PROBABILITY AND EVIDENCE*

I. SYNCHRONIC AND DIACHRONIC RATIONALITY

In 'The Aim of Inductive Logic', Rudolph Carnap has offered a clear statement of his conception of the way in which Inductive Logic or Confirmation Theory contributes to a theory of rational probability judgment.[1]

A theory of rational probability judgment is an integral part, according to Carnap, of a theory of rational decision making. Degrees of (subjective) probability, degrees of belief or degrees of credence, as Carnap calls them, are psychological dipositions to take risks of certain kinds and to avoid risks of other kinds. The degree of credence $Cr_{X,T}(H)$, which an agent X assigns to a sentence H at a time T, is the highest betting quotient at which X is willing to bet on H at time T.

A psychological study of credence would examine the general laws relating degrees of credence assigned to different sentences (or propositions) by an agent X at a given time and the manner in which degrees of credence accorded by X to given sentences vary over time. The agent's credence function at a given time represents his 'state' of probability judgment at that time. It is a point in a credal configuration space. Over time, the agent moves through credal configuration space on a path determined by prior points and other factors (such as the acquisition of new evidence).

A psychological theory of credence can, therefore, be divided into two parts: (a) Synchronic theory concerned with general cross sectional laws determining relations between values of credence functions for different arguments at a given time, and (b) diachronic, dynamic or learning principles which relate credence functions at given times.

Carnap is not concerned with psychology but with a theory of rational credence as a system of norms or prescriptions controlling the manner in which men ought ideally to fix their probability judgments. He contends that such a theory makes prescriptions regarding how an agent ought to fix his credence judgments at a given time and regarding the manner in which he should revise them over time. In other words, Carnap's normative theory is intended to parallel psychological theory in having a synchronic and a diachronic part.

Thus, Carnap invites us to think of a theory of rational credence as providing a blueprint for the design of a robot who at every moment of time would conform to the requirements of synchronic rationality and would revise his credence judgments over time in accordance with the dictates of diachronic rationality. By thinking about such robots, advice can be found for human beings in "their effort to make their decisions as rational as their limited abilities permit".[2]

Carnap's conception of the aim of a theory of rational probability judgment is highly misleading. If taken seriously, it presupposes that an ideally rational man is one who *at every instant of time* (or, at least, during every minimal interval of time) conforms to the conditions of synchronic rationality and whose judgments change from instant to instant in accordance with the dictates of diachronic rationality.

In order to construct a theory of this sort which will be applicable in real life, heavy reliance will have to be placed on our knowledge of human capacities for learning. Without more knowledge of the psychological processes involved in fixing credence judgments than we actually have, we have no grounds for claiming that any theory of diachronic rationality which we propose will be one to which ordinary mortals can conform even to a good degree of approximation.

Patrick Suppes has clearly and effectively argued this point. Although he apparently agrees with Carnap's conception of the aim of a theory of rational probability judgment and the image of an ideally rational man underlying it, he regards Carnap's own theories of rational probability judgment as inadequate:

The difficulty and subtlety of characterizing the mechanisms of information selection and at the same time a recognition of their importance in determining the actual behavior of men make me doubt that the rather simple Carnapian conception of inductive logic can be of much help in developing an adequate theory of rational behavior. Even the more powerful Bayesian approach provides a very thin and inadequate characterization of rationality because only one simple method for changing beliefs is admitted. It is my own view that there is little chance of defining an adequate concept of rationality until analytic tools are available to forge a sturdy link between the selection and use of evidence and processes of concept formation.[3]

This passage together with several other remarks clearly indicate that Suppes recognizes the psychologistic implications of Carnap's conception of rationality.[4] Any adequate diachronic theory of rational probability judgment must, in order to be applicable, be such that human beings can conform to its dictates. Such applicability can be determined only through studies in empirical psychology. At present, we do not have enough knowledge of such matters upon which to construct an adequate diachronic theory of rationality. Consequently, students of rational probability judgment and inductive logic in Carnap's sense ought to redirect their attention to learning theory. Suppes shares Carnap's image of an ideally rational man (or seems to) and bravely accepts the psychologistic consequences.

Those of us who are more timid see in Suppes' arguments a reductio ad absurdum. Carnap's image of an ideally rational man as one who meets conditions of rationality at every instant of time and revises his judgments in accordance with diachronic conditions from instant to instant is questionable precisely because it has the implications which Suppes sees so clearly.

But what would an alternative approach to Carnap's be like? Suppose that an agent indicates commitment to a given system of credence judgments. Either he or someone else notices that they deviate from certain conditions of rationality. The agent is not considered irrational for having deviated from these conditions. Perhaps, he was unaware of his deviation. Moreover, even if he were aware of it, he might not have been in a position to determine how he should adjust his beliefs in order to conform. But having recognized a deviation, he should, if he is rational, be committed to looking for a way to conform to standards of rationality.

Thus, recognition of a deviation from standards of rationality is not a sign of irrationality on the part of an agent unless the agent refuses to recognize any point in engaging in deliberations designed to rectify the deviation. A recognized deviation is a sufficient reason for embarking upon an inquiry. Consequently, conformity to conditions of rationality is a necessary condition for regarding one's situation as nonproblematic and inquiry as pointless.

Thus, principles of rationality can be regarded as stating necessary conditions for being in a certain kind of equilibrium in which critical review of one's probability judgments is unreasonable. These conditions are not

sufficient; for one's beliefs might conform to conditions of rational probability judgment and yet one might inquire in order to obtain new information (or for other motives). Nonetheless, to consider principles of rational probability judgment as conditions of 'rational equilibrium' suggests a quite different conception of the aim of a theory of rational probability judgment than that implied by Carnap and Suppes.

The chief point is that the distinction between synchronic and diachronic rationality loses its significance. All conditions of rationality are analogous to equilibrium conditions. In a sense, they are synchronic conditions; but they are not conditions which an agent must satisfy at every instant of time. Furthermore, in stating conditions of rational equilibrium, no prescription is made regarding the psychological path to be taken in moving from disequilibrium or from one equilibrium position to another. In other words, there are no norms prescribing rational learning processes.

Such a theory of rational probability equilibrium has, nonetheless, something to say about how probability judgments are to be revised in the light of new evidence. Consider the 'neoclassical' or 'marginalist' theory of consumer demand. Given a consumer's utility function, his income and the prices which he faces, that theory provides an account of how the consumer would allocate his income to the purchase of various commodities if he is 'in equilibrium'. Moreover, if some of the independent factors (for example, the consumer's utility function and his income) remain constant but other factors (e.g., the prices) change, the theory specifies what the new equilibrium demand would be. The theory can not predict, however, that if the prices do change under the circumstances described the consumer will attain the new equilibrium position or if he does how long the process of readjustment will take and what the behavioral and psychological properties of the readjustment process will be like. The theory of how demand responds to price changes is, in widely used jargon, a 'comparative static' theory which studies shifts from one equilibrium position to another.[5]

According to the view which shall be adopted here, a theory of rational probability revision is a normative analogue of such a comparative statical theory. Obtaining new evidence is analogous to a change in prices. The theory determines a new position of rational equilibrium when additional evidence is obtained. It makes neither predictions nor recommendations

as to how the equilibrium is to be attained but only recommends that ceteris paribus it should be.

To enjoin an agent to move toward rational equilibrium presupposes that the agent is able to do so. 'Ought' does after all imply 'can'. Thus, psychological considerations are, as Suppes insists, relevant to appraising the applicability of a theory of rational probability judgment even when interpreted as a comparative statical theory. But the abilities ascribed to an agent are of a rather different order on a comparative statical view than those which a theory prescribing the details of the psychological processes involved in attaining equilibrium presuppose. Indeed, judgments as to the feasibility of attaining equilibrium by some path or other can quite frequently be made on the basis of rather casual observation of human behavior.

For example, it does not take great learning in psychology to recognize that if a Bayesian theory of rational probability judgment is adopted then in some situations an agent will be incapable of making the calculations required to determine what his equilibrium is to be.

The same type of situation also suggests that the remedies for such difficulties in the application of Bayesian theories are not likely to be found by concentrating primary effort in the study of learning theory. Improved computer technology which will cheapen the cost of calculations looks more promising. In any event, if learning theory will be of help here, it is not likely that it will do so by pointing the way to a better theory of rational probability judgment but by devising techniques for enabling agents to calculate more quickly and exploit their memories more effectively. One ought not to be dogmatic here; but, at the present, Suppes' exhortation to students of probability and induction to bend their efforts to a study of learning remains unconvincing.

These remarks about the relations between studies of rational probability judgment and psychology are predicated on the assumption that students of rational probability judgment should discard Carnap's conception of such a theory as providing a design for a perfectly rational robot or learner characterizing in some detail the processes involved in changing probability judgments in response to the acquisition of new evidence or sensory stimulation.

Among the casualties resulting from the abandonment of this approach are principles of conditionalization construed in a diachronic, kinematic

or learning theoretic manner. So is the surrogate for conditionalization proposed by Richard Jeffrey. However, alternative formulations of conditionalization principles might be found which are compatible with an equilibrium and comparative statical approach to rational probability judgment. Such principles would be relatively immune to the objections raised by Suppes against diachronic principles of conditionalization. In subsequent sections, attention will be given to several equilibrium and comparative statical versions of conditionalization.

II. SUBJECTIVISM

Following Carnap, the judgments made by an agent X at a given time T regarding fair betting quotients will be represented by what he calls a 'credence function' $Cr_{X,T}(H)$. We shall assume, for the present, that the credence function is defined for all sentences in X's 'language' L – whatever that is.

The least controversial requirement for rational probability or 'credence' judgment is the following:

(I) $Cr_{X,T}(H)$ is strictly coherent.

As is well known, the requirement of strict coherence implies that the credence function at a given time must be a regular probability measure.[6]

According to the comparative statical approach adopted here (I) is a necessary condition for rational equilibrium. If a violation of coherence requirements is detected in X's credence judgments, he need not be considered irrational provided that he recognizes the need to readjust his judgments to avoid the violation.

Let K be the set of sentences which X accepts as evidence at T. According to (I), if X is in equilibrium, each element of K is accorded a credence value of 1. Moreover, K is a deductively closed set. We do not need to suppose that each member of K is explicitly listed. There are other ways in which X can characterize K – for example, by listing some subset of K and stating that K consists of the deductive consequences of items on the list.

One important feature of condition (I) is that if H is any sentence neither in K, nor inconsistent with an element in K, then for every real number r between 0 and 1 there is a credence function satisfying (I) which assigns

r to H. If (I) were the sole condition for rational equilibrium, the evidence available to an agent X would impose only very weak constraints on equilibrium judgments of credence for sentences whose truth or falsity is not deducible from the evidence. Consequently, one could not exploit empirical information to revise credence judgments.

Suppose that at T, X's total evidence is K. Between T and T', he 'acquires' in some sense additional evidence E so that his new total evidence becomes $K \& E$. What should his credence function, in equilibrium, be like at T'?

Let X's function $Cr_{X,T}(H) = f(H)$ having the following properties:

$f(H) = r$

$f(H/E) = f(H \& E)/f(E) = k$.

According to a widely held view, $Cr_{X,T'}(H)$ should equal $f(H/E) = Cr_{X,T}(H/E)$. The principle upon which this is usually based is the diachronically interpreted principle of conditionalization. However, there is a way to construct a principle of conditionalization similar to this one acceptable within the framework of a comparative statical theory.

(II)   If $Cr_{X,T}$ is an equilibrium function at T when K is the total evidence and E alone is acquired between T and T', then $Cr_{X,T'}$ is a new equilibrium credence function if and only if for every H, $Cr_{X,T'}(H/E) = Cr_{X,T}(H/E)$.

Given that (I) implies that $Cr_{X,T'}(E)$ must in equilibrium be equal to 1, (II) implies that $Cr_{X,T'}(H) = Cr_{X,T}(H/E)$.

Observe that (II) does not recommend that $Cr_{X,T'}(H)$ equal $Cr_{X,T}(H/E)$, but only that the equality obtain if X's credence judgments at T' are in rational equilibrium.

Now if the total evidence K at T does not uniquely determine the equilibrium credence function adopted by X at T, condition (II) does not by itself determine uniquely how credence judgments ought to change.

Suppose, for example, that not only is $f(H)$ a possible equilibrium credence function at T but that $g(H)$ is also an equilibrium function, where $g(H) = r'$ and $g(H/E) = k'$. Assume that X actually was in equilibrium at T and adopted $f(H)$ as his credence function. Condition (II) does not prevent X from adopting $g(H/E)$ as his equilibrium credence function at T'.

The same point can be made from a different direction. If X did not obtain any more evidence after T but remained with the same total evidence K, this does not mean that X cannot move from one equilibrium position to another. He can, according to (I) and (II), adopt $f(H)$ as his credence function at one time and $g(H)$ at another.

This result cannot be avoided by strengthening (II) with an additional rider which constrains X to stick with the equilibrium credence function which he first adopts unless his evidence changes and to conditionalize relative to that credence function when he obtains new evidence. Why should X be saddled with his past commitments in this way when, at the time when he made the commitment, it would have been perfectly rational for him to have adopted other commitments?

Writers on probability of subjectivist and personalist persuasion have often contended that they are able to accommodate the idea that empirical evidence does contribute to determining credence judgments. Yet, they insist that two agents sharing the same evidence can both be in rational equilibrium and have different – indeed, radically different – credence functions. They attempt to escape from extreme subjectivism here precisely by adopting a principle of conditionalization which is at worst untenably diachronic and at best saddles agents with their past judgments in a manner which cannot possibly be defended on their own principles.[7]

In my opinion, if we are to take seriously the idea that probability – i.e., credence – judgments ought to be revisable in the light of empirical evidence, we must be prepared to admit that empirical evidence does in some sense objectively confirm or disconfirm hypotheses. Thus, in spite of the many important contributions which personalists have made to probability theory, we must regard their position in the final analysis as excessively subjectivist and utterly incompatible with an empiricist epistemology.

One further point ought to be made before concluding this section. Condition (II) by itself adds no additional strength to condition (I). If the truth or falsity of H is not entailed by either K or $K \& E$, then for every r between 0 and 1 there is an equilibrium credence function when the evidence is K and an equilibrium function when the evidence is $K \& E$ such that H is accorded the credence value r by both credence functions. This emphasizes in another way the critical point already made. Unless we can add equilibrium conditions which specify the way in which the

total evidence K constrains the credence function, no comparative statical theory can offer an account of the way additional evidence alters the equilibrium credence function.

III. ON TOTAL EVIDENCE

One way in which one might hope to develop a suitable theory is through a variant of Carnap's own approach. If a measure of confirmation $c(H, K)$ suitable for X's language can be constructed, then the following equilibrium condition can be introduced.

(III)   If X's total evidence at T is K, then X's equilibrium credence function $\mathrm{Cr}_{X, T}$ at T is such that $\mathrm{Cr}_{X, T}(H) = c(H, K)$.

Observe that (III) is, in a sense, a form of conditionalization. Yet, it is neither diachronic nor does it involve explicit comparisons of credence functions in equilibrium at different times. Yet, it surely permits a rather powerful comparative statical theory. For example, if at T, X's total evidence is K and at T' is $K \& E$, it does imply that $\mathrm{Cr}_{X, T'}(H)$ should equal, in equilibrium, $\mathrm{Cr}_{X, T}(H/E)$. Here the objections raised previously do not hold; for the equilibrium positions both at T and T' are uniquely determined by the total evidence so that this relation obtains.

But this Carnapian approach has some well-known difficulties of its own. The notion of an a priori determined confirmation function is unacceptable on rather solid grounds to many who, though committed to the view that confirmation relations are objective, consider them to be empirical as well. Furthermore, Carnap has faced considerable technical difficulties in constructing suitable confirmation measures for even moderately rich languages.

But even if these objections and difficulties could be overcome, Carnap's confirmation functions would have no applicability in real life; for no one could ever under any realistic circumstances attain rational equilibrium. In order to attain rational equilibrium, X would have to determine what his equilibrium credence function is. To do this, he would have to identify his total evidence in such a manner as to allow him to use Carnap's confirmation function via (III) to determine his credence function. No one can review his total evidence in the manner required.[8]

In what sense does Carnap's theory require that the total evidence be

explicitly given before his confirmation measures are applied? The total evidence must be characterized as the deductive consequences of a specific list of axioms or of a set of axioms characterized by axiom schemata. It is only for evidence cast in some form such as this that Carnap's measures enable one to determine degrees of confirmation for hypotheses. And it is precisely such explicit statement of total evidence that no ordinary agent is able to provide.

There are, however, other ways of characterizing the total evidence which permit (III) to apply. X's total evidence at T might be described as confirming H to degree r. That is to say, the metalinguistic predicate $c(H, -) = r$ is declared to be true of the deductively closed set of sentences constituting X's total evidence. There may be several deductively closed sets of sentences in the language under consideration of which this predicate is true. However, in order to apply condition (III), it is not necessary to identify which of these closed sets constitutes X's total evidence. Since X's total evidence is trivially X's total evidence and $c(H, X$'s total evidence$) = r$, condition (III) recommends that in equilibrium $\mathrm{Cr}_{X, T}(H) = r$.[9]

Suppose that X cannot declare that $c(H, X$'s evidence at $T) = r$, but only that $c(H, X$'s evidence at $T)$ equals either r or s or falls in the interval between r and s, or is greater than $c(G, X$'s evidence at $T)$. In such an event, condition (III) does not prescribe a unique credence value for H at T. Nonetheless, it does constrain the credence value – e.g., in the first case, to the values r and s. There is an element of arbitrariness in the choice of a credence function here as long as the only characterization of X's total evidence is of the type given. But this concession to subjectivism is not excessive. After all, it may be possible for X to obtain more evidence and thereby constrain his credence judgment more definitely. If he has the opportunity of looking for such evidence before using his credence function to make a decision, he should do so. Otherwise, he will, indeed, have to select a credence function arbitrarily. But a theory of rational equilibrium should recognize that if an agent is not able to attain a suitable equilibrium, he may have to adopt a 'second best' approach and that such an approach might involve some arbitrary choice. A theory which insists that the scope of arbitrary choice, can, in principle, be indefinitely reduced recognizes this but does not, therefore, revert to extreme subjectivism.

But isn't the escape from subjectivism bogus? Suppose that X declares

his background to be a K such that $c(H, K) = r$, whereas Y declares his background to be K' such that $c(H, K') = s$. Is there any objective way to settle their differences?

Even if it were true that what is counted as evidence is an arbitrary affair, the subjectivism which results is different from the subjectivism popular among Bayesians according to which given evidence K and a sentence H not entailed by K for every r between 0 and 1 there is a legitimate credence function which accords H the value r. According to the idea suggested here, credence judgments would be constrained by empirical assumptions.

Moreover, there is surely no need to grant without further ado that what is admitted as evidence is an arbitrary matter.

Nonetheless, the objection carries some weight – or seems to. To claim that the evidence is K such that $c(H, K) = r$ is, indeed, an empirical claim. But unless the set K is recharacterized in a manner which allows its contents to be explicitly given, it is not clear that the truth or falsity of members of K can be checked by confrontation with observation reports. Given that no one can render his total evidence K explicit in this manner, assumptions about empirical evidence of the sort considered here are immune to serious empirical criticism.

Consider, however, that even though X cannot explicitly characterize his total evidence, he can surely explicitly cite some of it. Moreover, there is no relevant a priori or psychological constraint on which part he can render explicit and which he cannot. Our inability to explicitly review all of our empirical evidence all at once means, therefore, that we cannot bring all of the empirical assumptions upon which we base our credence judgments under critical scrutiny at the same time. But it does not mean that the total evidence is immune from criticism in a piecemeal fashion.

Thus, on many occasions it would be inappropriate for X to characterize his total evidence as a set of sentences K such that $c(H, K) = r$. If his credence judgments are being challenged, he might be required to cite explicitly evidence upon which his judgments are based. Let his explicitly cited evidence consist of propositions expressed by sentences E. X would then characterize his total evidence as consisting of the deductive consequences of the conjunction of E and B such that $c(H, B \& E) = r$. He might further characterize his implicit or background information B by identifying values of $c(H, B)$, $c(E, B)$, etc.

X's argument, therefore, in defense of a given credence judgment $\mathrm{Cr}_{X,T}(H)$ can then be reconstructed as follows:

(1) $c(H, \text{the deductive consequences of } E \text{ and } X\text{'s background information}) = r$.

(2) X's total evidence at T consists of the deductive consequences of E and X's background evidence at T.

(3) $\mathrm{Cr}_{X,T}(H) = r$.

This mode of argument renders pointless any concern about constructing confirmation functions. Given that we cannot explicitly characterize total evidence, we cannot directly apply them; and in order to make evidential assumptions sufficient to justify credence judgments, we do not need them.

Certain assumptions must, of course, be made about confirmation functions. Conditions (I) and (III) imply that confirmation measures must be probabilistic in structure. Moreover, it is assumed that statements such as $c(H, K) = r$ for explicit K and H are objectively true or false. This implies no more than that evidence objectively confirms hypotheses to various degrees.

Notice that we are free to regard statements like '$c(H, K) = r$' for explicit H and K as empirical. To be sure, if we follow the procedure adopted here and regard confirmation functions as taking sentences and sets of sentences in a given language L as arguments, then in the typical case confirmation statements will not be expressible in L. The empirical assumptions they assert are not part of the total evidence expressible in L. But L can be enlarged to include such confirmation statements when the demand arises. Or, alternatively, L can be enlarged to include statements K' such that for the confirmation function c' defined for the enlarged L', $c'(H, K \& K') = r$. In effect, Carnap's confirmation functions can always be viewed as conditional on evidence not explicitly expressible in the languages for which these functions are defined.

Thus, even though no one can explicitly cite his total evidence, there does not seem to be an a priori or a psychological bar preventing an investigator from explicitly citing part of it and from delving further into the implicit background information B, thereby enlarging the explicit part as the occasion requires. We do not need to conclude that any of X's empirical or confirmational assumptions are immune to explicit empirical criticism.

These remarks have a direct bearing on how credence judgments are revised in the light of new evidence. Suppose that at T X accords H a credence value of r and $\mathrm{Cr}_{X,T}(H/E)=s$, where E is not part of X's evidence.

If X is taken to be in rational equilibrium at T, his total evidence X must be such that $c(H, K)=r$ and $c(H, K\&E)=s$. By acquiring new evidence E subsequent to T, X is like the consumer who faces a change in prices. He has been thrown out of equilibrium and must find a new equilibrium position.

If at T' subsequent to obtaining E, X *does not retract any of the evidence he accepted at T*, he must regard his total evidence to be $K\&E$, where K is such that $c(H, K\&E)=s$. By condition (III), in equilibrium at T', $\mathrm{Cr}_{X,T'}(H)$ should be equal to the equilibrium $\mathrm{Cr}_{X,T}(H/E)$. This is the result recommended by customary principles of conditionalization. Observe, however, that this recommendation is not based upon a general principle of rationality prescribing that new credence judgments should equal past conditional credence judgments. A crucial rider has been added. It requires that such a result obtain only if past evidential assumptions have not been modified. Since, on some occasions, such modifications are legitimately called for, conditionalization cannot be imposed as a general principle for revising credence judgments in the light of new evidence.

Two further points deserve repetition here. Equilibrium condition (III) is a version of conditionalization relating confirmation measures to credence measures. It is an equilibrium condition, however, lacking any temporal features.

The second point is that even when X does not retract any of the evidence he accepted at T, $\mathrm{Cr}_{X,T'}(H)$ need not always equal in equilibrium $\mathrm{Cr}_{X,T}(H/E)$. For X may judge (both at T and T') that K is such that $c(H, K\&E)$ equals either r or s or that it falls in an interval between r and s. In that event, X is free to adopt $\mathrm{Cr}_{X,T}(H/E)=r$ and $\mathrm{Cr}_{X,T'}(H)=s$.

Thus, situations where the equilibrium positions at T and T' warrant equating $\mathrm{Cr}_{X,T'}(H)$ with $\mathrm{Cr}_{X,T}(H/E)$ are very special. They arise when the total evidence at T determines a unique equilibrium credence value for H given E and the addition of E to the total evidence between T and T' does not involve abandoning any of the evidence accepted at T.

IV. EMPIRICISM AND FALLIBILISM

According to the approach adopted here, the manner in which credence judgments are to be revised depends critically on the empirical assumptions which the agent makes. The totality of such assumptions cannot be explicitly cited by the agent all at once or in any reasonable period of time. However, any portion of the total evidence which is not rendered explicit can in principle be made so. Consequently, an agent's credence judgments are empirically arguable – i.e., they can be critically evaluated through an examination of some of the empirical assumptions upon which they are based.

Of course, to adopt such a view presupposes that sometimes it is legitimate for an agent to accept empirical propositions as evidence and accord them a credence value of 1. If no empirical propositions could legitimately be accepted as evidence, then all credence values would perforce be based upon a priori assumptions alone. If a priori evidence suffices to warrant assigning definite credence values to empirical hypotheses, then for all time there is only one equilibrium position. If, on the other hand, a priori evidence permits arbitrary choice between several equally legitimate credence functions, that choice remains the same for all time. Both of these views are untenable. It must be legitimate, therefore, to accept empirical statements as evidence.

This implication is one which many writers find difficult to concede. To accept E as evidence is to accord it a credence value of 1. This means that the agent X regards acting on E as without risk. Many philosophers consider this reasonable only if X has an infallible warrant of the truth of E. If it is agreed that no empirical statement can be provided with such a warrant, no empirical statement should rationally be accepted as evidence.[10]

This objection is predicated upon a dubious demand for infallibility. We may, I think, quite legitimately regard acting on E as being without serious risk even though we recognize the theoretical possibility that E is false. The fact that E might be false is not, in itself, sufficient to deprive us of the right to regard E as certain. It does, however, oblige us to keep an open mind regarding the truth of E – i.e., to be prepared to remove E from its evidential status when a serious challenge is made to according it that status. The difference between a priori evidence and empirical

evidence is not to be found in differences in credence values assigned to the a priori and the empirical but in the corrigibility of empirical evidential assumptions. The a priori consists of that which is immune to revision in the face of 'recalcitrant experience'. The empirical is more sensitive. This difference between the empirical and the a priori in no wise implies that in a given equilibrium state empirical and a priori assumptions are distinguishable with respect to the credence values assigned them. When engaged in deliberation for the purpose of making a choice between alternative policies, we regard acting on E as without risk if there is no serious doubt as to the truth of E. 'Cartesian' doubts regarding the truth of E do not count.

These remarks are nothing more than a brief restatement of the fallibilist position advocated by Pierce and Dewey. Fallibilism is not to be confused with the banal view that no empirical statement can be provided with an infallible warrant. Rather it is the position which claims that we are sometimes entitled to accept empirical statements as evidence and, hence, as true even though we lack such an infallible warrant. I have attempted to argue that fallibilism is the only tenable view on the basis of which a theory of rational probability judgment can be constructed which regards credence judgments as being grounded empirically. However, Richard Jeffrey, who is an articulate spokesman for the antifallibilist view, has recently proposed an account of rational probability judgment which claims to require that credence judgments be empirically grounded without necessitiating the admission of any empirical statements as evidence.[11]

Jeffrey's approach involves making a distinction between basing credence judgments on empirical evidence and basing credence judgments on direct responses to sensory stimulation.

When X makes an observation and admits an observation report E into evidence, we can regard his accepting E as evidence and according it a credence value of 1 as a causal response to the sensory stimulation. Jeffrey contends that one can respond to sensory stimulation by shifting a credence assignment to E from one nonextreme value to another. Indeed, he argues that even in those cases where X apparently accepts E as evidence in direct response to sensory stimulation, we can regard him as assigning E a credence value so close to 1 'as to make no odds'. Strictly speaking, a rational X never assigns a credence value of 1 to any empirical statement. Yet X's credence judgments are empirically based in the sense

that some of them are direct responses to sensory stimulation and the others are determined through conditions of rationality by those which are direct responses.

According to Jeffrey, the salient feature of credence judgments made in direct response to sensory stimulation is that the agent is compelled by the stimulation to make those judgments. Sensory stimulation in no wise justifies the responses but causes them to occur. Such compulsory credence judgments provide the empirical basis for all probabilistic belief.

Jeffrey does not maintain that compulsory credence judgments of the sort mentioned are invariably sound or legitimate. Nor need they be permanent. "New experience or new reflection (perhaps, prompted by the arguments of others) may lessen the bonds of compulsion, and may then establish new bonds; and the effect may be that the new state of belief is sounder than the old, or less sound."[12]

Why are compulsory responses reliable?

Quite simply put, the point is: coming to have suitable degrees of belief in response to experience is a matter of training – a *skill* which we begin acquiring in early childhood, and are never quite done polishing. The skill consists not only in coming to have appropriate degrees of belief in appropriate propositions under paradigmatically good conditions of observation, but also in coming to have appropriate degrees of belief between zero and 1 when conditions are less than ideal.[13]

Consider two agents X and Y who have observed a piece of cloth. X observes the piece of cloth under good light and responds by according the statement that the cloth is green a credence value of .999999. Y observes the cloth under poor light but responds by assigning G the same credence value. Both respond – indeed are compelled to respond – in the same way to different stimuli. Yet, the judgment of one of them, according to Jeffrey, would not be as sound or legitimate as that of the other.

If neither X nor Y accept any empirical assumptions as evidence, neither of them can render a verdict as to the reliability of their credence judgments. Thus, neither X, Y, nor anyone else who refuses to accept empirical evidence could criticize Y's response by noting that it was made under poor light. Indeed, X could not defend his response on the grounds that it was a response to sensory stimulation made under satisfactory conditions. To do so requires admission of empirical assumptions as evidence.

Of course, if no one was concerned with critical evaluation of credence

judgments, this would not matter. If human beings had perfected their skills in responding to sensory stimulation so that they were invariably reliable, perhaps, there would be no need for criticism. As in the case of the angels, being rational would be doing what comes naturally. But to prescribe for a community of angels is like designing one of Carnap's perfectly rational robots. It is to consider a rational agent as one conforming to conditions of rationality at every instant of time. Clearly, this approach is unacceptable. We are not angels nor are we such robots. Our skills are imperfect and our responses often less than reliable. Criticism is important.

But our credal responses to sensory stimulation cannot be criticized if we make no claims about them. If we do not claim that they are responses to sensory stimulation, we are not even committed to saying that they are empirically grounded in the sense that Jeffrey requires them to be. They could equally as well be compulsory responses to changes in metabolism. Moreover, if we recognize a distinction between reliable and unreliable responses to sensory stimulation, as Jeffrey himself does, we must defend our credal responses by claiming that they are made under satisfactory conditions. Otherwise, no commitment is made regarding the satisfactoriness of the judgment at all. The response remains immune to relevant criticism.

Jeffrey seems to think that the fact that credal responses to sensory stimulation are compulsory is important. Being compulsory, we cannot help having these responses. We must, if we are rational, adjust our other judgments to fit in with these compulsory responses.

In my scientific and practical undertakings I must make use of such compulsory beliefs. In attempting to understand or to affect the world, I cannot escape the fact that I am a part of it. I must rather make use of that fact as best I can.[14]

Of course, we must do what we must do. This is true of credal responses to changes in blood pressure or of unreliable responses to sensory stimulation as much as it is of reliable responses. There would be little point in criticism if once one is compelled to credence judgments, one was compelled to stick with the judgments indefinitely. But if such judgments can appropriately be criticized, it must in general be true that such responses can be discarded in due time. If so, we are not constrained to make use of all of our compulsory beliefs. When we are faced with making

decisions at the very instant when the beliefs are compulsory, we cannot help ourselves. In general, however, this will not be the case – at least for those cases where criteria of rationality have any applicability. Such criteria are designed for situations where an agent is capable of deliberation and can, in due time, remove the shackles of compulsion.

To be sure, an empiricist will require that some items admitted into evidence or some credence judgments should be based on reliable responses to sensory stimulation. But it is not the compulsory character of such responses which is decisive. It is of far greater importance that the responses be to sensory stimulation and, still more important, that they be reliable responses.

Perhaps, Jeffrey does not mean to deny that agents do admit empirical assumptions into evidence. He may only intend to deny that evidence is ever explicitly stated by citing sentences translatable into some suitably formalized language L. Thus, when X assigns a credence value of .999999 to G, he may declare that he has made a response under satisfactory circumstances to sensory stimulation. He has admitted a certain assumption into evidence; but he has not and may not be capable of rephrasing that assumption in a manner which indicates in more specific form what the conditions are. Jeffrey's point would then be that an agent can no more completely characterize the conditions of observation than he can explicitly cite his total evidence.

If this is, indeed, Jeffrey's point, it has the same merits as the observation that no one can explicitly cite his total evidence. This is indeed true. However, it is also true that one can explicitly describe some features of the conditions of observation. Moreover, the precision with which they are described is not limited in any a priori way; and if responses to sensory stimulation are to be critically evaluated, some explicit description of the conditions of observation will be required.

Thus, it seems to me that not only must an agent tacitly accept empirical statements as evidence. He must be capable of rendering some part of what is implicit explicit.

In sum, it seems to me that Jeffrey has failed to drive the wedge between responses to sensory stimulation and admitting empirical statements into evidence which would be required to avoid allowing agents to accept empirical statements into evidence and still retain the claim to an empiricist epistemology.

Empiricists must admit that some empirical statements can legitimately be accepted as evidence and accorded a credence value of 1. If they also admit that no empirical statements can be afforded an infallible guarantee, they must also adopt a fallibilistic point of view. Jeffrey's shrewd attempt to avoid a commitment to fallibilism does not succeed in nullifying this verdict.

V. ON RATIONAL ACCEPTANCE

The fallibilist and empiricist position adopted here insists that it is sometimes legitimate to accept as evidence and, therefore, to assign a credence value of 1 to empirical statements for which no infallible guarantee can be given. Such statements are always open to theoretical doubt; for it is logically possible that they are false. But judgments of risk upon which actions are based do not take into consideration such theoretical doubts. Only serious doubts are relevant to determining risk. A proposition whose truth is not judged problematic at a given time is a proposition to which a credence value of 1 can legitimately be assigned. Acting on it is without serious risk.

Thus, when X accepts E as evidence he regards accepting E as true as nonproblematic. Such acceptance may be caused by sensory stimulation. But cultural and psychological factors also determine what is accepted as evidence. There is nothing a priori wrong with accepting statements as evidence in response to a wide variety of factors. As long as no serious challenge is raised against such acceptance, they remain nonproblematic.

However, no evidential assumption is immune to serious challenge. When such an assumption is questioned, a rational agent will remove it from his evidential corpus and attempt to determine on the basis of the remaining nonproblematic evidence whether the problematic item should be reinstated as nonproblematic evidence or replaced by some other item.

If we are to avoid subjectivism and relativism, we must assume that objective standards regulate revision of the evidential corpus. Objectivity does not require that our initial evidential corpus be determined by objective standards but that the standards whereby that corpus is revised and corrected be objective.

One way in which the evidential corpus is revised is through observation. When observations are made under conditions which are nonproblematically reliable, the statements accepted as evidence in response to

sensory stimulation are nonproblematically accepted as evidence. New evidence obtained by observation can subsequently be questioned. It may be granted that observations made under conditions C are reliable but it may be seriously doubted whether the observations to which the report in question was a response were made under those conditions. Or the reliability of observations made under conditions C can be questioned. In both cases, whether the observation report is to be returned to the evidence or not can be settled only by inference from other evidence.

Much evidence does not, of course, consist of observation reports. Empirical generalizations and theoretical assumptions also appear in the evidential corpus – for example, in stating conditions of reliability of observation reports. Objective correction of such evidence again requires inference from evidence.

Thus, an important role must be assigned to criteria for rational acceptance which control conditions under which empirical statements can be added to the evidential corpus. Moreover, the criteria must include conditions for accepting statements H as evidence on the basis of evidence which does not entail H. To deny the relevance of criteria for inductive acceptance is to deny that evidential assumptions can be corrected and revised in an objective manner. Since, Richard Jeffrey to the contrary notwithstanding, rational credence judgments are based on empirical assumptions, to deny the possibility of objective criticism and revision of the evidential corpus is tantamount to denying that the empirical assumptions upon which probability judgments are based can be objectively criticised.

When can an empirical statement E be objectively and legitimately accepted as evidence via nondeductive inference from other evidence K? The short answer is when the other evidence K warrants regarding accepting E as true as nonproblematic. And accepting E as true is nonproblematic when E can legitimately be accepted as a correct answer to every problem or question which is recognized to be serious at the time when the status of E is under consideration.

From this point of view, we may distinguish three different senses in which a statement can be accepted as true via nondeductive inference from evidence K.

(i) H may be accepted as a true answer to a specific problem or question P for which H is a potential answer.

(ii) Sometimes H might be accepted as a true answer to a specific problem P but only tentatively so. This means that although H is the best answer on the evidence available, the evidence K might not so decisively settle the question in favor of H as to render further inquiry and the acquisition of additional evidence pointless. When, however, H is accepted as an answer to a specific problem P in a manner which renders pointless further inquiry to check on the propriety of adopting H as the correct answer to P, H is accepted as a final or terminal answer to P.

(iii) Both acceptance as a true answer to a specific problem P and acceptance as a final true answer to a specific problem P are relativized not only to the total evidence but to the problem P. In order to accept H as evidence, H must be accepted as a final answer relative to all problems which are regarded as serious problems at the time when such acceptance is being considered and for which H is a potential answer. If H is accepted as final relative to P but is not so accepted relative to Q for which H is a relevant answer, H cannot be accepted as evidence. In that case, the truth of H remains subject to serious doubt and is questionable.

This conception of the conditions under which an empirical hypothesis might legitimately be accepted as evidence via inference from other evidence is not intended as a final characterization. Rather it is to be regarded as the framework for guiding research into certain important questions pertaining to inductive inference.

In particular, its adoption suggests that a central objective of an account of scientific inquiry is to provide an analysis of the invariant features of specific inquiries where the aim is to provide true answers to given questions. This will include devising criteria for determining what are potential or relevant answers to specific problems (abduction), planning the acquisition of new evidence, criteria for weighing evidence in order to decide which of the potential answers ought to be accepted as true relative to the available evidence, criteria for deciding whether the answer accepted is final or whether additional inquiry is in order. Finally, it suggests that some attention be given to considering differences between serious and nonserious problems and questions.

Thus, criteria for rational inductive inference are to be studied within the framework of an account of rational problem solving. Indeed, an inductive inference is an attempt to justify a solution to a problem of a specific kind – namely, to a cognitive problem where the aim is to find

a correct answer to a given question. But an adequate account of rational problem solving relevant to cognitive problems should be a specialization of an account of rational problem solving applicable to practical decision problems as well. And Bayesian accounts of rational probability judgment are intended to be an integral part of a general account of problem solving. Thus, we have come full circle. I have argued that a theory of rational probability judgment which allows that probability judgments are empirically grounded must make room for legitimately accepting statements as evidence. But, according to the view adopted here, an account of rational acceptance ought itself to be embedded in an account of rational problem solving which, in turn, will make use of an account of rational probability judgment.

Thus, those who would build a theory of rational probability judgment without taking into consideration questions about rational acceptance and those who would worry about acceptance while ignoring probability are all committed to one sided images of the rational conduct of inquiry. The fundamental philosophical problem pertaining to probability and induction is not whether acceptance should be taken seriously but how an account of rational acceptance and rational probability judgment can be developed in a coherent and adequate manner.

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* This essay was written with partial support from the National Science Foundation, Grant No. GS-2050.

[1] R. Carnap [1].

[2] *Ibid.*, p. 309.

[3] P. Suppes [7].

[4] In addition to the paper mentioned see [6] as well as [8], which is Suppes' comment on R. C. Jeffrey's paper [3]. Jeffrey's reply is instructive.

[5] See P. A. Samuelson [4], p. 257, and footnote 8, p. 263.

[6] Whether strict coherence is plausible or whether more coherence should be required is debatable. However, adoption of the weaker assumption would complicate the exposition by requiring qualification of the claim that the set of sentences accepted as evidence is identical with the set of sentences accorded a credence value of 1. No point essential to this discussion would be lost, however, if the qualification were to be made.

[7] See, for example, L. S. Savage [5], pp. 46–55.

[8] Patrick Suppes has emphasized this point. See [6], p. 35.

[9] Of course, X might be mistaken in reporting that $c(H, X\text{'s total evidence}) = r$. Indeed, the metalinguistic predicate $c(H, \underline{\quad}) = r$ may be true of no non empty set of sentences in X's language. Observe, however, that X can be mistaken in what he takes to be his explicitly given evidence. The shift from explicit to implicit characterizations of evidence involves no change in this respect.

[10] See, for example, R. C. Jeffrey [2], pp. 156–7.

[11] *Op. cit.*, Ch. 11. Also [3].

[12] Jeffrey [3].

[13] *Op. cit.*, p. 179.

[14] *Ibid.*, p. 176.

DRACULA MEETS WOLFMAN:
ACCEPTANCE VS. PARTIAL BELIEF

One of the things I'd like to see come out of this conference is a clarification of the issues between Isaac Levi and me.[1] I take it that Levi's scruples about partial belief and probability kinematics are not idiosyncratic, nor are my scruples about his work on acceptance, so that the matter may be of general interest. When Dracula meets Wolfman in the movies it is not simply I-and-thou: They gibber and slaver for all vampires and all werewolves everywhere. So let it be with us.

I. MY KIND OF PRAGMATISM

Levi and I both seem to be pragmatists of sorts – of different sorts. Thus, he quotes with approval from Peirce ([11], p. 208), "The settlement of opinion is the sole end of inquiry", but my kind of pragmatism has deliberation, not opinion, as its focus: It tries to see how much mileage it can get out of the fact that man is an agent. Opinion and valuation come together in the matrix of deliberation, from which action emerges.

I do not mean to deny that men value understanding, and seek it for its own sake as well as for its practical value; but I do not see understanding as a matter of relief from agnosticism, nor do I find it illuminating to characterize scientists as seeking to replace doubt by true belief. (Still less do I find it illuminating to consider such questions as, 'Am I a *true* pragmatist?' and 'Have I betrayed fallibilism?" – see [11], p. 209.)

Deliberation can take a variety of forms, and has done, in the history of mankind. Our notion of what counts as rational decision-making keeps evolving – anyway, changing. The view that I find most satisfactory is very dimly adumbrated in the *Protagoras* (ca. 356) and Aristotle's *De Anima* (434^a 7–10); more sharply in *The Port-Royal Logic* (last Chapter), and more sharply still in Daniel Bernoulli's *Exposition of a New Theory on the Measurement of Risk* (1738); and has finally come into its own in the past 30 or 40 years through the work of Ramsey, De Finetti, Savage, and others. As an important social force, it has come into its own only