Response to Nick Bingham's comments on an early draft of our book *Probability and Finance: It's Only a Game!*

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In September 1998, the Danish Network on the History and Philosophy of Mathematics (http://mmf.ruc.dk/mathnet/) hosted a conference on the history and philosophy of probability at Roskilde University. Both of us were invited to speak at this conference, and the organizers even extended their hospitality to enable the two of us to take a few more days in Copenhagen after the conference to continue the collaboration that led to our book, *Probability and Finance: It's Only a Game* (Wiley, 2001).

Glenn Shafer's main contribution to the conference was a talk based on a draft chapter for the book, containing material that was later incorporated into Chapters 1 and 2. Another participant at the meeting, Nick Bingham, Professor of Statistics at Brunel University, was very engaged by Shafer's talk and responded to it energetically.

Most of the presentations at the conference were subsequently published as articles in a volume edited by the organizers: *Probability Theory: Philosophy, Recent History and Relations to Science*. This collection includes articles by both of us, but unfortunately it does not include an article based on the talk by Shafer that elicited Bingham's comments. We have two excuses for this. At first, we worried that inclusion of the chapter might compromise our effort to attract a publisher for the book. Then, after we obtained a contract with a publisher, we realized that we wanted to revise the chapter substantially (in part because of Bingham's comments), and this left us unwilling to publish an account that we felt was inadequate and rapidly evolving. We are embarrassed by this outcome, because our hosts very much deserved an article on this material from us after their magnificent Danish hospitality.

Even though we failed to provide an article following up on Shafer's talk, Bingham's article in *Probability Theory: Philosophy, Recent History and Relations to Science* includes a commentary on that talk, which is informed not only by our draft chapter but also by an early draft for the whole book, which we were circulating to publishers at the time and also shared with him. Bingham's commentary is balanced and on the whole quite complimentary, but the fact that it is directed towards a version of our book that was never published may create some confusion. To compound the confusion, the introduction to Bingham's article states that one of his purposes is

> ... to contribute to the discussion of the novel game-theoretic approach presented at the conference by Glenn Shafer and Volodya Vovk (see Vovk's contribution to this volume).

The reference to Vovk's article (which may have been inserted by the editors in desperation, for lack of any other reference) is misleading, because this article is concerned with a different topic—Kolmogorov's complexity conception of probability.

We are honored by Professor Bingham's having included comments on our work in his article, which was mainly concerned with the history and philosophy of the measure-theoretic foundation for probability. We hope that this response to his comments will allay the confusion arising from our failure to publish their actual target.

We begin by reproducing, with permission from Professor Bingham and Kluwer, the section of Professor Bingham's article that concerns our work.

1 "Critique of the Shafer-Vovk Approach Via Game Theory", by N. H. Bingham[1]

In the Shafer-Vovk approach (SV below for brevity), one visualizes a game between the statistician and nature (not in itself innovative, of course). Their approach to the strong law of large numbers may be summarised by saying that the statistician can force an outcome in which *either* the conclusion of the strong law of large numbers holds—convergence of the relevant sample mean to its appropriate expectation—*or* the statistician achieves a cumulative gain in the game which diverges to infinity. As one interprets this latter outcome as not to be observed in practice, one obtains an interpretation of the strong law of large numbers as representing what will actually happen, *and this without the need for an exceptional set of measure zero.* Not only the strong limit theorems mentioned above, but also their weak counterparts—the weak law of large numbers and the central limit theorem—find their place in this theory. There is some discussion of stochastic processes such as diffusions in this framework, using the machinery of non-standard analysis.

Shafer and Vovk apply their theory also to mathematical finance. In particular, they obtain the Black-Scholes partial differential equation.

Any theory that can produce versions of the classic limit theorems of probability theory on the one hand, and of the Black-Scholes theory of mathematical finance on the other, deserves to be taken seriously. The Shafer-Vovk theory shares the directness of approach of the Kolmogorov second theory,[2] and broadens our range of choices in approaching the treatment of probability. It is a pleasure to salute an impressively original approach, for which the authors deserve our thanks.

My reservations—which are considerable—lie not so much with the Shafer-Vovk theory itself, but with the authors' claims for it and against its measure-theoretic predecessor.

1. The authors claim—repeatedly and with some emphasis—that measure-

[1]This is Section 13 (pp. 41–43) of "Probability and Statistics: Some Thoughts at the Turn of the Millennium", by N. H. Bingham, pp. 15–49 of *Probability Theory: Philosophy, Recent History and Relations to Science*, Vincent F. Hendricks, Stig Andur Pedersen, and Klaus Frovin Jørgensen, editors. Volume 297 of the Synthese Library. Kluwer, Dordrecht, 2001.

[2]Note by Shafer and Vovk: Here Professor Bingham is referring to Kolmogorov's theory of algorithmic complexity, which he discusses in the preceding section of the article, Section 12.

theoretic probability is complicated, unnatural, cumbersome, and even ugly. The obvious response is that measure theory is a necessary part of the equipment of a fully-trained professional mathematician, if only because integration concepts less advanced than the measure-theoretic Lebesgue integral, such as the Riemann integral, may be easier to learn but are harder to handle and manipulate. One thinks of the ease with which Lebesgue's theorems of monotone or dominated convergence handle questions of interchange of limit and integral, for example. One thinks also of Wiener's use of the Lebesgue theory of integration to handle Fourier integrals—his work on generalised harmonic analysis (1930) and Tauberian theorems (1932), and his books on Fourier integrals (Wiener (1933), Paley and Wiener (1934))—which convinced a still sceptical mathematical public that one had to make the investment of learning measure theory, if only to do integration adequately. This remains as true in the early 2000s as it was in the early 1930s. To anyone knowing measure theory, seeing measure and integral put to use as probability and expectation is natural and painless.

2. The achievements of the measure-theoretic approach to probability—in pure probability, applied probability, statistics, and indeed pure mathematics—remain as impressive an edifice after the Shafer-Vovk attack as they were before. Being several decades more developed, measure-theoretic probability has been able to tackle a vastly broader range of problems, theoretical and practical, than the Shafer-Vovk game-theoretic approach. This is not said in criticism of the SV theory, for which it is still early days yet. It is said in criticism of the attacks by Shafer and Vovk on the measure-theoretic theory. They say of their own theory 'We can do everything that the measure theorists can do'. The vast range of applications that the measure-theoretic approach has so successfully handled to date—of which it was our pleasant task to discuss a small range above—provides a challenge which many years of work may enable SV to rise to. At the moment, however, the degrees of maturity of the two approaches are so different that a fair comparison is premature; meanwhile, a confrontational challenge of this kind between SV and measure-theoretic probability is rash verging upon suicidal. Such would indeed be typically the case for any new theory facing an established one. In any case, for myself I prefer a more inclusive and less confrontational approach.

2 Our Response

We worked on our book for nearly two and a half more years after our discussions with Professor Bingham at the Roskilde conference, and we tried, as we revised its early chapters, to follow his advice that we present our challenge to the measure-theoretic foundation for probability in a less confrontational way. Whether we succeeded in allaying his concerns we must leave for him to say.

On the whole, we agree with what Bingham has to say. Here are some salient points of agreement:

- We are very pleased that Bingham thinks the game-theoretic approach has already earned a place as one of a "range of choices in approaching the treatment of probability", and we agree with the implications of this way of putting the matter: Probability begins as a circle of ideas outside of pure mathematics, and there will always be choices in how to treat it mathematically.

- We agree that measure theory is a necessary part of the equipment of a fully trained mathematician. We also agree that anyone who already has this equipment will find it convenient and productive, on many occasions, to use measure and integral as probability and expectation.

- We agree that it is unnecessarily and unproductively confrontational for us to say, "We can do everything the measure theorists can do." No doubt Shafer did say this in his talk in Roskilde in 1998, but the formulations now in our book are more concrete and possibly less provocative.

Those who read the first few pages of our book[3] will see, however, that we have persisted in the thesis that our game-theoretic framework is an advance over the measure-theoretic framework as a mathematical and philosophical *foundation* for probability. What do we mean by this?

First, why do we say that the game-theoretic framework is better than the measure-theoretic framework as a *mathematical foundation* for probability? We have several reasons:

1. The game-theoretic versions of the classical limit theorems are more powerful, as it turns out, than the measure-theoretic versions of these

[3]The first chapter can be downloaded free from `http://www.cs.rhul.ac.uk/~vovk/book/`.

theorems. As we show in Section 8.1 of our book, the measure-theoretic versions can be deduced easily from the game-theoretic versions, but the opposite is not true. This is because the game-theoretic concept of a martingale is more general than the measure-theoretic concept of a martingale. In our game-theoretic framework, a martingale is simply the capital process of a player in a certain type of game, and this captures the essence of the intuitive concept of a martingale, without complications that belong more to the theory of integration. So to the extent that martingales are central to probability, our game-theoretic perspective is fundamental.

2. The game-theoretic framework does not need to be generalized, as the measure-theoretic framework does, to handle quantum mechanics or other topics where probabilities are determined only in the course of a process, partly as a result of events and decisions that are not themselves subject to a probabilistic theory. See Section 8.4 of our book.

3. Although the game-theoretic framework seems strange and perhaps therefore difficult for those already well-trained in measure theory, it is much more elementary, and we predict that most mathematicians will eventually agree that it is simpler and in this respect more elegant.

Measure-theoretic probability combines intuitions about gambling with an apparatus designed for the theory of integration. It is this mixture, not the separate parts, that can sometimes appear "complicated, unnatural, cumbersome, and even ugly". From a purely aesthetic viewpoint, the game-theoretic framework offers the advantage that the theory of integration can be set aside until it is really needed, and when it is needed it certainly will not appear as unnatural or ugly.

Formally, any probability measure determines a one-round probability game, in which a player is offered the opportunity to buy any random variable that has an expected value with respect to the measure (cf. pp. 180–182 of our book). So there is a trivial sense in which we can indeed "do everything that the measure theorists can do". But this is hardly interesting. The interesting part comes when the probability game has many rounds and we obtain strong results, such as the classical limit theorems, even though the player's opportunities to gamble are less extensive than those that would be given by probability measures.

Our touting of the game-theoretic framework must also be tempered by a recognition that it adds little or nothing to many applications of measure theory that lie at a great distance from the gambling ideas with which probability began, including very important applications in physics. We would not dispute, for example, the proposition that ergodic theory is essentially measure-theoretic and not game-theoretic.

Why do we say that the game-theoretic framework is better as a *philosophical foundation* for probability? Our thinking on this issue is outlined in Section 1.5 of our book. One of our main points is that probability games narrow the difference between the subjective and objective interpretations of probability. These interpretations both arise when it is hypothesized (this is *Cournot's principle* or the *efficient market hypothesis*) that the player who bets on each round cannot multiply a limited initial stake substantially without risking bankruptcy. If this hypothesis is offered merely as the opinion of the person setting the odds, then we may say that the probabilities (or upper and lower probabilities) that can be derived from the odds are subjective. If the hypothesis is offered as a hypothesis about the world, we may say that the probabilities are objective. Because the available bets may be limited (they may fall short of defining a full probability measure for future events), the bare hypothesis that a bettor cannot beat them can be more plausible than the notion, often associated with applications of measure-theoretic probability, that the phenomena being observed are somehow produced by a "stochastic mechanism" following a certain probability measure.

The game-theoretic framework also helps us understand applications of probability where Cournot's principle is *not* adopted. In this case, probabilities determined by the game cannot be interpreted as empirical predictions, but prices determined by the game can still be interpreted in terms of arbitrage. This interpretation is already well established in the theory of option pricing, and the second half of our book is devoted to showing how it can be freed from any lingering connection with stochastic assumptions.