# SLIGHTLY MORE REALISTIC PERSONAL PROBABILITY

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A person required to risk money on a remote digit of π would, in order to comply fully with the theory [of personal probability] have to compute that digit, though this would really be wasteful if the cost of computation were more than the prize involved. For the postulates of the theory imply that you should behave in accordance with the logical implications of all that you know. Is it possible to improve the theory in this respect, making allowance within it for the cost of thinking, or would that entail paradox?*

Like each of Professor Savage's difficulties in the theory of personal probability, his problem about the remote digit of π is entirely general. It concerns logical consequence as much as logical truth: his theory implies that if *e* entails *h* you should be as confident of *h* as of *e*. His own example is one of three distinct cases which militate against this part of his theory. In his example there is a known algorithm for working out of the relevant logical implications, but it is too costly for sensible use. A second case arises when there is no known algorithm for finding out whether the hypothesis *h* follows from the evidence *e*. Perhaps there are two subcases: in the first, the algorithm is not known to anyone; in the second, it is not accessible to the person who is making decisions. In either case the person who is as confident of *h* as of *e* is, though lucky, not reasonable, but prejudiced; a man who is less confident may be the sensible man who tailors his beliefs to the available evidence.

Intuitionist mathematicians offer ready examples for the first form of the second case. Does 777 occur in the decimal expansion of π? According to classical logic, any analytical definition either entails that 777 occurs, or entails that it does not, but we know no procedure sure to settle which it is. Complete confidence in either outcome is absurd. Yet complete confidence is demanded by personalism. If it is hard to imagine real life betting on such a question, recall the 15th century algorithm competitions. When Tartaglia knew the algorithm for solving cubic equations and Cardano did not, Cardano had to "risk money," or at least his reputation, on problems that could be solved only by an algorithm he did not know ([6], ch. 5).

A third case arises from undecideability. Suppose a man is to have a set of betting rates over a whole class of problems for which there exists no algorithm. It must be an infinite class because algorithms exists for all finite classes of problems. Such a man is prevented from systematically satisfying the demands of personal probability. For a concrete example, let our man have to bet about assertions of the form "*F* is a theorem of the predicate calculus," where *F* ranges over all well formed formulae of the calculus.

These three cases make distinct version of the difficulty suggested by Savage.

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* L. J. Savage, "Difficulties in the Theory of Personal Probability," in this issue of *Philosophy of Science*. Unless otherwise specified all references to Savage's work are to this article.

The third one, though it will appeal to logicians, might be discounted by a practical personalist on the grounds that we never do have to risk money over the whole range of an infinite undecideable class. Hence I shall attend mainly to the first two cases, although the third will also be kept in mind. The first and second cases do arise in serious practical matters. Many questions in probability theory are answered by Monte Carlo methods that yield only probable solutions with a range of uncertainty. Yet a computer technologist will often decide to use Monte Carlo methods: both when expensive exact solutions are theoretically available, and also when no algorithm for the exact solution is known. In either case he is rationally deciding to act against the axioms of personal probability. A slightly more realistic theory must show that his decision is reasonable.

Savage fears that any theory which is, in this respect, more realistic, will "entail paradox." This is especially plausible in the first two cases, for although we expect the precise analysis of recursive functions to help with the third, no analysis is already tailored for the other two. The difficulty seems to arise from some feature of what Savage calls "logical implication." Philosophers know, to their cost, the difficulty of getting any intuitively adequate analysis of relations among logical truths. The best known analysis of logical implication, namely C. I. Lewis' theory of strict implication, says that a self-contradictory proposition entails everything ([14], p. 250). Many philosophers balk at that result, but none has circulated an alternative which is, at present, widely accepted. It is plausible to guess that attempts to patch up personalism will sink into the same quagmires that have, in my opinion, swallowed up students of entailment.

## 1. A priori and a posteriori reasoning.

Plausible though such defeatism is, I shall argue against it. The argument goes near many philosophical quagmires, but we can skirt most of them in the way which, as Savage reminds us, so many other philosophical difficulties are evaded by personalism. A main strand in the argument can be sent out at once. Personalism is, says Savage, a theory for policing one's own potential decisions and systems of belief. Hence we distinguish between the theory and what it is about. In logician's parlance personalism is a metatheory. It is about, in part, various beliefs that are represented by propositions. Some aspects of Savage's problem may stem from over-willing acceptance of philosophical dogmas about propositions and our knowledge of them.

In particular I do not believe that the theory should acknowledge any distinction between facts found out by *a priori* reasoning and those discovered *a posteriori*. I am not referring to the current controversy as to whether there is a sharp distinction between analytic and synthetic truths. I insist only that actions based ultimately upon knowledge need not distinguish ways in which the knowledge is acquired.

Consider the problem of finding the surface of least area bounded by a closed curve in space. It is hard to establish even that there is a least area. Yet in the early 19th century the Belgian physicist Plateau could often answer by determining the film a soap bubble forms on a closed loop of wire; he knew enough about soap bubbles to be sure the film was of least area. The complete mathematical solutions had to wait for over a century ([4], p. 386). Yet the empirically obtained results should provide as much confidence for practical decisions as the later mathematical proofs—maybe more, considering several debacles that from time to time occurred in the calculus of variations! What matters to the decision maker is what he knows

or can find out; philosophical distinctions among the means of discovery are of no moment.

Take a pair of examples directly related to Savage's problem about the remote digit of π. Imagine a man taught binary notation, but not even told that it is a system of numbering. He is taught only the natural ordering of the binary numerals. He is also taught how to add and multiply in this notation, although he is not told what the operation means. He is asked to speculate on the relative magnitude of products of pairs of five-digit binary numbers. It does not matter to him much; say he risks no money at all, but can make a little every time he is right. His beliefs can be represented by betting odds in the way that Savage has taught us. Suppose that considering any pair of products of two five-digit binary numbers, his betting rate is 0.2 on the two products being equal, and 0.4 on each of the other two alternatives.

This man, whom we shall recall from time to time in what follows, is to be compared with another: someone who is first introduced to the mysteries of underground city transport, say that of the city of London. He is asked questions like, "Are there more stops travelling between Gloucester Rd. and King's Cross on the Picadilly or on the Circle line?" His odds parallel those of the first man, the binary computer. The two have much in common. Their betting rates hardly fit the facts as we know them. In each case, an elementary algorithm answers each question which can be put to them; each declines it as too expensive considering the trifling gains. In each case some "insight" short of working out complete answers would lead to more profitable betting odds.

Despite the parallel, personalism treats one man as sensible and the other as incoherent. We need a theory that puts both on a par. It should also explain why each man should find out more before wagering, if investigation is cheap enough. In trying to lessen the formal distinction between the two men, a remark of Savage's may suggests a fallacy we should avoid. He says that "the example about π does not adequately express the utter impracticality of knowing our own minds in the sense implied by the theory." I believe the example about π does not express the impracticality of knowing our own minds at all: it has nothing to do with knowing our own minds; it is a matter of knowing π. And our speculator on binary products may know his own mind full well; what he does not know is binary arithmetic.

## 2. Classical personalism.

**2. Classical personalism.** Personalists attribute probabilities to events, but Savage's problem arises out of logical implication, which is a relation between propositions. So it is natural to work in one of the formalisms that attribute probability to propositions rather than to events.

Classical personalism offers a theory of rational belief and reasonable decision. At any moment in his life a man will know a body of facts $f$. He is interested in some set of propositions. Associated with this set is a Boolean algebra $A$. $Prob_f(h)$ is to be a number representing the person's personal probability for $h$, when he knows $f$; for short, his probability given $f$. In one behavioural analysis, confidence is measured by the least favourable rate at which the person will bet about $h$. This leads to a well known argument for what I shall call the *static assumption* of personalism: For any $h \varepsilon A$, and at least any $f \subset A$, $Prob_f(h)$ is defined and satisfies the probability axioms. As de Finetti proved, the probability axioms give necessary and sufficient conditions that a person's odds not be open to a Dutch book, *i.e.* not open

to a book against him which is guaranteed a net gain [7]. Perhaps other arguments for the static assumption are more profound. Many readers will prefer those of F.P.Ramsey's [17] or Savage's ([19], ch. 3). But de Finetti's argument is so familiar, so simple, and by comparison so brief that it serves as a convenient reference point for the rest of this paper. I believe each point made in connection with the Dutch book argument can be transferred to the other famous arguments for the static assumption.

Probability given facts is not to be confused with conditional probability, which is defined in the usual way:

$$Prob_f(h/e) \ = \ \frac{Prob_f(he)}{Prob_f(e)}$$

for positive denominators. Conditional probabilities indicate how confident a person knowing only $f$ judges that he would be if he knew $e$ as well. The distinction between probability given facts and conditional probabilities is not found in the usual personalist writings. The terminology is copied from an objectivist paper of J.S.Williams ([25], p. 276). Formally the distinction is clear. The probability of $h$ given $f$ is a primitive to be circumscribed by the axioms of Kolmogorov. Conditional probability is defined as above. The latter is extraneous to the system, and introduced solely for convenience; the former is basic.

I say the distinction is fundamental to personalism yet personalists never use it explicitly. They never write "$f$" as a subscript to probabilities, nor express the idea in other ways. Why then introduce it? Because it will be crucial to our treatment of Savage's problem, and also because it makes explicit something fundamental to that part of Savage's theory which leads one to call his work Bayesian. Let me explain this after stating an implicit assumption of personalists which connects conditional probability with probability given facts. I call it the *dynamic assumption:*

$$Prob_{fv[e]} \ (h) \ = \ Prob_f(h/e).$$

The meaning is as follows. Suppose I know only $f$. I judge that if I knew $e$ as well, I would be confident of $h$ to degree $p$; behaviourally this judgement is shown by the conditional bets I would place. Now I find out that $e$ is the case. The dynamic assumption asserts that now my confidence in $h$ is $p$, as behaviourally shown in a readiness to place unconditional bets.

This assumption is not a tautology for personalism. It is a tautology for theories like Harold Jeffreys' [13], where a unique probability is associated with any pair $h,e$. Those theories do not need our distinction between probability given evidence and conditional probability. But personalists do need the distinction, and do need the dynamic assumption.

Since the assumption seems never to be stated explicitly in the classic personalist studies, how dare I say it is needed? Because it is essential to that "model of how opinion is modified in the light of experience" to which Savage refers above. This requires a digression, but it is so important to understanding personalism, and my modification of it, that the point deserves a section of its own.

**3. Conditional and given.** Savage's model of modifying opinion employs Bayes' theorem; that is why we speak of Bayesians today. Savage has stated the theorem "somewhat informally" in the following way (I use an innocent paraphrase of ([20], p. 15).

> $Prob(h/e) \propto Prob(e/h) \; Prob(h)$
>
> In words, the probability of h given the datum e is proportional to the product of the probability of observing e given h multiplied by the initial probability of h.

Well known properties of this theorem lead us to a model of learning from experience. My own catalogue of the properties, guilty of exactly the same confusion as I shall attribute to Savage's presentation, is given in ([9], ch.XIII). The idea of the model of learning is that $Prob(h/e)$ represents one's personal probability after one learns e. But formally the conditional probability represents no such thing. If, as in all of Savage's work, conditional probability is a defined notion, then $Prob(h/e)$ stands merely for the quotient of two probabilities. It in no way represents what I have learned after I take e as a new datum point. It is only when we make the dynamic assumption that we can conclude anything about learning from experience. To state the dynamic assumption we use probability given data, as opposed to conditional probability.

The conflation of two distinct concepts may explain why people favourable to personalism can say both that conditional probability is an "extraneous" defined notion, and also that, as D.V.Lindley puts it in discussing an address of Savage's "All probabilities are conditional" ([20], p. 83).

It may seem as if Lindley's position could let us avoid the distinction I have been urging. I said Jeffreys' interpersonal theory could get along with conditional probabilities taken as primitive. Why cannot the personalist do the same, as Lindley does in his own recent book [15]? Unfortunately we find the equivocation in a new guise. Lindley gives a betting rate justification of his axioms along personalist lines ([15], Vol.I,pp.32-36). It relies on reading $Prob\;(h/ef)$ as the rate, all conditional on f, at which I would bet on h conditional on e. Later in his Bayesian statistics, the same conditional probability symbol represents my confidence or betting rate for h when I know both e and f; when e is a sample, $Prob(h/ef)$ shows how beliefs are "changed by the sample according to Bayes' theorem" ([15], Vol.II,p.2).

The equivocation can be explained but not excused by the fact that a man knowing e would be incoherent if the rates offered on h unconditionally differed from his rates on h conditional on e. But no incoherence obtains when we shift from the point before e is known to the point after it is known. Thus, suppose to begin with both h and e are uncertain. A man offers odds of p,q,r, and $1-p-q-r$ on he, ~he, h~e and ~h~e respectively. His conditional rates fit in with this. Then e is found out to be true. The man revises his rates, betting 1 on e, 0 on ~e, and $p+s/p+q+s$ on h, and $q/p+q+s$ on ~h for some positive s. These new rates show how much the man has "learned" from e. His learning violates the dynamic assumption. It is non-Bayesian. But since the man announces his post-e rates only after e is discovered, and simultaneously cancels his pre-e rates, there is no system for betting with him which is guaranteed success in the sense of a Dutch book. It is of no avail to express all rates as conditional: then the man's $Prob(h/ef)$ before

learning $e$ differs from his $Prob(h/ef)$ after learning $e$. Why not, he says: the change represents how I have learned from $e$!

I am not here quarrelling with the dynamic assumption, although I know of no personalist defence of it. Probability dynamics is too little studied, although Richard Jeffrey's ([13], ch.11) is a good start at clarifying another aspect of the problem which I am here ignoring. Patrick Suppes' ([23], sec.4) is well aware of the matter I have just described, although the axiom he proposes does not seem sufficient to guarantee the dynamic assumption. One non-personalist defence of the dynamic assumption can, I believe, be derived from the continuity and differentiability argument of R.T.Cox ([5], ch.1) to which Shimony alludes in his essay in the present issue of *Philosophy of Science*. But that argument has never been favoured by personalists. And neither the Dutch book argument, nor any other in the personalist arsenal of proofs of the probability axioms, entails the dynamic assumption. Not one entails Bayesianism. So the personalist requires the dynamic assumption in order to be Bayesian. It is true that in consistency a personalist could abandon the Bayesian model of learning from experience. Salt could lose its savour.

**4. The betting rate interpretation.** Our digression into the concept of probability given facts was needed for our overall view of Savage's problem and its solution. For we propose a trivially Bayesian treatment of mathematical learning, in agreement with our view that learning mathematical facts, and learning empirical facts, are both learning facts. The model of how learning facts modifies opinion will be the same in each case, namely Bayesian. We can achieve this only by weakening the axioms for personal probability, but in such a way that no practical application of the classical theory is impeded. For a hint of how to proceed, re-examine the betting rate interpretation, where $Prob_f(h) = p$ if and only if $p$ is the largest number such that for any relatively small S I would exchange $pS$ for the right to collect S if $h$ is true, and nothing if $h$ is false.

Under the usual interpretation of a betting rate, de Finetti's theorem is valid: betting rates must satisfy the probability axioms or else be open to a Dutch book. But the usual interpretation involves a trifling idealization. In real life betting I will not collect on $h$ merely if it is true. It must be seen to be true. The bettors (or their heirs) must find out that $h$ is true, or at worst abide by the decision of a trusted arbiter who claims to know about $h$. This idea has, I think, been implicit in de Finetti's insistence that we can only bet on hypotheses of the sort that can be settled in finite time. But that insistence is not enough, for only a few of the hypotheses that can be settled in theory are ever in fact settled. Even if something can be in principle settled but in fact never is, there will be no pay-offs.

More realistically my personal probability for $h$ must be measured by $p$ when $p$ is the largest number such that I will contract with another party as follows. I agree to pay him $pS$ if we find out that $h$ is false. He agrees to pay me S in exchange for $pS$ if we find out that $h$ is true. No money changes hands until we settle the truth value of $h$. Of course like any other contract the "we" is less than literal: contracts can be inherited, bought, or adjudicated. But we discard the custom of leaving the stake in the hands of a bookmaker until the issue is settled: that custom is due to human dishonesty and has nothing essential to do with betting.

With this reinterpretation in mind, examine two of the probability axioms, say in a form adapted from Shimony's [21].

(1) *If some elements of f logically imply h, then $Prob_f(h) = 1$.*

(2) *If some elements of f logically imply that h and i are incompatible, then $Prob_f(h \lor i) = Prob_f(h) + Prob_f(i)$.*

The only other axiom for probabilities in finite algebras says that probabilities lie between 0 and 1. The axioms are sensible for the usual betting rate interpretation, for if my rates fail to satisfy either (1) or (2), then, in the usual interpretation, a Dutch book can be made against me. This does not hold for the more realistic interpretation. In the extreme case suppose there is no available way to find out if elements of $f$ logically imply $h$; $f$ could even be the null class, and $h$ a proposition of logic. Then, on the basis of knowledge of $f$ there is no absurdity in having a betting rate on $h$ less than 1, nor is there any known way to make a book against me with guaranteed profit. Though sufficient, the probability axioms are not necessary for avoiding a real life Dutch book.

John Vickers noticed this and in [24] suggested weakening (2). He proposed additivity only if there is a proof that the incompatibility of $h$ and $i$ follows from $f$. He rightly said that even this is too strong for strictly personal probability. To extend Vickers' line of thought we need to analyse more closely the possible states of affairs contemplated by a decision maker.

## 5. Possibilities.

Axioms (1) and (2) both use the concept of logical implication. As Shimony's [21] takes for granted in presenting probability, strict implication is the appropriate formal analysis of logical implication in this context. C.I.Lewis explained strict implication in terms of possibility: $e \to h$ if it is not logically possible for $e$ to be true while $h$ is false ([14], p.124). This implicit falling back on possibility should make us prick up our ears. Aristotle had a scale of modes: impossible, possible, probable, necessary. It is a tradition, which I do not admire, always to consider this as a scale of logical possibility, logical probability, *etc.* Savage snapped tradition by going to an opposite extreme: personal probability. Perhaps he gets into trouble because he is not completely radical. Just as logical probability is related to logical possibility, so personal probability demands a concept of personal possibility.

There is nothing sacred about logical possibility. We know how Quine has mocked it ([16], ch.1,2). A recent attempt to define what we commonly mean by possibility argues that though the concept is "objective" it falls short of logical possibility and is an epistemic concept [10]. That work was a by-product of trying to define "objective" probability short of logical probability. Likewise some concept of personal possibility should be a by-product of personal probability.

## 6. Personal possibility.

The personalist wants to choose among acts, given a partition into possible states of the world. As Savage says, a possible state of the world is a "possible list of all answers to questions that might be pertinent to the decision situation at hand." But the partition need not consist of distinct logical possibilities. It should consist of states of affairs each of which is "possible to the agent." Of course in English we don't say "possible to him" (and "possible for him" is something different; what [10] calls an M-occurrence of the word.) But personal

probability requires the odd "probable to him" or "probable for him" and personal possibility will need new locutions too.

For me, when is a proposition possible? When I do not know it to be false. Hence $p$ may be possible for me although, to use the rubric of Jaako Hintikka's ([11], p.3), it is not possible for all that I know that $p$ (i.e., $p$ may be possible for me when it is incompatible with facts I do know, so long as I do not know the incompatibility.)

What are the objects of personal probability? If, as in Carnap's ([1], p.27), logically equivalent propositions are identical, then propositions cannot be the objects. For $h$ and $i$ may be logically equivalent, and I may know $h$, yet, because I am ignorant of the equivalence, I may not know $i$; hence $\sim h$ would not be personally possible while $\sim i$ is. This is absurd if personal possibility applies to propositions. No tighter criterion of propositional identity has ever succeeded. Hence we must cast about for other objects for personal probablity. Sentences are the obvious choice. When $p$ is an unambiguous sentence that a person understands, I shall speak of $p$ being possible for him, and of his knowing $p$. This is not our normal way of speaking, but in the present context the meaning will be quite clear. We pretend that, as in a formal language, all sentences are unambiguous.

To attach knowledge to sentences is a blow against sound epistemology but is fine for personal probability, the theory of a person's choice. One can deliberate among only those possibilities expressed in sentences he can understand. Hence we abandon the idea of choosing within a Boolean algebra of propositions, and think of choosing among sentences in a language or "personal language" closed under what, in that language, correspond to the forming of conjunctions, negations, conditionals and alternations.

For an artifical example, recall the person comparing products of five-digit binary numbers. He need never employ any number over 961. Hence he need use only the following language. The terms are the first 961 binary numbers and the recursive result of writing "+" or "×" between two bracketted terms. The atomic sentences result from writing "=" or ">" between two terms. The closure of this under the Boolean sentential operations would be what I have called a language within which the person forms his beliefs about the problem at hand. It is not Boolean since the equivalence classes of sentential logic are not admitted.

It is not realistic to permit unending iteration of sentential operations, for there is an upper bound to the length of the sentences one can understand. A more realistic "personal language" would be the intersection of the closure under sentential operations, with the class of sentences a person understands. This can be characterized artificially, e.g. by limiting the sentences to 10,000 or fewer symbols. But I know of no difficulty in personal probability caused by ceaseless iteration, and I know no formal characterization of intelligibility which is not hopelessly artificial. Hence I shall not strive for realism in this matter.

We may notice, without elaboration, that tying personal probability to a personal language of sentences, or of intelligible sentences, makes one defect of personalism more transparent. Much scientific learning consists in devising new hypotheses or forming new concepts. The personalist difficulty over the unexpected hypothesis is explained in ([9], p.221); Patrick Suppes examines concept formation and personalism in [22]. Since new hypotheses and new concepts typically lead to newly intelligible sentences, they lead to a new personal language. So we should restrict

Bayesian learning to that learning which occurs when the personal language is unchanged; when experience or thought prompts a change in one's language, quite another analysis is called for.

**7. Knowledge.** It is fine to relate personal probability to sentences, but it is not inviting to explain "p is personally possible for me" as "I do not know that $p$ is false." For philosophers have never agreed on what knowledge is. They have agreed, at least since the *Gorgias,* that only what is true can be known. No other necessary condition is universally accepted. There is a long tradition of analysing knowledge as justified belief: for a man to know $p$, it is said, he must have good reasons for believing that $p$, must see these reasons to be good reasons, and must believe or even be certain that $p$. But this tradition is in a bad way, and like many other people, I suspect it is on the wrong track entirely.

The problem of what is knowledge is already a problem for personal probability, as noted by Savage above. Hence we will have achieved our aim of reducing Savage's list of difficulties by one, even if our treatment of the problem about π takes for granted the meaning of "knowledge." But one question we cannot evade. What are the closure conditions of knowledge? Despite the enduring argument of the *Meno,* knowledge is not closed under logical consequence. It is a tribute to Socrates' rhetoric that even today a good many philosophers agree with him, but at most they can be proposing a new, "divine," sense of knowledge. Using the verb "to know" in anything like its customary sense, it is at best a bad joke to say that once a student learns Peano's axioms he knows all their consequences.

Yet knowledge must surely have some closure conditions? If a man knows both $p$ and $p \supset q$, does he not thereby know $q$ as well? For him not to know $q$ would be for him to betray misunderstanding of the conditional, and hence to show that he does not know $p \supset q$ after all. So much is a natural conclusion to draw from Lewis Carroll's riddle about Achilles and the tortoise [3] when taken together with work like Gilbert Ryle's [18]. Yet closure under *modus ponens* leads disasterously near to the divine sense of knowledge. I think the solution to this dilemma is to say that indeed a man must know how to use *modus ponens* (the cash value being that when presented with $p$ and $p \supset q$ he can unswervingly infer $q$). If not, he does not understand the conditional. It in no way follows that knowledge is closed under *modus ponens.* Thinking otherwise must stem from confusing knowing how to get something (when certain conditions are met) and knowing that one gets it (when the conditions are met.)

Hence in what follows I adopt the very harsh view that a man can know how to use *modus ponens,* can know that the rule is valid, can know $p$, and can know $p \supset q$, and yet not know $q$, simply because he has not thought of putting them together. We should call this an examiner's view of knowledge.

**8. Slightly more realistic personal probability.** To sum up: A *personal language* based on a set of sentences which a person understands is the closure of the set under the sentential operations which, in his language, correspond to the formation of conjunctions, negations, conditionals and alterations. An element of a personal language is personally possible to the person if he does not know it to be false, in an examiner's sense of knowledge. Paralleling the Lewis definition of strict implication, we could say that an element $e$ of the personal language *personally implies* an ele-

ment $h$ if $e$ $(\sim h)$ or $(\sim h)e$ or $e$ or $\sim h$ is not personally possible to the person. Then slightly more realistic personal probability satisfies the dynamic assumption and also the static assumption restricted to the case in which personal implication replaces logical implication in the first two axioms.

Since personal implication is a degenerate concept with no closure conditions, it is more natural to express the axioms in terms of the fundamental concept of possibility. Then axioms (1) and (2) take the form:

(1) If given facts $f$, $\sim h$ is not possible, $Prob_f(h) = 1$.

(2) If given facts $f$, $hi$ or $ih$ is not possible, then $Prob_f(h \vee i) = Prob_f(h) + Prob_f(i)$.

In the theory of slightly more realistic personal probability, "possible" is construed as personally possible; in the classical theory it is construed as logically possible; other points are noted in a list below. First let us see how our theory works for the trifling example of a man comparing products of pairs of five-digit binary numbers.

We have settled on a personal language sufficient for his problem. What does he know? Nothing but the initial rules of calculation. For convenience of the example, we describe this knowledge as a set of facts about binary arithmetic, plus knowledge of how to infer by *modus ponens*. Let us represent the facts he knows as follows. I. All substitution instances of axioms for "=" which are sentences of the personal language. II. The ordering of the binary digits within the personal language, *viz.*, up to 961, he knows every true instance of $m > n$. III. The relation between "=" and ">", *viz.*, every true instance of $t > u \vee t = u \vee u > t$ for all terms of the personal language. IV. Simple addition, *viz.*, every true instance of $m+n=k$ up to 961. V. Recursive multipication, *viz.* every true instance of $m \times n = k + (m \times j)$ up to 961, where, if $n$ has $r+1$ digits, $k$ is the result of writing $r$ zeros to the right of $m$. VI. All substitution instances in the language of some set of axioms for the propositional calculus.

Evidently in this model we have amply idealized this man's knowledge, but even so, not up to the point of classical personalism. Specifically, we were concerned with this stupid man's betting rates on the $16^4$ elements of the form $m \times n > j \times k$, and on the $16^4$ elements of the form $m \times n = j \times k$, where $m,n,j$, and $k$ are five-digit binary numbers. A man can be consistent with the axioms of slightly more realistic personal probability if he assigns a betting rate of 0.4 on each inequality, and 0.2 on each equality, except, (to give him minimum good sense) if $m$ and $n$ are the same as $j$ and $k$, when he plumps for equality, and if $m > j$ while $n > k$ (and the like), when he bets solidly on the appropriate inequality. Although he cannot assign arbitrary odds to remaining elements of his language, there is a wide range of assignments that leaves him consistent with the axioms of slightly more realistic personal probability. Such a man is stupid, but speaking personally not much stupider than me. Personally, I would give lower odds for equality, larger for the inequalities, but otherwise my behaviour would not differ much. I know hardly any binary arithmetic.

## 9. A hierarchy.

According to how we construe "possible" in the axioms stated above we get a lattice of theories which includes the points in this list.

1. Realistic personalism. Possible = personally possible = not known to be false.

2. The theory of Vickers' [24]: possible = not proven to be inconsistent with given facts.
3. Hacking's theory: possible = possible (as analysed in [10]).
4. An algorithmic theory: possible = not provably inconsistent with the given facts according to any available algorithm.
5. Classical personalism: possible = logically possible.
6. God's theory: possible = not known by God to be false = true.

Note that theory 4 would avoid the second and third versions of Savage's difficulty— the cases where an algorithm is unknown or is impossible. But 4 remains open to Savage's objection, as do 2 and 3.

There are many more ways to fill in this epistemological list. People who are concerned with Savage's problem, and annoyed by my harsh examiner's sense of knowledge, will want to find more plausible points between 1 and 3. I hope they succeed. I must first show that even 1 evades some criticisms that might arise from devotion to 5. If you have a better theory than 1, which falls short of 5, there is every reason to expect that it too will avoid these criticisms. I have three criticisms in mind: the objection that anything short of 5 is too weak for personalism, a Dutch book objection, and the objection that anything less than 5 permits logical sloth. Each objection is unsound.

## 10. Is slightly more realistic personal probability mathematically weak?

Not for the purposes for which Savage recommends classical personalism. His theory is for policing one's own potential decisions and degrees of confidence. Might not the weaker theory be less good at detecting blunders? No. In the course of his personal police work a person proves theorems from the classical axioms and adapts his degrees of belief accordingly. But any correction deemed necessary by the classical personalist will be available to the realist. Suppose the classicist who knows $f$ settles on a coherent betting rate of $p$ on $h$ because he works out that, for him, in consistency, $Prob_f(h) = p$. Then the realist knowing $f'$ ($f$ plus the known logical truths which the classicist never bothers to mention) will settle on $p$ as well, proving that in consistency $Prob_{f'}(h) = p$.

In detail take the first time the classicist reasons, "I know $f$, which includes $e$. I prove $e$ logically implies $h$. By axiom (1), $Prob_f(h) = 1$; for me, in possession of $f$ and no more, the betting rate on $h$ is 1." The realistic *alter ego*, who includes logic among his store of facts, begins with some facts; like the classicist he proves $e \supset h$ and infers $h$ from his known $e$. By now he possesses $f'$, namely $f$ plus some logic and logical consequences of $f$, and concludes, by the realistic axiom (1), that $Prob_{f'}(h) = 1$. His metatheory differs from that of the classicist but he ends up with the same betting rates. Similarly for uses of axiom (2). Note that we are using a degenerate case of the dynamic assumption; the realist's reasoning can be represented as an application of Bayes' theorem. As an exercise one can apply this story to the model of the binary bettor when he takes the trouble to work out some binary products.

## 11. What about the Dutch Book argument?

It is said that necessary and sufficient conditions for a set of betting rates to escape a Dutch book is that they satisfy the classical axioms. We remarked earlier how this theorem fails for a more realistic

betting rate interpretation. But even more skepticism needs to be expressed. I quote de Finetti's original formulation:

> Once an individual has evaluated the probabilities of certain events, two cases can present themselves: either it is possible to bet with him in such a way as to be assured of gaining, or else this possibility does not exist. In the first case one clearly should say that the evaluation of the probabilities given by this individual contains an incoherence, an intrinsic contradiction ([7], p. 103).

Taken literally, the words are not quite right. For in order to bet with a person so as to be assured of winning, all that is required is that I know more than he does. If you bet on the outcome of a coin, but I know it is double-headed while you do not, and you offer odds on both heads and tails, I shall bet against tails and be assured of winning. But you are not incoherent or intrinsically inconsistent; you had the bad luck to bet with a crook.

It will be protested that I quibble: of course de Finetti meant "logically assured." Exactly such an interpretation is guaranteed for example by Shimony's definitions [21], although few other writers have been quite as careful as he. But I do not quibble. I urge that de Finetti's actual words are closer to an appropriate definition of coherence than the logician's gloss on them. Obviously I am not incoherent merely if someone knowing more than I can bet with me so as to be assured of winning. But, I contend, a man is incoherent if a person *knowing no more than that man does* is assured of winning.

If this is correct, it follows that a definition of incoherence must be tied to a definition of knowledge. Since no precise sense of knowledge is stronger than the examiner's sense, we want the following theorem. Suppose X knows no more (in the examiner's sense) than Y; then if Y's betting rates satisfy the slightly more realistic axioms, X cannot bet with Y in such a way that X knows (in the examiner's sense) that he will win from Y. This theorem holds.

Every stronger sense of "knowledge" will determine both a stronger definition of incoherence and a stronger set of probability axioms; thus whatever analysis you give to knowledge which takes you up the list from theory 1, you will discover a corresponding Dutch book theorem. The theorem does not discriminate among points on an epistemological list.

**12. What about logic?** We can surely insist that we do some logic: does not the slightly more realistic theory excuse a man from any cogent reasoning whatsoever? No. In the classical theory, the Dutch book argument is used to club a man into reasoning. There may be a better club to hand.

Notice that even for classical personalism, we need more than the Dutch book argument to make a man open his eyes and collect the information around him Since realistic personalism makes no distinction between finding out logical and empirical facts, we will require the same reason for harvesting logical information as for collecting empirical information. There are not two distinct kinds of decision, shall I do logic, and, shall I experiment. The question is, shall I find out what I can? The question is answered by a single maxim already accepted by personalists. I. J. Good calls it the principle of rationality [8]. It says one should act so as to maximize expected subjective utility. Good shows that if information is essentially free, acts based on more information cannot have less but can have more expected

subjective utility. This is, incidentally, the first formal reason in the literature for Carnap's requirement of total evidence ([1], p. 211), although the idea is anticipated at several places in Savage's ([25], e.g., p. 114, ex. 15). If follows that slightly more realistic axioms for personal probability, plus Good's principle, give a reason for getting facts. One is stupid if one declines to reason, not on account of the realistic version of the Dutch book argument, but because one is cutting down on expected utility. But the very judge which calls you stupid here does not call you stupid if you choose not to find out everything. It does not call you irrational if you fail to find out all the logical consequences of what you know. If the cost of information exceeds the gain in expected utility, you should decline the information.

**13. How to allow for the cost of thinking.** Good's theorem shows why to think when thinking is free, but thinking takes time and time is money. How should our model of the binary bettor allow for the cost of thinking? To answer we must import costs and prizes into the model. For each pair $m \times n$ and $j \times k$ in question, let our man be offered \$4 if he rightly calls them equal, \$2 if he rightly calls the first greater, and \$2 if he rightly calls the second greater. Recall that his personal odds were .4 on each inequality and .2 on equality, except for a few cases where I supposed that he found that right answer evident. The three simple strategies— bet on "equal" or "$m \times n$ greater" or "$j \times k$ greater"—each have subjective expectation of 80c. But our man may also undertake a calculating strategy: calculate which product is greater and bet accordingly. How does a calculation cost? Every calculation is a sequence of detachments, at least as we have constructed our model bettor. Now applying *modus ponens* is not simply a matter of detaching $q$ from $p$ and $p \supset q$; in the course of a significant calculation you must select the right $p$'s and $q$'s and that is not so easy. Indeed for me it is so time consuming that I personally set a price of 25c on every appropriate application of *modus ponens* needed by the binary computer. Now let $u_n$ be the number of occurrences of the digit one in $n$, and $u_k$ the same for $k$. As we have set up the model of our bettor, then, assuming he has efficient axioms for equality, he requires $u_n - 1$ detachments to evaluate $m \times n$, and $u_k - 1$ for $j \times k$; once he has evaluated each product, he requires two more detachments to be able to infer their relative magnitude. Thus it requires $u_n + u_k$ detachments in all.

The subjective expectation of any simple strategy, or mixture thereof, is 80c. The expected gross profit of the calculating strategy is \$2.40. Hence it is sensible to calculate when the cost of doing so is less than \$1.60; that is to say, when there are six or fewer occurrences of *one* in $n$ and $k$ together. When there are seven, it is better not to calculate.

**14. The cost of police work.** We called personalism a metatheory whose objects are beliefs and potential decisions. Our last calculation allowed for the cost of object level thinking. None of the costed detachments involved probability theory. We who look down on the binary bettor can say what his best strategy is. But personalism is for policing one's own decisions. Policing the bettor is not the same as the bettor policing himself. For among his costs will be what, in this special case, is the high cost of police work. He has to think harder to discover his best

strategy than he does to work out binary products. He has to allow for the high cost of metatheoretic thinking?

This is not Savage's question. His example concerned π. The cost of working out π can be analysed as in my simple model. But in real life there is a curious problem. The decision maker is faced with an initial meta-option. Should he invest in finding out his best object strategy, or would it be cheaper to gamble blindly? This question induces a formal regress. So it may be as Savage feared: accounting for the cost of thinking leads to "paradox," if regress be accounted paradox.

I do not find the regress paradoxical. You can allow for the cost of as much thinking as you like, up a long string of meta-metas. But you have to disregard the cost of thinking out the ultimate meta-decision. It is true that in our model of the binary bettor, first level meta-thinking costs more than object level thinking, and only a fool would disregard it. It is quite otherwise for the computer programmer who arranges a Monte Carlo solution rather than an exact one. His meta-thinking may take ten minutes of pencil time, while the object level thinking may take hours of computer time. It makes good sense to forget about the pencil time. All practical Bayesian business decision has to round off estimates of costs to one or two per cent. The cost of meta-thinking gets rounded off.

**15. Disclaimer.** Slightly more realistic personal probability is intended as a solution to Professor Savage's problem about the remote digit of π. It is not proposed that personalists should change the opening chapters of their books. The classical axioms plus the dynamic assumption provide a highly instructive model of scientific inference, especially of statistical inference. Like all models, this is both idealization and approximation. It is characteristic of the theory of personal probability that even when philosophical scruples invite one to replace the axioms by slightly more realistic assumptions, the entire substance of the theory remains. Now anyone who, like Professor Savage, thinks of personalism as a normative theory, may find this attitude complacent. For he has two difficulties which, though they seem separate, are closely related. "In what sense is this theory normative?" he asks. Later he questions the idea of the theory being "approximately valid." Let me, in closing, question whether there are *any* normative theories. I think there are only descriptive models of reasonable behaviour. If any were normative, I do not see how they could be approximately valid. But I believe there are not any. There are models of reasonable behaviour, and all models only approximate the truth. For all its defects, personalism is a good proxy.

*Acknowledgements*

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