COHERENCE

BRIAN SKYRMS

I. Pumping gold

A very rich and reliable bookie posts odds on a horse race. He values a contract which pays a pound of gold if Stewball wins at one ounce of gold. He values a contract which pays a pound of gold if Molly wins at one ounce of gold. He will buy or sell these contracts in any quantity at what he considers the fair price or better. Suppose that he also deals in contracts which pay a pound of gold if either Stewball or Molly win, and that he values these at four ounces of gold. If you had two ounces of gold, you could then buy separate contracts for Stewball and Molly for a net outlay of two ounces of gold and sell back a disjunctive contract on Stewball or Molly winning for four ounces of gold. With these you can now buy two seperate contracts each on Stewball and Molly, and sell back two disjunctive contracts, etc. If you don't have two ounces of gold, perhaps your banker will lend them to you on the strength of your prospects. You are able to make a *Dutch Book* against the bookie—a finite number of bets whose net gain is positive no matter what the outcome of the race—because his evaluation of the disjunctive contract does not cohere with his evaluation of the seperate contracts on its disjuncts.

Coherence requires that if the bookie values the seperate contracts at one ounce of gold each, he values the disjunctive contract which pays off exactly as holding both seperate contracts together would, at two ounces of gold. Coherence also requires that a contract that pays a pound of gold no matter what is worth a pound of gold; and that a contract that pays off a pound of gold in some circumstances and requires no payment otherwise, has non negative value (i.e. the bookie would not pay someone to take it off his hands.) What are we to make of these obvious, but nevertheless remarkable, facts?

deFinetti (1937) used them as a foundation for the theory of personal probability. The value to you (in pounds of gold) of a contract which pays one pound of gold if p; nothing otherwise is taken as your personal probability of p. Then if your personal probabilities are *coherent*—i.e. a cunning bettor cannot make a dutch book against you in a finite number of bets-your personal probabilities must satisfy the mathematical laws:

(1) Probability is non-negative
(2) Probability of a tautology is one.
(3) If p;q are mutually exclusive alternatives, the probability of their disjunction is the sum of their probabilities.

The mathematics of these classical dutch book arguments is rather trivial and is certainly well understood, but their epistemological status remains a matter of philosophical controversy. And the matter of how one is to change from one coherent probability assignment to another under the pressure of new evidence is one which is not settled by the classical static coherence arguments. In this paper I would like to do two things: (I) suggest that when properly viewed the classical coherence arguments have more epistemological force than many critics have been willing to grant them, and (II) report some recent work on *dynamic* coherence.

II. Preaching to the converted

The classical result is that a bookie whose posted betting ratios do not conform to the probability calculus can have a dutch book made against him. There are two approaches one can take in analyzing the significance of this result. One can (A) focus on the pragmatics of posting odds and attempt to construct an appropriate general theory or (B) one can try to locate the reason for the dutch book in a deeper inconsistency.

If one pursues course (A) it rapidly becomes clear that a general theory of optimal betting behavior will be extremely complicated and will lack the simple connection with degree of belief postulated in the classical dutch book situation. The bookie may know something about the bettor or bettors he is likely to encounter. He may know something about their propensities to bet when offered various options. Depending on the details he may be best advised to posted betting ratios which diverge widely from his true degrees of belief, and in certain situations would do well by posting *incoherent* betting ratios. To take an extreme and absurd case you can imagine that you will only face one bettor and that you know that when confronted with incoherence he will—for whatever reason—run the dutch book in reverse, pumping gold into your treasury instead of out. There are less trivial cases, however, whose analysis is of interest [e.g. see Adams and Rosenkrantz (1980)].

Ramsey [1931] takes the second approach:

> If anyone's mental condition violated these laws, his choice would depend on the precise form in which the option were offered him, which would be absurd. He could then have book made against him by a cunning bettor and would stand to lose in any event.

For Ramsey, the cunning bettor is a dramatic device and the possibility of a dutch book a striking symptom of a deeper incoherence. He suggests that coherence is just what logicians call "extensionality." If we are interested in the status of the dutch book theorems as a foundation for the theory of personal probability, this direction is clearly the one which merits investigation.

What will not do, is to confuse the two sorts of inquiry and to attempt to use the spectre of the cunning bettor as a bogie to coerce compliance with the laws of probability. Perhaps one should not let the bettor choose which side of the bet to take [Baille (1973)]. Perhaps one is not likely to run across a cunning enough bettor at all [Putnam (1975), Kyburg (1978), Chihara and Kennedy (1979), Glymour (1980)]. Must a rational person always act as if he is confronted by a cunning bettor, ever when he isn't [Jackson and Pargetter (1976)]? If one is interested in the status of the theory of personal probability, these concerns operate at the level of the dramatic device, rather than that of the underlying logic.

Let us look a little more closely at the structure of the dutch book argument for additivity, keeping in mind all the while the Ramsey point of view. Assume for the time being that all that is of value in the betting situation is payoffs in gold, and that gold has constant marginal utility (i.e. an ounce of gold counts for as much to the bookie whether added to big winnings or big losses). A *betting arrangement, B,* is a function from possible states of affairs to payoffs in gold. A *bet* on a proposition, p, is a betting arrangement which returns some gain, a, whever p, is true and some loss, b, whenever p is false. The *aggregate,* $B^1 \# B^2$, of two betting arrangements is the betting arrangement which has at each possible state of affairs, w, the sum of the payoffs at w of the constituent bets, B^1; B^2:

$$B^1 \# B^2 (w) = B^1 (w) \# B_2 (w) \qquad \text{for all } w.$$

Aggregation corresponds to the net effect of holding several bets. If the bookie is to be consistent, his prices should be additive over aggregation:

$$V (B^1 \# B^2) = V (B^1) + V (B^2)$$

since if he sells you B^1 for an ounce of gold and B^2 for two ounces of gold, he has *in effect* sold you $B^1 \# B^2$ for three ounces of gold. But then it is simply a matter of logic that if $p;q$ are incompatible propositions, the aggregate of a bet that returns one pound of gold if p; nothing otherwise and a bet that returns one pound of gold if q nothing otherwise *is* a bet on the proposition p *or* q which returns one pound of gold if p or q; nothing otherwise. So, by definition, probabilities of incompatible alternatives add. [As an aside I might add that the argument works just as well for countable as for finite additivity. See Adams (1962).]

Some critics [e.g. Kyburg (1978), Schick (forthcoming)] have argued that the dutch book argument falls short essentially because additivity over aggregation fails in many real world situations. Non-

monetary goods exhibit complementarities. A bet which returns a sow and a boar if the coin comes up heads may be worth more to you than the sum of the values of the separate bets. And monetary goods may have declining marginal utility. A bet which returns 100 pounds of gold if p may not have for you 100 times the value of a bet which returns one pound of gold if p. These phenomonae have been remarked upon at least since Daniel Bernoulli, and Ramsey and deFinetti were well aware of them. To discuss them with any clarity, we need a theory of utility.

von Neumann and Morgenstern (1944) rediscovering an idea of Ramsey (1931) suggested that we measure utility roughly as follows. We take the least desirable payoff in the decision situation and conventionally choose it as the zero point of our utility scale; we take the most desirable payoff and conventionally give it utility of one. Then using some chance device with known chances, and whose outcomes are "ethically neutral" we judge another possible payoff to have value x if we are indifferent between it and a wager which gives a chance x of getting the most desirable payoff, otherwise the least desirable payoff. [Ramsey goes further, dispensing with the known objective chances in favor of subjective surrogates, but there is no time here to go into these details.] In terms of such a theory of utility, complimentary goods and declining marginal utility of monetary instruments makes perfect sense. What then do these phenomonae show about additivity over aggregation?

Let us say that the *physical* aggregate of two bets, B^1; B^2 pays off in each possible situation both the physical goods that B^1 pays off and the physical goods that B^2 pays off. Let us say that B^3 is the *mathematical* aggregate of B_1 and B_2 if B_3 is such that at each possible situation, the utility of its payoff is equal to the sum of the utilities of the payoffs of B^1 and B^2. The payoffs are in goods which exhibit complimentarities or variable marginal utilities, a physical aggregate of two bets may not be a mathematical aggregate of them. Such cases provide examples of failures of additivity of utility over physical aggregation, but not failure of additivity over mathematical aggregation. To say that such examples provide counter examples to the additivity of value over aggregation is, I shall argue, a little like saying that the fact that adding a quart of water to a quart of alcohol does not yield two quarts of fluid provides a counter example to the laws of arithmetic.

In the general setting in which we are now working, the mathematical aggregate of two bets need not represent the net payoff resulting from undertaking both B^1 and B^2. What is the point of mathematical aggregation? It is part of a descriptive language. Suppose that propositions $p;q$ are incompatible and that one has a betting arrangement that has a payoff of utility x if either p or q is true and a payoff of utility zero otherwise. Such a betting arrangement can be correctly described either as a bet on the disjunctive proposition, *p or q* or as the mathematical aggregate of two bets, one of which yields a payoff of utility x if p is true, utility zero otherwise; the other of which yeilds a payoff of utility x if q is true, utility zero otherwise. It is additivity over *mathematical* aggregation, not physical aggregation, that is important for the dutch book argument.

Now we have abstracted away from the inessential features of bookmaking so that bets and betting arrangements have given way to random variables (i.e. the functions that give the payoffs in utility in each possible situation). Additivity of payoffs *in a situation:*

$$U\ [B_1 \# B_2\ (w) = U\ [B_1\ (w)\ ] + U\ [B_2\ (w)\ ]$$

which fails for physical aggregation, holds *by definition* for mathematical aggregation. Then, if we grant (1) The constant betting arrangement, C, which returns utility V in every possible circumstance has expected utility V, $EU(C) = V$ and (2) If $EU\ (B^1) = EU\ (B^{1\prime})$ and $EU\ (B^{2\prime}) = EU\ (B^{2\prime})$ then $EU\ (B^1 \# B2) = EU\ (B^1 \# B^{2\prime})$ then it follows that expected utility is additive over mathematical aggregation:

$$EU\ (B^1 \# B^2) = EU\ (B^1) + EU\ (B^2)$$

If $p;q$ are incompatible the following are descriptions of the same betting arrangement: (I) A bet on *p or q* which returns utility one if *p or q*; utility zero otherwise, (II) The mathematical aggregation of a bet on p which returns utility one if p; zero otherwise and a bet on q which returns utility one if q; zero otherwise. Then by additivity over mathematical aggregation; the definition of probability in terms of expected utility; and the principle that a betting arrangement gets the same expected utility no matter how described we get the additivity law for probability.

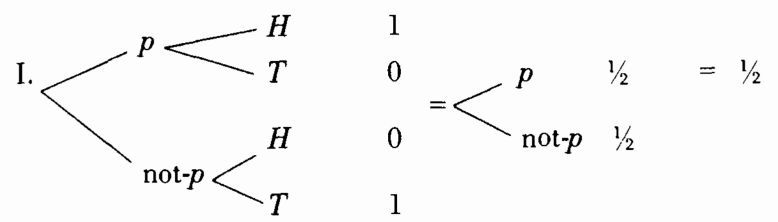
Allow me to make a brief remark about the Ramsey extensionality principle. Belief is notoriously non-extensional. Why should degree of belief obey an extensionality principle? The answer is, of course, the fact that the two descriptions designate the same betting arrangement is an immediate consequence of the underlying truth functional logic. Ramsey's view is that coherence is a kind of *consistency* and that the theory of personal probability is a branch of *logic.*

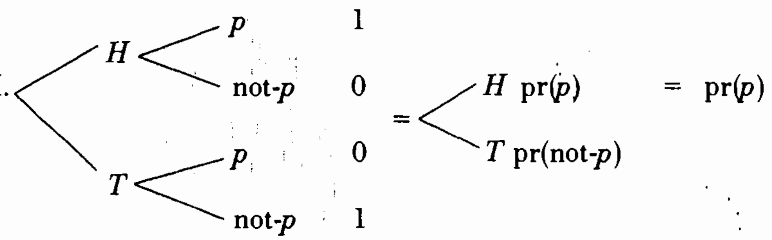
Persistent critics of the argument will want to attack (2) above, though it is hard to see how. Since we have passed to utility theory, and mathematical aggregation, there is no way in which payoffs can interact at worlds other than by addition. Since our abstracted "bets" are not events or propositions but random variables there is no way for a "bet" to influence the probabilities salient to the evaluation of an aggregate. For those who are nevertheless still dubious about (2) it is possible to found additivity on more restricted substitution principles within utility theory [e.g. see Anscombe and Aumann (1963) and Raiffa (1968).] Those who are dubious about even the restricted substitution principles should consider what we can do in special cases with (1) alone.

Consider this twist on Raiffa. Suppose that there is a known fair coin, so that CHANCE (H) = CHANCE (T) = ½ is known. Suppose p is an "ethically neutral" proposition in the sense of Ramsey, i.e. that the truth or falsity of p makes no difference to the decision maker no matter what the payoffs. Suppose also that p is such that the decision maker is indifferent between the gambles (i) utility 1 if p; utility 0 if not p, and (ii) utility 1 if not p; utility zero if p. Consider a gamble with the payoff matrix:

	p	not p
H	1	0
T	0	1

This gamble can be represented two ways:



II.


The first equality in I comes from the known chance and definition of utility; the second fom principle (1) page 9. The first equality in II comes from the definition of personal probability of p and the subjects stated indifference; the second from principle (1) page 9. I and II are descriptions of the same betting arrangement. The different orders in I and II are artifacts of the description rather than indicative of any temporal or causal order. So consistency requires that $\mathrm{pr}(p) = \mathrm{pr}(\text{not-}p) = \frac{1}{2}$. Along the same lines we can validate an n-fold equiprobable partition for any n. If a critic lets us get this far, getting the rest of the way is not difficult.

Once we have seen the foregoing, do we really need the physical probabilities as input? If p is ethically neutral and the subject is indifferent between gambles (i) and (ii) we can define $\mathrm{pr}(p) = \mathrm{pr}(\text{not-}p) = \frac{1}{2}$; and similarly for the appropriate generalization to n-fold partitions. If we pursue this line we, like Ramsey, will have followed the dutch book theorems to deeper and deeper levels until it leads to the representation theorem.

This is not to say that the qualitative assumptions of the representation theorems for probability and utility are above question; but rather to emphasize that criticism of the dutch book arguments, if it is to be more than superficial, must question utility theory and the representation theorems as well.

I have spent this much time chewing over the classical dutch book theorems in hopes of generating some sympathy for Ramsey's view that they touch something fundamental. It is time now to survey some newer results regarding dynamic coherence.

III. New horizons

As Kyburg (1978) and Hacking (1967) point out, a theory of personal probability which addresses only coherence at a time, and

neglects probability change is seriously incomplete. Kyburg puts it nicely:

> It might be maintained, and would be by anyone who regarded the theory of subjective probability as providing insights into scientific inference, that its main function is dynamic: it is the changes in the probability function that are wrought by empirical evidence, through the mediation of Bayes' theorem (or a generalization thereof) that give the theory its philosophical importance. [Kyburg (1978) 176]

The dynamic rule of probability change that Kyburg has in mind is *Bayes' rule* or the principle of *conditionalization.* That is if one gets as new evidence just the proposition, e, where e is a proposition (measurable set) in ones probability space with positive prior probability, one should update by taking as a new probability for any proposition, q, its old probability conditional on p:

$$\mathrm{Pr}^{\mathrm{new}}(q) = \mathrm{Pr}^{\mathrm{old}}(e \,\&\, q) / \mathrm{Pr}^{\mathrm{old}}(p)$$

Both Ramsey and deFinetti endorse the principle of conditionalization:

> . . . obviously if p is the fact observed, my degree of belief in q after the observation should be equal to my degree of belief in q given p before, or by the multiplication law to the quotient of my degree of belief in $p\&q$ by my degree of belief in p. [Ramsey (1931) 192.]
>
> The aquisition of a further piece of information, H—in other words *experience*, since experience is nothing more than the aquisition of further information—acts always and only in the way we have just described . . . As a result of this the probabilities are $P(E/H)$ instead of $P(E)$. [deFinetti (1974) 141] [1]

But nowhere in Ramsey and deFinetti do we find an explicit coherence argument for conditionalization. Kyburg sees this as a deficiency in the theory of personal probability:

> But the really serious problem is that there is nothing in the theory that says a person should *change* his degrees of belief in response to evidence in accordance with Bayes' theorem. On the contrary, the whole thrust of the subjectivistic theory is to claim that the history of an individual's beliefs is irrelevant to their rationality: all that counts at a given time is that they conform to the requirements of coherence. [Kyburg (1978) 176-177]

Hacking (1967) calls it the "dynamic assumption of personalism" and argues that no coherence argument for conditionalization is possible.

deFinetti (1937) does show that there is a coherence argument for the ratio definition of conditional probability:

$$\Pr(q/p) = \Pr(q \,\&\, p) / \Pr(p)$$

based on conditional bets, i.e. bets called off if the condition is not fulfilled. Let the conditional probability of q given p be defined as the fair betting quotient for a conditional bet on q on the condition p. A conditional bet on q conditional on p can be represented as the aggregate of unconditional bets on $p \,\&\, q$ and against p. For the two ways of evaluating the conditional bet to agree, the ratio measure of conditional probability is required. But as Hacking (1967)[2] points out, this is a *static* coherence argument regarding coherence of conditional and unconditional probabilities at the same time; and falls short, by itself, of justifying the rule of conditionalization.

It does not fall far short, however. If you have an epistemic *rule* for updating degrees of belief, and that rule disagrees with the rule of conditionalization, then a dynamic dutch book can be made against you. Omitting details, the heart of the matter is that if a cunning bettor knows your rule for updating degrees of belief, there are two ways in which he can make a bet on q conditional on p with you. He can make a conditional bet now at your going conditional betting ratio, or he can reserve an amount to bet on q if the condition p is satisfied. For the two conditional betting ratios to coincide, your rule must be to update by conditionalization. This argument, due to David Lewis, is reported in Teller (1973) (1976). An elementary exposition is available in the second and third editions of my *Choice and Chance*. I want to emphasize that here again possibility of a dutch book is a symptom of a deeper pragmatic inconsistency: the comittment to two different betting ratios for the same conditional bet.

Lewis' argument for conditionalization fills the gap that Kyburg and Hacking found in the theory of personal probability. Is the theory then complete? One can argue that it is only if the conditions suitable for conditionalization exist whenever we learn from experience, i.e. the results of an observation are always summed up as rendering certain some proposition in the observers probability space. Thus put, the view seems at best extremely dubious. It appears that Ramsey did not hold it. His discussion of conditionalization begins:

> Since observation changes (in degree at least) my opinion about the fact observed, some of my degrees of belief after the observation are necessarily inconsistent with those I had before. We have therefore to explain how exactly the observation should modify my degrees of belief . . .

There follows the endorsement of the rule of conditionalization already quoted. The tantalizing parenthetical phrase, suggestive of a theory of uncertain observation, is not followed up.[3]

Such a theory, where observation raises the probability of a proposition, but does not raise it all the way to one, is developed in Jeffrey (1965) (1968). Jeffrey proposes a generalization of conditionalization on p, where the probabilities conditional on p are kept constant but the probability of p need not go to one. Such a change, he calls a change by probability kinematics on p. More generally, if $\{p_i\}$ is a finite partition, all of whose members have positive prior probability, a change is by probability kinematics on the partition if:

$$\Pr_{new}\,(q \text{ given } P_i) = \Pr_{old}\,(q \text{ given } P_i)$$

Jeffrey sees probability kinematics as a more general method of updating suitable for uncertain observations whose informational content is captured by the probability shifts of the members of the partition. He uses the example of the observation of the color of a cloth by candlelight with probability change by probability kinematics on a partition of colors. For a discussion of probability kinematics and its relation to Bayesian statistics, see Diaconis and Zabell (1982).

Is there a coherence argument for probability kinematics? One approach is to try to reduce probability kinematics to conditionalization on a larger space. Thus, if we introduce higher order probabilities, we might suppose that the uncertain observer did learn something for certain—not p but rather that the probability of p is .99. If we put restrictions on the higher order probability measure that guarantee that $\mathrm{pr}(p) = .99$ only carries first order information about p, then we get belief change by probability kinematics on p. Likewise for the more general form of probability kinematics. [Armendt (1980), Skyrms (1980 a, b), Good (1981)]. For the special case where the partition is $\{p, \text{not } p\}$ the condition:

SUFFICIENCY: $\mathrm{PR}[q \text{ given } p \;\&\; \mathrm{pr}(p) = a] = \mathrm{PR}[q \text{ given } p]$

(for first order p,q) guarantees that conditionalization on $\mathrm{pr}(p) = a$ effects a change on the probability over the first order statements by probability kinematics on the partition $\{p, \text{not } p\}$. The additional condition:

MILLER: $\mathrm{PR}[p \text{ given } \mathrm{pr}(p) = a] = a$

guarantees that conditioning on $\mathrm{pr}(p) = a$ gets you to a posterior distribution in which the probability of p does equal a. Roughly speaking, MILLER together with the theorem on total probability gives you:

EXPECTATION: $\mathrm{PR}[p] = \mathrm{EXPECTATION}\ [\mathrm{pr}(p)]$

For more detail see the references. The only difficulty with this approach is that it solves a slightly different problem than the one posed by Jeffrey. He does not suppose that we are presented with "The Given in probabilistic terms" but rather that there is no proposition which we learn for certain [Jeffrey (1983) 183].

It is, however, also possible to solve the problem in Jeffrey's own terms and give a dutch book argument for probability kinematics using only first order probabilities [Skyrms (forthcoming)]. The argument is again to be made precise as an argument addressed to *rules* or *strategies* for changing degrees of belief, in a certain sort of epistemic situation. The trick is to consider not only the uncertain observation, but also to introduce a subsequent observation which removes the uncertainty.

For definiteness, suppose that the question at issue concerns the color and flavor of a jellybean. The bookie has an initial opinion and posts odds; observes the bean under dim light, and posts odds again; then the lights are turned up (of more definitely, the true color announced) and the bookie posts odds again. The bookie has a *strategy* for dealing with this situation. The strategy can no longer be thought of as a *function* since there is no "given" at the time of uncertain observation, but must rather be thought of as a relation, i.e. as a set of quadruples $<\mathrm{Pr}_1, \mathrm{Pr}_2, \mathrm{COLOR}, \mathrm{Pr}_3>$ which delimit his possible moves. There must be, however a way of guaranteeing that under dim light he only observed the color of the bean rather than tasteing it, or more precisely that his *strategy* treats the observation qualitatively as one confined to color. We therefore require that his probability at time 3 after the true color is announced not depend on the results of the imperfect information reflected in Pr_2 but rather be determined by his initial probability together with the color announced. This can be thought of as the requirement that the partition of colors be *qualitatively sufficient with respect to his strategy.* Then we can show that for a suitable notion of coherence, coherence requires a strategy which proceeds from the initial probability to the one modified by uncertain observation by probability kinematics on the partition of colors.

If the bookie has higher order initial probabilities about the possible pr_2s on the color-flavor space that may eventuate from uncertain observation, then we can say more. If the bookie's strategy does not proceed almost everywhere in his pr_1 (i.e. if at the initial time he gives any positive probability at all to the proposition that his moves will violate probability kinematics) the he is subject to an unconditional dutch book, [Skyrms (forthcoming)].

Higher order probabilities also bring in an additional coherence requirement. Goldstein (1983) and van Fraassen (1984) show that coherence requires the aforementioned *Expectation Principle*, that pr_1 be equal to the prior expectation of pr_2. That is, the bookies initial probability over the color-flavor space must be the "center of mass" of the possible probabilities at time two, with the higher order initial probabilities being taken as the measure of mass, or else he lays himself open to an unconditional dutch book.

One can also pursue converse dynamic dutch book theorems in the domains under consideration. Let a call a bookie's strategy in the uncertain observation game with the jelly bean *catholic* if it allows for pr_2 which distribute probabilities over the colors in every posible way consistent with giving each color non-zero probability. Then in that game, a *catholic* strategy of belief change by *probability kinematics* is coherent. [Skyrms (1987)]

IV. Invitation

The first half of this paper was concerned with arguing that dutch book arguments, properly interpreted, touch deep features in the logic of practical reason. The second half—an incomplete survey of recent work in dynamic and higher order coherence—is intended to show that the field is not closed. There is no reason to think that the sort of model situation used to analyze probability kinematics for uncertain observations is the only model of epistemological interest. For example, there is the statistical model investigated by Cornfield (1969), Freedman and Purves (1969), Heath and Sudderth (1972) (1978), Lane and Sudderth (1983) wherein the chance of an experimental outcome given the state of nature is known but no prior probability over the states of nature is assumed, the experiment is performed and the bookie posts odds according to some rule. You can think of other models of interest. The contribution of investigations of coherence to epistemology is an unfinished story.[4]

NOTES

1. And deFinetti (1937): " . . . observation can only give us information capable of influencing our opinion. The meaning of this statement is very precise: it means that to the probability of the fact conditioned on this information—a probability very distinct from that of the same fact not conditioned on anything else—we can indeed attribute a different value." Compare also deFinetti (1976) section VII.

2. I take it that Levi (1980), pp. 81-82 is making the same point in his discussion of "confirmational conditionalization" vs. "temporal credal conditionalization." So is Seidenfeld (1985) 280 ftnt 7. Both Levi and Seidenfeld, like Hacking, are skeptical about the possibility of a dutch book argument for conditionalization.

3. Did Ramsey think about such matters at all? As Diaconis and Zabell (1982) point out, there is a partial anticipation of the "probability kinematics" of Jeffrey (1965) (1968) in Donkin (1851). It is therefore of some interest that Ramsey took notes on Donkin's paper, underlining some of the passages relevant to our concerns here. Richard Nollan, curator of special collections at the Hillman library of the University of Pittsburgh, was kind enough to allow me to examine the Ramsey papers in the Archives for Scientific Philosophy in the Twentieth Century of which the notes in question are item 003-13-01.

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REFERENCES

E. Adams, "On Rational Betting Systems," *Archive fur Mathematische Logik und Grundlagenforshung*, vol. 6 (1962), pp. 7-29, 112-128.

E. Adams, and R. Rosenkrantz, "Applying the Jeffrey Decision Model to Rational Betting and Information Acquisition," *Theory and Decision*, vol. 12 (1980) pp. 1-20.

B. Armendt, "Is there a Dutch Book Argument for Probability Kinematics?" *Philosophy of Science*, vol. 47 (1980), pp. 583-588.

F. J. Anscombe and R. J. Aumann, "A Definition of Subjective Probability," *Annals of Mathematical Statistics*, vol. 34 (1963), pp. 199-205.

P. Baillie, "Confirmation and the Dutch Book Argument," *British Journal for Philosophy of Science*, vol. 24 (1973), pp. 393-397.

R. J. Buehler, "Coherent Preferences," *The Annals of Statistics*, vol. 4 (1976), pp. 1051-1064.

C. Chihara and R. Kennedy, "The Dutch Book Argument: Its Logical Flaws, Its Subjective Sources," *Philosophical Studies*, vol. 36 (1979), pp. 19-33.

J. Cornfield, "The Bayesian Outlook and Its Application," *Biometrics* vol. 25 (1969), pp. 617-658.

B. de Finetti, "La Prevision: ses lois logiques, ses sources subjectives," *Annales de l'Institut Henri Poincare*, vol. 7 (1937), pp. 1-68. trans. as "Foresight: its logical laws, its subjective sources" in H. E. Kyburg, Jr. and H. Smokler (ed.), *Studies in Subjective Probability* (Huntington, N.Y.: Krieger, 1980).

B. de Finetti, *Theory of Probability*, vol. I (New York: Wiley, 1974).

B. de Finetti, "Probability: Beware of Falsifications," *Scientia*, vol. 111 (1976), pp. 283-303, reprinted in Kyburg and Smokler (1980).

P. Diaconis and S. Zabell, "Updating Subjective Probability," *Journal of the American Statistical Association*, vol. 77 (1982), pp. 822-830.

W. F. Donkin, "On Certain Questions Relating to the Theory of Probabilities," *The Philosophical Magazine*, Ser. 4, 1 (1851), pp. 353-361.

Z. Domotor, "Probability Kinematics and the Representation of Belief Change," *Philosophy of Science*, vol. 47 (1980), pp. 384-404.

Ellery Eells, *Rational Decision and Causality* (Cambridge: Cambridge University Press, 1982).

D. A. Freedman and R. A. Purves, "Bayes Method for Bookies," *The Annals of Mathematical Statistics*, vol. 40 (1969), pp. 1177-1186.

M. Goldstein, "Revising Previsions: A Geometric Interpretation," *Journal of the Royal Statistical Society B*, vol. 43 (1981), pp. 105-130.

M. Goldstein, "The Prevision of a Prevision," *Journal of the American Statistical Association*, vol. 78 (1983), pp. 817-819.

I. J. Good, "The Weight of Evidence provided by Uncertain Testimony or from an Uncertain Event," *Journal of Statistical Computation and Simulation*, vol. 13 (1981), pp. 56-60.

C. Glymour, *Theory and Evidence* (Princeton: Princeton University Press, (1980), pp. 71-72.

I. Hacking, "Slightly More Realistic Personal Probability," *Philosophy of Science*, vol. 34 (1967), pp. 311-325.

D. Heath and W. Sudderth, "On a Theorem of de Finetti, Oddsmaking and Game Theory," *Annals of Mathematical Statistics*, vol. 43 (1972), pp. 2072-2077.

D. Heath and W. Sudderth, "On Finitely Additive Priors, Coherence, and Extended Admissibility," *The Annals of Statistics*, vol. 6 (1978), pp. 333-345.

F. Jackson and R. Pargetter, "A Modified Dutch Book Argument" *Philosophical Studies*, vol. 29 (1976), pp. 403-407.

R. Jeffrey *The Logic of Decision* (N. Y.: McGraw Hill, 1965); 2nd ed. (Chicago: University of Chicago Press; 1983).

R. Jeffrey, "Probable Knowledge," in I. Lakatos (ed.), *The Problem of Inductive Logic* (Amsterdam, North Holland, 1968), reprinted in Kyburg and Smokler (1980).

M. Kaplan, "Decision Theory as Philosophy," *Philosophy of Science* vol. 50 (1983), pp. 549-577.

J. Kemeny, "Fair Bets and Inductive Probabilities," *Journal of Symbolic Logic*, vol. 20 (1955), pp. 251-262.

H. Kyburg, "Subjective Probability: Criticisms, Reflections and Problems," *The Journal of Philosophical Logic*, vol. 7 (1978), pp. 157-180.

H. Kyburg and H. Smokler (eds.), *Studies in Subjective Probability* (Huntington, New York: Krieger, 1980).

D. Lane and W. Sudderth, "Coherent and Continuous Inference," *The Annals of Statistics*, vol. 11 (1983), pp. 114-120.

R. Lehman, "On Confirmation and Rational Betting," *Journal of Symbolic Logic*, vol. 20 (1955), pp. 251-262.

Isaac Levi, *The Enterprise of Knowledge* (Cambridge, Mass.: MIT, 1980).

H. Putnam, "Probability Theory and Confirmation," in *Mathematics, Matter and Method* (Cambridge: Cambridge University Press, 1975).

H. Raiffa, *Decision Analysis* (Reading, Massachusetts: Addison-Wesley, 1968).

F. P. Ramsey, "Truth and Probability," in R.B. Braithwaite (ed.), *The Foundations of Mathematics and Other Essays* (N.Y.: Harcourt Brace, (1931) and in Kyburg and Smokler (1980).

F. Schick, "Dutch Books and Money Pumps," *The Journal of Philosophy*, forthcoming.

A. Shimony, "Coherence and the Axioms of Confirmation," *Journal of Symbolic Logic*, vol. 20 (1955), pp. 1-28.

A. Shimony, "Amplifying Personal Probability," *Philosophy of Science*, vol. 34 (1967), pp. 326-333.

T. Seidenfeld, "Calibration, Coherence and Scoring Rules," *Philosophy of Science*, vol. 52 (1985), pp. 274-294.

T. Seidenfeld and M.J. Schervish, "A Conflict between Finite Additivity and Avoiding Dutch Book," *Philosophy of Science*, vol. 50 (1983), pp. 398-412.

B. Skyrms, *Causal Necessity* (New Haven: Yale University Press, 1980a) Appendix 2.

B. Skyrms, "Higher Order Degrees of Belief,"in D.H. Mellor (ed.), *Prospects for pragmatism* (Cambridge: Cambridge University Press, 1980b).

B. Skyrms, "Zeno's Paradox of Measure" in R.S. Cohen and L.

Lauden (ed.), *Physics, Philosophy, and Psychoanalysis (Dordrecht: Reidel, 1983), pp. 223-54.*

B. Skyrms, Pragmatics and Empiricism (New Haven: Yale University Press, 1984).

B. Skyrms, *Choice and Chance: An Introduction to Inductive Logic* (Belmont, California: Wadsworth, 1986).

B. Skyrms, "Dynamic Coherence and Probability Kinematics," *Philosophy of Science*, forthcoming, 1987.

S. Spielman, "Physical Probability and Bayesian Statistics," *Synthese*, vol. 36 (1977), pp. 235-269.

P. Teller, "Conditionalization and Observation," *Synthese*, vol. 26 (1973), pp. 218-258.

P. Teller, "Conditionalization, Observation, and Change of Preference," in W. Harper and C. Hooker (eds.) *Foundations of Probability Theory, Statistical Inference, and Statistical Theories of Science* (Dordrecht-Holland: D. Reidel, 1976), pp. 205-253.

B. van Fraassen, "Belief and the Will," *Journal of Philosophy*, vol. 81 (1984), pp. 235-256.

B. van Fraassen, "Rational Belief and Probability Kinematics," *Philosophy of Science*, vol. 47 (1980), pp. 165-187.

J. Vickers, "Some Remarks on Coherence and Subjective Probability," *Philosophy of Science*, vol. 32 (1965), pp. 32-38.

J. von Neumann and O. Morgenstern, *Theory of Games and Economic Behavior* (Princeton, New Jersey: Princeton University Press, 1944).

P.M. Williams, "Bayesian Conditionalization and the Principle of Minimum Information," *British Journal for the Philosophy of Science*, vol. 31 (1980), pp. 131-144.

PEIRCE MEETS KUHN: THE VERIFICATIONIST THEORY OF TRUTH WITHOUT A FIXED METHODOLOGY OF VERIFICATION

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VERIFICATIONISM, that hardy perennial that seems to outlive each of the philosophical doctrines thought to provide its broad theoretical justification, is enjoying yet another renaissance, thanks in large part to the work of Dummet and Putnam.[1] Dummett seems chiefly concerned to defend an account of meaning as determined by verification-conditions, with a verificationist theory of truth a significant corollary. For Putnam, on the other hand, verificationism about truth plays the central role. In what follows, I shall be concerned exclusively with the consequences of adopting a verificiationist account of truth.

I. THE VERIFICATIONIST ACCOUNT OF TRUTH

On the sort of account I have in mind, truth, as Putnam puts it, is "an idealization of rational acceptability."[2] Roughly, a statement is true if and only if it would ultimately be accepted by an ideal community of rational investigators. The classic statement of a view of this kind is Peirce's:

> The opinion which is fated to be ultimately agreed to by all who investigate, is what we mean by the truth, and the object represented in this opinion is the real . . .