Perhaps enough has now been said, to indicate the nature and objectives of the theory of higher-order preference. Like the theory of first-order preference, it simply refuses to countenance as preferences, rankings that are intransitive or have certain other failings —human though those failings be.[19] But the important point is that the higher-order theory does countenance various other failings—or misfortunes, or conflicts, or tensions, or "contradictions" in some Hegelian sense. It gives us a canvas on which to paint some very complex attitudinal scenes, from life. That one cannot paint intransitive preference rankings on that canvas makes it all the more interesting that one *can* paint poor Akrates there, in the various postures we have seen above.

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ON INDETERMINATE PROBABILITIES *

SOME men disclaim certainty about anything. I am certain that they deceive themselves. Be that as it may, only the arrogant and foolish maintain that they are certain about everything. It is appropriate, therefore, to consider how judgments of uncertainty discriminate between hypotheses with respect to grades of uncertainty, probability, belief, or credence. Discriminations of this sort are relevant to the conduct of deliberations aimed at making choices between rival policies not only in the context of games of chance, but in moral, political, economic, or scientific decision making. If agent X wishes to promote some aim or system of values, he

[19] My thought is that someone who says he prefers A to B and B to C but not A to C is simply mistaken about his preferences. Others prefer to say that the theory imposes an idealization, and is false of much actual preference. Still others (e.g., Amélie Rorty) hold that intransitivities may be quite in order, e.g., when A is preferred to B in one respect (or, under one description) and B is preferred to C in (or, under) another. But throughout, I am concerned with preference *all things considered*, so that one can prefer buying a Datsun to buying a Porsche even though one prefers the Porsche qua fast (e.g., since one prefers the Datsun qua cheap, and takes that desideratum to outweigh speed under the circumstances). *Pref* = preference *tout court* = preference on the balance.

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will (*ceteris paribus*) favor a policy that guarantees him against failure over a policy that does not. Where no guarantee is to be obtained, he will (or should) favor a policy that reduces the probability of failure to the greatest degree feasible. At any rate, this is so when X is engaged in deliberate decision making (as opposed to habitual or routine choice).

Two problems suggest themselves, therefore, for philosophical consideration:

The Problem of Rational Credence: Suppose that an ideally rational agent X is committed at time t to adopting as certain a given system of sentences $K_{X,t}$ (in a suitably regimented L) and to assigning to sentences in L that are not in $K_{X,t}$ various degrees of (personal) probability, belief, or credence. The problem is to specify conditions that X's "corpus of knowledge" $K_{X,t}$ and his "credal state" $B_{X,t}$ (i.e., his system of judgments of probability or credence) should satisfy in order to be reasonable.

The Problem of Rational Choice: Given a corpus $K_{X,t}$ and a credal state $B_{X,t}$ at t, how should X make decisions between alternative policies from which he must choose one at t?

Consideration of these two problems should lead to examination of a third. A rational agent X is entitled to count as certain at t not only logical, mathematical, and set-theoretical truths supplemented by suitably produced testimony of the senses, but theories, laws, and statistical claims as well. At the same time, the revisability of X's corpus at t should be recognized not only by others but by X himself. Moreover, just as X's judgments of certainty are liable to revision, so too are his judgments of probability or credence. Indeed, the two types of modification are apparently interdependent, and this interdependence itself deserves examination. The third problem, therefore, is as follows:

The Problem of Revision: Under what conditions should X modify his corpus $K_{X,t}$ or his credal state $B_{X,t}$, and, if he should do so, how should he choose between alternative ways of making revisions?

In this essay, I shall not attempt to solve the problem of revision. However, I shall indicate how a prima facie obstacle to offering anything other than a dogmatic or antirationalistic answer to the question can be eliminated.

The obstacle is a serious one; for it derives from a very attractive system of answers to the problem of rational credence and the problem of rational choice. I allude to what is called the "bayesian" view. Bayesians do not agree with one another in their answers to these questions in all respects. The views of Harold Jeffreys and the early views of Rudolf Carnap are not consonant in important ways with the ideas of Bruno de Finetti and Leonard J. Savage (or the later Carnap). Nonetheless, the answers these and a host of other authors offer to the first two questions share certain important ramifications for the problem of revision. One of these implications is the commitment to either dogmatism or antirationalism.

Of course, identifying an objectionable consequence of bayesianism, where the objection is grounded on a question of philosophical principle, is in itself unlikely to persuade devoted bayesians to abandon their position. Such authors will be tempted to modify philosophical principle so as to disarm the objection; and they will have good reasons for doing so. Bayesian doctrine does offer answers to the first two questions. These answers are derivable from a system of principles which are precise and simple. Even the disputes between bayesians can be formulated with considerable precision. Furthermore, the prescriptions bayesians recommend for making choices appear to conform to presystematic judgment at least in some contexts of decision. Rival attempts to answer the problems of rational credence and rational choice seem either eclectic or patently inadequate when compared with the bayesian approach.

Thus, it is not enough to complain of the defects of bayesianism. The serious challenge is to construct an alternative system of answers to the problems of rational credence and choice which preserves the virtues of bayesianism without its vices—in particular, the defects it exhibits relevant to the problem of revision.

In this paper, I shall outline just such a rival view.

I

X's corpus of knowledge $K_{X,t}$ at t identifies a set of options A_1, A_2, . . . , A_n as the options from which he will choose (at t' identical with or later than t) at least and at most one. In addition, $K_{X,t}$ implies that at least and at most one of the hypotheses h_1, h_2, . . . , h_m is true and that each of the h_j's is consistent with $K_{X,t}$. Finally, $K_{X,t}$ implies that, if X chooses A_i when h_j is true, the hypothesis o_{ij} asserting the occurrence of some "possible consequence" of A_i is true.

The problem of rational choice is to specify criteria for evaluating various choices of A_is from among those feasible for X according to what he knows at t. Such criteria may be construed as specifying conditions for "admissibility." Option A_i is admissible if and only if X is permitted as a rational agent to choose A_i from among the feasible options. If A_i is uniquely admissible, X is obliged, as a rational agent, to choose it. In general, however, unique admissibility cannot be guaranteed, and no theory of rational choice pretends to guarantee it.

Bayesians begin their answer to the problem of rational choice by assuming that X is an ideally rational agent in the following sense:

(i) X has a system of evaluations for the possible consequences (the o_{ij}s) representable by a real-valued "utility" function $u(o_{ij})$ unique up to a linear transformation (i.e., where utility assignments are nonarbitrary once a 0 point and a unit are chosen—as in the case of measuring temperature).

(ii) X has a system of assignments of degrees of credence to the o_{ij}s, given the choice of A_i representable by a real-valued function $Q(o_{ij}; A_i)$ conforming to the requirements of the calculus of probabilities. Often X will assign credence values to the "states of nature" $h_1, h_2, \ldots, h_n$ so that the h_js are probabilistically independent of the option chosen. When this is so, $Q(o_{ij}; A_i)$ equals the unconditional credence (given $K_{X,t}$) $Q(h_j)$. In the sequel, I shall suppose that we are dealing with situations of this kind.

Given such a utility function $u(o_{ij})$ and Q-function $Q(h_j)$, let $E(A_i) = \sum_{j=1}^{m} u(o_{ij})Q(h_j)$. $E(A_i)$ is the expected utility of the option A_i.

Bayesians adopt as their fundamental principle of rational choice the principle that an option is admissible only if it bears maximum expected utility among all the feasible options.

Very few serious writers on the topic of rational choice object to the principle of maximizing expected utility in those cases where X's values and credal state can be represented by a utility function unique up to a linear transformation and a unique probability function. The doubts typically registered concern the applicability of this principle. That is to say, critics doubt that ordinary men have the ability under normal circumstances to satisfy the conditions of ideal rationality stipulated by strict bayesians even to a modest degree of approximation.

The bayesian riposte to doubts about applicability is to insist that rational men should meet the requirements for applying the principle of maximizing expected utility and that, appearances to the contrary notwithstanding, men are quite capable of meeting these requirements and often do so.

I am not concerned to speculate on our capacities for meeting strict bayesian requirements for credal (and value) rationality. But even if men have, at least to a good degree of approximation, the abilities bayesians attribute to them, there are many situations where, in my opinion, rational men *ought not* to have precise utility functions and precise probability judgments. That is to say, on some occasions, we should avoid satisfying the conditions for applying the principle of maximizing expected utility even if we have the ability to satisfy them.

In this essay, reference to the question of utility will be made from time to time. I shall not, however, attempt to explain why I think it is sometimes (indeed, often) irrational to evaluate consequences by means of a utility function unique up to a linear transformation. My chief concern is to argue that rational men should sometimes avoid adopting numerically precise probability judgments.

The bayesian answer to the problem of rational choice presupposes at least part of an answer to the problem of rational credence. For a strict bayesian, a rational agent has a credal state representable by a numerically precise function on sentences (or pairs of sentences when conditional probability is considered) obeying the dictates of the calculus of probabilities.

There are, to be sure, serious disputes among bayesians concerning credal rationality. In his early writings, Carnap believed that principles of "inductive logic" could be formulated so that, given X's corpus $K_{X,t}$, X's credal state at t would be required by the principles of inductive logic to be represented by a specific Q-function that would be the same for anyone having that corpus.[1] Others (including the later Carnap[2]) despair of identifying such strong principles. Nonetheless, bayesian critics of the early Carnap's program for inductive logic continue to insist that ideally rational agents should assign precise probabilities to hypotheses.

II

X's corpus of knowledge $K_{X,t}$ shall be construed to be the set of sentences (in L) to whose certain truth X is committed at t. I am not suggesting that X is explicitly or consciously certain of the truth of every sentence in $K_{X,t}$, but only that he is committed to being certain. X might be certain at t of the truth of h and, hence, be committed to being certain of $h \vee g$, without actually being certain. Should it be brought to X's attention, however, that $h \vee g$ is a deductive consequence of h, he would be obliged as a rational agent

[1] *Logical Foundations of Probability* (Chicago: University Press, 2nd ed., 1962), pp. 219–241.

[2] "Inductive Logic and Rational Decisions," in Carnap and R. C. Jeffrey, eds., *Studies in Inductive Logic and Probability* (Berkeley: UCLA Press, 1971), p. 27.

either to cease being certain of h or to take $h \vee g$ to be certain. The latter alternative amounts to retaining his commitment; the former to abandoning it.

In this sense, X's corpus of knowledge at t should be a deductively closed set of sentences. Insofar as we restrict our attention to changes in knowledge and credence which are changes in commitments, modifications of corpora of knowledge are shifts from deductively closed sets of sentences to other deductively closed sets of sentences. Such modifications come in three varieties:

(1) *Expansions,* where X strengthens his corpus by adding new items. Some examples of expansion are acquiring new items via observation, from the testimony of others and through inductive or nondeductive inference leading to the "acceptance" of statistical claims, laws, or theories into the corpus.

(2) *Contractions,* where X weakens his corpus by removing items. This can happen when X detects an inconsistency in his corpus due to his having added at some previous expansion step an observation report that contradicts assumptions already in his corpus, or when X finds himself in disagreement with Y (whose views he respects on the point at issue) and wishes to resolve the dispute without begging the question.

(3) *Replacements,* where X shifts from a theory containing one assumption to another containing an assumption contradicting the first. This can happen when X substitutes one theory for another in his corpus.

No matter which kind of modification is made, I shall suppose that there is a "weakest" potential corpus UK (the "urcorpus") of sentences in L such that no rational agent should contract that corpus. UK is the deductively closed set of sentences in L such that every potential corpus in L is an expansion of UK (or is UK itself). I shall suppose that UK contains logical truths, set-theoretical truths, mathematical truths, and whatever else might be granted immunity from removal from the status of knowledge. (The items in UK are in this sense incorrigible.)

Replacement poses special problems for an account of the revision of knowledge. At t when X's corpus is $K_{X,t}$, why should he shift to a corpus K^* which is obtained by deleting items from $K_{X,t}$ and replacing them with other items inconsistent with the first? From X's point of view, at t, he is replacing a theory which he is certain is true by another which he is certain is false.

The puzzle can be avoided by regarding replacements for purposes of analysis as involving two steps: (a) contraction to a corpus relative to which no question is begged concerning the rival theories, and (b) subsequent expansion based on the information available in the contracted corpus, supplemented, perhaps, by the results of experiments conducted in the interim.

Those who insist on attempting to justify replacements without decomposing them into contractions followed by expansions confront the predicament that they cannot justify such shifts without begging questions. Such justification is no justification. The conclusion that beckons is that all replacements are forms of "conversion" to which men are subjected under revolutionary stress. This is the view which Thomas Kuhn has made so popular and which stands opposed to views that look to the formulation of objective criteria for the evaluation of proposed modifications of knowledge.

III

How does all this relate to bayesian views about the revision of credal states?

Consider X's corpus of knowledge $K_{X,t}$ at t. X's credal state $B_{X,t}$ at t is, according to strict bayesians, determined by $K_{X,t}$. Strict bayesians disagree among themselves concerning the appropriate way in which to formulate this determination. The following characterization captures the orthodox view in all its essentials.

Let K be any potential corpus (i.e., let it be UK or an expansion thereof). Let $C_{X,t}(K)$ be X's judgment at t as to what his credal state should be were he to adopt K as his corpus of knowledge. I shall suppose that X is committed to judgments of this sort for every feasible K in L. The resulting function from potential corpora of knowledge to potential credal states shall be called X's "confirmational commitment" at t.

According to strict bayesians, no matter what corpus K is (provided it is consistent), $C_{X,t}(K)$ is representable by a probability function where all sentences in K receive probability 1. In particular, $C_{X,t}(UK)$ is representable by a function $P(x;y)$—which I shall call a P-function, to contrast it with a Q-function representing $C_{X,t}(K)$ where K is an expansion of UK.

Strict bayesians adopt the following principle, which imposes restrictions upon confirmational commitments:

Confirmational Conditionalization: If K is obtained from UK by adding e (consistent with UK) to UK and forming the deductive closure, $P(x;y)$ represents $C_{X,t}(UK)$ and $Q(x;y)$ represents $C_{X,t}(K)$, $Q(h;f) = P(h;f\&e)$.

In virtue of this principle, X's confirmational commitment is defined by specifying $C_{X,t}(UK) = C_{X,t}$ and employing confirmational conditionalization.[3] X's credal state at t, $B_{X,t}$, is then determined by $K_{X,t}$ and $C_{X,t}$ according to the following principle:

Total Knowledge: $C_{X,t}(K_{X,t}) = B_{X,t}$

Notice that the principle of confirmational conditionalization, even when taken together with the principle of total knowledge, does not prescribe how X should modify his credal state given a change in his corpus of knowledge.

To see this, suppose that at t_1 X's corpus is K_1 and that at t_2 his corpus K_2 is obtained from K_1 by adding e (consistent with K_1) and forming the deductive closure. From confirmational conditionalization and total knowledge, we can conclude that *if X does not alter his confirmational commitment in the interim from t_1 to t_2*, then, if Q_1 represents B_{X,t_1} and Q_2 represents B_{X,t_2}, $Q_2(h;f) = Q_1(h;f\&e)$. Should X renege at t_2 on the confirmational commitment he adopted at t_1, the change in knowledge just described need not and will not, in general, lead to a modification of credal state of the sort indicated.

Nonetheless, strict bayesians unanimously suppose that a rational agent will, save under unusual circumstances, modify his credal state in the fashion indicated. This mode of revising credal states is often called "conditionalization"; to distinguish it from confirmational conditionalization and other types of conditionalization, I shall call it "intertemporal credal conditionalization." I contend that the strict bayesian endorsement of intertemporal credal conditionalization presupposes commitment to the following principle:

Confirmational Tenacity: For every X, t, and t', $C_{X,t} = C_{X,t'}$

Thus, strict bayesians have an answer to the problem of revising credal states. X's confirmational commitment is to be held fixed over time. Given such a fixed commitment, the credal state he should adopt is determined for each possible modification of his corpus of

[3] Confirmational commitments built on the principle of confirmational conditionalization are called "credibilities" by Carnap (*ibid.*, pp. 17–19). The analogy is not quite perfect. According to Carnap, a credibility function represents a permanent disposition of X to modify his credal states in the light of changes in his corpus of knowledge. When credibility is rational, it can be represented by a "confirmation function." Since I wish to allow for modifications of confirmational commitments as well as bodies of knowledge and credal states, I assign dates to confirmational commitments. Throughout I gloss over Carnap's distinction between credibility functions and confirmation functions (*ibid.*, pp. 24–27).

knowledge which is a consistent expansion of *UK*. The problem of revising credal states reduces, therefore, to the problem of revising corpora of knowledge.

Is this answer to the problem of revision satisfactory? It would be, in my opinion, if the program for inductive logic envisaged by Carnap in his early writings on the subject could be realized. Inductive logic would then be strong enough to single out a standard *P*-function that all rational agents should adopt as their confirmational commitment. A fortiori, all such agents should hold that commitment fast at all times.

Few bayesians now think an inductive logic of the requisite power can be constructed. Their reasons (which, in my opinion, are sound) need not detain us. In response to this skepticism, most bayesians no longer require that all rational agents endorse a single standard confirmational commitment. They hold that rational *X* is perfectly free to pick any confirmational commitment consonant with the principles of inductive logic. Rational *Y* is quite free to pick a different commitment. However, bayesians tend to insist that, once *X* and *Y* have chosen their respective commitments, they should hold them fixed. To do this is to follow the probabilistic analogue of the method of tenacity so justly criticized by Peirce in "Fixation of Belief."

In the spirit of Peirce, it would have been far better to say that a rational *X* should not modify his confirmational commitment capriciously—i.e., without justification. To follow this approach, however, demands consideration of criteria for justified modifications of confirmational commitments. Bayesians not only fail to do this, but, as I shall now argue, they cannot do so without great difficulty. Given the bayesian answer to the problem of rational credence, no shift can be justified. If I am right, for bayesians, either tenacity should be obeyed, or, if not, justification is gratuitous. I think this implication of bayesian doctrine is to be deplored and should lead to scrutiny of other approaches.

IV

Modifying a confirmational commitment is not quite the same as modifying a corpus of knowledge. Yet, shifting from a confirmational commitment represented by a precise probability function to another confirmational commitment represented by a different precise probability function seems analogous to replacement in the following sense: The shift from confirmational commitment C_1 to confirmational commitment C_2 involves a shift to a confirmational commitment conflicting with C_1 in the sense that the *P*-function

X uses to determine his credal state relative to his corpus when C_1 is adopted yields different precise subjective probability or credence assignments for hypotheses from those which X would make were he to adopt C_2 (and keep his corpus constant).

From X's vantage point at t when he endorses C_1, C_2 is illegitimate. He cannot justify shifting to C_2. At least, he cannot justify a direct shift. Can he do so indirectly by first performing a shift analogous to contraction from C_1 to C_3, which begs no questions concerning the merits of C_1 and C_2? Not from a strict bayesian point of view; for C_3 would, like C_1 and C_2, have to be representable by a precise P-function. The shift from C_1 to C_3 would be as problematic as the shift from C_1 to C_2.

Thus, from a bayesian point of view, no shift from one confirmational commitment to another can be justified. A rational man should conform to confirmational tenacity so that no justification is needed or else hold that some shifts are permitted without justification. Carnap sometimes seems to recognize shifts in confirmational commitments as a result of conceptual change.[4] Alternatively, one might allow shifts in confirmational commitment due to conversion under revolutionary stress. Except for the minimal requirement that the shift be to a commitment obeying requirements of inductive logic, no critical control is to be exercised. Bayesians are committed to being dogmatically tenacious or arbitrarily capricious.

The source of the difficulty should be apparent. Bayesians restrict the confirmational commitments a rational agent may adopt to those representable by numerically precise probability functions. This precludes shifting from a confirmational commitment C_1 to a confirmational commitment C_3 that begs no questions as to the merits of C_1 and another commitment C_2 that conflicts with C_1. My thesis is that not only are rational men allowed to make shifts to non-question-begging commitments but that on many occasions they ought to do so. That is to say, it is sometimes appropriate for a rational agent to adopt a confirmational commitment that is indeterminate in the sense that it cannot be represented by a numerically precise probability function. If we relax the stringent requirements imposed by bayesians on confirmational commitments and credal states so as to allow for such shifts, there is at least some hope that we can avoid endorsement of tenacity or capriciousness. Within the strict bayesian framework, we cannot expect to do so

[4] "A Basic System of Inductive Logic," in Carnap and Jeffrey, *op. cit.*, pp. 51–52.

except by clinging desperately to Carnap's early program for constructing an inductive logic so strong as to single out a standard P-function to represent the uniquely rational confirmational commitment (for a given language).

I propose to explore one way of relaxing strict bayesian requirements. The basic idea is to represent a credal state (confirmational commitment) by a *set* of Q-functions (P-functions). When the set is single-membered, the credal state (confirmational commitment) will be indistinguishable in all relevant respects from a strict bayesian credal state (confirmational commitment).

On this view, if X starts at t with the precise (i.e., single-membered) confirmational commitment C_1, he can then shift to a confirmational commitment that has as members all the P-functions in C_1 as well as the P-functions in some other confirmational commitment C_2. (As the technical formulation will indicate, other P-functions will be members of C_3 as well.)

C_3 will be "weaker" than C_1 or C_2 in that it will allow more P-functions to be "permissible" than either of the other two confirmational commitments alone does. It will allow as permissible all P-functions recognized as such according to C_1 and according to C_2. In this sense, the shift to C_3 will beg no questions as to the permissibility of the P-functions in the other two confirmational commitments.

Of course, the notion of a permissible P-function (and the correlative notion of a permissible Q-function according to a credal state) require elucidation. I shall offer only an indirect clarification. The account of rational credence (and confirmational commitment) based on the new proposal will be supplemented by criteria for rational choice which indicate how permissibility determines the admissibility of options. By indicating the connections between permissibility and rational choice, permissibility will have been characterized indirectly.

V

To simplify the technical details, I shall restrict the discussion to characterizing credal states and confirmational commitments for sentences in a given language L which belong to a set M generated as follows: Let $h_1, h_2, \ldots, h_n$ be a finite set of sentences in L all consistent with the urcorpus UK for L and such that UK logically implies the truth of at least and at most one h_i. M is the set of sentences in L which are equivalent, given UK, to a disjunction of zero or more distinct h_is. (A disjunction of zero h_is is, as usual, a sentence inconsistent with UK.)

With this understanding, X's credal state at t will be a set $B_{X,t}$ of functions $Q(x;y)$ where the sentences substituted for 'x' are in M and the sentences substituted for 'y' are in M and are consistent with $K_{X,t}$. When the sentence substituted for 'y' is a member of $K_{X,t}$, I shall write $Q(x) = Q(x;y)$.

The set $B_{X,t}$ must satisfy the following three conditions:

(1) *Nonemptiness:* $B_{X,t}$ is nonempty.
(2) *Convexity:* $B_{X,t}$ is a convex set—i.e., every weighted average of Q-functions in $B_{X,t}$ is in $B_{X,t}$.
(3) *Coherence:* Every Q-function in $B_{X,t}$ is a probability measure where $Q(h;e) = 1$ if and only if h is deductively implied by e and $K_{X,t}$.

Every Q-function in $B_{X,t}$ is "permissible" according to $B_{X,t}$.

As before, X's confirmational commitment $C_{X,t}(K)$ is a function from feasible corpora of knowledge to potential credal states that X at t considers to be the credal states he should adopt were he to adopt K as his corpus of knowledge. The value of the function for given K, therefore, is a nonempty, convex set of Q-functions relative to K. $C_{X,t}(UK) = C_{X,t}$ is, therefore, a nonempty convex set of P-functions. The principle of confirmational conditionalization introduced previously must now be modified to conform to the new characterization of confirmational commitments and credal states:

Confirmational Conditionalization: Let K be obtained from UK by adding e (consistent with UK) to UK and forming the deductive closure. $C_{X,t}(K)$ is the set of Q-functions such that $Q(h;f) = P(h;f\&e)$ for some permissible P-function in $C_{X,t} = C_{X,t}(UK)$.

$B_{X,t}$ can be determined, as before, as follows:

Total Knowledge: $B_{X,t} = C_{X,t}(K_{X,t})$

Thus, X's confirmational commitment is defined by specifying the value of $C_{X,t}(UK)$.

A strict bayesian confirmational commitment, of course, allows a single P-function to be uniquely permissible. However, confirmational commitments are possible which contain more than one P-function. In general, I shall say that one confirmational commitment is stronger than another if the set of its P-functions is a subset of the set of P-functions in the other commitment.

On this view, the weakest confirmational commitment possible is that which contains all the P-functions that meet the requirements of inductive logic. I shall continue to follow Carnap in understanding inductive logic to be a system of principles that impose constraints on probability functions eligible for membership in confirmational commitments.

In contrast, the strongest confirmational commitment would be the empty one—which is inconsistent with our first requirement of nonemptiness. A strongest "consistent" confirmational commitment is single-membered.

We can, by the way, extend the notion of a confirmational commitment so as to define it for an inconsistent corpus. We can require that $C_{X,t}(K)$ where K is inconsistent, be empty. This means that our previous requirement that a credal state be nonempty is to be restricted to cases where K is consistent. Thus, X might adopt a consistent confirmational commitment (i.e., one that is nonempty). Yet, if he should, unfortunately, endorse an inconsistent K, his credal state should be empty.

As noted previously, strict bayesians have differed among themselves as to what constitutes a complete system of principles of inductive logic. These differences persist on the view I am now proposing. They may be viewed, however, in a new light. The disagreements over inductive logic turn out to be disagreements over what constitutes the "weakest" possible confirmational commitment —which I shall call "*CIL*(*UK*)."

"Coherentists" like de Finetti and Savage claim that the principle of coherence constitutes a complete inductive logic. On their view, $CIL(UK)$ is the set of all P-functions obeying the calculus of probabilities defined over M.

Some authors are prepared to add a further principle to the principle of coherence. This principle determines permissible Q-values for hypotheses about the outcome of a specific experiment on a chance device, given suitable knowledge about the experiment to be performed and the chances of possible outcomes of experiments of that type.

There is considerable controversy concerning the formulation of such a principle of "direct inference." In large measure, the controversy reflects disagreements over the interpretation of "chance" or "statistical probability," concerning the so-called "problem of the reference class" and random sampling. Indeed, the reason coherentists do not endorse a principle linking objective chance with credence is that they either deny the intelligibility of the notion of objective chance or argue in favor of dispensing with that notion.

Setting these controversies to one side, I shall call anyone who holds that a complete inductive logic consists of the coherence principle and an additional principle of direct inference from knowledge of chance to outcomes of random experiments an "objectivist."

There are many authors who are neither coherentists nor objectivists because they wish to supplement the principles of coherence and direct inference with additional principles. Some follow J. M. Keynes, Jeffreys, and Carnap in adding principles of symmetry of various kinds. Others, like I. Hacking,[5] introduce principles of irrelevance or other criteria which attempt to utilize knowledge about chances in a manner different from that employed in direct inference. Approaches of this sort stem by and large from the work of R. A. Fisher. I lack a good tag for this somewhat heterogeneous group of viewpoints. They all agree, however, in denying that objectivist inductive logic is a complete inductive logic.

Attempting to classify the views of historically given authors concerning inductive logic is fraught with risk. I shall not undertake a tedious and thankless task of textual analysis in the vain hope of convincing the reader that many eminent authors have been committed to an inductive logic whether they have said so or not. Yet much critical insight into controversies concerning probability, induction, and statistical inference can be obtained by reading the parties to the discussion as if they were committed to some form of inductive logic. If I am right, far from being a dead issue, inductive logic remains very much alive and debated (at least implicitly) not only by bayesians of the Keynes-Jeffreys-Carnap persuasion but by objectivists (to whose number I think J. Neyman, H. Reichenbach, and, with some qualifications, H. Kyburg belong) and the many authors, like Hacking, who are associated with the tradition of Fisher in various ways.

Assuming, for the sake of the argument, that the debate concerning what constitutes a complete set of principles of inductive logic is settled (I, for one, would defend and will defend elsewhere adopting a variant of an objectivist inductive logic), there is yet another dimension to debates among students of probability, induction, and statistical inference.

Some authors seem to endorse the view that a rational agent should adopt the weakest confirmational commitment, *CIL,* consonant with inductive logic and hold it fast. They are, in effect, advocating confirmational tenacity. They do so, however, on the grounds that one should not venture to endorse a confirmational commitment stronger than the weakest allowed by inductive logic. (Their view is analogous to one that would require adopting the

[5] *Logic and Statistical Inference* (New York: Cambridge, 1965), p. 135.

weakest corpus of knowledge *UK* and holding it fast.) I shall call advocates of such a view "necessitarians."

Again, classifying historically given authors is a risky business. However, Keynes, Jeffreys, and Carnap (in his early work) seem to be clear examples of necessitarians. What is more interesting is the implication that anyone is a necessitarian who insists that the only conditions under which a numerically precise probability can be assigned to a statement (other than a statement that is certainly true or false) are those derivable via direct inference from knowledge of chances. Such authors, on my view, are committed to saying that, when numerical probabilities are not assignable in this way, any numerical value is a permissible assignment provided that it is derived from Q-functions allowed by inductive logic.

To illustrate, suppose that X knows that a given coin has a .4 or a .6 chance of landing heads on a toss. Let h_1 be the first hypothesis that the chance is .4, and h_2 the second hypothesis. Let g be the hypothesis that the coin will land heads on the next toss. By direct inference, every permissible Q-function in X's credal state must be such that $Q(g;h_1) = .4$ and $Q(g;h_2) = .6$. By coherence, every Q-function in his credal state must be such that $Q(h_2) = 1 - Q(h_1)$, where $Q(h_1)$ is some real number between 0 and 1 and $Q(g) = Q(g;h_1)Q(h_1) + Q(g;h_2)Q(h_2) = .4Q(h_1) + .6(1 - Q(h_1))$.

According to the authors I have in mind, there is no unique numerical value that a rational X should adopt as uniquely permissible for $Q(h_1)$. As I am interpreting such authors as Kyburg, Neyman, Reichenbach, and Salmon, they mean to say that X's credal state should consist of all Q-functions meeting the conditions specified. The upshot is that the set of permissible Q-values for g should consist of all Q-values in the interval from .4 to .6. If I am reading them right, they endorse an objectivist logic and, at the same time, insist that X should adopt *CIL* as his confirmational commitment. They are "objectivist necessitarians."

The early Carnap, as noted previously, had hoped to identify an inductive logic that singled out a unique P-function as eligible for membership in confirmational commitments. Had his hope been realized, a rational agent would perforce have had to be a necessitarian. The weakest confirmational commitment would have been the strongest consistent one as well. Confirmational tenacity would have been necessitated by the principles of inductive logic.

But if Carnap's program is abandoned, necessitarianism is by no means the only response that one can make. Indeed, it seems to be of doubtful tenability, if for no other reason than that credal states formed on a necessitarian basis seem to be too weak for use in practical decision making or statistical inference. (Many objectivist necessitarians seem to deny this; but the matter is much too complicated to discuss here.)

Personalists, like de Finetti and Savage, abandon necessitarianism but continue to endorse confirmational tenacity—at least during normal periods free from revolutionary stress. It is this position that I contended earlier leads to dogmatism or capriciousness with respect to confirmational commitment.

The view I favor is *revisionism.* This view agrees with the personalist position in allowing rational men to adopt confirmational commitments stronger than *CIL.* It insists, however, that such commitments are open to revision. It sees as a fundamental epistemological problem the task of providing an account of the conditions under which such revision is appropriate and criteria for evaluating proposed changes in confirmational commitment on those occasions when such shifts are needed.

I shall not offer an account of the revision of confirmational commitments. The point I wish to emphasize here is that, once one abandons the strict bayesian approach to credal rationality and allows credal states to contain more than one permissible Q-function in the manner I am suggesting, the revisionist position can be seriously entertained. The strict bayesian view precludes it and leaves us with the dubious alternatives of necessitarianism and personalism. By relaxing the strict bayesian requirements on credal rationality, we can at least ask a question about revision which could not be asked before.

VI

According to the approach I am proposing, X's credal state at t is characterized by a set of Q-functions defined over sentences in a set M. Such a representation describes X's credal state globally. Nothing has been said thus far as to how individual sentences in M are to be assigned grades of credence or how the degrees of credence assigned to two or more sentences are to be compared with one another. The following definitions seem to qualify for this purpose:

Def. 1: $Cr_{X,t}(h;e)$ is the set of real numbers r such that there is a Q-function in $B_{X,t}$ according to which $Q(h;e) = r$.

Def. 2: $c_{X,t}(h;e)$ is the set of real numbers r such that there is a P-function in $C_{X,t}$ according to which $P(h;e) = r$.

In virtue of the convexity requirement, both the credence function $Cr_{X,t}(h;e)$ and the confirmation function $c_{X,t}(h;e)$ will take sets of values that are subintervals of the unit line—i.e., the interval from 0 to 1. The lower and upper bounds of such intervals have properties which have been investigated by I. J. Good,[6] C. A. B. Smith,[7] and A. P. Dempster.[8]

A partial ordering with respect to comparative credence or with respect to comparative confirmation can be defined as follows:

Def. 3: $(h;e) \overset{Cr_{X,t}}{\leq} (h';e')$ if and only if, for every Q-function in $B_{X,t}$, $Q(h;e) \leq Q(h';e')$.

Def. 4: $(h;e) \overset{C_{X,t}}{\leq} (h';e')$ if and only if, for every P-function in $C_{X,t}$, $P(h;e) \leq P(h';e')$.

The partial orderings induced by credal states and confirmational commitments conform to the requirements of B. O. Koopman's axioms for comparative probability.[9] Koopman pioneered in efforts to relax the stringent requirements imposed by bayesians on rational credence. Within the framework of his system, he was able not only to specify conditions of rational comparative probability judgment but to identify ways of generating interval-valued credence functions.

According to Koopman's approach, however, any two credal states (confirmational commitments) represented by the same partial ordering of the elements of M are indistinguishable. My proposal allows for important differences. Several distinct convex sets of probability distributions over the elements of M can induce the same partial ordering on the elements of M according to definitions 3 and 4.

Dempster, Good, Kyburg, Smith, and F. Schick, have all proposed modifying bayesian doctrine by allowing credal states and confirmational commitments to be represented by interval-valued probability functions.[10] Good, Smith, and Dempster have also explored the

[6] "Subjective Probability as the Measure of a Non-measurable Set," in P. Suppes, E. Nagel, and A. Tarski, *Logic, Methodology, and the Philosophy of Science* (Stanford: University Press, 1962), pp. 319–329.

[7] "Consistency in Statistical Inference and Decision" (with discussion), *Journal of the Royal Statistical Society*, series B, XXIII (1961): 1–25.

[8] "Upper and Lower Probabilities Induced by a mutivalued Mapping," *Annals of Mathematical Statistics*, XXXVIII (1967): 325–339.

[9] "The Bases of Probability," *Bulletin of the American Mathematical Society*, XLVI (1940): 763–774.

[10] Dempster, *op. cit.*; Good, *op. cit.*; Kyburg, *Probability and the Logic of Rational Belief* (Middletown, Conn.: Wesleyan Univ. Press, 1961): Smith, *op. cit.*; Schick, *Explication and Inductive Logic*, doctoral dissertation, Columbia University, 1958.

representation of credal states defined by interval-valued credence functions by means of sets of probability measures. Smith and Dempster explicitly consider convex sets of measures. Nonetheless, all these authors, including Dempster and Smith, seem to regard credal states (and confirmational commitments) represented by the same interval-valued function as indistinguishable. In contrast, my proposal recognizes credal states as different even though they generate the identical interval-valued function—provided they are different convex sets of Q-functions.[11]

Thus, the chief difference between my proposal and other efforts to come to grips with "indeterminate" probability judgments is that my proposal recognizes significant differences between credal states (confirmational commitments) where other proposals recognize none. Is this a virtue, or are the fine distinctions allowed by my proposal so much excess conceptual baggage?

I think that the distinctions between credal states recognized by the proposals introduced here are significant. Agents X and Y, who confront the same set of feasible options and evaluate the possible consequences in the same way may, nonetheless, be obliged as rational agents to choose different options if their credal states are different, even though their credal states define the same interval-valued credence function. That is to say, according to the decision theory that supplements the account of rational credence just introduced, differences in credal states recognized by my theory but not by Dempster's or Smith's, do warrant different choices in otherwise similar contexts of choice.

To explain this claim, we must turn to a consideration of rational choice. We would have to do so anyhow. One of the demands that can fairly be made of those who propose theories rival to bayesianism is that they furnish answers not only to the problems of rational credence and revision but to the questions about rational choice. Furthermore, the motivation for requiring credal states to be nonempty, convex sets of probability measures and the explanation of the notion of a permissible Q-function are best understood within the context of an account of rational choice. For all these reasons, therefore, it is time to discuss rational choice.

[11] The difference between my approach and Smith's was drawn to my attention by Howard Stein. To all intents and purposes, both Dempster and Smith represent credal states by the largest convex sets that generate the interval-valued functions characterizing those credal states. Dempster (332/3) is actually more restrictive than Smith. Dempster, by the way, wrongly attributes to Smith the position I adopt. To my knowledge, Dempster is the first to consider this position in print—even if only to misattribute it to Smith.

VII

Consider, once more, a situation where X faces a decision problem of the type described in section I. No longer, however, will it be supposed that X's credal state for the "states of nature" $h_1, h_2, \ldots, h_n$ and for the possible consequences $o_{i1}, o_{i2}, \ldots, o_{im}$ conditional on X choosing A_i are representable by a single Q-function. Instead, the credal state will be required only to be a nonempty convex set of Q-functions.[12]

Although I have not focused attention here on the dubiety of requiring X's evaluations of the o_{ij}s to be representable by a utility function unique up to a linear transformation, I do believe that rational men can have indeterminate preferences and will, for the sake of generality, relax the bayesian requirement as follows: X's system of evaluations of the possible consequences of the feasible options is to be represented by a set G of "permissible" u-functions defined over the o_{ij}s which is (a) nonempty, (b) convex, and such that all linear transformations of u-functions in G are also in G. A bayesian G is, in effect, such that all u-functions in it are linear transformations of one another. It is this latter requirement that I am abandoning.

In those situations where X satisfies strict bayesian conditions so that his credal state contains only a single Q-function and G contains all and only those u-functions which are linear transformations of some specific u-function u_1, an admissible option A_i is, according to the principle of maximizing expected utility, an option that bears maximum expected utility $E(A_i) = \sum_{i=1}^{m} Q(h_i)u_1(o_{ij})$. Notice that, if any linear transformation of u_1 is substituted for u_1 in the computation of expected utility, the ranking of options with respect to expected utility remains unaltered. Hence we can say that, according to strict bayesians, an option is admissible if it bears maximum expected utility relative to the uniquely permissible Q-function and to any of the permissible u-functions in G (all of which are linear transformations of u_1).

There is an obvious generalization of this idea applicable to situations where $B_{X,t}$ contains more than one permissible Q-function and G contains u-functions that are not linear transformations of one another. I shall say that A_i is *E-admissible* if and only if there is at least one Q-function in $B_{X,t}$ and one u-function in G such that

[12] As in section I, I am supposing that "states of nature" are "independent" of options in the sense that, for every permissible Q-function, $Q(h_j) = Q(o_{ij};A_i)$. I have done this to facilitate the exposition. No question of fundamental importance is, in my opinion, thereby seriously altered.

$E(A_i)$ defined relative to that Q-function and u-function is a maximum among all the feasible options. The generalization I propose is the following:

E-admissibility: All admissible options are *E*-admissible.

The principle of *E*-admissibility is by no means novel. I. J. Good, for example, endorsed it at one time. Indeed, Good went further than this. He endorsed the converse principle that all *E*-admissible options are admissible as well.[13]

I disagree with Good's view on this. When *X*'s credal state and goals select more than one option as *E*-admissible, there may be and sometimes are other considerations than *E*-admissibility which *X*, as a rational agent, should employ in choosing between them.

There are occasions where *X* identifies two or more options as *E*-admissible and where, in addition, he has the opportunity to defer decision between them. If that opportunity is itself *E*-admissible, he should as a rational agent "keep his options open." Notice that in making this claim I am not saying that the option of deferring choice between the other *E*-admissible options is "better" than the other *E*-admissible options relative to *X*'s credence and values and the assessments of expected utility based thereon. In general, *E*-admissible options will not be comparable with respect to expected utility (although sometimes they will be). The injunction to keep one's options open is a criterion of choice that is based not on appraisals of expected utility but on the "option-preserving" features of options. Deferring choice is better than the other *E*-admissible options in this respect, but not with respect to expected utility.

Thus, a *P-admissible* option is an option that is (a) *E*-admissible and (b) "best" with respect to *E*-admissible option preservation among all *E*-admissible options. I shall not attempt to provide an adequate explication of clause (b) here. In the subsequent discussion, I shall consider situations where there are no opportunities to defer choice. Nonetheless, it is important to notice that, given a suitably formulated surrogate for (b), the following principle holds:

P-admissibility: All admissible options are *P*-admissible.

My disagreement with Good goes still further than this; for I reject not only the converse of *E*-admissibility but that of *P*-admissibility as well.

[13] "Rational Decisions," *Journal of the Royal Statistical Society*, Ser. B, XIV (1952): 114.

To illustrate, consider a situation that satisfies strict bayesian requirements. X knows that a coin with a .5 chance of landing heads is to be tossed once. g is the hypothesis that the coin will land heads. Under the circumstances, we might say that X's credal state is such that all permissible Q-functions assign g the value $Q(g) = .5$. Suppose that X is offered a gamble on g where X gains a dollar if g is true and loses one if g is false. (I shall assume that X has neither a taste for nor an aversion to gambling and that, for such small sums, money is linear with utility). He has two options: to accept the gamble and to reject it. If he rejects it, he neither gains nor loses.

Under the circumstances described, the principle of maximizing expected utility may be invoked. It indicates that both options are optimal and, hence, in my terms E-admissible. Since there are no opportunities for delaying choice, both options (on a suitably formulated version of P-admissibility) become P-admissible.

Bayesians—and Good would agree with this—tend to hold that rational X is free to choose either way. Not only are both options E-admissible. They are both admissible. Yet, in my opinion, rational X should refuse the gamble. The reason is not that refusal is better in the sense that it has higher expected utility than accepting the gamble. The options come out equal on this kind of appraisal. Refusing the gamble is "better," however, with respect to the security against loss it furnishes X. If X refuses the gamble, he loses nothing. If he accepts the gamble, he might lose something. This appeal to security does not carry weight, in my opinion, when accepting the gamble bears higher expected utility than refusing it. However, in that absurdly hypothetical situation where they bear precisely the same expected utility, the question of security does become critical.

These considerations can be brought to bear on the more general situation where two or more options are E-admissible (even though they are not equal with respect to expected utility) and where the principle of P-admissibility does not weed out any options.

An S-admissible option (i.e., option admissible with respect to security) is an option that is P-admissible and such that there is a permissible u-function in G relative to which the minimum u-value assigned a possible consequence o_{ij} of option A_i is a maximum among all P-admissible options.[14]

[14] The possible consequences of a "mixed act" constructed by choosing between "pure options" A_i and A_j, with the aid of a chance device with known chance probability of selecting one or the other option is the set of possible consequences of either A_i or A_j. Consequently, the security level of such a mixed option for a given u-function is the lowest of the security levels belonging to A_i and A_j. Thus, my conception of security levels for mixed acts differs from that employed by

S-admissibility: All admissible options are *S*-admissible.

I cannot think of additional criteria for admissibility which seem adequate. (But then I have no precise conditions of adequacy.) I think, perhaps, we should keep an open mind on this matter. Nonetheless, for the present, I shall tentatively assume that the converse of *S*-admissibility holds. This assumption will not alter the main course of the subsequent argument.

Even without detailed exploration of the ramifications of this decision theory, some of its main features are immediately apparent. It conforms to the strict bayesian injunction to maximize expected utility in those situations where X has a precise credal state and G contains u-functions that are all linear transformations of one another. In this sense, bayesian decision theory is a special case of mine.

Similarly, the proposed decision theory identifies situations where the well-known maximin criterion is applied legitimately. Customarily maximin is used to select that option from among all the *feasible* options which maximizes the minimum gain. This recommendation is legitimate, according to my theory, provided (1) G contains all and only u-functions that are linear transformations of one another, and (2) all feasible options are *P*-admissible. But even if condition (1) is satisfied, it can be the case that the maximin solution from among all the feasible options is not itself *E*-admissible and so cannot be considered to be *S*-admissible.

Finally, my proposal is able to discriminate between and cover a wider variety of situations where neither maximizing expected utility nor maximining can be invoked with much plausibility. Moreover, it does so with the aid of a unified system of criteria of rational credence and rational choice. Thus, it does offer answers to just those questions which Bayesian theory purports to solve. Moverover, it escapes the bayesian commitment to the dubious doctrines of necessitarianism or personalism.

VIII

Some elementary properties of credal states as nonempty convex sets will be illustrated and explained by applying the decision theory just outlined to simple gambling situations. Suppose X knows that

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von Neumann and Morgenstern and by Wald in formulating maximin (or minimax) principles. For this reason, starting with a set of *P*-admissible pure options, one cannot increase the security level by forming mixtures of them. In any case, mixtures of *E*-admissible options are not always themselves *E*-admissible. I shall leave mixed options out of account in the subsequent discussion. See D. Luce and H. Raiffa, *Games and Decisions* (New York: Wiley, 1958), pp. 68–71, 74–76, 278–280.

a coin is to be tossed and has either a .4 or .6 chance of landing heads. g is the hypothesis that the coin will land heads. I shall suppose that X has neither a taste for nor an aversion to gambling and that X's values are such that G is a set of u-functions that are linear transformations of the monetary payoffs of the gambles to be considered.

Case 1: X is offered a gamble on a take-it-or-leave-it basis where he receives $S - P$ dollars if g is true and loses P dollars if g is false. (Both S and P are positive.)

Case 2: X is offered a gamble on a take-it-or-leave-it basis where he loses P dollars if g is true and receives $S - P$ dollars if g is false. (S and P have the same values as in case 1.)

h_1 is the hypothesis that the chance of heads is .4, and h_2 is the hypothesis that the chance of heads is .6. By the reasoning of page 405, every permissible Q-function in X's credal state should be such that $Q(g) = .4Q(h_1) + .6[1 - Q(h_1)]$.

According to strict bayesians, X should, therefore, adopt a credal state that selects a single such Q-function as permissible. This can be done by selecting a single value for $Q(h_1)$. If that value is r, $Q(g) = .4r + .6(1 - r) = .6 - .2r$.

Hence, the bayesian will find that accepting the case 1 gamble is uniquely admissible if and only if $Q(g) > P/S$, and will find accepting the case 2 gamble uniquely admissible if and only if $Q(\sim g) > P/S$. (Otherwise rejecting the gamble for the appropriate case is uniquely admissible, assuming that ties in expected utility are settled in favor of rejection.) Hence, if P/S is less than .5, a bayesian must preclude the possibility of accepting the gamble being inadmissible both in case 1 and in case 2.

Suppose, however, that $Cr_{X,t}(h_1)$ takes a nondegenerate interval as a value. For simplicity, let that interval be [0, 1]. The set of permissible Q-values for g must be all values of $.6 - .2r$ where r takes any value from 0 to 1. Hence, $Cr_{X,t}(g) = [.4, .6]$.

Under these conditions, my proposal holds that, when P/S falls in the interval from .4 to .6, both options are E-admissible (and P-admissible) in case 1. The same is true in case 2. But in both case 1 and case 2, rejecting the gamble is uniquely S-admissible. Hence, in both cases, X should reject the gamble. *This is true even when P/S is less than .5.* In this case, my proposal allows a rational agent a system of choices that a strict bayesian would forbid. In adopting this position, I am following the analysis advocated by C. A. B. Smith for handling pairwise choices between accepting and rejecting gambles. Smith's procedure, in brief, is to characterize X's degree of credence for g by a pair of numbers (the "lower pignic probability" and the "upper pignic probability" for g) as follows: The lower pignic probability $\underline{s}$ represents the least upper bound of betting quotients P/S for which X is prepared to accept gambles on g for positive S. The upper pignic probability $\bar{s}$ for g is $1 - t$, where t is the least upper bound of betting quotients P/S for which X is prepared to accept gambles on $\sim g$ for positive S. Smith requires that $\underline{s} \leq \bar{s}$, but does not insist on equality as bayesians do. Given Smith's definitions of upper and lower pignic probabilities, it should be fairly clear that, in case 1 and case 2 where $Cr_{X,t}(g) = [.4, .6]$, Smith's analysis and mine coincide.[15]

Before leaving cases 1 and 2, it should be noted that, if X's credal state were empty, no option in case 1 would be admissible and no option in case 2 would be admissible either. If X is confronted with a case 1 predicament and an empty credal state, he would be constrained to act and yet as a rational agent enjoined not to act. The untenability of this result is to be blamed on adopting an empty credal state. Only when X's corpus is inconsistent, should a rational agent have an empty credal state. But, of course, if X finds his corpus inconsistent, he should contract to a consistent one.

Case 3: A_1 is accepting both the case 1 and the case 2 gamble jointly with a net payoff if g is true or false of $S - 2P$.

This is an example of decision making under certainty. Everyone agrees that if P is greater than $2S$ the gamble should be rejected; for it leads to certain loss. If P is less than $2S$ X should accept the gamble; for it leads to a certain gain. These results, by the way, are implied by the criteria proposed here as well as by the strict bayesian view.

[15] Smith, *op. cit.*, pp. 3–5, 6–7. The agreement applies only to pairwise choices where one option is a gamble in which there are two possible payoffs and the other is refusing to gamble with 0 gain and 0 loss. In this kind of situation, it is clear that Smith endorses the principle of E-admissibility, but not its converse. However, in the later sections of his paper where Smith considers decision problems with three or more options or where the possible consequences of an option to be considered are greater than 2, Smith seems (but I am not clear about this) to endorse the converse of the principle of E-admissibility—counter to the analysis on the basis of which he defines lower and upper pignic probabilities. Thus, it seems to me that either Smith has contradicted himself or (as is more likely) he simply does not have a general theory of rational choice. The latter sections of the paper may then be read as interesting explorations of technical matters pertaining to the construction of such a theory, but not as actually advocating the converse of E-admissibility. At any rate, since it is the theory Smith propounds in the first part of his seminal essay which interests me, I shall interpret him in the subsequent discussion as having no general theory of rational choice beyond that governing the simple gambling situations just described.

Strict bayesians often defend requiring that Q-functions conform to the requirements of the calculus of probabilities by an appeal to the fact that, when credal states contain but a single Q-function, a necessary and sufficient condition for having credal states that do not license sure losses (dutch books) is having a Q-function obeying the calculus of probabilities. The arguments also support the conclusion that, even when more than one Q-function is permissible according to a credal state, if all permissible Q-functions obey the coherence principle, no dutch book can become E-admissible and, hence, admissible.

Case 4: B_1 is accepting the case 1 gamble, B_2 is accepting the case 2 gamble, and B_3 is rejecting both gambles.

Let the credal state be such that all values between 0 and 1 are permissible Q-values for h_1 and, hence, all values between .4 and .6 are permissible for g.

If P/S is greater than .6, B_3 is uniquely E-admissible and, hence, admissible. If P/S is less than .4, B_3 is E-inadmissible. The other two options are E-admissible and admissible.

If P/S is greater than or equal to .4 and less than .5, B_3 remains inadmissible and the other two admissible.

If P/S is greater than or equal to .5 and less than .6, all three options are E-admissible; but B_3 *is* uniquely S-admissible. Hence, B_3 should be chosen when P/S is greater than or equal to .5.

Three comments are worth making about these results.

(i) I am not sure what analysis Smith would propose of situations like case 4. At any rate, his theory does not seem to cover it (but see footnote 15).

(ii) When P/S is between .4 and .5, my theory recommends rejecting the gamble in case 1, rejecting the gamble in case 2, and yet recommends accepting one or the other of these gambles in case 4. This violates the so-called "principle of independence of irrelevant alternatives." [16]

[16] See Luce and Raiffa, *op. cit.*, pp. 288/9. Because the analysis offered by Smith and me for cases 1 and 2 seems perfectly appropriate and the analysis for case 4 also appears impeccable, I conclude that there is something wrong with the principle of independence of irrelevant alternatives.

A hint as to the source of the trouble can be obtained by noting that if 'E-admissible' is substituted for 'optimal' in the various formulations of the principle cited by Luce and Raiffa, p. 289, the principle of independence of irrelevant alternatives stands. The principle fails because S-admissibility is used to supplement E-admissibility in weeding out options from the admissible set.

Mention should be made in passing that even when 'E-admissible' is substituted for 'optimal' in Axiom 9 of Luce and Raiffa, p. 292, the axiom is falsified. Thus, when $.5 \leq P/S \leq .6$ in case 4, all three options are E-admissible, yet some mixtures of B_1 and B_2 will not be.

(iii) If the convexity requirement for credal states were violated by removing as permissible values for g all values from $(S - P)/S$ to P/S, where P/S is greater than .5 and less than .6, but leaving all other values from .4 to .6, then—counter to the analysis given previously, B_3 would not be E-admissible in case 4. The peculiarity of that result is that B_1 is E-admissible because, for permissible Q-values from .6 down to P/S, it bears maximum expected utility, with B_3 a close second. B_2 is E-admissible because, for Q-values from .4 to $(S - P)/S$, B_2 is optimal, with B_3 again a close second. If the values between $(S - P)/S$ and P/S are also permissible, B_3 is E-admissible because it is optimal for those values. To eliminate such intermediate values and allow the surrounding values to retain their permissibility seems objectionable. Convexity guarantees against this.

> *Case 5:* X is offered a gamble on a take-it-or-leave-it basis in which he wins 15 cents if f_1 is true, loses 30 cents if f_2 is true, and wins 40 cents if f_3 is true.

Suppose X's corpus of knowledge contains the following information:

Situation a: X knows that the ratios of red, white, and blue balls in the urn are either (i) 1/8, 3/8, 4/8 respectively; (ii) 1/8, 4/8, 3/8; (iii) 2/8, 4/8, 2/8; or (iv) 4/8, 3/8, 1/8.

X's credal state for the f_is is determined by his credal state for the four hypotheses about the contents of the urn according to a more complex variant of the arguments used to obtain credence values for g in the first four cases. If we allow all Q-functions compatible with inductive logic of an objectivist kind to be permissible, X's credal state for the f_is is the convex set of all weighted averages of the four triples of ratios. $Cr_{X,t}(f_1) = (1/8,\ 4/8)$, $Cr_{X,t}(f_2) = (3/8,\ 4/8)$, and $Cr_{X,t}(f_3) = (1/8,\ 4/8)$. Both accepting and rejecting the gamble are E-admissible. Rejecting the gamble, however, is uniquely S-admissible. X should reject the gamble.

Situation b: X knows that the ratios of red, white, and blue balls is correctly described by (i), (ii), or (iv), but not by (iii). Calculation reveals that the interval-valued credence function is the same as in situation *a*. Yet it can be shown that accepting the gamble is uniquely E-admissible and, hence, admissible. X should accept the gamble.

Now we can imagine situations that are related as *a* and *b* are to one another except that the credal states do not reflect differences in statistical knowledge. Then, from the point of view of Dempster and Smith, the credal states would be indistinguishable. Because the set of permissible Q-distributions over the f_is would remain different for situations *a* and *b*, my view would recognize differences and recommend different choices. If the answer to the problem of rational choice proposed here is acceptable, the capacity of the account of credal rationality to make fine distinctions is a virtue rather than a gratuitous piece of pedantry.

The point has its ramifications for an account of the improvement of confirmational commitments; the variety of discriminations that can be made between confirmational commitments generates a variety of potential shifts in confirmational commitments subject to critical review. For intervalists, a shift from situation *a* to *b* is no shift at all. On the view proposed here, it is significant.

The examples used in this section may be used to illustrate one final point. The objective or statistical or chance probability distributions figuring in chance statements can be viewed as assumptions or hypotheses. Probabilities in this sense can be unknown. We can talk of a set of simple or precise chance distributions among which X suspends judgment. Such *possible* probability distributions represent hypotheses which are possibly true and which are themselves objects of appraisal with respect to credal probability. *Permissible* probability distributions which, in our examples, are defined over such *possible* probability distributions (like the hypotheses h_1 and h_2 of cases 1, 2, 3, and 4) are not themselves possibly true hypotheses. No probability distributions of a still higher type can be defined over them.[17]

I have scratched the surface of some of the questions raised by the proposals made in this essay. Much more needs to be done. I

[17] I mention this because I. J. Good, whose seminal ideas have been an important influence on the proposals made in this essay, confuses permissible with possible probabilities. As a consequence, he introduces a hierarchy of types of probability (Good, *op. cit.*, p. 327). For criticism of such views, see Savage, *The Foundations of Statistics* (New York: Wiley, 1954), p. 58. In fairness to Good, it should be mentioned that his possible credal probabilities are interpreted not as possibly true statistical hypotheses but as hypotheses entertained by X about his own unknown strictly bayesian credal state. Good is concerned with situations where strict bayesian agents having precise probability judgments cannot identify their credal states before decision and must make choices on the basis of partial information about themselves. [In *Decision and Value* (New York: Wiley, 1964), P. C. Fishburn devotes himself to the same question.] My proposals do not deal with this problem. I reject Good's and Fishburn's view that every rational agent is at bottom a strict bayesian limited only by his lack of self-knowledge, computational facility, and memory. To the contrary, I claim that, even without such limitations, rational agents should not have precise bayesian credal states. The difference in problem under consideration and presuppositions about rational agents has substantial technical ramifications which cannot be developed here.

do believe, however, that these proposals offer fertile soil for cultivation not only by statisticians and decision theorists but by philosophers interested in what, in my opinion, ought to be the main problem for epistemology—to wit, the improvement (and, hence, revision) of human knowledge and belief.

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BOOK REVIEWS

The Matter of Chance. D. H. MELLOR. New York: Cambridge University Press, 1971. xiii, 190 p. $13.00.

Quite a few very different accounts of the meaning of probability attributions have been forthcoming, which have been called "dispositional" accounts. Not surprisingly, there are as many different dispositional accounts as there are philosophers espousing them; in fact, there are more, since some philosophers have propounded more than one distinct theory they have called a dispositional theory of probability.

In one of Popper's accounts, the disposition is attributed to the test situation, the display is what would be the limit of the long-run frequency in a potential, but non-actual, infinite sequence of tests. The connection between test situation and display is never made fully clear, and, as I argued in "Is Probability a Dispositional Property?,"[1] it cannot be made clear. In another of Popper's accounts, the disposition is again attributed to the test situation, the display being the actual outcomes of single trials. Here the theory is rather question-begging, since the relation of display to test is certainly not that of an ordinary display to the ordinary actualization of a test situation characterized by a dispositional feature, but is mediated rather by a notion of the "weight" of the disposition. These "dispositional weights," however, on reflection turn out to be just the "chances" we wanted an explication of in the first place; and the addition of dispositional notions to the account is unilluminating.

In what is the clearest and most explicit account of dispositional probability, Isaac Levi takes the disposition as attributable to the test situation, the display to be actual frequencies of outcomes in se-

[1] This JOURNAL, LXVII, 11 (June 11, 1970): 355–366.