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Some Philosophical Reflections on de Finetti's Thought

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My reflections are divided into four parts. The first deals with de Finetti's pragmatism, the second with his rejection of determinism and indeterminism, the third with the problem of axiomatizing qualitative probability, and the fourth on coherence and consistency.

1 De Finetti's pragmatism

Throughout his life, as far as I have a good sense of that, de Finetti held to a strong form of pragmatism. Here is an important quotation from him in his later years from the translation of his *Theory of Probability* (de Finetti 1975: vol. 2, 201):

> In the philosophical arena, the problem of induction, its meaning, use and justification, has given rise to endless controversy, which, in the absence of an appropriate probabilistic framework, has inevitably been fruitless, leaving the major issues unresolved. It seems to me that the question was correctly formulated by Hume (if I interpret him correctly – others may disagree) and the pragmatists (of whom I particularly admire the work of Giovanni Vailati.)

And here is de Finetti's footnote on Vailati (de Finetti 1975: vol. 2, 201):

> Cf. G. Vailati, *Scritti* (Edited by Seeber), Florence (1911). Giovanni Vailati, a mathematician of the Peano school, was an original, profound and committed supporter of pragmatism in Italy

Bruno de Finetti, Radical Probabilist
Maria Carla Galavotti (ed.)

> (which had several features – which I, in fact, approve of – distinguishing it from the American version of Peirce, James, etc.). ...

Let me say something about Giovanni Vailati. He admired Peirce and James, but the form of pragmatism he developed in Italy had several features that distinguished it from the American version. Here is a quotation from Vailati in 1909:

> The term "pragmatism," according to its original creator Ch. S. Peirce, appeared for the first time in 1871, in a series of debates between the members of the Metaphysical Club in Cambridge, Mass. Peirce found that this was the proper word to indicate the method followed by Berkeley in his investigations of the concepts of "substance," "matter," "reality," etc. – even if such a method was not explicitly formulated by the author. ... Peirce thought that this procedure used by Berkeley was an instance of a more general methodological process, which could be described like this: *the only way to determine and clarify the [meaning] of an assertion is to indicate which particular experiences, according to such assertion, are going to take place, or would take place given specific circumstances.* ... "Pragmatism" can be conceived to have a "utilitarian" character only to the extent that it makes it possible to get rid of a certain number of "useless" issues... For instance, when we have two assertions and we are not able to identify the particular experiences that should occur in order to make one of the assertions true and not the other, it is not proper to inquire which of the two is true. In a case like this the two assertions, according to Peirce, have to be considered simply as two different ways to say the same thing. ... Peirce's methodological rule appears to be, for what has been said so far, an indication of the importance of examining our assertions to identify a part that implies some predictions, because that is the part that can be confirmed or refuted by further experiences. ... To entertain a certain belief instead of another means this, for the pragmatist: to have a certain kind of expectations, different from the expectations he would have, having he had a different belief. ... In fact, besides our beliefs regarding the future, we have at least as many beliefs that, apparently, are only facts of the present or the past. Nevertheless, if we look closer at such beliefs we can see that a reference to the future is always an essential part of their meaning. A typical example of this, examined by Berkeley, is represented by judgments on the existence of material objects. In his

> Theory of Vision – that is, after all, in every respects a theory of "prediction" [previsione]. The common opinion is that the size, position and distance of objects are perceived in the same way as we perceive their color. Instead our visual perceptions are not able to provide this kind of information immediately. Distances, shapes, and dimensions of objects are not "seen" by us, but rather "foreseen", or inferred from the signs provided by actual visual perceptions. ... (Vailati 1909, trans. by Claudia Arrighi)

This long passage is especially marked by its orientation of pragmatism towards predicting the future. One cannot help but think that de Finetti's own theory of prevision was much influenced by what Vailati has to say, especially in the last paragraph focused on Berkeley. It is not possible to sketch all the ways in which de Finetti's thought was influenced by Vailati, but it is quite clear that there is a natural sympathy of ideas here evident to any careful reader. It is unfortunate that the work of the Italian pragmatists is not more available in English.

One way of offering more detail is to examine de Finetti's allegiance to operationalism, exemplified earlier in the work of the American physicist P.W. Bridgman, explicitly referenced by de Finetti in several places, but especially in the final chapter of his 1937 Paris lectures. He refers there to Bridgman's influential work *The Logic of Modern Physics*, published in 1927.

Here is de Finetti's own very clear statement about operationalism:

> With each of our assertions, a question invariably surges into our mind: has this assertion really any meaning? To give only one example, we know that the notion of simultaneity seemed, not very long ago, perfectly clear and sure, to the point that it had been thought possible to consider time as a notion given *a priori*. Why do we no longer believe this today? Because we have been taught the necessity of conceiving of every notion from a point of view which can be called "operational". Every notion is only a word without meaning so long as it is not known how to verify practically any statement at all where this notion comes up; in the example given above, this practical verification is furnished us by Einstein's procedure employing light signals. An analogous evolution took place some time ago in the mathematical sciences: once, for example, the problem of knowing if 1 – 1 + 1 – 1 + ... = ½ or not was considered in a nebulous, mysterious, metaphysical way; it sufficed to define what was to be understood by "limit" (for exam-

> ple ordinary limit, limit in the sense of Cesaro) and all the obscurities vanished. (de Finetti 1937/1964: 148)

Later, de Finetti goes on to say, "We only apply the notion of probability in order to make likely predictions" (p. 150). This additional focus almost exclusively on prediction takes us immediately back to Vailati's form of pragmatism. I think it is fair to say that of all the major figures in the foundations of probability, it is de Finetti who is most deeply committed to pragmatism and operationalism as the general philosophical foundations of his thought.

2 Rejection of determinism and indeterminism

Again, let me begin with a quotation from de Finetti, an important one, also from vol. 2 of *The Theory of Probability*:

> Before turning to another topic, it would perhaps be appropriate to clarify certain views on the theme of determinism, given the connection with discussions pertaining to the present theory, and given that we have commented upon it (even though in order to decide that it was not relevant). In my opinion, the attachment to determinism as an *exigency of thought* is now incomprehensible. Both classical statistical mechanics (or Mendelian hereditary) and quantum physics provide explanations – in the form of coherent theories, accepted by many people – of apparently deterministic phenomena. The mere existence of such explanations should be sufficient to give the lie for evermore to the dogmatism of this point of view. (de Finetti 1975: vol. 2, 324)

Almost everyone who has read very much of de Finetti at all knows very well his strong rejection of determinism, well stated in the passage that I have quoted, but to be found in many other places in his writing. I'll have more to say about this later. Here is another quote stating his animosity to determinism in another way, bringing out, in this case, his sympathy with the views of von Neumann:

> The foundations of physics are those we have today (perhaps for many decades, perhaps centuries), and I think it unlikely that they can be interpreted (or adapted) in deterministic versions, like those that are apparently yearned for by people who invoke the possible existence of 'hidden parameters', or similar devices. I hold this view not only because von Neumann's arguments against such an

> idea seem to me convincing (vN, pp. 313-328), but also because I can see no reason to yearn for such a thing, or to value it – apart from an anachronistic and nostalgic prejudice in favour of the scientific fashion of the nineteenth century. If anything, I find it, on the contrary, distasteful; it leaves me somewhat bewildered to have to admit that the evolution of the system (i.e., of its functions ψ) is deterministic in character (instead of, for example, being a random process) so that indeterminism merely creeps in because of the observation, rather than completely dominating the scene. (de Finetti 1975: vol. 2, 325)

Maria Carla Galavotti has brought to my attention the important fact that de Finetti rejected any absolute or metaphysical concept of indeterminism, as well as that of determinism. Here is a revealing quotation, published late in de Finetti's career:

> The alternative [between determinism and indeterminism] is undecidable and … illusory. These are metaphysical diatribes over 'things in themselves'; science is concerned with what 'appears to us', and it is not strange that, in order to study these phenomena it may in some cases seem more useful to imagine them from this or that standpoint, by means of deterministic theories… or indeterministic ones. (de Finetti 1976: 299-300)

For a more extended discussion of this topic, see Galavotti (1989). Rather than pursue this general topic, I prefer to sketch a view about determinism and indeterminism that is not de Finetti's, but is one with which I think he would be in much agreement if it were laid out in detail, with the appropriate theorems that I refer to. A good starting point is the entropy of familiar Bernoulli or Markov stochastic processes. I do not write down the equation, but note that the notion of entropy here is the rate of entropy change, not the entropy at a cross-sectional moment of a stationary process. The beginning of this story is the theorem proved by Kolmogorov and Sinai in 1958:

THEOREM 1. (Kolmogorov 1958, Kolmogorov 1959 and Sinai 1959). *If two Bernoulli or Markov processes are isomorphic then their entropies are the same.*

Until they proved this theorem the seemingly simple problem was open, whether or not the Bernoulli process with probability 1/2 heads and 1/2 tails, and the Bernoulli process of a three-sided coin, with probability 1/3 for each side, are isomorphic. The theorem proves that they are not, because their entropies are not the same, and therefore by contraposition, they are not isomorphic. The more difficult and surprising converse theorem was proved by Ornstein in 1970 for the case of Bernoulli processes:

THEOREM 2. (Ornstein 1970). *If two Bernoulli processes have the same entropy they are isomorphic.*

In a rather short time, and in a rather straightforward way, Theorem 2 was generalized to:

THEOREM 3. *Any two irreducible, stationary, finite-state discrete Markov processes are isomorphic if and only if they have the same periodicity and the same entropy.*

And, this theorem has as a natural corollary the following: an irreducible, stationary, finite-state discrete Markov process is isomorphic to a Bernoulli process of the same entropy if and only if it is aperiodic. The surprising and important development, particularly with the proof of Ornstein's theorem in 1970, is that entropy is a complete invariant for the measure-theoretic isomorphism of ergodic Bernoulli or Markov processes. The word "complete" here means that to know if two such processes are isomorphic, we need only know if a single number is the same for both of them, namely the entropy rate.

Now I want to apply these ideas to a new view of determinism and indeterminism. A good place to begin is the following quote from Peirce in 1892:

> Try to verify any law of nature, and you will find that the more precise your observations, the more certain they will be to show irregular departures from the law. We are accustomed to ascribe these, and I do not say wrongly, to errors of observation; yet we cannot usually account for such errors in any antecedently probable way. Trace their causes back far enough, and you will be forced to admit they are always due to arbitrary determination, or chance. (Peirce 1892/1955: 331).

Peirce is making, in some ways, an obvious but still very important point. The verification of deterministic laws can scarcely ever be said to be complete, because there will be errors of measurement if continuous quantities are involved. And, even in discrete processes involving large numbers, errors are almost always found in the collection and analysis of data. It is a way of saying that the notion of a strictly deterministic law is an idealized characterization of scientific laws.

I now turn to the beautiful example of Sinai billiards (named after the Russian mathematician Ya. G. Sinai for his important results, 1970). They are so named, because, unlike the idealized deterministic periodic motion of an idealized particle of classical mechanics, chaos is introduced by placing a convex object in the middle of the billiard table. The particle is reflected off this convex object according to the same physical laws as for the sides of the table. So, particles as idealized billiard balls satisfy the following rules: we have a rectangular box with reflecting sides, the classical law of reflection holds: the angle of reflection equals the angle of incidence, and there is no dissipation of energy. For Sinai billiards the convex obstacle is also an ideal reflector. For this physical situation we have the following idealized theorem: the motion of a Sinai billiard ball is ergodic, and as a corollary of that such motion is strongly chaotic.

It is widespread folklore in discussions of chaos by physicists that most important physical examples of chaos are deterministic. On the other hand, there is a variety of evidence, especially mathematical arguments, that associated with chaos, particularly in the strongest chaotic examples, are phenomena that can only be regarded as genuinely random or stochastic in nature. It would be easy to argue that one has got to choose either the deterministic or stochastic view of phenomena, and at least for a given set of cases, it is not possible to move back and forth in a coherent fashion. It is this view, also perhaps part of the folklore, that we want to argue very much against in the present discussion. We will be depending on general ideas from ergodic theory and in particular on the strong kind of isomorphism theorems proved by Donald Ornstein and his colleagues. Before we turn to the details, there are one or two other points we want to discuss in a very intuitive fashion. For example, if we use a billiard model of a mechanical particle, and we consider the deterministic model in the case of an ergodic motion, that is, one, for example, where there is a convex obstacle in the middle of the billiard table, then there is an empirically indistinguishable stochastic model. The response to this isomorphism might be, "Well yes, but for the case of ergodic motion where the convex object is present we should choose either the simple Newtonian model". Because this Newtonian model works so well in the nonergodic periodic case when there is no convex object, it is natural to say that it is not a real choice between the deterministic or stochastic models. Because of its generalizability the choice seems obviously to be the deterministic model.

But this argument can run too far and into trouble when we turn to a wider set of cases. On the same line we would be pushed to argue that the only kind of complete physical model for quantum mechanics must be a deterministic one, for example the kind advocated by Bohm, but the evidence, once we turn to quantum mechanical phenomena, seems far from persuasive for selecting as the unique intuitively correct model the deterministic one. Here there is much to be said for choosing the stochastic model, which is much closer in spirit to the standard interpretation of classical quantum mechanics. Our point, without going into details, is that whether we intuitively believe the model should be deterministic or stochastic will vary with the particular physical phenomena we are considering. What is fundamental is that independent of this variation of choice of examples or experiments is that when we do have chaotic phenomena, especially when we have ergodic phenomena, then we are in a position to choose either a deterministic or stochastic model. When such a choice between different models has occurred previously in physics – and it has occurred repeatedly in a variety of examples, such as free choice of a frame of reference in Galilean relativity, or choice between the Heisenberg or Schroedinger representation of quantum mechanics –, the natural move is toward a more abstract concept of invariance. What is especially interesting about the empirical indistinguishability and the resulting abstract invariance in the present billiard case, is that at the mathematical level the different kinds of models are inconsistent, that is, the assumption of both the deterministic and the stochastic model leads to a contradiction when fully spelled out. On the other hand, it leads to no contradiction at the level of observations, as we shall see in an important class of ergodic cases. (The remarks in the preceding paragraph and this one are close to ones made several years ago in a joint article with Acacio de Barros (Suppes and de Barros 1996).)

We can go further in terms of Sinai billiards with the concept of measure-theoretic isomorphism. To keep things in the context of finite-state discrete processes, we can form a finite partition of the free surface on the billiard table. This constitutes a finite partition of the space of possible trajectories for the billiard and we correspondingly make time discrete in terms of movement from one element of the partition to another. With these constructive approximations, the following theorem has been proved:

THEOREM 4. (Gallavotti and Ornstein 1974). *With the discrete approximation by a finite partition of the continuous flow just described above, the discrete deterministic model of the billiard is isomorphic in the measure-theoretic sense to a finite-state discrete Bernoulli process model of the motion of the billiard.*

It should be noted that instead of this theorem, we could have stated a theorem for continuous time and such results are to be found in the paper by Gallavotti and Ornstein. What the Gallavotti and Ornstein theorem shows is that the discrete mechanics of billiard balls is in the measure-theoretic sense isomorphic to a discrete Bernoulli analysis of the same phenomena. However, it is to be emphasized that in order to claim that intuitively the two kinds of analysis are observationally indistinguishable we need a stricter concept of isomorphism.

To show why this is so, we do not have to consider something as complicated as the billiard example but only compare a first-order Markov process and a Bernoulli process that have the same entropy rate and therefore are isomorphic in the measure-theoretic sense. It is easy to show by very direct statistical tests whether a given sample path of any length, which is meant to approximate an infinite sequence, comes from a Bernoulli process or a first-order Markov process with strong transition dependencies. There is, for example, a simple chi-square test for distinguishing between the two. It is a test for first-order versus zero-order dependency. The analysis is statistical and, of course, cannot be inferred from a single observation, but the data are usually decisive even for finite sample paths that consist of no more than 100 or 200 trials.

A natural stricter concept is that of α-congruence due to Ornstein and Weiss (1991). My explanation is intuitive in terms of trajectories of Sinai billiard balls. Let D_t be a classical mechanics deterministic model with α the bound away from zero of errors of measurement, and let S_t be a stochastic model such that the predictions of the trajectory of a Sinai billiard such that D_t and S_t satisfy

(i) Both models predict correctly the trajectory within the error bound α.

(ii) The theoretical predicted trajectories τ of models D_t and S_t are such that for any two points x and y at time t, the distance between $\delta_t(x,y)$ (deterministic model) and $\sigma_t(x,y)$ (stochastic model) is less than α, except for a set of exceptional points whose probability of occurring is less than α.

So this concept of congruence is a probabilistic one, as a generalization of the familiar Euclidean distance between points in a plane. Using α-congruence the following remarkable theorem can be proved.

THEOREM 5. (Ornstein and Weiss 1991). *There are physical processes which can equally well be analyzed as deterministic systems of classical mechanics or as indeterministic Markov processes, no matter how many observations are made, if observations have an accuracy bounded away from zero.*

I think de Finetti would have been pleased that for important chaotic examples of physical systems there is no observable distinction between deterministic and indeterministic theories of the physical systems. There being no observable distinction resonates nicely with his pragmatism and operationalism.

Here is a very qualitative description of the kind of Markov process cleverly constructed by Ornstein and Weiss to satisfy their Theorem.

1. Pick $\alpha \succ 0$.
2. Define a (biased) coin-tossing stochastic process.
3. Finitely partition the table.
4. The ball will always be in one element of the finite partition.
5. It stays in each element p of the partition for time $t(p)$.
6. The ball then jumps to one of a pair of points according to a toss of the coin.
7. This pair of points depend on p.

Here are some final remarks on Ornstein and Weiss' results that are meant to be in de Finetti's spirit.

1. Such explicit theorems for concrete systems are difficult, but seem likely to be true for a wide variety of chaotic phenomena.

2. Such results are important in making the case that any thesis of universal determinism or indeterminism is transcendental i.e., beyond experience.
3. Transcendental, not false.

3 Axiomatizing qualitative probability

One of the things for which de Finetti is justly famous is his emphasis on the qualitative nature of probability and the possibility of expressing many of our ordinary ideas about probability in qualitative as well as subjective terms (de Finetti 1937/1964). These ideas are expressed well in a long passage from the famous 1937 Paris lectures, which I quote:

> Let us consider the notion of probability as it is conceived by all of us in everyday life. Let us consider a well-defined event and suppose that we do not know in advance whether it will occur or not; the doubt about its occurrence to which we are subjects lends itself to comparison, and, consequently, to gradation. If we acknowledge only, first, that one uncertain event can only appear to us (a) equally probable, (b) more probable, or (c) less probable than another; second, that an uncertain event always seems to us more probable than an impossible event and less probable than a necessary event; and finally, third, that when we judge an event E' more probable than an event E, which is itself judged more probable than an event E'', the event E' can only appear more probable than E'' (transitive property), it will suffice to add to these three evidently trivial axioms a fourth, itself of a purely qualitative nature, in order to construct rigorously the whole theory of probability. This fourth axiom tells us that inequalities are preserved in logical sums: if E is incompatible with E_1 and with E_2, then $E_1 \vee E$ will be more or less probable than $E_2 \vee E$, or they will be equally probable, according to whether E_1 is more or less probable than E_2, or they are equally probable. More generally, it may be deduced from this that two inequalities, such as
>
> E_1 is more probable than E_2,
>
> E_1' is more probable than E_2',
>
> can be added to give

$E_1 \vee E_1'$ is more probable than $E_2 \vee E_2'$,

> provided that the events added are incompatible with each other (E_1 with E_1', E_2 with E_2'). It can then be shown that when we have events for which we know a subdivision into possible cases that we judge to be equally probable, the comparison between their probabilities can be reduced to the purely arithmetic comparison of the ratio between the number of favorable cases and the number of possible cases (not because the judgment then has an objective value, but because everything substantial and thus subjective is already included in the judgment that the cases constituting the division are equally probable). This ratio can then be chosen as the appropriate index to measure a probability, and applied in general, even in cases other than those in which one can effectively employ the criterion that governs us there. In these other cases one can evaluate this index by comparison: it will be in fact a number, uniquely determined, such that to numbers greater or less than that number will correspond events respectively more probable or less probable than the event considered. Thus, while starting out from a purely qualitative system of axioms, one arrives at a quantitative measure of probability, and then at the theorem of total probability which permits the construction of the whole calculus of probabilities. (de Finetti 1937/1964: 100-101)

In Definition 1, I express these axioms numbered as by de Finetti, with only slight changes. Axiom D0 just defines the formal structures used, consisting of a nonempty set Ω, an algebra of sets representing events closed under union and complementation, and the qualitative relation $\pm$ expressing (weakly) more probable than:

DEFINITION 1. A *structure* $\mathbf{\Omega} = (\Omega, \Im, \pm)$ *is a qualitative probability structure if and only if the following axioms are satisfied for all A, B, and C in* $\Im$:

D0. $\Im$ *is an algebra of sets on* Ω;

D1. $A \pm B$ *or* $B \pm A$;

D2. *If* $A \neq \varnothing$, *then* $A \succ \varnothing$ *and* $\Omega \pm A$.

D3. *If* $A \pm B$ *and* $B \pm C$, *then* $A \pm C$;

D4. If $A \cap C = \varnothing$ *and* $B \cap C = \varnothing$, *then* $A \pm B$ *if and only if* $A \cup C \pm B \cup C$.

De Finetti's subdivision remark after formulating the fourth axiom, the one expressing additivity, is one way to get immediately a quantitative representation in the finite case. For this purpose we define equivalence $\approx$ of events in the standard way: $A \approx B$ if and only if $A \pm B$ and $B \pm A$. I will call this Axiom 5, the subdivision axiom, which has a deceptively simple formulation, but it has as a consequence that if the set of possible outcomes is finite, then the possible outcomes all have the same probability.

DEFINITION 2. *Let* Ω *be a finite set, and let* $(\Omega, \Im, \pm)$ *be a qualitative probability structure. This structure is* uniform *when* Axiom 5 *is also satisfied*:

AXIOM 5. *If* $A \pm B$ *then there is a* C *in* $\Im$ *such that* $A \approx B \cup C$.

We then have as a straightforward elementary theorem the existence of a unique strictly agreeing probability measure P for such structures when they are finite.

THEOREM 6. *Let* $(\Omega, \Im, \pm)$ *be a uniform finite qualitative probability structure. Then this structure has a strictly agreeing unique probability measure* P, *i.e., for events* A *and* B

$$P(A) \geq P(B) \text{ if and only if } A \pm B.$$

I stated this theorem and gave the elementary proof in Suppes (1969: 6) before I read de Finetti in any detail. Kraft, Pratt, and Seidenberg (1959) showed that finite qualitative probability structures, in the sense of Definition 1, do not in general have a strictly agreeing probability measure. Their counterexample has five possible outcomes.

There is a further step that de Finetti did not formally take. By having a set of standard events of equal probability, it is possible to approximate the probability of any event even when the space of possible outcomes is infinite.

Here is the definition of the structure and the theorem of approximate measurement of belief I gave in Suppes (1974) with acknowledgment of the earlier work of the godfathers of modern Bayesian thought – Ramsey, de Finetti, and Savage.

From a formal standpoint, the basic structures to which the axioms apply are quadruples $(\Omega, \Im, \mathcal{S}, \pm)$, where as before, Ω is a nonempty set, $\Im$ is an algebra of subsets of Ω, and $\pm$ is the qualitative probability relation, $\mathcal{S}$ is a similar finite algebra of sets, intuitively the events that are used for standard measurements, and I shall refer to the events in $\mathcal{S}$ as *standard events*, *S*, *T*, etc.

DEFINITION 3. *A structure* $\mathbf{\Omega} = (\Omega, \Im, \mathcal{S}, \pm)$ *is a finite approximate qualitative probability structure if and only if* Ω *is a nonempty set,* $\Im$ *and* $\mathcal{S}$ *are algebras of sets on* Ω, *and the following axioms are satisfied for every A, B* and C in $\Im$ and every S and T in $\mathcal{S}$:

Axiom 1. $(\Omega, \Im, \pm)$ *is a qualitative probability structure in the sense of Definition 1;*

Axiom 2. $\mathcal{S}$ *is a finite subset of* $\Im$, *and* $(\Omega, \mathcal{S}, \pm)$ *is a uniform qualitative probability structure.*

A minimal element of $\mathcal{S}$ is any event A in $\mathcal{S}$ such that $A \neq \varnothing$, and it is not the case that there is a nonempty B in $\mathcal{S}$ such that B is a proper subset of A. A *minimal open interval* (S, S') of $\mathcal{S}$ is such that $S \prec S'$ and $S' - S$ is equivalent to a minimal element of $\mathcal{S}$. Axiom 5, stated earlier, is the main structural axiom, which holds only for the finite subalgebra and not for the general algebra.

In stating the representation and uniqueness theorem for structures satisfying Definition 3, in addition to an ordinary probability measure on the standard events, I shall use upper and lower probabilities to express the inexact measurement of arbitrary events. A good discussion of the quantitative properties one expects of such upper and lower probabilities is found in Good (1962). All of his properties are not needed here because he dealt with conditional probabilities. The following properties are fundamental, where $P_*(A)$ is the lower probability of an event A and $P^*(A)$ is the upper probability (for every A and B in $\Im$):

I. $P_*(A) \geq 0$.

II. $P_*(\Omega) = P^*(\Omega) = 1$.

III. If $A \cap B = \varnothing$ then

$$P_*(A) + P_*(B) \leq P_*(A \cup B) \leq P_*(A) + P^*(B) \leq P^*(A \cup B) \leq P^*(A) + P^*(B)$$

Condition (I) corresponds to Good's Axiom D2 and (III) to his Axiom D3.

For standard events $P(S) = P_*(S) = P^*(S)$. For an arbitrary event A not equivalent in qualitative probability to a standard event, I think of its "true" probability as lying in the open interval $(P_*(A), P^*(A))$.

In the fourth part of Theorem 7, I define a certain relation and state it is a semiorder with an implication from the semiorder relation holding to an inequality for upper and lower probabilities. Semiorders have been fairly widely discussed in the literature as a generalization of simple orders, first introduced by Duncan Luce. I use here the axioms given by Scott and Suppes (1958). A structure $(A, {}^*\succ)$ where A is a nonempty set and ${}^*\succ$ is a binary relation on A is a *semiorder* if and only if for all $a, b, c, d \in A$:

Axiom 1. Not $a \,{}^*\!\succ a$;

Axiom 2. If $a \,{}^*\!\succ b$ *and* $c \,{}^*\!\succ d$ *then either* $a \,{}^*\!\succ d$ *or* $c \,{}^*\!\succ b$;

Axiom 3. If $a \,{}^*\!\succ b$ *and* $b \,{}^*\!\succ c$ *then either* $a \,{}^*\!\succ d$ *or* $d \,{}^*\!\succ c$.

THEOREM 7. *Let* $\mathbf{\Omega} = (\Omega, \mathfrak{I}, \mathcal{S}, \pm)$ *be a finite approximate qualitative probability structure. Then*

(i) there exists a probability measure P *on* $\mathcal{S}$ *such that for any two standard events* S *and* T

$$S \pm T \text{ if and only if } P(S) \geq P(T),$$

(ii) the measure P *is unique and assigns the same positive probability to each minimal event of* $\mathcal{S}$,

(iii) if we define P_* *and* P^* *as follows:*

(a) for any event A *in* $\mathfrak{I}$ *equivalent to some standard event* S,

$$P_*(A) = P^*(A) = P(S),$$

(b) for any A *in* $\mathfrak{I}$ *not equivalent to some standard event* S, *but lying in the minimal open interval* (S, S') *for standard events* S *and* S'

$$P_*(A) = P(S) \text{ and } P^*(A) = P(S'),$$

then P_* *and* P^* *satisfy conditions (I)--(III) for upper and lower probabilities on* $\mathfrak{I}$, *and*

(c) if n *is the number of minimal elements in* $\mathcal{S}$ *then for every* A *in* $\mathfrak{I}$

$$P^*(A) - P_*(A) \leq 1/n,$$

(iv) if we define for A and B in $\Im$

$$A \,{}^*\!\succ B \text{ if and only if } \exists S \text{ in } \mathfrak{S} \text{ such that } A \succ S \succ B,$$

then ${}^*\!\succ$ is a semiorder on $\Im$, if $A \,{}^*\!\succ B$ then $P_*(A) \geq P^*(B)$, and if $P_*(A) \geq P^*(B)$ then $A \pm B$.

This theorem expresses a simple constructive result about approximate measurement of subjective probability. It is, I believe, very much in the spirit of informal remarks that occur in de Finetti's writings about such approximations.

Finally, I consider an extension of Definition 1 to give necessary and sufficient conditions for the existence of a unique strictly agreeing probability measure. In brief, by enlarging the structures to include elementary random variables, Definition 1 can be extended to give necessary and sufficient conditions for all sets Ω, finite or infinite.

If A is a set, $\mathbf{A}^i$ is its indicator function, which is a random variable. Thus, if A is an event

$$\mathbf{A}^i(\omega) = \begin{cases} 1 & \text{if } \omega \in A, \\ 0 & \text{otherwise.} \end{cases}$$

We need to go slightly beyond the indicator functions. The move is from an algebra of events to the algebra $\Im^*$ of extended indicator functions relative to $\Im$. The algebra $\Im^*$ is just the smallest semigroup (under function addition) containing the indicator functions of all events in $\Im$. In other words, $\Im^*$ is the intersection of all sets with the property that if A is in $\Im$ then $\mathbf{A}^i$ is in $\Im^*$ and if $\mathbf{A}^*$ and $\mathbf{B}^*$ are in $\Im^*$, then $\mathbf{A}^* + \mathbf{B}^*$ is in $\Im^*$.

Then, to have $\mathbf{A}^* \pm \mathbf{B}^*$ is to have, intuitively, the expected value of $\mathbf{A}^*$ equal to or greater than the expected value of $\mathbf{B}^*$. The qualitative comparison is now not one about the probable occurrences of events, but about the qualitative expected value of certain restricted random variables. What the representation theorem below shows is that very simple necessary and sufficient conditions on the qualitative comparison of extended indicator functions guarantee existence of a strictly agreeing, finitely additive measure P, whether the set Ω of possible outcomes is finite or infinite, i.e., $P(A) \pm P(B)$ if and only if $A \pm B$.

DEFINITION 4 (Suppes and Zanotti 1976). *Let Ω be a nonempty set, let $\Im$ be an algebra of sets on Ω, and let $\pm$ be a binary relation on $\Im^*$, the algebra of extended indicator functions relative to $\Im$. Then the qualitative algebra $(\Omega, \Im, \pm)$ is qualitatively satisfactory if and only if the following axioms are satisfied for every $\mathbf{A}^*, \mathbf{B}^*$, and $\mathbf{C}^*$ in $\Im^*$:*

Axiom 1. *The relation $\pm$ is a weak ordering of $\Im^*$;*
Axiom 2. $\Omega^i \succ \varnothing^i$;
Axiom 3. $\mathbf{A}^* \pm \varnothing^i$;
Axiom 4. $\mathbf{A}^* \pm \mathbf{B}^*$ *if and only if* $\mathbf{A}^* + \mathbf{C}^* \pm \mathbf{B}^* + \mathbf{C}^*$;
Axiom 5. *If* $\mathbf{A}^* \succ \mathbf{B}^*$ *then for every* $\mathbf{C}^*$ and $\mathbf{D}^*$ in $\Im^*$ *there is a positive integer n such that* $n\mathbf{A}^* + \mathbf{C}^* \pm n\mathbf{B}^* + \mathbf{D}^*$.

These axioms in terms of qualitative expectation fit in with de Finetti's framework of analysis. Only Axiom 5 needs explanation. It is one standard form of an Archimedean axiom.

THEOREM 8. *Let Ω be a nonempty set, let $\Im$ be an algebra of sets on Ω, and let $\pm$ be a binary relation on $\Im$. Then a necessary and sufficient condition that there exists a strictly agreeing probability measure on $\Im$ is that there be an extension of $\pm$ from $\Im$* to *$\Im^*$ such that the qualitative algebra of extended indicator functions $(\Omega, \Im^*, \pm)$ is qualitatively satisfactory.*

Moreover, if $(\Omega, \Im^*, \pm)$ is qualitatively satisfactory, then there is a unique strictly agreeing expectation function on $\Im^*$ and this expectation function generates a unique strictly agreeing probability measure on $\Im$.

4 Coherence and consistency

I summarize my main points, which, I realize, not everyone will agree with.

1. Coherence is an important concept for de Finetti and almost all subjectivists.
2. Prior to experimentation, coherence replaces truth as the central philosophical concept.
3. But coherence is not a strong enough constraint on experimental scientists or engineers prior to actual experimentation. We must believe their priors are based on serious past experience. This is a point not well enough recognized by some Bayesians.
4. It is reasonable to hold that even for the hypothesis being tested, "Not all priors are equal."
5. My focus is on coherence itself and its formal complexity as a problem.
6. In most advanced work in statistics, the problem of coherence is assumed, not investigated. It amounts to assuming the family of random variables for the problem at hand has a joint distribution, even when the prior information is far from being decisive on this point. As an example, the nonexistence of joint distributions for pairwise correlations is a familiar aspect of quantum-entanglement experiments.
7. As Fréchet pointed out, it is easy for persons to have irrational, i.e., incoherent probabilities for complex situations.
8. Here is a simple artificial example. We are given three random variables $\mathbf{X}$, $\mathbf{Y}$, and $\mathbf{Z}$ with possible values ± 1 and expectations

$$E(\mathbf{X}) = E(\mathbf{Y}) = E(\mathbf{Z}) = 0,$$

and

$$E(\mathbf{XY}) = 0.6$$
$$E(\mathbf{YZ}) = 0.7$$

Given a meteorological story or something similar, respondents are asked their prior for $E(\mathbf{XZ}) = ?$

Bob says, I estimate $E(\mathbf{XZ}) = 0.25$.

Question: Is Bob coherent? Answer: No.

We have the following inequalities for the three correlations of $\mathbf{X}$, $\mathbf{Y}$, and $\mathbf{Z}$ to have a joint distribution (Suppes and Zanotti 1981):

$$-1 \leq E(\mathbf{XY}) + E(\mathbf{YZ}) + E(\mathbf{XZ}) \leq 1 + 2Min(E(\mathbf{XY}), E(\mathbf{YZ}), E(\mathbf{XZ})).$$

The values assigned to the correlations do not satisfy these inequalities, so Bob is incoherent, even though he is unlikely to know it. An objection to this example is that I am assigning priors to the correlations, but this is really no different than asking for the three pairwise distributions, and usually the triple moment $E(\mathbf{XYZ})$ would be ignored. The essential point is that there is little if any discussion in the literature of complicated priors and their problem of coherence. Betting quotients are not constructed in fact for anything very complicated. I am just expressing here my skepticism that for complicated stochastic processes and related entities, proof of coherence can be taken as a serious requirement, any more than proof of consistency is a prerequisite to do classical mathematical analysis.

9. Consistency has been an ideal but unattainable goal of pure mathematics since the revolutionary results of Gödel in the 1930s. Based on this experience, coherence seems hard to guarantee in advanced work in probability and statistics, a point that does not seem to be fully appreciated among Bayesian statisticians. I am just sorry I did not ever discuss this problem with de Finetti, on the several occasions when we had long philosophical conversations. But I do want to make clear I think he would have had a lot to say, and would have had no difficulty in putting his own touch on how to think about such problems. Here is a quotation from 1961, the publication of the proceedings of a colloquium entitled *La Décision* in 1960 in Paris, the occasion of my first meeting with de Finetti:

> Sans doute, dans les évaluations pratiques complexes tout homme réel est incapable d'échapper à des contradictions. Malheureusement, la qualification de "comportement rationnel" employée parfois, assez improprement, pour le comportement conforme à la théorie, a fait souvent soupçonner qu'on prétendait que tous les hommes (les fous exceptés) seraient infailliblement et automatiquement conduits par leur propre psychologie à s'y tenir; ou que c'est la psychologie que l'on a imaginée pour un type d'homme hautement idéalisé. Ni l'un ni l'autre, comme on vient de voir.
>
> Un fait différent est par contre d'admettre explicitement comme un des effets de l'idéalisation propre à tout schème: mes décisions réelles dépendent aussi de facteurs accessoires qu'il faut considérer à côté du schème principal. (de Finetti 1961: 164)

We had at that 1960 meeting a lively discussion of the special role of the axiom of choice in the foundations of mathematics, but I can no longer remember any of the particular remarks either one of us made.

The details are gone from our several meetings over two decades, but even now, more than twenty years after de Finetti's death, I remember vividly one lasting impression. Of the many mathematicians and statisticians I have known over my long life, Bruno de Finetti was the most deeply philosophical.

References

Bridgman, P.W. (1927). *The Logic of Modern Physics*. New York: Macmillan.

de Finetti, B. (1931). "Sul Significato Soggettivo della Probabilità". *Fundamenta Mathematicae* XVII, pp. 298-329. English edition: "On the Subjective Meaning of Probability". English translation in B. de Finetti, *Probabilità e Induzione* (*Induction and Probability*), ed. by P. Monari and D. Cocchi. Bologna: CLUEB, 1992, pp. 291-321.

de Finetti, B. (1937/1964). "La prévision: ses lois logiques, ses sources subjectives". *Annales de l'Institut Henri Poincaré* 7, pp. 1-68. Translated in *Studies in Subjective Probability*, ed. by H.E. Kyburg, Jr. and H.E. Smokler. New York: Wiley, 1964, pp. 93-158.

de Finetti, B. (1961). "Dans quel sens la théorie de la decision est-elle et doit-elle être 'normative'?". In *La Décision.* Paris: Éditions Du Centre National De La Recherche Scientifique, pp. 159-169.

de Finetti, B. (1975). *Theory of Probability*, vol. 2. Translated by A. Machi and A. Smith. New York: Wiley.

de Finetti, B. (1976). "Probability: Beware of Falsifications!". *Scientia* 70, pp. 299-300.

Galavotti, M.C. (1989). "Anti-realism in the Philosophy of Probability: Bruno de Finetti's Subjectivism". *Erkenntnis* 31, pp. 239-261.

Gallavotti, G. and Ornstein, D.S. (1974). "Billiards and Bernoulli Schemes". *Comm. Math. Phys*. 38, pp. 83-101.

Good, I.J. (1962). "Subjective Probability as the Measure of a Non-measurable Set". In *Logic, Methodology, and Philosophy of Science: Proceedings of the 1960 International Congress*, ed. by E. Nagel, P. Suppes and A. Tarski. Stanford: Stanford University Press, pp. 319-329.

Kolmogorov, A.N. (1958). "A New Metric Invariant of Transient Dynamical Systems and Automorphisms in Lebesgue Spaces". *Dokl. Akad. Nauk. SSSR* 119, pp. 861-864. (Russian) MR 21 #2035a.

Kolmogorov, A.N. (1959). "Entropy per Unit Time as a Metric Invariant of Automorphism. *Dokl. Akad. Nauk SSSR* 124, pp. 754-755. (Russian) MR 21 #2035b.

Kraft, C.H., Pratt, J.W. and Seidenberg, A. (1959). "Intuitive Probability on Finite Sets". *Annals of Math. Statistics* 30, pp. 408-419.

Ornstein, D.S. (1970). "Bernoulli Shifts with the Same Entropy are Isomorphic". *Advances in Mathematics* 4, pp. 337-352.

Ornstein, D.S. and Weiss, B. (1991). "Statistical Properties of Chaotic Systems". *Bull. Am. Math. Soc. (New Series)* 24, pp. 11-116.

Peirce, C. (1892). "The Doctrine of Necessity Examined". In *Philosophical Writings of Peirce*, ed. by J. Buchler. New York: Dover, 1955, pp. 324-338.

Scott, D. and Suppes, P. (1958). "Foundational Aspects of Theories of Measurement". *Journal of Symbolic Logic* 23, pp. 113-128.

Sinai, Ya. G. (1959). "On the Notion of Entropy of a Dynamical System". *Dokl. Akad. Nauk SSSR* 124, pp. 768-771.

Sinai, Ya. G. (1970). "Dynamical Systems with Elastic Reflections: Ergodic Properties of Displacing Billiards". *Dokl. Akad. Nauk SSSR* 124, pp. 137-189.

Suppes, P. (1969). *Studies in the methodology and foundations of science: Selected papers from 1951-1969*. Dordrecht: Reidel.

Suppes, P. (1974). "The Measurement of Belief". *Journal of the Royal Statistical Society (Series B)* 36, pp. 160-191.

Suppes, P. (2006). "Transitive Indistinguishability and Approximate Measurement with Standard Finite Ratio-scale Representations". *Journal of Mathematical Psychology* 50, pp. 329-336.

Suppes, P. and de Barros, J.A. (1996). "Photons, Billiards and Chaos". In *Law and Prediction in the Light of Chaos Research, Lecture Notes in Physics*, ed. by P. Weingartner and G. Schurz. Berlin: Springer-Verlag, pp. 189-201.

Suppes, P. and Zanotti, M. (1976). "Necessary and Sufficient Conditions for Existence of a Unique Measure Strictly Agreeing with a Qualitative Probability Ordering". *Journal of Philosophical Logic* 5, pp. 431-438.

Suppes, P. and Zanotti, M. (1981). "When are Probabilistic Explanations Possible?". *Synthese* 48, pp. 191-199.

Vailati, G. (1909). "The Origins and Fundamental Idea of Pragmatism". *Rivista di Psicologia Applicata* 1. Reprinted in *Scritti di G. Vailati*. Leipzig: J.A. Barth, 1911, pp. 36-48.