probabilities without consideration of the utility of acceptance or rejection.

---

[13]The distinction might be made in terms of factual support as opposed to degree of confirmation -- see Kemeny and Oppenheim (ref. note 11, above) -- but there seems to be no need to appeal to a distinction this fine.

[14]Incompatible statements can, of course, be confirmed by the same evidence. See Goodman, Fact, Fiction and Forecast pp. 69ff. If one makes φp small enough, however, to permit such support, in the case of the statement which supports both incompatible statements, the contrary of the supporting statement, which will disconfirm both statements, will also be admitted.

In view of such considerations as the above, we shall exclude highest level hypotheses from our subject matter. The exclusion, perhaps, is not as indicative of narrowness of conclusion as might at first seem. If such a highest level hypothesis is a reformulation of already accepted and confirmed theorizing, then its empirical relevance is indirect -- though not by any means insignificant -- and the utility of accepting it results from its function in simplifying and clarifying extant theories. We shall consider such hypotheses (reformulations of extant theories) as limit cases of highly interesting and unimportant hypotheses. If, on the other hand, the highest level hypothesis in question is not a reformulation, but a novel assertion in the form of a very inclusive generalization, then its degree of interest would not be nearly so high -- such a hypothesis would contain novel terms and exhibit a low degree of entrenchment. It's importance, however, might be very high. One is minded of current theorizing in extra-sensory perception; the terms concerned have very little significant scientific history, hence are not well entrenched; but the acceptance of the hypothesis would require not only modification of laws of psychology and biology, but also of physics and chemistry.[15]

---

[15] I fail to find a clear presentation of these hypotheses. One gets some idea, however, of what an articulated theory of extrasensory perception would be like from J. B. Rhine, New Frontiers of the Mind (New York, 1937).

The question as regards such hypotheses is Goodman's inquiry; ought they be projected? If only interesting hypotheses are to be projected, then they ought not. If one considers some other property than interest, then perhaps they ought. The situation seems counter exemplary to the theory of projection, and provides an interesting case for study of that theory and its limitations: What is of most interest to us here is that a case which proves difficult for the theory of importance to deal with, shows itself to be difficult for the theory of projection to deal with. As we remarked before, this would seem to be indicative of the degree to which an adequate theory of importance must depend upon an adequate theory of projection, or interest.

The above comments indicate that our caveat about hypotheses of highest level generality need not perhaps be as stringent as it is. There are cases in which importance functions of highest level generalizations are indicative and relevant. There are also, however, cases in which such functions are neither indicative nor relevant. This leads us to remark that where the theory works on highest level generalizations, it works; and hence to consider highest level generalizations properly outside the subject matter of our comments. We shall make reference to limit cases in our illustrative development, but we do not intend to offer conclusive information as regards these limit cases.

4.1 In previous sections we have spoken of "the four points in the outcome space" of a decision situation. This locution, convenient initially to describe a way of viewing decision situations, is sufficiently misleading to require explicative comments: The outcome space is perhaps best considered as a <u>dense</u> set of points, this sense set may be partitioned into four subsets, mapping every point in each subset to an 'ideal' representative point, or, mapping every point in the outcome space to one and only one point in another space of four points. The generic ordering illustrated in Figure 2 imperfectly ordered the four points in this second space. A more nearly complete ordering of outcomes must attend to two tasks: (1) An ordering of points (or of some significant subset of points) <u>in</u> the partitions of the original dense outcome space, and (2) An ordering of the partitions <u>of</u> the dense outcome space, or a more complete ordering of the four discrete points in the simplified space. In this section we shall attempt this more comprehensive ordering, first within each partition, then among the partitions.

Our first attempt at ordering will be made, as was said, in terms of value. It is assumed that, in order to have value or disvalue, an outcome must make a felt experiential difference which is mainly to be exemplified in changing what is accepted by science. If an outcome requires that science reject certain theories which it once accepted, then this is an outcome which makes a difference; similarly for outcomes which require the acceptance of what was once rejected. If an outcome requires no such variation in acceptance, then it is to be considered generally better than bad outcomes and worse than good ones. Outcomes are to be considered good if they consist in changing policy so that true statements are accepted and false ones rejected, they are to be called bad if they bring about the converse.

In order to make this evaluation of outcomes relevant to appraisal of decisions irrespective of specific outcomes, some means is obviously required to functionally relate the two values. We shall call that decision x_0 best in a given situation if for all x, $u(x_0) \geqslant u(x)$. Where $u(x)$ is the sum of all products, $p(y)v(y)$, where $p(y)$ is the probability that y will occur given that x occurs, and $v(y)$ is the value of y[16] (in this case the variation in scientific policy). Symbolically

$$u(x) = \sum_{i=1}^{n} [p(y_i, x)v(y_i)]$$

The set of y's can be, of course, infinite; and one faces an analogous problem to that of deciding which statements are to be admitted to an evidence set for the computation of degree of confirmation. Much the same sort of qualification can be made as regards utility (for such is the

---

[16]By 'value' is meant, e.g., 'felt good', not mathematical or logical value. See note 1, this chapter.

conventional name for the function u(x)); the number of y's included in the set is directly proportional to the inquirer's need to ascertain exactly what the results will be. We shall not define this formally here, the reader is referred to the contemporary literature for various formal treatments.[17]

4.2 Consider two hypotheses H_0 and H_1 such that H_1 has a high importance index and H_0 has a low importance index. If H_0 and H_1 are both true, then they both ought to be accepted, but the obligation is stronger in one case than the other. Acceptance of H_0 results in little modification of truth claims,[18] while acceptance of the highly important H_1 results in extensive modification. Theories which were heretofore accepted are rejected and, what is decisive, true (interesting) hypotheses tend to confirm true (interesting) theories and disconfirm false (interesting) theories. Thus the acceptance of a true H_1 tends to disconfirm a large body of false and relevant theory, while the acceptance of a true H_0 brings about little such modification.

---

[17]See R. D. Luce and H. Raiffa, <u>Games and Decisions</u> (New York, 1957), pp. 19-23. Savage, <u>op. cit</u>., pp. 70-76, Davidson, et. al., <u>op. cit</u>., pp. 9-12.

[18]The expression 'truth claim' is used as equivalent to 'what is accepted' in the preceding section. The change is made to avoid such confusing locutions as "Acceptance results in modification of what is accepted.' There is no intention to imply that science 'claims' anything.

In the limit case where H_o is a reformulation of extant theorizing, what results from its acceptance is a more economical and facile expression of what has previously been expressed. Reformulations are, so to speak, accepted with little regard for direct empirical evidence; the evidence has been regarded in the acceptance of the lower level generalizations, and the acceptance of the minimally important but highly interesting hypothesis is dictated by considerations of systematic economy. This is not to say that systematic economy is an unimportant end, far from it;[19] but it is to say that the benefits which accrue to science from such economical moves is a product of advances in formal techniques, and that no important empirical scientific decision is needed to adopt unimportant reformulations.

We might say, then that all other factors being equal, if H_1 is more important than H_o, then the consequences of accepting the true H_1 are more valuable than those of accepting the true H_o. Utilizing a conventional and suggestive notation, we shall let "$v[A,T](H_i)$" abbreviate "the value of accepting H_i when it is true". The comparison may then be phrased in a formula;

III.1.1 If $(\psi H_1 > \psi H_o)$ then $v[A,T](H_1) > v[A,T](H_o)$

If H_o, as above, is low in importance and true, then to reject it is to tend to reject its supporting and

[19] See e.g. Goodman, Structure of Appearance, chap. III.

supported hypotheses. Since in the case of a true and interesting H_o, $T(H_o)$ -- the including theory -- would tend to be true, rejection in this case would be tantamount to considering a true theory disconfirmed. If H_1 is high in importance, true and rejected, then the evil is double edged in that a true theory is considered disconfirmed and a false one confirmed. Here the phrase "all other things equal" becomes essential; we assume in the comparison that the amount of false theory confirmed is the same in both cases, that the hypotheses differ, therefore, only in the amount of true theory disconfirmed. All other factors equal, the rejection of a true H_1, is of greater disvalue than the rejection of a true H_o, when H_1 is more important than H_o.

One might choose to think that the realms of value and disvalue present two discrete distributive realms. This amounts to claiming that the relation 'better than' is not connected in its field. Such, it would seem, was the assumption behind our initial generic ordering. We are now prepared to recant any such assumption that we might have made, and explicitly to assume that good and bad outcomes may be represented on a linear scale, unique up to the assignment of a zero and a unit. On this assumption -- which, it might be remarked, the author has found more intuitively satisfactory with closer acquaintance -- to speak of disvalue is to speak of less value. The advantages of this for a scheme of ordering such as that we contemplate are so obvious as not to need mention, but one might object that such a scheme of ordering does injustice to the nature of good and evil, or the customary notions of the distinction between the two disjunctive realms.

To the protagonist of disjunction we remark that he is free, if he chooses, to represent the states of good and evil by positive and negative numbers respectively. If he does this, however, it would seem that he at least implicitly assumes a point of status quo -- that the point of zero increment or loss is the neutral point of value; and to assume this is to assume that the ordering has a great many more properties to it than would seem justified. It is to assume, most importantly, that the scale is completely orderable, that values are meaningfully additive, and that the intervals between value assignments signify 'real' units of difference between values. Our proposal that a scale of partial comparative ordering be utilized carries none of these disagreeable results with it. If the protagonist of disjunction could meaningfully explicate his disjunction, then there would be reason to heed his admonitions. Until such explication is forthcoming, however, we shall assume that the assignment of value indices to points in the outcome space indicates no more than a partial comparative ordering.

The arguments put forth here are the traditional and conventional arguments for the use of utility theory.[20] It should be no secret, by now, that we intend to implement our measurement of values by reference to the vocabulary and theorems of utility and decision theory. The vocabulary we have introduced so far is obviously kindred to that of utility theory, and the introduction of concepts has been unashamedly pointed toward an eventual association through formulations of these theories. It might be objected that though utility theory and decision theory are good things, they are not philosophical sorts of things. We mention this objection only because it seems to be somewhat prevalent. If the philosophical relevance of these theories is recognized, then there is no need to argue for this relevance. If it is not recognized, then this is not the place to make such argument. The doubtful reader is referred to the literature.[21]

We shall henceforth consider 'x has more disvalue than y' as equivalent to 'x has less value than y'. Textually we shall speak of value and disvalue for verbal

---

[20]Luce and Raiffa, Games and Decisions, chaps. 1 and 2 contains a quite favorable and persuasive introduction to utility theory. Savage, Foundations of Statistics, chap. 5, deals more formally with the introduction of utility, while Savage in chapts. 1 through 4 presents an interesting and clear general exposition. The philosophical problems, as well, become evident in such works as Braithwaite, Theory of Games. The strongest presentation and argument, however, would still seem to be the classic treatment in Von Neumann and Morgenstern, Theory of Games and Economic Behavior chaps. I and II.

[21]See note 20, above.

facility and intuitive comprehension, but in formal notation we utilize only expressions of positive value.

To return, at last, to the comparison with respect to true and rejected hypotheses: We said that if H_1 is more important than H_0, then the rejection of a true H_1 -- all other things being equal -- is of greater disvalue (of less value) than the rejection of a true H_0. Hence the formula:

III.1.2 If $\psi H_1 > \psi H_0$ Then $v[R,T](H_1) < v[R,T](H_0)$

If H_0 is accepted and false; if it is of low importance its acceptance does not call for the rejection of a large body of knowledge, thus does not necessarily tend to disconfirm a large body of true extant theory. Since it is false however, its acceptance does tend to confirm a body of accepted false theory. The case is analogous to accepting true and unimportant hypotheses -- if the hypothesis is very interesting and not at all important, it may be considered as a reformulation. In the case of accepting true and unimportant hypotheses, the benefits accrued were largely systematic benefits. So, in this case of accepting the false and unimportant hypothesis, the disadvantages are largely those of making it easier for ourselves to persist in some mistake. This sort of disadvantage to be sure, is insidious as can be: One only infrequently tests such reformulations, and just incorporates them into accepted doctrine. We should say that if there is a disadvantage then it is insidious. It might very well be, however, that such simplified systematization would make the errors of false hypotheses more readily discoverable, and then we should remark how fortunate was the acceptance of this false hypothesis. But I think that the fact that we should call ourselves fortunate in such a case is sufficient to persuade against the assignment of a generic positive value to the acceptance of false hypotheses, however unimportant. The argument seems to indicate that some low index of disvalue should be assigned to the acceptance of unimportant false hypotheses.

If, on the other hand, the highly important H_1 is accepted and false, the accrued disvalue is accordingly large. $T'(H_1)$ would tend to be true. The more important is H_1 the more disvalue is accrued. If, in short, H_o is less important than H_1, then -- all else equal -- more disvalue (less value) accrues by the acceptance of H_1 if false than by acceptance of H_o if false.

III.1.3 If $\forall H_1 > \forall H_o$ then $v[A,F](H_1) < v[A,F](H_o)$

The remaining case is similar to the preceding three cases; If H_1 is more important than H_o, then to reject the false H_1 is of more value than to reject the false H_o, since by rejecting the false H_1 we make more of a difference in truth claims than we do by rejecting the false H_o. This difference, furthermore, is a good difference, since false hypotheses tend to confirm false (interesting) theories and to disconfirm true(interesting) theories. In the limit case, again, the false H_0 is a reformulation of extant theory, and much the same considerations hold as were relevant in appraising the disvalue of accepting false and unimportant hypotheses. All other factors equal, there is more value resultant upon the rejection of H_1 than of H_0, where H_0 and H_1 are false, and H_1 is more important than H_0.

III.1.4 If $\psi H_1 > \psi H_0$ then $v[R,F](H_1) > v[R,F](H_0)$

4.3 For convenience of reference we reproduce the four formulas from the preceding section;

If $\psi H_1 > H_0$ then

III.1.1 $v[A,T](H_1) > v[A,T](H_0)$

III.1.2 $v[R,T](H_1) < v[R,T](H_0)$

III.1.3 $v[A,F](H_1) < v[A,F](H_0)$

III.1.4 $v[R,F](H_1) > v[R,F](H_0)$

An examination of III.1.1 reveals that the value of accepting a true hypothesis varies proportionately with the importance of the hypothesis -- the more important is the hypothesis, the better it be accepted if true. Suppose we choose to express this as

III.2.1 $v[A,T](H) = (\psi H)(f_H)$ for some f_H

The functional expression 'f_H' is innocuously vague. It would seem unjustifiedly restrictive here to assume that III.1.1 permitted replacement of 'f_H' with a constant, we have been cautiously avoiding assuming any additivity or multiplicativity of importance arrays, and replacement with a constant would violate just these caveats. The utilization of f_H rather than a constant corresponds to our use of the phrase 'other factors being equal' in the discursive presentation of III.1.1 through III.1.4. This qualification was omitted from the formulas because no adequate way was found to express it which would not render the formalism excessively cumbersome and, perhaps, defeat the explicative end of the previous section. No such omission need be condoned here, however, for the utilization of functional notation is sufficient qualification. The analogues of III.1.2, III.1.3, III.1.4 are written similarly;

III.2.2	$v[R,T](H)=j_H/\Psi H$	for some j_H
III.2.3	$v[A,F](H)=g_H/\Psi H$	for some g_H
III.2.4	$v[R,F](H)=(\Psi H)(k_H)$	for some k_H

The use of different functional notation in each case assures that the multiplicative and divisible formats are harmless. The point, let us repeat, is an explicative one. We shall make no further demonstrative use of III.2.1 through III.2.4 but shall develop another formula set which will be the foundation of a final partially ordered matrix set.

Let us compare III.2.1 and III.2.4. The first of these expresses the value of accepting true H_i as a function of importance, while the latter expresses the value of rejecting false H_i as a function of importance. The question to which we now address ourselves is this; For a given H_i how does $v[A,T](H_i)$ compare with $v[R,F](H_i)$?

It is obvious that, abstracting from variations in f_H and k_H, $v[A,T](H)$ and $v[R,F](H)$ vary proportionately with variation in ψH. Some unpacking of the functional notations must be effected. It is noteworthy that 'ψH' expresses nothing about the degree of confirmation of the including theory, but only permits consideration of H's rival theory as a function of H's acceptance or rejection. Let us now explicitly consider a function of H's degree of confirmation, namely $v_H(F)$. If the including theory explains more than the rival theory, that is to say if $\psi H < v_H(F)$, then a more significant change in truth claims is effected by accepting the theory if true than by rejecting it if false. In both cases the changes in truth claims are for the good, but in one case more good is brought about than in the other. The converse variation can also be seen to hold: If the rival theory explains more than the including theory, then the change in truth claims is greater when the false theory is rejected than it is when the true one is accepted. These considerations give rise to two formulas.

III.4.1 $[\psi H < v_H(F)] \supset v[R,F](H) < v[A,T](H)$

III.4.2 $[v_H(F) < \psi H] \supset v[A,T](H) < v[R,F](H)$

The analogous development of the formulas for comparison of true rejection and false acceptance is obvious:

III.4.15 $[\psi H < v_H(F)] \supset v[R,T](H) < v[A,F](H)$

III.4.25 $[v_H(F) < \psi H] \supset v[A,F](H) < v[R,T](H)$

We shall now repeat formally the assumption made in the first generic value assignments;

III.3.1 $v[R,T](H) < v[R,F](H)$ for all H

III.3.2 $v[R,T](H) < v[A,T](H)$ for all H

III.3.3 $v[A,F](H) < v[R,F](H)$ for all H

III.3.4 $v[A,F])H) < v[A,T](H)$ for all H

Two orderings then become evident, as consequences of the two distinct conditionals.[22]

T.III.1 $[\psi H < v_H(F)] \supset v[R,T](H) < v[A,F](H) < v[R,F](H) < v[A,T](H)$ [from III.4.1, III.4.15, III.3.3]

T.III.2 $[v_H(F) < \psi H] \supset v[A,F](H) < v[R,T](H) < v[A,T](H) < v[R,F](H)$ [from III.4.2, III.4.25, III.3.2]

We are now prepared to assert two matrices corresponding to T.III.1 and T.III.2. We shall use letters, 'a', 'b', 'c', and 'd', in the matrix positions, stipulating an ordering among a, b, c, and d, to avoid numerical postulations of intervals among the outcomes.

---

[22]In accordance with our simplifying assumption (cf. note 7 above and p. 79, this chapter) we use the strong relation ' < ' rather than its weaker counterpart ' $\leqslant$ '. Notice that in formula set III.4 this results in a weaker assumption, while in the set III.3 the assumption is stronger. We would be prepared to assert a set of formulas as corresponding to the III.4 set in which ' < ' was replaced throughout by '='. Given this last mentioned set it is evident that many interesting analogues of the III.4 set could be developed. It is felt, however, that such additional development would not aid in establishing the matrices which are the goal of this chapter.

	H is true	H is false
accept	d	b
reject	a	c

if $[\psi H < v_H(F)]$

where $a < b < c < d$

FIGURE 3

	H is true	H is false
accept	c	a
reject	b	d

if $[v_H(F) < \psi H]$

where $a < b < c < d$

FIGURE 4

In accordance with our assumption of section III.4.1,[23] we are now in a position to indicate a scheme for computing $u(x)$ for $x=[A,T]$, $x=[A,F]$, $x=[R,T]$, $x=[R,F]$. We shall consider that the probability of H's being true is $p(H,E u F)$, or the degree of confirmation of H relative to some φp. Then, where $p(H,E u F)=\chi$,

	H is true	H is false
accept	χd	$(1 - \chi)b$
reject	χa	$(1 - \chi)c$

if $[\psi H < v_H(F)]$

where $a < b < c < d$

FIGURE 5

[23] pp. 89 ff., this chapter.

	H is true	H is false	
accept	χc	$(1-\chi)a$	if $[v_H(F) < \chi H]$
reject	χb	$(1-\chi)d$	where $a < b < c < d$

FIGURE 6

Figures 5 and 6 present a matrix depiction of our conclusions. We avoid the use of numerical constants in the place of a, b, c, and d for obvious reasons -- we feel that the use of numbers in these positions, especially since the values are to be multiplicatively combined with probability values, would present too great a temptation to conceive of the ordering function (importance) as metricized. This, of course, weakens considerably our conclusions; no definitive information is offered. Let us hope, however, that the tabular presentation makes evident the factors of valuation ineluctably present in decisions to accept and reject hypotheses.

CHAPTER IV

CONCLUSION

1.1 In this chapter some of the conclusions of the thesis will be examined. Part of this examination will be in the form of extrapolation beyond the primary concerns of the thesis, and part of it will be criticism of the notions developed in the first three chapters. We shall attempt some return to the concerns of the first chapter and investigate the possibilities for resolution of some of the problems that were raised there.

It should be kept in mind that what has been presented could not pretend to be a theory of decision making. We remarked in chapter I that such theories must be empirically based and not the a priori verbalizations of philosophers. In this respect, the thesis is incomplete. It is a proposal for founding the empirical inquiries which would be requisite to an adequate theory of scientific decision making; not a completed theory. If the preceding chapters have value, then this must lie in their worth as a speculative inquiry.

To note, however, that an inquiry is speculative rather than empirical-theoretical is not at all to excuse shortcomings. It would be overly optimistic to expect empirically founded recommendations from a speculative work, but there are nevertheless criteria which must be met by any adequate speculations, just as there are criteria for the appraisal of empirical theories. The inquiry must not run counter to what is empirically known; it must provide clearly defined opportunities for empirical theorizing; it must provide meaningful discursive comments by way of justifying its tentative definitions. We have tried to keep these and other related _caveats_ in mind throughout the constructive parts of the thesis.

The method of the thesis, its general tenor and _modus operandi_, so to speak, has been constructivist. We construe this method generally as follows: Some problem or set of problems is pointed out, a partial system of definitions and axioms is articulated similar to a proposed system for empirical investigation. In the articulation of the system further problems become evident and these are analyzed with an eye to making them amenable to theoretical solution. The interrelatedness of proposed definitions and axioms is indicated, and the relevance of available empirical knowledge is recognized wherever possible. What should result is a set of recommendations for theory construction in the area of the inquiry.

Constructivism is, I think, the system-building aspect of analytic philosophy. Philosophy is probably forever priveleged (or doomed according to your dispositions) to be speculative. Empirical inquiries we call scientific, and we raise our eyebrows at scientists for speculating beyond the licenses of their evidence. A goodly part of our philosophical heritage is in the form of speculative discourse, and to ignore this heritage on the grounds that speculative philosophy is not observationally meaningful is to subscribe to an empiricism so crude as not even to merit serious refutation. The analytical traditions in philosophy, however, should teach us to speculate cautiously with an awareness of what speculation commits us to. Wittgenstein once remarked that if we were given all the books of a large library in a huge disordered stack and told to place them on the shelves in proper order, we might very well place a small set properly ordered on some shelf, even with no assurance that that shelf was where the books belonged. The discovery that these four books belong together is of use even if one does not know where they will end up in the general scheme. The task of speculative philosophy is not at all unlike this sorting of the disordered heap, and I should think that the initial tentative orderings of concepts has a worth analogous to that of the ordering of the small set of books.

Constructivism as a method recognizes the limits of speculative discourse and attempts to make explicit its assumptions and transgressions of these limits. If philosophy is a handmaiden of the sciences, constructivism tries to be an Ariadne-like handmaiden, to provide the proper strings for science to follow through the labyrinth of the universe.

1.2 The disciplines associated with decision theory, utility theory and the Theory of Games are coming of age. More and more sophisticated techniques are being developed for empirical theorizing in these areas, and the work shows itself to be of growing and momentous relevance to many areas. At the same time -- in the same breath, we have tried to say -- confirmation theory as a field of tremendous scope and problems has begun to become systematized and amenable to serious discourse. It is evident that scientific decisions enter into techniques of confirmation, and it is equally evident that no self-conscious attempt has been made to show the applicability of decision theory and its allied disciplines to the problems of confirmation of scientific hypotheses. The two endeavors cry for unification in many of their aspects while work goes on more or less independently in each of them. Some of this unification has been accomplished, I should think, unconsciously, in the work of such men as Savage, Wald, Davidson, and Suppes,[1] to name a few. That it is unconscious would seem to be the case in that very little specific note is made of the applicability to problems other than those being specifically dealt with. I am sure that these writers would agree that

---

[1]Savage, op. cit., A. Wald, Statistical Decision Functions (New York, 1950). Davidson et. al., op. cit.

scientific decision situations present very fruitful opportunities for the implementation of techniques of decision theory, but none of them has to my knowledge concerned himself self-consciously to bring about this implementation. Similarly, such writers as Goodman, Hempel and Carnap would probably welcome assistance from the decision-theoretical direction in dealing with the problems of confirmation theory, but, to my knowledge, none of the people so concerned have made specific utilizations of the available techniques in problems of confirmation.

Our attempts herein should not be interpreted as efforts at theorization, any more than metaphysics should be interpreted as physics. In light of the fact that very little attempt has been made at unification of the disciplines of decision theory and confirmation theory, we have tried to indicate what the direction of such an inquiry would be were it commenced. The thesis attempts to provide tentative answers to some of the problems which would doubtless arise in such an attempt, gratia argumentum so to speak. The efforts take the form, largely, of theory construction rather than of comments about the conjectured problems. This technique -- for better or for worse -- surely does make a wealth of problems evident. What had started off to be the subject matter of a portion of the first chapter of the thesis raged out of control to become more than the subject matter of the thesis entire.

It is hoped that we have indicated at least that the scientific decision situation is ripe for serious consideration by decision theorists. The fact that science has always been largely a self conscious endeavor makes it quite aptly suited for the application of formal techniques. In the decisions of everyday life, e.g. the buying of thread, one tends to feel that the decision theoretician is being too picayune and pedantic when he recommends a precise and formal appraisal of alternatives: The cost of computation through necessarily cumbersome formal procedures more than outweighs the advantages which might be gained from making the 'right' decision. I think that very few decision theorists would claim that they intend their recommendations to be implemented in the buying of thread or the making of omelets. The worth of the procedures is in their availability for implementation in more important decisions where the cost of appraisal of alternatives is <u>not</u> greater than the cost of an error. Science, I should think, provides just such a set of decisions. The decisions of scientists are momentous; it is worth a good deal of trouble to assure oneself that he is choosing rightly. The goals of science as an institution are established with reasonable facility and with a large measure of unanimity by the concerned parties. The alternatives available may be clearly defined and the results of these alternatives are at least partially predictable with some accuracy. This thesis may be taken as a recommendation that decision theorists and confirmation theorists make use of this suitability of science, and that scientists make use of what can be offered by the decisions theorists and the confirmation theorists.

2.1 A proposal of the thesis which should be further commented upon, it would seem, is the establishment of the rule of rejection for degree of confirmation. Degree of confirmation probability lent itself quite well to our needs in formulating the concept of importance, but we noted that if the theory were to aspire at all to practicality some procedure for appraising degree of confirmation without recourse to at least very large statement sets was required. The procedure which we advocated was inclusion of all statements which changed the confirmation function by more than a specified amount (φp).[2]

This rule was phrased so that the need for precision in determination of degree of confirmation should be interpreted as a function of which statements were to be included in the evidence set taken as confirmatory. But the rule proposed might be taken as a not very sophisticated *petitio principii*: What is being measured is not stated, so no interpretation is available of the meaning of the rule of rejection. It is required, by the rule, that any statement which changes the value of p(H,E) by an amount

---

[2] *Supra*, Chapt. II, pp. 41-43.

greater than φp be included in the evidence set. The question can still be asked, what statements are these? or, What does the rule of rejection insure as to inclusiveness? Attempts at answering these questions would raise more problems than we are prepared to deal with in the confines of this thesis, or, perhaps, out of the confines of this thesis. We might reply that only statements which express evidence of more than a certain relevance (where that relevance is measured by φp) are included. But at this stage of inquiry, such a comment is akin to the pronouncement of Molière's physician about the dormative power of opium. Or, if we chose to consider the answer genuinely informative, i.e. if we assume that amount of evidence is distinct from degree of confirmation, we then involve ourselves in a commitment to explain this difference. Kemeny and Oppenheim, as we have mentioned, make such an attempt at defining what they call 'factual support'.[3] And we should be in agreement, if not with their results, at least with the tenor of the effort. It seems intuitively plausible that degree of confirmation __is__ distinct from factual support and that the two concepts should be related in some vital way. If, however, this second horn of the dilemma is chosen, we should then be required to show how it is that increment of degree of confirmation by a statement measures or is functionally related to factual support. In short, the problem

---

[3]Kemeny, __op. cit.__

then becomes one of establishing a functional relationship between two disparately defined empirical properties. Thus either alternative interpretation of the question requires interpretative comments about degree of confirmation.

I should think, however, that the dilemma can be ignored without completely disastrous weaknesses of a priorism. The thesis does not purport to provide an adequate interpretation of the calculus of probability. It assumes that some adequate interpretation can be provided, and that our rule of rejection will be interpretatively adequate. There seem to be good reasons for believing that degree of confirmation will be shown to be a suitable interpretation for the calculus. Perhaps I am overly optimistic in this, but the work already accomplished by such men as Carnap and Savage[4] offers as sound a basis for optimism as does any other work in the field of probabilistic inference.

2.2 Decision situations have been classified in many ways. One mode of classification consists in calling the situation in question a case of decision under certainty, risk, or uncertainty, according to the degree of certainty inherent in the probability measure associated with alternatives.[5] If the eventuation of each alternative is known

---

[4]Carnap, Foundations. Savage, op. cit.

[5]See, for example, Luce, op. cit., pp. 275-377, where a summary discussion is presented. And R. M. Thrall, C. H. Coombs and R. L. Davis (eds), Decision Processes (New York, 1954) pp. 45-61 and 225-287.

with certainty, then the situation is one under certainty. If the eventuations are only known probably, then the situation is classified as being one under risk. If, finally, nothing whatever is known -- i.e., no probability distribution is offered over the outcome space -- then the situation is one under complete uncertainty. The scientific decision situation as we depicted it is one under risk.

The tripartite division alluded to above is worthy of mention since our classification of the scientific decision situation runs counter to the customary classification. Several writers have chosen to call the scientific decision situation a case of decision under uncertainty. Our objections to this are two. (1) The concept of decision under uncertainty is not a clear one and (2) Even if it were clear the scientific decision situation is not legitimately so classifiable.

I should think that the notion of decision under certainty could be made considerably more clear. Thus, one might define cases of decision under what might be called practical certainty. Generally, a statement x could be said to be practically certain to an individual A relative to a body of statements S when (1) the truth of x is relevant to the truth of S and (2) the truth of x remains unquestioned for A in investigating the truth of S.[6] In accordance with this notion, decision situations under (practical) certainty would be situations in which all alternatives were practically certain to eventuate in specified ways.

What should be pointed out is that an adequate definition of practical certainty will recognize the relation as depending upon an individual who believes the statement and some body of other beliefs relative to belief in which the statement is practically certain. I should think that, <u>mutatis mutandis</u>, much the same could be said for defining practical uncertainty. Thus, the eventuation of an alternative is practically certain if one of the members in the probability space is construed as close to one, and the eventuation is practically uncertain if every eventuation is construed as close to zero. Under this interpretation there can be any number of points in the space of a practically certain decision, since all that is required is that some one member be close to one, but practical uncertainty

---

6 . The sort of definition I have in mind depends upon an adequate definition of '<u>practically equiprobable</u>' which would have, roughly, the following import: If one could specify under what conditions two statements were practically equiprobable then, where an equiprobability class is a class of statements which are equiprobable by pairs; x is <u>practically certain</u> if x is a member of some equiprobability class, X, where X is defined by some probability, p*, such that p* is greater than all p(H,E) of the theory (statement set) S under consideration.

can only occur in the cases where there are very many elements in the space. This lack of isomorphism in the concepts argues that practical uncertainty is not to be made meaningful in the same manner as practical certainty, if it is to be made meaningful at all. In fact, we should say, one ought not make decisions under practical uncertainty. It might be objected that such decisions are sometimes necessary, that we must occasionally act when we have no grounds for believing that one eventuation is more probable than another. We maintain that such situations are not amenable to treatment under decision theoretical procedures, that decision theory gives no divination into the future, and is therefore incapable of advising when no knowledge is available. If the individual is not able to increment his information set before making the decision, at least to the point where the decision can be considered as a case of decision under risk, then there is no point in analyzing this information set with formal techniques which require the consideration of information sets in the making of decisions.

In addition to these objections, it seems that science can quite appropriately be considered as a case of decision under risk. Significant probabilities are usually associable with the alternatives of the scientific decision situation, and the techniques of decision theory -- as we pointed out above -- are admirably suited for the handling of such situations. To consider the scientific decision situation as one in which no information is available to the decision maker is to voice not only a damaging scepticism but also an ignorance of the confirmatory and inferential procedures available to the scientific theorizer.

3.1 Throughout the thesis we noted a simplifying assumption which could very well be damaging to any conclusions which might be attained. We assumed, in our initial generic value assignments that it was always good to accept true and reject false hypotheses and that it was always bad to accept false and reject true hypotheses. We indicated at that time that the assumption was too simple, that it might sometimes be good, e.g. to accept a false hypothesis. We should like now to examine this assumption in a little more detail and, perhaps, to find that it may not be as restrictive as might first seem.

But first, we shall discuss under what conditions the assumption could be erroneous: Scientific theories are to be interpreted as languages which make possible accurate and empirically meaningful discourse about certain fields. The field of a theory may be taken to be the union of the ranges of its lowest-level variables. Thus the field of astro-physics is all visible celestial phenomena, the field of psychology is all human behavior, the field of Game Theory (construed as an empirical-descriptive theory) is all competitive (i.e. Game) situations. If the theory in question permits one to predict and explain occurrences in the field, then the theory serves to some extent at least one of its purposes. These purposes, it might be pointed out, include, in addition to being accurate as regards purported predictions, being a viable instrument for the formation of new hypotheses.

If theories are considered thus as languages, then an important interpretative task is to consider the overall adequacy of theories as explicative of events in their fields. Assuming this to be the case, one can readily see that the number of true statements made in a theory is neither a necessary nor a sufficient condition for the excellence of the theory. There are certain important structural properties which contribute to the viability and effectiveness of theorizing and which might, indeed, be maximized in theories which contained false statements or minimized in theories which contained nothing but true ones. This is not to say that the endeavors of science are removed from truth, far from it, but it is to attend to the fact that truth -- as regards general statements of not direct empirical import -- is more a predicate applying to a language than to specific statements of a language. It has been shown that definition of one constant in terms of a set of other constants can only be made meaningful relative to a certain set of sentences, the assumptions necessary for the definition. We shall not go into the formal proof of this here, but the point is nevertheless of great import for the philosophy of science and the allied disciplines of theory construction and confirmation theory. Definitions in interpreted languages are not mere arbitrary abbreviations, they are assertions of complex relationships between different semantical and structural aspects of the language.[7] Since this is the case, the interpretation of a theory can not at all be taken to be a mere task of finding empirical states of affairs for as many of the statements of the theory as possible and then calling the other statements 'transcendental statements' and excusing their empirical meaninglessness on the grounds that they help out the interpretation of the lower level statements. The point here is that the protocol-statement thesis of empirical meaning is not effective, it is too naive and, for the most part, wrong. The language is the unit of meaning, whether the language in question be natural or artificial and just to this extent does the language determine truth.

Should we then apply the predicates 'true' and 'false' only to languages? I think not, statements are certainly true or false, but to call a statement true or false is only meaningful in the context of a certain language. If truth consists in correspondence with reality, then the language is the relation which establishes the correspondence.

---

[7]Cf. Goodman, The Structure of Appearance, chap. I.

3.2 All of this argues very strongly for our contention that the number of true statements in a theory is neither a necessary nor a sufficient condition for the adequacy of the theory. And this, I should hope, shows a part of the difficulty with our simplifying assumption. That the assumption is not an oversimplification may be shown by what follows.

Consider a decision situation of n alternatives, $\{a1, \ldots, an\}$. One makes a decision when he chooses one of the alternatives and acts so as to consummate it. If each decision situation -- each set of alternatives -- in a set of such situations can be resolved in a certain way, if there is a function defined over the ai in each alternative-set such that this function is maximized, minimized or whatever, then we say that a strategy is available for the superset of which each of the alternative-sets is a member. Thus the function f(x), if defined for each alternative set over the ai, makes possible the establishment of a strategy. Whether the strategy is a good or a bad one is here beside the point. If the function can be defined (with certain general restrictions) over each of the ai in each alternative set, then it is possible to stipulate a strategy which isolates a unique member of each set.[8] Such a strategy might

---

[8]The strategy need not, of course, isolate a unique member in each decision set: The decision set is mapped to another set such that members of the decision set, $x_1, \ldots x_n$ which result in equivalent values for u(x) are mapped to the same element of the second set, x'_k. Each member of the decision set is thus mapped to some x'_k, and to choose some x'_k is to choose any one of n alternative equivalent strategies in the decision set.

be simply, "Maximize f(x)." For simple games, the strategy is usually extensionally specified, e.g., "at move 1 make choice a, at move 2 make choice a2, ...". Given a set of k alternative-sets, each of which contains n alternatives, there are n^k strategies specifiable. A strategy, then, can be considered as a class or a set of decisions, where this set is defined by the strategy-function.

But the descriptive task need not end here, it can, in fact, be made infinitely regressive. Given a set of strategy functions, we can still define functions which take the strategy functions as values. Thus; $F[\varphi(x)]$, $G[\varphi(x)]$...etc., where '$\varphi(x)$' is variable. Each of these higher order functions may be called a <u>policy</u>, and a <u>policy decision</u> is then a decision as to which policy to choose. Thus, if we believe in the maximin theorem, a good policy is the one which says "Maximize u(x).", but bear in mind that there are many alternatives to this policy. If I am gambling on a roulette wheel, I may choose that policy which ensures that I shall lose at most so much, or that policy which ensures that I win at least so much, or that policy which ensures that I win at most so much if it rains in Tibet and at most so much otherwise, etc. ad infinitum.

In the scientific decision situation as we have pictured it in chaps. II and III, a decision is made when one accepts (rejects) a given hypothesis. A strategy as a means of determining a priori the making of decisions would be e.g., "Always accept the hypothesis with the highest degree of confirmation." A policy -- the policy we have advocated, in fact -- would be "Act so as to maximize u(x), where u(x) is defined over the field of hypotheses as a function of importance and degree of confirmation." The policy then dictates choices of strategy for specific alternative sets, that strategy always being chosen which maximizes u(x).

The assumption that it is always good to accept true, always bad to accept false hypotheses, etc. is made in the establishment of that policy, it is made as a part of defining u(x). This, let us point out again, is only one of a number (probably infinite) of policies which might be chosen. Before one could describe an interesting subset of these policies it would be necessary that more information be available as to what exactly have been the results of accepting and rejecting various hypotheses. In the absence of such information we venture to assume that the policy outlined in the preceding chapters is a relatively safe one. That it is, in fact <u>the best policy of those which can presently be specified</u>.

It is our contention, then, that the assumption is justified. That those cases in which a violation of the policy (and thus adherence to another policy) would be advantageous cannot presently be specified with adequate accuracy so as to make reasonable definition of the alternative and superior policies possible. We maintain that until such specification is forthcoming, our policy is the best one.

4.1 The conclusions of the thesis may be summarized as follows: The scientific decision to accept or reject a hypothesis is in part a decision of value and, thus, such decisions are liable to value theoretical scrutiny just as are other decisions of a legal or moral nature. More generally, the distinction of 'fact' from 'value' is an artificial one and, though perhaps useful, is not to be sanctified with any priveleged ontological status. Our knowledge and our values are not only interdependent but in large part indistinguishable. A theory of value which aspires to relevance must recognize this and engender appropriate relations with a theory of knowledge. Conversely, theories of knowledge must recognize the ineluctability of valuational considerations in the attainment of knowledge.

The scientific decision -- as an evaluation -- is subject to scrutiny with respect to goal orientation and probable results of a deontological sort. "Which hypothesis ought the scientist to accept?" is a genuine moral question and is to be treated with all the ethical seriousness usually lavished upon questions of non-scientific obligation.

In the preceding chapters some attempt has been made to specify a mode of making these valutional commitments explicit in scientific decisions. The attempt is, perforce, narrowly conceived and only recognizes value in the form of 'truth claims' of science. We should hope, however, that the feasibility of certain modes of evaluation has been adequately indicated; that it is fruitful to utilize the formal tools becoming available for the analysis and self-conscious (recognition) of values in science. Science is not to be conceived of as an undirected machine which requires the armchair decisions of moralists to point it in the 'right' direction, it is to be conceived of as a method of human inquiry, perhaps the best available method of human inquiry, and it is directed -- for better or worse -- by the humans who use it, not by their self-appointed moral advisers. If the scientist thinks of himself as a pure fact-seeker, he is thinking a harmful lie; he is just as morally responsible for the results of his decisions as is anyone.

I had hoped to be able -- after introducing utility-theoretical notions as applicable to scientific decisions -- to deal with some of the problems resultant upon this introduction. The application of the theorems of game theory is of particular interest, e.g. the making meaningful of the notion of a mixed strategy; and the consideration of other sorts of value than that resident in truth claims. The brief thesis, however, is perhaps already overburdened by the plethora of problems which cropped up in its conception, and it is felt that further speculation without resolution of some of these problems would exceed the bounds of philosophical prudence. Some solution of the problems raised and speculation on the bases of these solutions might be the task of a larger and, it is hoped, forthcoming work.

BIBLIOGRAPHY

Blackwell, David with M. A. Girshick. Theory of Games and Statistical Decisions. New York: J. Wiley and Sons, 1954.

Braithwaite, R. B. (1). Scientific Explanation. Cambridge: Cambridge University Press, 1955.

Braithwaite, R. B. (2). The Theory of Games as a Tool for the Moral Philosopher. Cambridge: Cambridge University Press, 1955.

Carnap, Rudolf (1). "Empiricism, Semantics and Ontology", Revue Internationale de Philosophie, XI (1950), 20-40.

Carnap, Rudolf (2). Logical Foundations of Probability. Chicago: University of Chicago Press, 1950.

Carnap, Rudolf (3). "Testability and Meaning", Philosophy of Science, 3, 1936 and 4, 1937. Also found slightly abridged in Feigl and Brodbeck, Readings, pp. 47-92.

Coombs. C. H., See Thrall.

Craig, William. "Replacement of Auxiliary Expressions", Philosophical Review, 65 (1956), 38-55.

Davidson, Donald and P. Suppes with S. Siegel. Decision Making. Stanford: Stanford University Press, 1957.

Davis, R. L.. See Thrall.

Dewey, John (1). Human Nature and Conduct. New York: Henry Holt and Co., 1922.

Dewey, John (2). The Quest for Certainty. New York: Minton, Balch and Co., 1929.

Girshick. M. A., See Blackwell.

Goodman, Nelson (1), Fact, Fiction and Forecast. Cambridge, Mass: Harvard University Press, 1955.

Goodman, Nelson (2). "On Infirmities of Confirmation Theory". Philosophy and Phenomenological Research, VIII, (1947).

Goodman, Nelson (3), The Structure of Appearance. Cambridge, Mass.: Harvard University Press, 1953.

Hempel, C. G. (1), "Fundamentals of Concept Formation in Empirical Science". Encyclopedia of Unified Science, II; 7. Chicago: University of Chicago Press, 1952.

Hempel, C. G. (2) with Paul Oppenheim. "The Logic of Explanation". Philosophy of Science, XV, (1948). Also found in Feigl and Brodbeck, Readings, pp. 319-352.

Hempel, C. G. (3), "The Theoretician's Dilemma". Minnesota Studies, Vol I, 37-98.

Kemeny, J. G. with Paul Oppenheim. "Degree of Factual Support". Philosophy of Science, XIX, (1952), 307-324.

Keynes, John M., A Treatise on Probability. London: Macmillan and Co., 1921.

Khinchin, A. I. Mathematical Foundations of Information Theory. New York: Dover Publications, 1957.

Lewis, C. I. An Analysis of Knowledge and Valuation. Chicago: Open Court Publishing Co., 1945.

Luce, R. D. and H. Raiffa. Games and Decisions. New York: John Wiley and Sons, 1957.

Morgenstern, Oscar. See Von Neumann (2).

Nagel, Ernest. "Principles of the Theory of Probability". Encyclopedia of Unified Science I;6. Chicago: University of Chicago Press, 1939.

Oppenheim, Paul. See Hempel (2) and Kemeny.

Quine, W. V. (1). "On Carnap's Views on Ontology". Philosophical Studies, VIII (1951).

Quine, W. V. (2). From a Logical Point of View. Cambridge, Mass.: Harvard University Press, 1953.

Quine, W. V. (3). Methods of Logic. New York: Henry Holt and Co., 1959, (Revised Edition).

Raiffa, Howard. See Luce.

Ramsey, F. D. Foundations of Mathematics. London: Routledge and Kegan Paul, 1931.

Reichenbach, Hans. "Logical Foundations of the Concept of Probability". Feigl and Brodbeck, Readings, 456-474.

Rhine, J. B. New Frontiers of the Mind. New York: Rhinehart and Co., 1937.

Rudner, Richard (1). Can Science Provide Ethical Bases? Read before the Philosophy Colloquium, Michigan State University, Spring, 1958.

Rudner, Richard (2). "The Scientist Qua Scientist Makes Value Judgments". Philosophy of Science, XX, (1953), 1-6.

Russell, Bertrand. Human Knowledge, Its Scope and Limits. New York: Simon and Schuster, 1948.

Savage, L. J. Foundations of Statistics. New York: J. Wiley and Sons, 1954.

Siegel, Sidney. See Davidson.

Stevenson, C. L. Ethics and Language. New Hartford, Conn.: Yale University Press, 1944.

Suppes, Patrick. See Davidson.

Thrall, R. M., C. H. Coombs and R. L. Davis (Eds.). Decision Processes. New York: J. Wiley and Sons, 1954.

Von Neumann (1). "Zur Theorie der Gesellschaftsspiele". _Mathematische Annalen_. c (1928).

Von Neumann, J. (2) and Oscar Morgensen. _The Theory of Games and Economic Behavior_. Princeton, 1944.

Wald, Abraham. _Statistical Decision Functions_. New York: J. Wiley and Sons, 1950.

READINGS

Minnesota Studies in the Philosophy of Science. 2 vols. Minneapolis: University of Minnesota Press, 1958.

Readings in the Philosophy of Science. H. Feigl and May Brodbeck, Eds. New York: Appleton, Century Crofts, 1953.