Brit. J. Phil. Sci. **59** (2008), 1–23

De Finetti, Countable Additivity, Consistency and Coherence

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ABSTRACT

Many people believe that there is a Dutch Book argument establishing that the principle of countable additivity is a condition of coherence. De Finetti himself did not, but for reasons that are at first sight perplexing. I show that he rejected countable additivity, and hence the Dutch Book argument for it, because countable additivity conflicted with intuitive principles about the scope of authentic consistency constraints. These he often claimed were logical in nature, but he never attempted to relate this idea to deductive logic and its own concept of consistency. This I do, showing that at one level the definitions of deductive and probabilistic consistency are identical, differing only in the nature of the constraints imposed. In the probabilistic case I believe that R.T. Cox's 'scale-free' axioms for subjective probability are the most suitable candidates.

1 Introduction

Many people have pointed out that there appear to be mathematically sound Dutch Book arguments for countable additivity (the latest I am aware of is due to Williamson [1999]). Here's a quickie. Let I_{A}, I_{B}, $I_{\mathrm{C}}, \ldots$ be the indicators of the propositions/events A, B, C, . . . , defined on some appropriate underlying possibility-space Ω, where the indicator of A is a function taking the value 1 on those elements of Ω making A true and 0 otherwise. Following de Finetti ([1937]), a bet on A with *betting quotient p* and *stake S*, denominated in some currency, say US dollars, has the form $S(I_{\mathrm{A}} - p)$, $S \neq 0$, with the sign of S

doi:10.1093/bjps/axm042

indicating the direction of the bet. Thus one side of the bet pays the other the amount pS to receive S dollars if A is true. Suppose all the members of a countable disjoint family $\{A_i: i = 1,2, \ldots\}$ and its disjunction (or union, if the propositions are represented explicitly as sets) $\cup A_i$ are in the domain of a finitely additive probability function P, and that $p_i = P(A_i)$. A well-known consequence of finite additivity is that $\Sigma p_i \leq 1$, from which it is easy to infer that the countably infinite sum $\Sigma S(I_{A_i} - p_i)$ exists for all states in Ω and is identically equal to $S(I_{\cup A_i} - \Sigma p_i)$. Hence if $P(\cup A_i)$ differs from Σp_i, anyone 'owning' the function P and agreeing to all the bets at the P-rates could be Dutch Booked, that is to say made to lose come what may, since the bettor would in effect be betting on the event $\cup A_i$ at two different betting rates: an opponent would be assured of a profit by buying the bet at the cheaper rate and selling it back at the dearer.

It is generally believed that invulnerability to a Dutch Book is necessary and sufficient for the *coherence* of the relevant set of probability-evaluations, and more generally for the coherence of what de Finetti calls 'previsions', that is to say subjective estimates of random quantities which have the formal properties of expectations (thus your probability of A is your prevision of I_A). It would appear to follow that a violator of countable additivity ought to be regarded as incoherent. Given that de Finetti was responsible for (a) introducing both the concept of coherence, and the condition of invulnerability to a Dutch Book as a necessary and sufficient condition, into the probabilistic literature, and (b) exhibiting a Dutch Book against a particular type of violation of countable additivity, it is therefore on the face of it rather surprising that de Finetti himself denies this conclusion; yet he does.

The reasons for his denial are puzzling and apparently contradictory, and if only for that reason merit discussion in their own right. In what follows I shall discuss them and, I hope, make clear exactly what de Finetti's argument was, and why it is nonetheless correct. This paper is not however primarily an exercise in de Finetti exegesis, or even an application of the principle of charity. His discussion is important for two reasons. First, it raises in a very pointed way the question of Dutch Book arguments in general, arguments which are still the subject of a good deal of controversy but whose status, I think, de Finetti's own observations (and, more importantly, reservations) clarify a great deal: to the point, perhaps surprisingly, of explicitly *denying* the commonly-held view that any set of fair betting quotients which for some set of stakes generates a loss in all eventualities is incoherent. Second, his discussion suggests a view of the nature of the fundamental principles of subjective probability which most people do not associate at all with him: that these principles are nothing less than a logic of uncertain inference. Since this is a view which is still a long way from being accepted, or even seriously entertained, by mainstream Bayesians, it is a matter of some interest to see if it can be justified.

2 Coherence and Consistency

I shall start by asking a question, to which probably most of us think we already know the answer because it was famously given by de Finetti. The question is: what does it mean to say that an assignment π of probability-evaluations is coherent? De Finetti certainly does appear to give an unambiguous answer, or rather two which he proved equivalent: (i) subjected to a quadratic scoring rule (exacting a penalty equal to the sum of the squares of the differences between the values in π and the values of the indicator functions), there is no other assignment which would reduce the penalty uniformly, i.e. over all possible joint values of the indicators; (ii) considered as the agent's fair betting quotients the evaluations in π cannot combine with a choice of stakes to generate combinations of bets that necessarily result in a net loss (or gain).[1] The entire theory of probability, he tells us in the *Introduction* to his ([1972]), is simply the deductive closure of the 'coherency conditions [which are] necessary and sufficient to preserve the individual from sure losses' (p. xiv).

So that's settled then. Not quite. We need to keep in mind that even though de Finetti could read and speak English, the English-language texts one reads are generally not his own. The Italian word now nearly always translated in his work as 'coherence' is '*coerenza*', yet the standard English translation of '*coerenza*', I am told, would be 'consistency'. In their preface to (de Finetti [1974]), the translators report that they follow the policy of the English translations of (de Finetti [1937], [1972]) in using 'coherence' to translate '*coerenza*' ([1974], p. xiv). To say the least this is strange, since the translator[2] of the papers included in (de Finetti [1972]), *uniformly* uses 'consistency' and not 'coherence'! (To the best of my knowledge nobody has ever before pointed this out.) Moreover Henry Kyburg, the translator of de Finetti's famous paper ([1937]), published in French, actually tells us that de Finetti himself found the translation 'consistent' for '*cohérent*' "perfectly acceptable" (and '*cohérent*' would also usually translate 'consistent'); nevertheless Kyburg adopted 'coherent' on the ground that 'consistent' *chez* logicians just means non-contradictoriness while *cohérence*, in de Finetti's sense, imposes additional constraints on beliefs (Kyburg and Smokler [1980], p. 55).

[1] ([1974], p. 87). The fair betting quotient on A is the value x such that the agent is indifferent between betting on/against A at the odds $x{:}1-x$ for small stakes. Possibly more realistic accounts allow an interval of values rather than a single point, but I shall not discuss these here. Criterion (i) is a condition of *admissibility*: your choice cannot be dominated by another, while (ii) is of course the famous no-Dutch Book criterion. In what follows I shall focus almost exclusively on (ii). Where there are infinitely many possible states the condition in (ii) is that the net loss or gain is bounded away from zero.

[2] Giandomenico Majone.

Why is this important? As will become apparent later, not only is Kyburg's reason for eschewing the translation 'consistent' not a compelling one, but de Finetti himself often appeals to criteria of coherence/consistency which, though not extending beyond the intuitive and informal, seem to have much in common with those of logical consistency and are certainly in strong tension with the tidy operationalistic conditions (i) and (ii) above. This tension develops into almost open conflict in de Finetti's discussion of the countable additivity issue where it is decisively resolved in favour of the informal criteria, obliging him to place severe limitations on the latter, to the point of denying *explicitly* that the existence of a Dutch Book necessarily signals incoherence.

This trumping of the operationalistic by informal, quasi-logical criteria should not be altogether surprising to anyone who has paid attention to de Finetti's more philosophical observations, since there is a good deal of evidence, particularly in his earlier work, that he saw the laws of probability very much as *laws of logic*. There is of course the title of his ([1937]), in which the laws of probability are '*lois logiques*', but there is much else besides to make it reasonably convincing that this was no careless choice of words. Consider this, for example:

> It is beyond doubt that probability theory can be considered as a multi-valued logic (precisely: with a continuous range of values), and that this point of view is the most suitable to clarify the foundational aspects of the notion and the logic of probability. (de Finetti [1936], parenthesis in the original; quoted in Coletti and Scozzafava [2002], p. 61. These values were not supposed to be additional truth-values, but probability-values 'superimposed' [*sic*] on the logic of truth-values.)

Similar observations are scattered liberally throughout de Finetti's earlier writings, and while his later views—as expressed in his book ([1974])—emphasised the operationalism, we will see that same appeal to quasi-logical considerations emerge even there in his discussion of the countable additivity question.

The discussion of these issues will occupy the first part of this paper, which falls into two main parts. The second proceeds from observing that some of the formal features which de Finetti seemed to regard as particularly salient to the logic of subjective probability have striking analogues in classical deductive logic: for example, probability-values assigned consistently to any set of propositions can be extended to the propositions in any field that includes these, just as can, *mutatis mutandis*, truth-value assignments; compactness; and the idea that de Finetti appeals to in defending finite additivity (as opposed to countable), that an authentic consistency constraint should issue in a non-ampliative logic of inference. He himself never pursued these analogies, and the apparent conceptual chasm between deductive consistency and what are often described as rationality constraints on probability functions which so impressed Kyburg makes any such project look at the very least unpromising. I shall show nevertheless that it can be carried through to give a definition of probabilistic consistency formally very similar to the usual model-theoretic definition for classical propositional logic, and I shall end by showing that R.T. Cox's 'scale-free' approach supplies a *logical* rationale for finite, but not countable, additivity.

3 The Infinite Fair Lottery

Given de Finetti's often-stated view that the rules of probability are rules which permit all and only those probability evaluations which, *qua* fair betting quotients, avoid certain loss, his treatment of a simple lottery example is very puzzling if not downright inconsistent. The example is a subjective probability distribution over a countable partition $\{E_n: n = 1, 2, \ldots\}$ which assigns probability $p_n = P(E_n) = 0$, for all n, a distribution de Finetti regards as appropriate (in fact mandatory) if, for example, the E_n describe the possible outcomes of choosing an integer 'at random' ([1974], p. 120). By finite additivity $p = \Sigma p_n \leq 1$, but if $p < 1$ the countable sum of bets against each E_n with stake 1 (1 here just signifies a small unit of some currency) will result in a certain loss of $1 - p$, in this case 1 since $p = 0$. In other words, there is a Dutch Book against any owner of this assignment, and a very simple one at that. Only $p = 1$, and hence a strongly asymmetrical distribution over the E_n, avoids it. Maher sums up the general opinion in concluding that 'de Finetti cannot consistently reject countable additivity' ([1993], p. 200), a conclusion which indeed seems inescapable.

But de Finetti certainly does reject countable additivity as an axiom, and responds to the charge that the uniform distribution $p_n = 0$ is incoherent with the following rhetorical question:

> If this lack of symmetry [induced by countable additivity] does not reflect the actual judgment of the subject, perhaps because he is indifferent toward all the possible outcomes, how could we then include in the definition of consistency (in a purely formal sense) a condition which does not allow him to assign equal values, necessarily zero, to all the probabilities p_n? ([1972], p. 91)

According to the conventional account, apparently endorsed by de Finetti himself, he should easily have been able to answer his own question: the assignment is Dutch-Bookable, and so (it would appear) by his own criterion incoherent. Yet de Finetti denies that the conclusion follows. His explanation has, however, puzzled commentators ever since:

> in reality the argument is circular, for only if we know that complete additivity holds can we think of extending the notion of combinations of fair

> bets to combinations of an *infinite* number of bets, with the corresponding sequence of betting odds. ([1972], p. 91; emphasis in the original)

What commentators have taken de Finetti as saying, not unreasonably since that is what it looks as if he meant, is that countable additivity must be assumed in the mathematical operation of extending sums of fair bets to the countably infinite. There the critical unanimity ends. One view is that if this is what he means then he is simply wrong. Thus Spielman: 'De Finetti is mistaken. Countable additivity is not directly presupposed' ([1977], p. 256). Indeed, the lottery example itself seems to be a simple counterexample: it is a well-defined (i.e. convergent) denumerably infinite sum of presumptively fair bets whose betting quotients are not countably additive. Skyrms, on the other hand, claims that de Finetti is correct, but that it is the convergence of the *payoffs* that he is alluding to. To support this claim Skyrms changes the example (in Skyrms's own version the initial stake is \$100) to one in which the bets are \$101 against $\$1/2^n$ on ticket n (so that the gain is still \$100 in any event), remarking that it 'reveals clearly what the first may not, that in each case I am assuming *sigma-additivity* of the payoff-values in totaling up my net gain in the infinite system of bets' ([1983], p. 249). But that is not right: no assumption of any sort is required to observe a net deficit of 1 (or \$100) in the first case, nor is 'sigma-additivity' involved in the second, only the elementary analysis involved in summing a simple absolutely convergent series. Spielman's view seems on the face of it the right one: all that is presupposed in extending sums of fair bets consistently to the countably infinite is that the net payoffs converge, which they trivially do in this example; in which case de Finetti has merely made a childish error—something that is surely incredible.

What makes de Finetti's commentary even more puzzling is that he had already provided a proof, in the same discussion, that violating finite additivity implies incoherence ([1972], pp. 77–8); a proof which, *as he knew*, is easily extended to countable sums of bets. That proof holds, however, the key to the puzzle. The result actually proved is:

(a) If the domain of a non-negative real-valued function P is a field (or algebra), with the probability of the certain event 1, then if finite additivity is violated there is a finite sum X of bets each of which is fair according to P but such that $X > 0$ (i.e. $X(s) > 0$ for all outcomes s).

But the form in which the theorem is stated is:

(b) If the domain of a non-negative real-valued function P is a field, with the probability of the certain event 1, then if finite additivity is violated there is a fair sum X of bets (fair relative to P) and $X > 0$. ([1972], p. 77; for reasons of clarity I am paraphrasing de Finetti's own formulations of (a) and (b))

On the basis of (b), and the reasonable principle that it is inconsistent to consider fair a bet that produces a uniform loss in any case, we can conclude that violating finite additivity is inconsistent. In fact, in de Finetti's original paper, whose translation forms one of the chapters in (de Finetti [1972]), he adopts as an explicit axiom the statement that it is '*un'incoerenza*' to maintain that a bet is fair which generates certain loss. The translator, I think very reasonably, translates the entire sentence as 'it would be inconsistent to consider fair a bet that produces ... a loss in any case' ([1972], p. 84), and 'inconsistent' certainly seems the appropriate word here: given the meaning of 'fair' (see Section 2) it is as close as the vernacular usually gets to a formal logical contradiction. De Finetti's view seems to have been that it actually *is* a formal contradiction: 'one clearly should say that the evaluation of the probabilities given by this individual contains ... an intrinsic contradiction' ([1937], p. 63; see also the quotation from de Finetti terminating this paper).

However, the passage from (a) to (b) clearly requires an assumption allowing the replacement of 'a finite sum of fair bets' with 'a fair finite sum of bets'. De Finetti does explicitly make such an assumption, which I shall call (A):

(A) A finite sum of bets is fair with respect to P just in case each is fair with respect to P.

In his 1972 book an equivalent form of (A) is introduced as a *definition* of fairness for arbitrary finite sums (p. 77). Its substantive nature is identified explicitly in (de Finetti [1974]), where it is called the *hypothesis of rigidity*, for which he claims a utility-theoretic justification: small enough monetary stakes can be regarded as utilities, and standard utility theory that the expected utility of any finite sum of fair, i.e. zero-expected-utility, bets is also 0. De Finetti dispenses with the full utility treatment not just because of the simplicity dealing only with money-valued bets affords, but also the practical impossibility of having payoffs measured in 'utiles' ([1974], pp. 80–2). The simplicity is paid for, however, by the introduction of additional independent postulates, and the finite additivity of fairness is one.

4 The Puzzle Resolved—But Replaced by Another

We are now in a position to give a complete explanation of de Finetti's *prima facie* puzzling claim that

> the argument [that the existence of a uniformly positive gain from the sum of bets based on $p_n = 0$ implies incoherence] is circular, for only if we know that complete additivity holds can we think of extending the notion of combinations of fair bets to combinations of an *infinite* number of bets.

The explanation is in two parts: (i) by 'extending the notion of combinations of fair bets to combinations of an infinite number of bets' de Finetti actually means *extending* (*A*) *to include countably infinite sums*; (ii) the extension of (A) to countable sums *entails* countable additivity, and conversely.[3] De Finetti's 'puzzling' claim is therefore merely an implicit reference to the mathematical fact that extending (A) to the countably infinite case is tantamount to assuming countable additivity. Hence a valid Dutch Book argument that violating countable additivity is inconsistent presupposes what it sets out to prove, and de Finetti was therefore entirely correct to say that the argument is circular.

So the fact that a Dutch Book can be made against bets on all the outcomes in the countable lottery does not, at any rate for de Finetti, show that they are incoherent. On the contrary, because (A), and thereby the scope of the definition of coherence itself,[4] is restricted to finite sums, they are a coherent assignment. Since 'coherent' in the literature has become almost exclusively a label for de Finetti's operationalistic criteria, and the way he has been using the term in this discussion is strongly in tension with these, from now on I am going to follow the translator of de Finetti's papers in (de Finetti [1972]) and use the term 'consistent' rather than 'coherent'. This usage will also assist in bringing out some interesting features that suggest some analogies with deductive properties. Thus, we see that finite additivity can be seen as in effect producing a *compact* logic of uncertainty, which countable additivity does not. With finite additivity, if an assignment is inconsistent then some finite subset is; with countable additivity, on the other hand, the countable lottery above is an inconsistent assignment every finite subset of which is consistent, so compactness fails. Though consistent, however, the sum of the bets in the countable lottery yields a loss in all circumstances, and hence they are also *jointly unfair*. Far from exhibiting inconsistency, therefore, the fact that if you were to bet at your fair betting rates on each outcome you could be made to lose overall merely exhibits your own imprudence in taking on a demonstrably unfair sum of individually fair bets.

We see, therefore, that for de Finetti the ability of Dutch Book arguments, and inadmissibility arguments generally, to reveal inconsistency in a set S of probability evaluations is very limited: only if S permits a Dutch Book for some finite sum of bets is it inconsistent, and even then only by courtesy of the additivity-of-fairness principle (A). By themselves, therefore, these arguments are completely silent on the question of consistency. Only in conjunction with (A) do they entail inconsistency in the elicited previsions, while if (A) is not extended to include in its scope suitably convergent infinite sums they merely

[3] The proof is simple. Suppose there are countably many disjoint events E_i in the domain D of P whose union $\cup E_i$ is also in D. By the extended version of (A), the countable sum X of the fair bets $1(I_{E_i} - P(E_i))$ is fair. But $X = \Sigma I_{E_i} - \Sigma P(E_i) = 1(I_{\cup E_i} - \Sigma P(E_i))$. This is fair just in case $P(\cup E_i) = \Sigma E_i$. The converse is proved using simple properties of expectations.

[4] A footnote to the no-Dutch Book criterion of coherence in (de Finetti [1974], p. 87) tells us that it applies to *finite* combinations of bets, but no explanation is offered. That such a significant restriction appears only in a footnote is in itself surprising.

confirm, as in the countable lottery example, that the sum of an infinite set of fair bets is not necessarily fair. That may settle the issue of what de Finetti meant, and why, though at first sight perplexing, his claim is correct that the Dutch Bookability of a uniform distribution over a countable partition does not reveal incoherence/inconsistency. It does, however, raise a couple of difficult questions of its own. One is why de Finetti placed so much significance on Dutch Book arguments. If one needs (A) to transform a Dutch Book argument into a demonstration of inconsistency, and if (A) *directly* implies finite additivity (as we easily see from the proof for the countable case),[5] then why bother with Dutch Book considerations at all? Logically speaking, establishing that a violation of finite additivity implies Dutch-Bookability is just a redundant step. That is one question; another is why (A) should be restricted to finite sums only.

Let us take these in turn. What is certainly true is that invulnerability to a (finite!) Dutch Book was for de Finetti a concept of deep *theoretical* importance, for it turns out to be a very powerful analytic tool. Thus, not only does it characterise exactly the class of finitely additive probability functions (so-called *Dutch Book Theorem*), but it is also the key to a much deeper result, which is that consistency is what logicians call an *absolute* property: the consistency of any assignment P[E] to a class E of propositions depends only on P[E] itself. This is an immediate corollary of de Finetti's result ([1972] p. 78, itself a corollary of what he called the 'Fundamental Theorem of Probability' [1974], p. 112) that P[E] is extendable to a finitely additive probability function on any algebra which includes E just in case no set of bets on finitely many members of E at odds given by P is subject to certain loss or gain. We have noted one formal analogy which finite additivity creates with first order logic, namely compactness; now we have another, since any consistent assignment of truth-values, i.e. any assignment satisfying the truth-table constraints, to any set E of propositions is independent of the language in which E is formulated. These features are more than merely suggestive: a probabilistic model theory canonically extending a classical truth-valuation in a relational structure was actually constructed forty years ago by Gaifman ([1964]) and further developed by Scott and Krauss ([1966]), while the results above can be straightforwardly derived within a probabilistic logical semantics based on notions of model, consistency and consequence formally very similar to the usual deductive ones.

And so to the other question: why limit (A) to finite sums? The restriction does look on the face of it somewhat arbitrary, since there seems to be no reason in principle why, say, Savage's theory, or any utility theory which according to de Finetti underwrites (A), should not be supplemented by an appropriate continuity axiom from which the countable version of (A), and a more restrictive

[5] It is also straightforward to show that it implies the multiplication rule $P(\mathrm{A\&B}) = P(\mathrm{A} \mid \mathrm{B})P(\mathrm{B})$.

definition of consistency, would follow (Villegas [1964], pp. 1794–6). The question was implicitly posed by de Finetti himself:

> From the subjectivistic viewpoint, the problem is to determine whether in assigning probabilities $p_1, p_2, \ldots, p_n, \ldots$ to a complete denumerable class of incompatible events, it is necessary for consistency that the sum of the p_n's be 1 … [but] this statement of the problem is not quite unequivocal since the very definition of consistency could be modified ([1972], pp. 90–1)

The answer that he gave was to reject the extended version of (A) precisely *because* it entails countable additivity, and because he believed that there are compelling reasons for seeing in countable additivity an axiom so strong that it goes far beyond merely delimiting the boundaries of consistent assignments. To these I now turn.

5 Countable Additivity, Conglomerability and Dutch Books

The common argument (and the only argument that Kolmogorov himself supplies) that countable additivity is justified by the technical convenience it procures[6] is dismissed by de Finetti with the observation that mathematics should be servant rather than master, and in particular the servant of reflective intuition about what should and what should not be regarded as determining mere consistency. He illustrates the workings of this intuition as it bears on the additivity problem with some informal examples. One of these is the countably infinite lottery: it does seem strange that a mere criterion of consistency should forbid a uniform distribution over a countable disjoint set, as is certainly possible with either a finite one or a bounded interval of real numbers, and instead demand a heavily skewed one:

> If this lack of symmetry does not reflect the actual judgment of the subject … how could we then include in the definition of consistency (in a purely formal sense) a condition which does not allow him to assign equal values (necessarily zero) to all the probabilities p_n? Should we force him, against his own judgment, to assign practically the entire probability to some finite set of events, perhaps chosen arbitrarily? Such limitations on the choice of the probabilities are altogether extraneous to the essence of the consistency condition. ([1972], pp. 91–2)[7]

[6] Countable additivity is equivalent to continuity in the sense of Kolmogorov's Axiom 5, on which most of modern mathematical probability relies. It ensures among other things that probability functions can be uniquely recovered (on the Borel sets) from distribution functions (Cramér [1946], pp. 51–3); for example, only if continuity is assumed does the jump at a discontinuity point a of $F(x)$ give the probability that $X = a$.

[7] This is not an endorsement of the Principle of Indifference, which is prescriptive rather than simply permissive.

The objection becomes even more compelling if you first assign a uniform distribution to a variate ranging over the unit interval, and then receive information simply that its value is a rational number. It seems implausible to regard this information *by itself* as implying that the corrected distribution over the countable set of rationals must be heavily skewed. Indeed, countable additivity seems in effect to be *adding content* not contained in the conditioning information, to wit, that some of the probabilities are now much more likely than others: 'Here the *content* of my judgment enters into the picture' (de Finetti [1974], p. 123, emphasis in the original; he did not say why he thought *finite* additivity itself should be immune to this charge, and this is a point I shall return to in the next section).

Although he did not, de Finetti might have made the same general comment about the celebrated Bayesian convergence-of-opinion theorems, which in their strong 'with probability 1' formulation require countable additivity. Since for a countably additive distribution over a countable partition a finite subset will carry a probability $1 - \varepsilon$ where ε tends to zero this implies that if a hypothesis H about a data source generating countably infinite data sequences is false the probability that it will be falsified after any given finite number of observations must tend to 0. It follows that sufficient positive evidence will push the probability of H arbitrarily close to 1 (Kelly [1994], p. 321–30; Kelly's own assessment could easily have come from de Finetti himself:

> If probabilistic convergence theorems are to serve as a philosophical antidote to the logical reliabilist's concerns about local underdetermination and inductive demons, then countable additivity is elevated from the status of a mere technical convenience to that of a central epistemological axiom favoring scientific realism. (p. 323))

A further objection, that de Finetti mentions but, since he repudiates objective probability, does not stress, is the fact that long-run relative frequencies are not generally countably additive ([1972], pp. 89–90). For those who believe that subjective probability should be set equal to the objective probability where that is known, and accept a long-run frequency interpretation of objective chances (and most working physicists seem to), this supplies a compelling reason for refusing to adopt countable additivity as a universal principle. Though people have brought methodological objections against basing objective probabilities on limiting relative frequencies (but that is another story) there is absolutely no doubt that it is a consistent mathematical theory. Not only is it consistent, but it is easy to find models, and in particular models of the de Finetti uniform distribution over a countable partition: e.g. let a von Mises Collective be any permutation of the set N of natural numbers and the attributes the singletons $\{n\}$, $n = 0, 1, 2, \ldots$ The limiting relative frequency of each exists and is equal to 0, though the countable union (disjunction) has of course limiting relative frequency 1 (von Mises's axiom of randomness, in Church's recursion-theoretic form, is also satisfied, if rather trivially). It is well known that events with well-defined limiting relative frequencies do not always form a field, but it follows from a result of Kadane and O'Hagan ([1995]) that the uniform distribution over the singletons of N can be extended to all subsets of N.

There are of course also arguments against rejecting countable additivity, and de Finetti cites those he thinks most important, only to dismiss them after due consideration. Probably the most important, as far as the literature is concerned at any rate, is the fact that non-countably additive probability functions whose domains include countably infinite partitions are *non-conglomerable* with respect to at least one such partition, and conversely. A probability P is conglomerable with respect to a countable partition $B = \{B_i: i = 1, 2, \ldots\}$, if for every proposition A in the domain of P and numbers x, y such that $x \leq P(A \mid B_i) \leq y$ for all B_i in B, $P(A)$ lies within the same bounds; nonconglomerability is just the negation of conglomerability. I shall follow the usual custom and truncate 'conglomerable/nonconglomerable with respect to countable partitions' to just 'conglomerable/nonconglomerable'.

Nonconglomerability sounds rather like a failure of an infinitary logical rule of 'or'-introduction. The appearance is illusory, however, for that would require parsing a conditional probability $P(A \mid B)$ as 'the probability of A if B is true', a parsing which as de Finetti shows is untenable ([1972], p. 104). Nevertheless, there are some curious, if not disturbing, features of nonconglomerability. Consider again the uniform distribution in de Finetti's fair countable lottery, and the following scenario described, de Finetti tells us ([1972], p. 205), by Lester Dubins in a letter to Savage. Two different mechanisms, A and B, for randomly generating a positive integer N are each selected with probability $1/2$, and we are given that $P(N = n \mid A) = 2^{-n}$, while $P(N = n \mid B) = 0$ for all n (thus violating countable additivity). It follows that $P(B \mid N = n) = 0$, for all n, though $P(B) = 1/2$: P is nonconglomerable in the partition $\{\{n\}, n = 1,2,3,\ldots\}$. Note that in this example all the conditional probabilities are fully determinate, and that we also have, in the limiting relative frequency model, a concrete model of a random distribution over N. But now we seem to have a paradox, because the identity $P(N = n \mid B) = 0$ for all n tells us that no individual value of N conveys any discriminatory information, yet one should nevertheless bet at infinite odds on A and against B after any given observation even though their unconditional probabilities are $1/2$! In other words, you know in advance of making any observation that *whatever its outcome* it will decide you with certainty in favour of A, yet even armed with that foreknowledge you still only assign A a prior probability of $1/2$. As Kadane *et al.* ([1996]) neatly put it, with any nonconglomerable probability function you are always liable to 'reason to a foregone conclusion' (whereas such reasoning is always precluded by countable additivity). Worse, you seem to be reasoning inconsistently: since '$N = n$' must be true for some n, it looks as if you are assigning different probabilities to the same proposition (both to A and to B). But that conclusion is certainly incorrect: to see inconsistency here is to fall into the fallacy de Finetti exposed, of thinking that a nonconglomerable distribution is in conflict, or at any rate marked tension, with deductive logic.

It is a fact, however, that this probability function is open to a Dutch Book.[8] The Dutch Book can of course be avoided by refraining from assigning a positive probability to any alternative like A which assigns positive probability to each $N = n$ (thus avoiding $P(N = n) > 0$ for each n). Unfortunately this strategy has only limited effectiveness since nonconglomerability *in itself* is open to a similar penalty. For suppose that $P(\mathrm{C}\,|\,\mathrm{D}_i) \leq k < 1$ and $P(\mathrm{C}) > k$, where D_i, $i = 1, 2, \ldots$ is a countable partition. This immediately leads to a violation of the 'principle' of countable dominance, which says that if act F is weakly preferred to act G conditional on each member of a countable partition, then F is weakly preferred to G unconditionally. Let W be a bet paying \$1 if C is true and $\mathrm{W}\,|\,\mathrm{D}_i$ be the same bet conditional on D_i. Suppose that, as usual, conditional preferences are represented by conditional expectations and unconditional preferences by unconditional expectations. Then a straight payment of \$$k$ is weakly preferred to W conditional on B_i, for each i, whereas W is preferred to a payment of \$$k + d$ for some $d > 0$. The penalty for the violation is of course a Dutch Book. For if you own P you will regard selling a conditional bet on C given D_i with stake \$1 and betting quotient b as at worst fair, for each i, and buying a bet on C with betting quotient $k + d$, for some $d > 0$, and stake \$1 as more than fair. Since one of the D_i must be true, the net gain from all these bets is $-$\$$d$.

Since admissibility and invulnerability to Dutch Books are what de Finetti is usually taken to claim coherence is all about, failures like these might be thought (at least) troubling for him; yet he does not even mention them. But this should come as no surprise, for in these examples we have in disguised forms just a reprise of the countable lottery example. As Milne observes ([unpublished], p. 7), the assumption behind the Dutch Books above are the same as in that example, namely that you will regard as fair infinite sets (i.e. sums) of bets at your individual fair betting rate. But as we saw de Finetti point out, to see in such Dutch Books a conviction of inconsistency is to beg that very question.

But that defence presents de Finetti, and advocates of Dutch Book arguments in general, with a problem: if those for countable additivity, dominance, conglomerability etc. beg the question by presupposing the countable version of (A) or some broadly equivalent additivity-of-fairness principle, then it is difficult to see why the familiar Dutch Book arguments for *finite* additivity and the multiplication rule do not share the same suspicion by presupposing (A)

[8] It is particularly easy to construct one against $P(\mathrm{B}\,|\,\mathrm{N} = k) = 0$, for all k, and $P(\mathrm{B}) = 0.5$.

itself.[9] De Finetti regarded (A) as authorised by expected utility theory subject to the proviso that the stakes are small enough for the effects of risk-aversion to be ignorable. But in no practical case will this be true unless the stakes are so small as to invalidate the betting scenario altogether as a reliable way of eliciting degrees of belief, a fact Ramsey cited as a reason for eschewing an approach in terms of money bets altogether ([1926], p. 176). Nor is it practically possible to invoke pure utility-scaled bets, as de Finetti pointed out:

> it would be practically impossible to proceed with transactions, because the real magnitudes in which they have to be expressed ... would have to be adjusted to the continuous and complex variations in a unit of measure [utility] that nobody would be able to observe. ([1974], p. 81)

Even for sums of two bets (A) is still therefore a substantive postulate, and as such one which not only can one consistently reject but also in appropriate circumstances deem false. As far as I am aware the first to point this out in the philosophical literature was Schick; as he observes, the Dutch Book argument for the binary addition principle contains

> the unspoken assumption ... that the value I place on [the bets taken] together is the sum of the values I put on them singly. This, however, is not always true – it isn't always true of *me*. ([1986], p. 113)[10]

Quite so. One might for example reject that 'unspoken assumption' if one thinks it conflicts with the probability evaluations one thinks the circumstances render appropriate, indeed even possibly mandate. De Finetti rejected its countable version for precisely such reasons, and it is not difficult to construct plausible rejection-scenarios even in the finite case. They may even be logico-mathematically compelling. For example, there is a 'dynamic' Dutch Book argument for the so-called Reflection Principle, which states that $P(\mathrm{B} \mid Q(\mathrm{B}) = r) = r$, where Q is a future probability function. But suppose in the Dubins example earlier that $P(Q(\mathrm{B}) = 0) = 1$ (you have reasoned that you are certain to observe $N = k$ for some k and that you will update your P-function to Q to accommodate this information), while $P(\mathrm{B}) = 0.5$. Then the ordinary probability axioms imply that $P(\mathrm{B} \mid Q(\mathrm{B}) = 0) = 0.5$. Indeed, it is quite easy to manufacture counterexamples.

There is however still a large body of opinion which holds fast to the idea that Dutch Book arguments, *including* that for countable additivity, are symptoms of a genuine inconsistency: of evaluating uncertain options differently depending

[9] This is the *tu quoque* I take it Spielman is directing against de Finetti in (Spielman [1977], p. 256).

[10] Though as we noted, in the form of postulate (A) it was 'spoken' by de Finetti himself—perhaps rather too quietly, since it is subsequently ignored in the usual accounts of his work.

on how they are expressed or described. The idea originates with Ramsey, who in a much-quoted observation observed that

> If anyone's mental condition violated these [probability] laws, his choice would depend on the precise form in which the options were offered him, which would be absurd. He could then have a book made against him by a cunning bettor and would then stand to lose in any event. ([1926], p. 80)

Skyrms, who quotes the passage, underlines the point: 'what is basic [to the Dutch Book argument for the binary addition principle for probabilities] is the consistency condition that you evaluate a betting arrangement independently of how it is described' ([1984], pp. 21–2). If the preceding paragraphs are correct then Ramsey and Skyrms are just wrong. Let us see. Pointing out that the sum of two bets on the propositions A and B for a dollar stake with betting quotients equal to your personal probabilities $P(\mathrm{A})$, $P(\mathrm{B})$ is a bet on the disjunction $\mathrm{A}\vee\mathrm{B}$ with the same stake and betting quotient $P(\mathrm{A}) + P(\mathrm{B})$, Skyrms concludes (using p, q where I have used A, B):

> if you are to be *consistent*, your personal probability for p *or* q had better be … probability(p) + probability(q). ([1984], p. 21; emphasis in the original.)

–it 'had better be' because the penalty for violation is a Dutch Book. As Skyrms notes, and as we saw in Section 1 above, the argument is straightforwardly extended to the countably infinite case.

But the argument is fallacious. Consistency in the Ramsey–Skyrms sense amounts only to the condition that one's evaluation be a *functional*; it certainly does not follow that it must be an *additive* functional. To ensure that it is, one must stipulate it. Thus, to proceed from the facts that $P(\mathrm{A})$ is my fair betting quotient on A, and $P(\mathrm{B})$ is my fair betting quotient on B, to the conclusion that the betting quotient determined by the sum of two bets at those odds is my fair betting quotient I clearly need the additional premise that I regard the sum of two fair bets as fair: i.e., *one needs* (*A*)—or at any rate the two-dimensional version of (A)—as de Finetti saw. Curiously, in a footnote to his text Skyrms implicitly concedes the point by making what amounts to just that assumption: specifically, that if the expected value $EV(\mathrm{W}_1)$ of a bet W_1 is equal to $EV(\mathrm{W}'_1)$, and $EV(\mathrm{W}_2) = EV(\mathrm{W}'_2)$, then $EV(\mathrm{W}_1 + \mathrm{W}_2) = EV(\mathrm{W}'_1 + \mathrm{W}'_2)$, where the sums are pointwise defined ([1984], p. 123, note 4). Skyrms himself shows that this is just a form of (A) restricted to the sum of two bets: setting W'_1 and W'_2 equal to straight payments $\$x_1$ and $\$x_2$ of the expected values of W_1 and W_2, we infer that $EV(\mathrm{W}_1 + \mathrm{W}_2) = EV(\$x_1 + \$x_2) = \$x_1 + \$x_2$ (by another assumption) $=$ $EV(\mathrm{W}_1) + EV(\mathrm{W}_2)$.

It is almost time to draw a line under Dutch Book arguments. The final item to consider brings back the focus onto countable additivity: it is the claim by Seidenfeld and Schervish ([1983]) that violating countable additivity causes the agent to be inconsistent. If the preceding observations are correct then this is wrong, but the mistake, if there is one, is certainly not immediately obvious. Their argument proceeds from the observation that countable additivity for infinite partitions is equivalent to nonconglomerability along some margin. Let Y be a gamble whose value on each member D_i of a partition is the conditional prize $\mathrm{W}\,|\,\mathrm{D}_i$ where as earlier W pays 1 if C is true and 0 if not, and $\mathrm{W}\,|\,\mathrm{D}_i$ is the same gamble conditional on D_i for some C. So $Y = \Sigma(\mathrm{W}\,|\,\mathrm{D}_i)\mathrm{I}_{\mathrm{D}_i}$. Let P be the nonconglomerable distribution, $P(\mathrm{C}\,|\,\mathrm{D}_i) \leq k$, $P(\mathrm{C}) > k$, for every $i = 1, 2, \ldots$. I shall omit currency symbols for the sake of notational simplicity. The value to the agent of $\mathrm{W}\,|\,\mathrm{D}_i$ is $P(\mathrm{C}\,|\,\mathrm{D}_i)$, which is no greater than k for all i. Hence the expected value of this value, i.e. of the prevision y^+ of Y, is not greater than k. Since one of the D_i must be true, Y will pay 1 if C is true and 0 if not. Hence $Y = \mathrm{W}$, and so the bet $-(\mathrm{W} - y^+)$ is fair. But the value you put on $-(\mathrm{W} - y^+)$ is no greater than $-d$ and so $-(\mathrm{W} - y^+)$ is unfair ([1983], p. 410). Since y^+ is a fair price for selling W, it follows that k is a weakly favourable one. On the other hand, $(k + d/2)$ is a favourable price for buying W. Hence you are inconsistent and can be made to lose.

In this Dutch Book argument no additional assumption like (A), or even its countable version, seems to be presupposed. So where does it go wrong? Seidenfeld and Schervish claim that the argument depends on allowing outcomes (of Y) to be gambles ([1983], p. 410), and as we shall see this is indeed the nub of the matter. To make the situation clearer it will help to write the conditional bet $\mathrm{W}\,|\,\mathrm{D}_i$ you value at $P(\mathrm{C}\,|\,\mathrm{D}_i)$ in canonical random variable form, as the quantity $I_{\mathrm{C\&D}_i} + P(\mathrm{C}\,|\,\mathrm{D}_i)I_{\neg\mathrm{D}_i}$ (because by the definition of a conditional betting quotient, this is the random quantity you would exchange $P(\mathrm{C}\,|\,\mathrm{D}_i)$ for, paying 1 when both C and D_i are true, 0 when C is false and D_i is true, and $P(\mathrm{C}\,|\,\mathrm{D}_i)$ when D_i is false, i.e. when the conditional bet is called off and you get your money back). Thus $Y = \Sigma[I_{\mathrm{C\&D}_i} + P(\mathrm{C}\,|\,\mathrm{D}_i)I_{\neg\mathrm{D}_i}]I_{\mathrm{D}_i}$. Since every possible state is in some D_i, it is not difficult to see that $Y(s) = I_{\mathrm{C}}(s) = \mathrm{W}(s)$ for every s in the state-space, and so $Y = \mathrm{W}$ as Seidenfeld and Schervish claim. But then it is not true that the outcome for s in D_j, say, is the gamble, in the sense of (measurable) *function*, $I_{\mathrm{C\&D}_j} + \mathrm{P}(\mathrm{C}\,|\,\mathrm{D}_j)I_{\neg\mathrm{D}_j}$. If Seidenfeld and Schervish really want to regard the outcome of Y at D_j as the *function* of s, $\lambda s[I_{\mathrm{C\&D}_j}(s) + P(\mathrm{C}\,|\,\mathrm{D}_j)I_{\neg\mathrm{D}_j}(s)]$,[11] then Y is not W, and not even a measurable function but a functional of higher type. Either way, the inconsistency vanishes.

[11] The functional lambda notation is very useful for distinguishing between functions, i.e. entities of higher type, and their values at given arguments.

6 The Probability Axioms and Cox's Theorem

De Finetti's argument that a purely 'formal' principle should not forbid *in principle* a uniform distribution over the elementary events (atoms) in a power set algebra has, I believe, a very strong intuitive pull. In addition there is the fact that we get a nicely analogical development of a logical probability without countable additivity, manifested in the properties of the absoluteness of consistency, and compactness, neither of which obtain under countable additivity. But something is nevertheless missing, and that is a type of completeness theorem telling us that the rules of probability extend to finite but not countable additivity. Dutch Book arguments certainly don't have this character, and neither does the usual utility-based one, whose classic exposition is Savage ([1954]), since as observed earlier a continuity condition can be straightforwardly added to give countable additivity.[12] On the other hand, those accounts leave too much to be desired on other grounds: enough has been said (I hope) to rule out Dutch Book arguments as independent authentic validators of probabilistic principles, while the problems besetting utility-based accounts seem to be no less severe: there are not only the familiar utility paradoxes but also the much deeper problem of unequivocally separating out utilities from probabilities described in (Schervish *et al*. [1990]).

Much work in the foundations of probability in the last sixty or so years has consisted in investigating qualitative axioms for a constrained preference relation. But the qualitative form of additivity which is necessary for an agreeing probability function means that such stand-alone systems will to that extent be question-begging, at the same time also failing to draw a sharp conceptual line at countable additivity (and as remarked earlier one can always append an appropriate continuity assumption in the manner of Villegas [1964]). An alternative approach is to start immediately with a quantitative notion and think of general principles that *any* acceptable numerical measure of uncertainty should obey. One might think this a less promising avenue than the measurement theory approach because of the capacity of representation theorems to generate unique or almost unique measures from assumptions which, because they are only qualitative, seem to possess much greater generality. But R.T. Cox ([1961]) and I.J. Good ([1950]) independently proved this assessment to be incorrect: they showed how strikingly little in the way of constraints on a numerical measure suffice to yield the finitely additive probability functions as canonical representations. It is not just the generality of the assumptions that makes the Cox–Good result so significant: unlike some of those which have to be imposed on a qualitative probability ordering, the assumptions used by Cox, and to a somewhat lesser extent Good, seem to have the property of being

[12] There are of course also well-known difficulties with this type of account.

uniformly self-evidently analytic principles of numerical epistemic probability *whatever particular scale it might be measured in*.[13]

Cox, whose treatment rather than Good's I shall refer to in what follows, identified three such invariant principles. Let M be an admissible real-valued *conditional* measure (since one of the objectives is to generate the multiplication principle), taking values in an interval of the real line. Then for any jointly consistent B, C, D

i. $M(\neg A \mid C) = f(M(A \mid C))$, for some real-valued, twice-differentiable function $f(x)$ decreasing in x.

ii. $M(A\&B \mid C) = g(M(A \mid B\&C), M(B \mid C))$, for some real-valued $g(x,y)$ with continuous partial derivatives, increasing in x and y.[14]

iii. If $A \Leftrightarrow B$ and $C \Leftrightarrow D$, then $M(A \mid C) = M(B \mid D)$.

That the probability of ¬A should depend in a smoothly decreasing way on that of A seems intuitively compelling. Though there are non-additive theories which uncouple these two quantities, like Shafer's theory of non-additive belief functions ([1976]) and some systems of fuzzy probability, if anything deserves to be taken as a fundamental principle then surely it is that as your belief in a proposition's being true increases, so your belief in its being false should diminish. There should be even less difficulty in accepting (iii) as fundamental. So we come to (ii), which in conjunction with (iii) implies that the right hand side of (ii) is symmetric in A and B. (ii) tells us that the probability of A and B both being true is determined jointly by that of A being true given B and that of B. Why should that be the case? Well, knowing how likely A would be if I could assume B was true will not of course tell me how likely A is; for that I would also need to know how likely B is. But once I know that then it seems that I should know, at any rate in principle, how likely both A and B are. Nothing in this piece of informal conditional reasoning depends on any scale of measurement of these quantities, or on any particular operational definition of conditional probability (such as is the case in, for example, the usual Dutch Book argument for the multiplication rule).

[13] Cox was a working physicist and his point of departure was a typical one: to look for *invariant* principles:

> to consider first . . . what principles of probable inference will hold however probability is measured. Such principles, if there are any, will play in the theory of probable inference a part like that of Carnot's principle in thermodynamics, which holds for all possible scales of temperature, or like the parts played in mechanics by the equations of Lagrange and Hamilton, which have the same form no matter what system of coordinates is used in the description of motion. ([1961], p. 1)

[14] The differentiability conditions were dispensed with in subsequent derivations of substantially the same result by Aczél ([1966], pp. 320–4) and Paris ([1994], pp. 24–32).

Cox believed, I think correctly, that these three rules deserve to be regarded as fundamental. His next, and major, step was to show that, constrained by the rules of propositional logic, a necessary and sufficient condition for (i)–(iii) is that there exists a strictly increasing real-valued function $h(x)$ taking values in [0,1] such that

$$h(M(\neg A|C)) = 1 - h(M(A|C))$$
$$h(M(A\&B|C)) = h(M(A|B\&C))h(M(B|C))$$[15]

Thus a simple rescaling of M produces the finitely-additive probability axioms, and since no other constraint than the rules of propositional logic was imposed there is no loss of generality in taking the finitely-additive probability axioms themselves as the general solution of (i)–(iii). Two things need to be said about this. Firstly, it is obvious that there are other rescalings of (i)–(iii) that are not probability functions, like the odds scale. Does this not refute the claim that those axioms are consistency constraints? No. It has already been observed that such rescalings change nothing at the fundamental level, any more than the fact that arbitrary transformations of 0 and 1 for 'false' and 'true' generate different arithmetical representations of the propositional connectives but change nothing otherwise[16] (or should change nothing: see the next section where this basic principle is apparently violated).

Secondly and more importantly from the present perspective, Cox's result does not extend to endorsing countable additivity. There are infinitary propositional languages whose properties are reasonably well understood: $L_{\omega_1,\omega}$, for example, which like a first order language allows only finite strings of quantifiers but unlike first order languages permits countable conjunctions and disjunctions, even has a weak completeness theorem (Scott [1965]). But there is no natural extension of Cox's proof which would exploit that additional structure to yield countable additivity. The question was raised earlier whether a unified account existed of which finite additivity is a consequence and countable additivity is not. Here we have an answer, and one endorsing de Finetti's view that countable additivity is an assumption that can be added if thought appropriate to the problem, but not an axiom held to be invoked in every case, appropriate or not (e.g. the countable fair lottery!).

[15] There has been some controversy about Cox's proof (see for example Halpern [1999]), in particular about his inference of the associativity of g from the proof that for any A,B,C,D in the background propositional language such that A&B&C is consistent, $g(g(M(B\,|\,C), M(A\,|\,B\&C)), M(D\,|\,A\&B\&C)) = g(M(B\,|\,C), g(M(A\,|\,B\&C), M(D\,|\,A\&B\&C)))$. Associativity only follows if there are enough propositions whose values approximate any given triple of real numbers in D arbitrarily closely. To ensure this Paris introduces a further assumption ([1994], p. 24, Co 5), but Cox's differentiability conditions imply that f and g are fixed under changes in values of M for given arguments; in other words we are considering not just actual but also *possible* values of $M(A\,|\,C)$, whence associativity does follow.

[16] Earman's summary dismissal seems to overlook this basic point ([1992], p. 45).

7 Truth and Probability

There is one further desideratum which should be satisfied by any account of epistemic probability which proposes to be more that an exercise in *l'art pour l'art*. It should arguably connect at the boundary with general properties of *truth*. Indeed, a traditional view of epistemic probability is that it is a measure, admittedly not of objective truth or even partial truth (which nobody has ever succeeded in defining in a compelling way), but of *truthlikeness*, and (very importantly) one which can be updated with additional information. What one person believes to be true, however, another may not, but since we are interested in *invariant* principles the most we should arguably ask is that the propositions assigned T by *every* valuation and those assigned F by *every* valuation always appear in the respective certainty-classes, i.e. those assigned the maximum and minimum probability values, and that certainty of truth should be closed under deduction and certainty of falsity under converse-deduction. This is easily seen to be the case for all Cox's admissible measures (it follows from the fact that each of them, conditioned on a tautology, can be represented by a finitely additive probability function).

A more ambitious attempt to use truth-oriented constraints to elicit a significant property of belief functions—no less than that they should be formally probabilities—is made in a paper by James M. Joyce ([1998]), employing a dominance argument formally reminiscent of de Finetti's well-known dominance argument for the finitely additive probability axioms relative to a quadratic scoring rule. Joyce by contrast uses a scoring rule based on truth-accuracy, or more accurately, inaccuracy (!), one which, roughly speaking, averages distance between the values of a belief function b and a truth-valuation w taking the value 1 for 'true' and 0 for 'false' ([1998], p. 593). Joyce shows that any non-probabilistic belief function can be dominated with respect to accuracy by a finitely additive probabilistic one. Details of his argument have been questioned,[17] but a more fundamental objection is to its robustness: it is not clear that it is inaccuracy with respect to *truth* that Joyce's measure represents, depending as it seems to do on the (purely conventional) use of 1 as the numerical proxy for 'true' rather than 0. Indeed, by changing these values round one gets a very different result. A perfectly accurate belief function b with respect to $W = 1 - w$ is now only *dually* probabilistic, with b assigning the value 0 to a tautology, etc., and Joyce's proof would show that for any probabilistic belief function there is a non-probabilistic one strictly less inaccurate than it with respect to all W-valuations. Of course, the definition of dominance (with respect to distance between belief-values and w-values) could be transformed correspondingly so that larger numerical discrepancies between b-values and

[17] (Maher [2001]).

W-values correspond to smaller 'distances' from the truth, but that would hardly *explain* the probability axioms as taking the form they do.[18]

8 Conclusion: 'Logical Omniscience'

An important corollary of a logical approach to epistemic probability is a natural solution of the so-called 'problem of logical omniscience' that dogs the position, held by the large majority of contemporary Bayesians, that valid probabilistic principles are canons of rational belief.[19] The problem is this: because the probability axioms require various deductive relationships to constrain assignments of probability (logical truths are assigned probability 1, and probabilities add over pairs of mutually inconsistent propositions), rationality on the probability = rational belief view would be forced to include the ability to decide whether those relationships hold in any given case, a decision problem which is well-known to be in principle beyond the algorithmic ability of a Universal Turing Machine. On the present account that problem is no problem at all, for the simple reason that this account does not pretend to lay down principles of rational belief. It is not even *about* rational belief (whatever that is). Just as deductive consistency is a predicate applicable to truth-value assignments *in themselves*, whoever makes them or whether those individuals are rational or not,[20] probabilistic consistency is a predicate to be applied to probability assignments in themselves, independently of who makes them or why. Though it has largely been overlooked in accounts of his work, this was a point de Finetti stressed and I will simply end with the following apposite quotation:

> To speak of coherent or incoherent (consistent or inconsistent) individuals has been interpreted as a criticism of people who do not accept a specific behavior rule ... It is better to speak of coherence (consistency) of probability evaluations rather than of individuals, not only to avoid this charge, but *because the notion belongs strictly to the evaluations and only*

[18] Jeffrey also seems to be a victim of reading too much into the conventional association of 1 with 'true' and 0 with 'false'. He claimed that because probabilities are the expected values of the corresponding indicator functions they are therefore estimates of truth-values ([1986]). If he were right then the estimate of the truth-value of A could just as correctly be $P(\neg A)$ (by taking truth-values to be given by *W*). Reasoning similar to Jeffrey's often appears in discussions of quantum logic: the eigenvalues of a projector are 1 and 0 and the probability of a 'quantum proposition' is the expected value of a projector projecting onto the corresponding subspace of Hilbert space.

[19] Thus Savage in his classic work ([1954]) claims to be constructing 'a highly idealized theory of the behavior of a "rational" person with respect to decisions'. (p. 7)

[20] Mathematicians, asked what their proofs rest on, might refer the enquirer back to the axioms of ZFC, Zermelo–Fraenkel set theory plus the Axiom of Choice. But they are not irrational in doing this and at the same time acknowledging that as far as anyone knows or can know ZFC could be inconsistent (Gödel's second incompleteness theorem implies that any proof of the consistency of ZFC, or any nontrivial mathematical theory, begs the question).

indirectly to the individuals ([1937], p. 63; my emphasis but parentheses in the original).

Acknowledgements

I would like to thank an anonymous referee for very helpful comments, David Makinson likewise, and the members of the workshop on Dutch Book arguments in Prague in August 2005 for comments on an earlier version.

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