MARK KAPLAN

RATIONAL ACCEPTANCE*

When should a rational person accept a proposition? It is hard to think of a question which has enjoyed a more comfortable place in epistemology and the philosophy of science. Even in periods during which there has been wide-spread disagreement over what sort of conditions an adequate answer should satisfy, the integrity of the question itself has never been widely challenged. One need only to look at the history of the last few decades for illustration.

Consider epistemology. In the last thirty years foundationalism, once the dominant approach to the theory of rational acceptance, has been subjected to a great deal of criticism and is now widely considered to be discredited. But if foundationalism is a thing of the past (and I am not sure it is), the question it sought to answer is not. In fact, it is fair to say that foundationalism has come into disrepute largely because a new approach to rational acceptance — stressing coherence, reflective equilibrium and webs of belief — has become more attractive to theorists of rational acceptance.

The story in the philosophy of science is not materially different. In the wake of work by Thomas Kuhn, N. R. Hanson and Paul Feyerabend, the integrity of a once-dominant approach to the philosophy of scientific rationality has been severely questioned — and the approach itself abandoned — by many philosophers of science. But while, for these philosophers, many questions associated with the old approach have lost their appeal (e.g., 'What is the criterion of cognitive significance?' 'How are generalizations confirmed by their instances?'), the question with which this paper began is not among them. The new historically-oriented philosophers of science are, if anything, even more preoccupied with rational acceptance than were their predecessors in the older tradition.

My message is that this comfortable place the theory of rational acceptance enjoys in epistemology and the philosophy of science has not been earned. For almost twenty years there have been signs that the theory of rational acceptance suffers from deep foundational difficulties — difficulties which

plague foundationalist and anti-foundationalist, pre-Kuhnian and post-Kuhnian alike. Indicative of their depth is the fact that these difficulties not only raise questions about the most fundamental canons of rational acceptance, but ultimately call into question the very intelligibility of what we are saying when we say of someone that she accepts a proposition.

My aim in this paper is, first, to say what these foundational difficulties are and, second, to make a case for one way of resolving them. The upshot will be that, if we are to have a theory of rational acceptance at all, it will have to be a rather different sort of theory, about a rather different sort of thing, than we have hitherto anticipated.

I

To see the present infirmity of the theory of rational acceptance we must begin with a pair of puzzles which have enjoyed renown (the first more than the second) since the early 1960s. The puzzles arise from the joint consequences of two apparently innocent and credible claims about the canons of rational acceptance.

The first claim is that a rational person need not be certain that a proposition is true before she is entitled − and indeed required − to accept it; she need only be sufficiently confident that it is true (at least more confident that it is true than that it is false). If we imagine, for the sake of convenience, that a person's degree of confidence that a proposition is true is something measurable, and if we let '1' represent the maximum degree of confidence (she is certain it is true), '0' the minimum (she is certain it is false) and '0.5' the midway point (she is as confident of its truth as she is of its falsehood), we can state this first claim as follows (where P is any proposition and X is any person):

(1)   There is a number n, $0.5 \leqslant n < 1$, such that, *if X is rational, then* X will accept P if and only if X has a degree of confidence greater than n that P.

The second claim is that, in accepting propositions, a rational person is subject to a certain sort of consistency constraint. In particular, except where her logical acumen fails her,

(2)   If X is rational, then

(a) X will accept the conjunction of any propositions she accepts;

(b) X will accept all the consequences of every proposition she accepts; and

(c) X will not accept any contradiction.

Now the puzzles. The first is a reductio argument which exploits (2) and only that part of (1) which says that, for a rational person, a high degree of confidence is *sufficient* to warrant acceptance, i.e.:

(1a)   There is a number $n, 0.5 \leqslant n < 1$, such that *if X is rational, then X* will accept P if X has a degree of confidence greater than n that P.

The second puzzle is a reductio which exploits (2) and that part of (1) which says that, for a rational person, a high degree of confidence is *necessary* for acceptance to be warranted, i.e.:

(1b)   There is a number $n, 0.5 \leqslant n < 1$, such that *if X* is rational, *then X* will accept P only if X has a degree of confidence greater than n that P.

Let us look at the first puzzle.[1]

Suppose we pick a value for n in (1a): let n be equal to 0.9. Suppose that X is rational, and X is certain (and thus by (1a), accepts the claim) that there is a one-thousand ticket fair lottery in which one and only one ticket will win. Suppose that, accordingly, X has, for each ticket t_i in the lottery, a degree of confidence equal to 0.999 that t_i will lose. Since X is rational, by (1a) and our choice of n, she will accept with respect to each $i, 0 < i \leqslant 1000$, the proposition that t_i will lose. Since X is rational, she will by (2a) accept the proposition that t_1 and t_2 and ... and t_{1000} will lose and that there are one thousand tickets in the lottery of which one and only one will win. Let us suppose that it is as obvious to X as it is to us that this proposition entails a contradiction. So, by (2b), X will accept a contradiction. But then, by (2c), X is not rational — which contradicts our supposition that she *is* rational.

The argument will obviously work for any value of n less than 0.9. And by modifying the case to increase the number of tickets in the lottery, the argument will work for any value of n greater than 0.9 but less than 1.

Now the second puzzle, employing (1b).[2]

Suppose X is rational and a professional historian. X has, after long work

and research, written a piece of history which now fills a rather hefty book. As she reads over her magnum opus she reaffirms, for each sentence in the book, her acceptance of the proposition which that sentence expresses in English. But, having finished reading, she recognizes that she has written a greater many things and that, given the ambition of her work and her human fallibility, her book is very likely to be erroneous in some detail or other. So X has a very low degree of confidence — below 0.5 — that the conjunction of the proposition expressed in her book is true. Since X is rational and since, for each propositions in her book X accepts that proposition, X is willing by (2a) to accept the conjunction of all the propositions expressed in her book. But since X is rational and her degree of confidence in the truth of that conjunction is below 0.5, X will by (1b) not accept that conjunction. And this contradicts what has just been claimed.

To dissolve the puzzles, we have to modify at least one of the two claims, (1) and (2), with which we began. Our interest, of course, is in making as minimal a modification as is needed to block the reductios. We can distinguish three distinct strategies for solving the puzzles by making such modifications.

The first strategy for dissolving the puzzles is to say that they show that (1) is too permissive. That is, we can draw the moral that, contrary to (1), a person will if rational, accept a proposition P if and only if she is certain that P is true. Thus, in the case of the lottery, X will be enjoined from accepting any prediction stronger than the claim that one ticket will win — and the puzzle will be dissolved.

But how does this strategy enable us to dissolve the second puzzle? Only by enjoining the historian from accepting much of what she wrote in her book. And here lies the strategy's unsavory characteristic: it is a strategy only a skeptic could love. For it is no less true in science than it is history that there is very little of which any rational person is certain. And so with every other field of theoretical inquiry. If we take this strategy we commit ourselves to the unfortunate claim that it is irrational to accept most scientific and other theoretical doctrine.

The second strategy is to admit (1) and reconsider the endorsement of (2). That is, we can draw from the puzzles the moral that not all the clauses in (2) are correct.[3] The denial of either (2a) or (2b) would alone suffice to defuse both puzzles. Unfortunately, the denial of any one of the three clauses would also suffice to undermine the force of one of our most powerful tools of rational criticism — the reductio argument.

In putting forth a reductio argument, a critic derives a contradiction from the conjunction of a set of propositions each of which an investigator accepts. On pain of irrationality, the investigator is forced to abandon at least one of the propositions in the set.

Suppose we ask what it is about rationality which requires this of the investigator. There are, after all, other ways in which the investigator might respond to the critic's reductio argument without abandoning any proposition in the set. The investigator might claim that her acceptance of each proposition in the set does not commit her to accept their conjunction; she might argue that, therefore, insofar as the critic has derived a contradiction only from the conjunction of the propositions in the set, the critic has not cast aspersion on any proposition in the set. Or the investigator might admit that she is committed to accept the conjunction of the propositions but deny that she is thereby committed to accept its consequences; she might argue that, as a result, the fact that the conjunction entails a contradiction casts no aspersion on the conjunction. Or the investigator might admit that her acceptance of each proposition in the set does commit to accept a contradiction, but she may deny that there is anything wrong with accepting contradictions.

The answer is clearly that what forces the investigator, on pain of irrationality, to cease acceptance of at least one proposition in the set is the fact that the canons of rationality prohibit these alternate responses. They dictate that, the investigator will not be rational if, while she accepts each proposition in the set, she is unwilling to accept their conjunction; or if while she accepts their conjunction, she is not willing to accept its consequence; or if she is willing to accept a contradiction. That is, what prohibits the rational investigator from ignoring the critic's reductio argument is simply (2). In other words, if we abandon (2), we license rational persons to blithely ignore reductio arguments.[4]

Finally, the third strategy: we can admit that certainty is not a prerequisite for rational acceptability, admit that (2) is proper and conclude that therefore both (1a) and (1b) are incorrect. That is, from the first puzzle, we can draw the moral that, for a rational person, no degree of confidence short of certainty is alone sufficient to warrant acceptance; from the second, the moral that, for a rational person, a degree of confidence greater than 0.5 is not necessary to warrant rational acceptance.[5]

This strategy unfortunately carries with it obvious burdens. It requires our

acquiescing in the claim that a rational person may be prohibited from accepting some propositions of whose truth she is nearly certain (as in the lottery case) and required to accept some propositions of whose falsehood she is nearly certain (as in the historian case). Moreover, since on this strategy we are neither enjoining acceptance of all merely probable propositions (as we do on the first strategy) nor endorsing the acceptance of every garden variety implausibility, the third strategy requires that we find some way of distinguishing the acceptable probable propositions from the allegedly unacceptable ones and the allegedly acceptable improbabilities from the unacceptable ones.

Given that there is no strategy for modifying (1) or (2) which does not entail taking at least one of the three strategies already surveyed, we can conclude that the following *normative difficulty* afflicts the theory of rational acceptance: we have two canons of rational acceptance, (1) and (2), such that we cannot endorse both without engendering paradox, and cannot abandon either without drastically revising our intuitions concerning rational acceptability. But this is not the worst of our troubles. For this normative difficulty lays bare a second, deeper, difficulty in the theory of rational acceptance.

Thus far we have presumed that, while we may be puzzled about the conditions under which a rational person ought to accept a proposition P, we have a clear fix on what it *is* for a person to accept P. But our discussion raises serious doubt as to whether we can legitimately claim even that much insight into the theory of rational acceptance.

It is fair to say (although the evidence is not easily gathered) that virtually all philosophers have taken acceptance to be exactly what one would innocently expect it to be: a state of confidence. Not very many have written as if they thought acceptance was a state of certainty, and it is just as well.[6] To be certain that P is true is to be certain that anything that entails P's denial is false (indeed, as certain as one is that a contradiction is false). Were we to view acceptance as a state of certainty, then, we would expect a person who accepts a theory T to perforce view any theory which clearly denied T to be as incredible as a contradiction. And, of course, we expect no such thing. A person may accept the theory that Hitler died in his bunker yet view the theory that Hitler escaped to Argentina as credible to some degree — more credible, for example, than the theory that Hitler turned into a butterfly or the theory that Hitler both died and did not die.

More plausible than the certainty-view of acceptance is the view that to accept P is to have a degree of confidence above a high threshold short of certainty. It is this, the *confidence-threshold view*, which I suspect most philosophers hold. And it is this view which is fundamentally undermined by the normative difficulty we have been discussing.

Consider the part of (1) which contributed to the lottery puzzle. Recall that (1a) states that there is a number n, $0.5 \leqslant n < 1$, such that *if X is rational, then X* will accept P if X has a degree of confidence that P greater than n. Suppose that we flesh out the confidence-threshold view by picking a number k to be the threshold. k, of course, will be less than 1. Substitute 'X will have a degree of confidence greater than k that P' for 'X will accept P' in (1a) and the problem with the confidence-threshold view becomes clear. (1a) now says that there is an n, $0.5 \leqslant n < 1$, such that, *if X is rational, then X* will have a degree of confidence greater than k that P if X has a degree of confidence greater than n that P. But this is just an instantiation of a trivial truth of arithmetic: for any k, $k < 1$, there is an n, $0.5 \leqslant n < 1$, such that $k < n$. The talk of rationality in (1a) becomes entirely superfluous – (1a) is true simply by the definition of 'accept'. And, insofar as the first and third strategies controvert (1a), the confidence-threshold view makes them incoherent – for they entail, on this view, the denial of a truth of arithmetic.

What is wrong with the confidence-threshold view is *not* that it supports the second strategy over the others – after all, we cannot resolve the normative difficulty without supporting one strategy over its rivals. What is wrong is that it does so without introducing any normative considerations whatsoever. On the contrary, it supports the second strategy by undermining the very claim that there can be any normative difficulty at all – for, by its definition of 'accept', the confidence-threshold view makes every proposal which denies (1a) *ipso facto* incoherent! In short, if there is even the least bit of integrity in our claim to have exposited a normative difficulty in the theory of rational acceptance – if (1a) is more than just a trivial truth devoid of normative content and if the first and third strategies are more than incoherent proposals to controvert arithmetic – then the confidence-threshold view is mistaken.[7] Which is to say that, being neither a state of certainty nor a state of confidence above a threshold, acceptance is not a state of confidence at all.

Hence the conclusion that there is a second, *psychological difficulty* plaguing the theory of rational acceptance: the mere intelligibility of the

normative difficulty in the theory of rational acceptance entails that accep-
tance is not the state of confidence we had always thought it was. This new
difficulty, then, lies in trying to understand what, if anything, worthwhile we
could be saying of X when we say that X accepts P.

II

As I indicated, my intention is neither to smother the reader in a jumble of
conflicting intuitions nor to fashion from that jumble a story about how, given
its foundational difficulties, the theory of rational acceptance is impossible.
My intention is rather a constructive one: to provide new foundations for the
theory of rational acceptance. Needless to say, the first step must be to resolve
the psychological difficulty in the theory of rational acceptance — to say
what sort of state acceptance is.

The reader may notice that here, as before, I present the psychological
difficulty — the indefinability of acceptance-talk in terms of confidence-
talk — as a sign that we have a tenuous grasp on acceptance-talk. It is not
obvious that this is fair. Why not say instead that the indefinability of accep-
tance-talk in terms of confidence-talk casts aspersion upon the intelligibility
of the latter? The reason is that ascriptions of degrees of confidence to
persons independently enjoy a sort of intelligibility which, given the
indefinability of acceptance-talk in terms of confidence-talk, ascriptions of
acceptance do not. Indeed, it is this very sort of intelligibility which is the
proper object of our search for an account of what sort of state acceptance
is. What we need is a story about how the acceptance of propositions
impinges upon human practice. We need to know what difference a person's
acceptance of a proposition makes to the way she will behave — whether
within or without the context of inquiry. We need an account of how the
attribution of acceptance to persons contributes to our understanding of
human conduct.

In contrast, we already have such an account for our attributions of
degrees of confidence to persons: the Bayesian theory of rational decision.
The beauty of the Bayesian theory of rational decision is that it weaves
confidence-talk into a powerful normative account of the workings of
rational deliberation. Beginning with some intuitive normative constraints
on rational preference and the homely insight that X is more confident that

P than she is that Q just in case, given the choice, X would prefer to bet on the truth of P, the theory derives the following remarkable results.

Consider the components of manageable decision-problem: a set of possible acts open to the agent X; a   set of mutually exclusive and jointly exhaustive hypotheses describing possible states of the world; for each act and hypothesis a description of what outcome the act will have if the hypothesis is true. If X is rational (and endowed with sufficient logical accumen and mathematical ability) then (i) for each hypothesis, X's degree of confidence that this hypothesis is true will be measurable on a scale from 0 to 1; (ii) this assignment of numbers to hypotheses (X's *degree-of-confidence function*) will satisfy the axioms of the probability calculus; (iii) there will be a second function, X's *utility function*, which measures the preferability for X of the outcomes; and (iv), X's solution to her decision problem will be determined by the two functions – she will choose to perform an act which bears maximum *expected utility* (an act which receives the maximum value as a result of adding together the weighted utility of each of its possible outcomes, the utility of each outcome being weighted by X's degree of confidence that the hypothesis, on whose truth .the occurrence of the outcome would depend, is in fact true).[8]

The question posed by the psychological difficulty in the theory of rational acceptance is, then, properly put as follows: Given that acceptance is not a state of confidence and given the Bayesian account of the place confidence-talk enjoys in the story of human endeavors, what legitimate place in that story can we find for attibutions of acceptance?[9]

The few attempts that have been made to state explicitly how acceptance is reflected in human conduct have, unfortunately, been versions of an act-analysis: to accept P is to be disposed to act (or, to act in certain circumstances) as if P were true. It is unfortunate because the analysis entails the claim that the acceptance of propositions is a decisive doxastic input into decision-problems – or, at least, into those decision-problems in which the agent accepts one of the hypotheses describing the possible states of the world. And, given the Bayesian theory of rational decision, this claim is false. For in the case of a rational person confronted by a manageable decision-problem, it is her degree-of-confidence function which provides the decisive doxastic input – and acceptance, as we have seen, is not a state of confidence.[10]

It is a mistake to suppose that the proper role of acceptance-talk is to describe the doxastic input into rational deliberation. This is, rather, the role of confidence-talk. The proper function of acceptance-talk is to describe a certain feature of our behavioral repertoire – the practice of defending propositions in the context of inquiry.

Were it the case that an investigator habitually indicated, each time she defended a proposition, just how confident she was that this proposition was true, we could see the practice of defending propositions in the context of inquiry as a straightforward means of expressing one's degree of confidence in the truth of propositions. But, typically, an investigator does nothing of the sort. What she defends in books, lectures, and teachings, she defends without any significant qualification.[11] This is where acceptance-talk earns its keep. We will not always say, simply on the evidence of a person's having defended P in a lecture or in a scholarly journal, that she accepts P. We may have reason to suspect, for example, that she fears for her safety if she does not publicly espouse P. But we *will* say she accepts P insofar as we are willing to say – whether on the basis of a critical interpretation of her text or, as in the more common cases, on the basis of what she defends in word and in print – that she would defend P were here sole aim to defend the truth. My suggestion is that we should view 'X accepts P' as just shorthand for 'X would defend P were her aim to defend the truth'.[12]

In so doing, we not only say how acceptance impinges upon human practice but we locate it in a rather prominent role. After all, the practice of defending propositions as if one's aim were to defend the truth is, by all appearances, one to which we attach a great importance. The amount of time and energy we devote to the books and papers which are but the most conspicuous products of this practice attests to our preoccupation with what investigators are willing to defend and the advisability of their being disposed to defend what they do.

It may nonetheless seem, on first blush, that this account of acceptance will not be helpful with regard to the psychological difficulty it is supposed to resolve. One may think that, on this account, our acceptance-ascriptions will be trivially determined by our degree-of-confidence-ascriptions – and in exactly the same way as they are on the confidence-threshold view of acceptance. If so, the defect of the confidence-threshold view will just recur in a new form.

But we actually have no reason to expect this result. There is a kind of

person — a rational person — of whom we should expect that her willing-
ness to defend a proposition P when her aim is to defend the truth may *not*
be a function of whether her degree-of-confidence that P is true is above
a threshold. As we saw, one of the morals of the Bayesian account of rational
decision is that, in the case of a rational person, what she will decide to do
will in general be an intricate function of two things: her degree-of-confidence
function and her aims. We have no right to presume that her decisions as to
what to defend when her aim is to defend the truth will be determined in any
more straightforward a way. Thus we have no grounds for thinking that an
analogue of the confidence-threshold view of acceptance is being proposed
here. Let us then push ahead and see whether we can make headway on our
normative difficulty.

III

The truth about a matter is more than just *a* truth about the matter. Long
after we knew *a* truth about the Watergate break-in (e.g. that Howard Hunt
participated) *the* truth about Watergate was still being sought. The truth
about a matter is just the comprehensive true story about the matter. The
aim to defend the truth, taken quite literally, is not then an aim which one
can, in general, expect to achieve. By 'the aim to defend the truth' I mean,
rather, the aim to defend as comprehensive a part of the truth as one can.

  Not surprisingly, the desire for truth and the desire for comprehensiveness
often conflict. When an investigator chooses what to include in her theory
of some matter, the choice between including a stronger claim rather than a
weaker one will often amount to a choice to *either* indulge her desire for
comprehensiveness at the cost of increasing her risk of frustrating her desire
for truth *or* override the desire for comprehensiveness so as not to incur
that risk. Thus a theory of rational acceptance, in my sense of 'acceptance', is
a theory which says how a rational person ought to adjudicate between these
oft-competing desires so as to decide what she should be disposed to defend
when her aim to defend the truth — i.e., what she should accept.

  One consequence of this way of looking at a theory of rational accept-
ance is that the prospects for successfully constructing such a theory —
i.e., for resolving the first, normative, difficulty in the theory of rational
acceptance — become a good deal brighter. It is not that the normative diffi-
culty ceases to be a genuine difficulty nor that the distasteful consequences

of adopting the strategies cease to be genuine consequences. It is rather that, once we adopt the point of view developed in the last paragraph, there is one strategy whose hitherto distasteful consequences cease to cloy. Not only can we find a rationale for embracing these consequences, but we can see how they came to seem so distasteful.

The strategy I have in mind is neither the first nor the second. Given our sense of 'acceptance', the doctrines the first two strategies must respectively embrace (it is irrational to accept most of science, a rational person may safely ignore reductio arguments) seem if anything, even more repugnant than they did earlier. It is the third strategy which assumes a new plausibility.

According to the third strategy, we are to conclude from our two puzzles that, for a rational person, a high degree of confidence is neither sufficient nor necessary to warrant acceptance. Recall that what seemed unpalatable about this moral was that it both prohibits a rational person to accept some propositions of whose truth she is all but certain (e.g., for some ticket in the lottery, the proposition that this ticket will lose) and requires her to accept some propositions of whose falsehood she is all but certain (e.g., in the case of the historian, the conjunction of all the propositions expressed in her book). But notice how much of this unpalatability derived from the assumption, at that point still unchallenged, that acceptance is a state of confidence.

Consider first the claim that (a) no matter how confident she is that P, a rational person, so long as she is not certain that P, may be required not to accept P. From the point of view of someone who sees acceptance as a state of confidence, (a) is tantamount to the assertion that acceptance is a state of certainty — it entails that, for every degree of confidence short of certainty, it is possible for a rational person to have that degree of confidence that P yet fail to accept P. It is no wonder, then, that (a) should have seemed bizarre. The view that acceptance is a state of certainty is, as we noted earlier, not even initially plausible.

Once we recognize, however, that acceptance is no state of confidence and we adopt the view of acceptance I am proposing, (a) loses its bizarre air. The aim to defend the truth is, as we noted, a complex of two aims: the aim to defend a truth and the aim to defend something maximally comprehensive. Each decision to defend something stronger than what one already defends, involves (from the agent's point of view) a risk of frustrating the first aim in favor of satisfying the second — so long as the proposition to be added is not one of which the agent is certain. The moral which the third strategy

On the view of acceptance being adopted here, the historian is faced with a straightforward choice: *either* (i) defend the conjunction of the propositions in her book and, hence, the proposition that everything she defends in it is true; *or* (ii) defend the denial of the conjunction and, hence, the proposition that not everything she defends in the book is true; *or* (iii) do neither (i) nor (ii). (Given the consistency constraint rightly imposed by (2), the historian cannot on pain of irrationality choose both (i) and (ii).) If she chooses to take the first option, she will achieve a great deal of comprehensiveness at the cost of incurring a high risk of defending something false. Option (ii) offers very little comprehensiveness but also very little risk of falsehood. Option (iii) offers no comprehensiveness and no risk of falsehood. If X is well-disposed towards the second rational course described above – if she feels that large trade-offs of the desire for truth in favor of the desire for comprehensiveness are worthwhile – she may be entirely rational to choose the first option and reject the others. That is, she may be rational to reject (i.e., accept the denial of) the claim that not everything she accepts is true.

<center>IV</center>

The last section, of course, provides nothing more than a sketch of how the prima facie unpalatable consequences of the third strategy become sweeter once we adopt the view of acceptance I have proposed. The integrity of this sketch ultimately depends upon the viability of its picture of rational investigators as agents who adjudicate between the competing aims of truth and comprehensiveness when deciding what to accept. And nothing has yet been said about how such an adjudication might work – in particular about how an adjudicator might be able to tell acceptable improbabilities from unacceptable ones.[13] What we need is a theory which will fill out the sketch – a theory which will execute the third strategy by telling us, for any proposition P, when a person if rational will accept P.

It is beyond the scope of this paper to present such a theory. My purpose here has rather been to argue that it is a kind of theory worth having.[14] But it is only right that I should point out before concluding that such a theory of rational acceptance will turn out to be a rather different sort of theory than we may have expected.

I suggested earlier that our preoccupation with what people defend (as if their aim were to defend the truth) indicates that acceptance, in the sense of

draws from the lottery puzzle — and which (a) states — is simply that there are conditions (which hold in the lottery case) under which no such risk is worth it.

Much the same story can be told for the second consequence of the third strategy which we found so distasteful: that (b) a rational person may be required to accept some propositions of whose falsehood she is all but certain. (b) is not a claim to the effect that despite the evidence of her fallibility, the historian ought to become extremely confident that everything she said in her book is true (although, thinking of acceptance as a state of confidence, we may well have read (b) this way). Rather, on the view of acceptance I am suggesting, (b) simply says that the historian should be willing, when her aim is to defend the truth, to defend the claim that everything she said in her book is true.

Of course, in saying this much, (b) is requiring the historian to defend a proposition she is nearly certain is false. But, given that her aim in so doing is to defend the truth, the rationale for the requirement is not hard to find. As Karl Popper has been saying for many years, one cannot have theories which are the least bit comprehensive unless one is willing to run a high risk of defending something false. Given this fact, one rational course is to decide that the subordination of the desire for truth to the desire for comprehensiveness required in order to justify defending such theories is simply not worth it. One then refrains from defending comprehensive theories such as the historian's. Another course, also rational, is to decide that the trade-off of one desire against the other is worth it. Then one will be willing to defend theories like the historian's. The message in (b) is that we should recognize that since we are not prepared to censure the historian in our second puzzle for having written her book — for having defended each proposition expressed in her book as if her aim were to defend the truth — we are committed to the second rational course and, thus, need suffer no qualms in requiring the historian to defend their conjunction.

The understandable temptation to suppose that the historian should accept the proposition that not everything she accepts is true is to be resisted. It is indeed true that she should be extremely confident that not everything she accepts is true. But given the view of acceptance adopted here and the rationale it provides for rejecting (1a) and (1b), it neither follows (as it might from (1a)) that the historian is rational to accept the claim that not everything she accepts is true, nor (as it would from (1b)) that she is not rational to accept its denial.

the word being employed here, is a prominent feature of rational inquiry. I could have added that, in addition, our attempts to grapple with decision problems concerning what to accept (as with other decision problems) may serve as important spurs to inquiry. Nonetheless, there is a sense in which, on the view of acceptance I have been proposing, a theory of rational acceptance is epistemologically epiphenomenal. After all, one important determinant of what X will accept will be her degree-of-confidence function − but the contour of that function is, presumably, in no way determined by what she accepts, since acceptance is not a state of confidence. Indeed, most of the traditional problems of epistemology, e.g., the controversy over foundationalism to which we alluded at the outset, presumably arise not in the theory of rational acceptance but rather in the theory of rational degree-of-confidence functions.

Some may, therefore, complain that in the final analysis our account makes less of the theory of rational acceptance than has always seemed appropriate. No doubt it does. But for this I think we need not apologize. Rather, we can respectfully ask the philosopher who would accord acceptance a more pivotal role in epistemology to tell us how *her* acceptance-talk fits into a comparably comprehensive account of rational human inquiry and practice.

University of Wisconsin − Milwaukee

NOTES

[1] What follows is a variant of an argument due to H. Kyburg, Jr., Probability and the Logic of Rational Belief (Wesleyan University Press, Middletown, 1961), p. 463, and C. G. Hempel, 'Deductive-nomological vs statistical explanation', in Minnesota Studies in the Philosophy of Science, Vol. 3, ed. by H. Feigl and G. Maxwell (University of Minnesota Press, Minneapolis, 1962), pp. 163−166.
[2] The following argument is a variant of the one whose name serves as a title to D. C. Makinson's 'The paradox of the preface', Analysis XXV (1965), pp. 205−207.
[3] This move has been championed by Henry Kyburg in a number of places, e.g., 'Conjunctivitis', in Induction, Acceptance and Rational Belief, ed. by M. Swain (D. Reidel, Dordrecht, 1970), pp. 55-82. See also Richard Foley, 'Justified inconsistent beliefs', American Philosophical Quarterly XVI (1979), pp. 247−257. Foley's paper contains numerous references to like-minded writers.

⁴ Indeed, it is hard to see how our two reductios can convince us that we must abandon (2) – once we recognize that it is only because we are committed to (2) that we feel these reductios have any critical force at all.
⁵ It has been popular to deny (1a) but, owing to the relative obscurity in which the second puzzle has languished, few seem to have felt the need to confront or abandon (1b). Isaac Levi is an exception. He boldly advocates the third strategy in Gambling with Truth (Knopf, New York, 1967). So does Keith Lehrer. See Knowledge (Clarendon, Oxford, 1974) and 'The racehorse paradox', in Midwest Studies in Philosophy, Vol. 5, ed. by P. French, T. Uehling, and H. Wettstein (University of Minnesota Press, Minneapolis, 1980), pp. 183–192.
⁶ See, however, Issac Levi's discussions of what he calls 'acceptance as evidence' in Gambling with Truth (op. cit.) and 'knowledge' in The Enterprise of Knowledge (MIT, Cambridge, 1980). I discuss Levi's view in a review of the latter book (forthcoming in The Philosophical Review).
⁷ Let me put this point another way. Both the first and third strategies say that, no matter how confident X is that P at a given time, she may (provided she is not certain that P) be required to refrain from accepting P at that time. One may feel that this is not a justifiable prescription – that it is not a prescription a person ought to follow – but it does seem (and has apparently seemed to every writer on the subject) to be a prescription a person could conceivably follow. But, on the confidence-threshold view of acceptance, the prescription is not one a person can even follow. For once X's degree of confidence that P exceeds k, it is not in her power to comply with the prescription since, by definition, she already accepts P. Hence, even if the first and third strategies are nothing more than repugnant alternatives to the second, the confidence-threshold view of acceptance is mistaken.
    It is a consequence of this point, of course, that any philosopher who adopts the confidence-threshold view and pursues any strategy other than the second is (provided there is no unseen flaw in our description of the lottery case) guilty of inconsistency. See, for example, Keith Lehrer who, in Knowledge (ibid.), both explicitly endorses the confidence-threshold view of the nature of belief (p. 13, p. 63) and pursues the third strategy with respect to the lottery (pp. 192–198).
⁸ The classic exposition is to be found in L. J. Savage, The Foundations of Statistics, 2nd ed. (Dover, New York, 1972).
⁹ Note that I am not arguing that in order to maintain that acceptance is a state of confidence one must jettison probability theory. I am rather arguing that to maintain that acceptance is a state of confidence one must jettison the Bayesian theory of rational decision. It is worth noting because, for some adherents to Bayesian decision-theory, an argument for the second claim is tantamount to an argument for the first. These philosophers and statisticians hold that probability theory is intelligible only if it is a theory about the degree-of-confidence function a rational person ought to have – and, thus, it is a correct theory only insofar as the Bayesian theory of rational degree-of-confidence functions is correct. There is, however, nothing in the Bayesian theory of decision which entails this condition of adequacy on an analysis of probability claims and there is nothing in my enthusiasm for that theory of decision which commits me any such condition of adequacy.
    None of this is meant, of course, to provide comfort to those who would argue that the indefinability of acceptance-talk in terms of confidence-talk casts aspersion on the latter. For while I do want to say that one can argue this way without thereby undermining probability theory, I also want to say that one cannot argue this way without forgoing a remarkably deep and compelling account of the relation between rational inquiry and practice – i.e. the Bayesian theory of rational decision.
¹⁰ Indeed, even if the confidence-threshold view were correct, the act-analysis would still be mistaken. See R. Jeffrey, 'Valuation and the acceptance of scientific hypotheses',

Philosophy of Science XXIII (1956), pp. 237–246 and I. Levi, *op. cit.*, pp. 7–16.

[11] Except, of course, when she explicitly qualifies the claim probabilistically. It is true that some investigators are more apt than others to insist on the tentativeness of their theories, or to draw attention to parts they wish to advocate more tentatively. But, in the end, the general admission of tentativeness serves merely as a syncategoramic expression of modesty or open-mindedness, and the local tentativeness, if genuine, occurs only in the context of a bona fide defense of the rest of the theory.

[12] This account of acceptance-talk was inspired by R. B. DeSousa's 'How to give a piece of your mind: Or, the logic of belief and assent', Review of Metaphysics XXV (1971), pp. 52–79, especially pp. 62–63. Note that, what I mean in saying 'X would defend P' is just that X would assert P or assent to P (*not* that X would argue for P).

[13] Some may indeed think that the adjudication is not possible given that it seems that (i) comprehensiveness does not lend itself to measurement and that (ii) propositions not related to one another by entailment are not comparable with respect to comprehensiveness. Although I think that (i) and (ii) are both true, I also think that a theory of how the adjudication should work can be developed —without assuming that there is more to go on than an ordering of propositions related by entailment.

[14] I construct this kind of theory in 'A Bayesian theory of rational acceptance', The Journal of Philosophy LXXVIII (1981), pp. 305–330.