Grades of Inductive Skepticism

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There is not a unique inductive skeptical position; there are grades of inductive skepticism. There is nothing much to say about complete skepticism, but some more restricted skeptical positions may be profitably analyzed.

The classic modern statement of inductive skepticism comes from David Hume, although he reminds us of its ancient sources. Inductive reasoning is not justified by relations of ideas: "That the sun will *not* rise tomorrow is no less intelligible a proposition than that it will rise. We should in vain, therefore, attempt to demonstrate its falsehood" (Hume 1748/1777, Sec. IV, Pt. I, 21). To try to justify it inductively is to beg the question: "It is impossible, therefore, that any arguments from experience can prove this resemblance of the past to the future, since all these arguments are founded on the supposition of that resemblance" (Sec. IV, Pt. II, 32).

One might simply read Hume and give up. This is the position taken by Karl Popper in the early twentieth century. "Yet if we want to find a way of justifying inductive inferences, we must first try to establish a *principle of induction*. . . . Now this principle of induction cannot be a purely logical truth . . . if we try to regard its truth as known from experience, then the very same problems which occasioned its introduction will arise all over again." Hume, according to Popper, has shown the impossibility of inductive logic: "My own view is that the various difficulties of inductive logic here sketched are insurmountable" (Popper 1934/1968, 29), and Popper concludes that the logic of scientific inquiry must be purely deductive.

Received December 2013; revised February 2014.

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†This article was given as the Wesley Salmon lecture at the University of Pittsburgh, October 2013. Thanks to Brad Armendt, Simon Huttegger, Sandy Zabell, Kevin Zollman, and two anonymous referees for helpful comments.

Philosophy of Science, 81 (July 2014) pp. 303–312. 0031-8248/2014/8103-0006$10.00
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https://doi.org/10.1086/676637 Published online by Cambridge University Press

One might well ask why Popper did not think to apply the tropes of Agrippa the skeptic to deductive reasoning, just as Hume had applied them to inductive reasoning.[1] There is an infinite regress—mathematics is justified by set theory, whose consistency is proved by stronger set theory, and so on—or, regarding mathematics as a whole, a circularity. Or why not ask why one should accept any argument at all? Trying to answer a thoroughgoing skeptic is a fool's game.

But it is possible, and sometimes quite reasonable, to be skeptical about some things but not others. There are *grades of inductive skepticism*, which differ in what the skeptic calls into question and what he is willing to accept. For each grade, a discussion of whether such a skeptic's doubts are justified in his own terms might actually be worthwhile. Here, I assume that my skeptics are probabilistically coherent.[2]

Hume floated a global skepticism, but there is a much more focused skeptical challenge that he might well have raised, were he a better mathematician and more in touch with the current ferment in probability theory. The challenge was raised retrospectively by Richard Price. Pascal and Fermat had their correspondence (1654), which rapidly became known to the intellectual elite of Europe.[3] Christaan Huygens had written a monograph on probability (1656), which was translated into Latin (1656/1657) and then twice into English (1656/1692, 1656/1714). Jacob Bernoulli's *Ars Conjectandi*, including the text of Huygens, had been published posthumously (1713/2005). The first edition of de Moivre's *The Doctrine of Chances* had been published (1718). There was intense interest in practical applications of the new theory of probability. In particular, there was interest in arguing not only from known chances in gaming to probabilities of outcomes but also in the inference from data to chances.

Bernoulli claimed that he had cracked this problem: "what you cannot deduce a priori, you can at least deduce a posteriori—that is, you will be able to make a deduction from the many observed outcomes of similar events. For it may be presumed that every single thing is able to happen and not to happen in as many cases as it was previously observed to have happened or not to have happened in like circumstances" (1713/2005, chap. 4; de Moivre made similar claims in the second and third editions of

1. Agrippa applied them to all kinds of reasoning. Thus, in the account of Diogenes: "And in order that we may know that an argument constitutes a demonstration, we require a criterion; but again, in order that we may know that it is a criterion we require a demonstration" (1925), bk. IX [90].

2. Where I need it, this includes sigma coherence. Coherence has many virtues, which I will not detail here. See, e.g., Kolmogorov (1933/1950), de Finetti (1937/1980, 1972, 1974, 1975), and Joyce (1998).

3. Largely through Father Mersenne's Académie Parisienne.

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The Doctrine of Chances). Bernoulli had proved that with enough trials it would become "morally certain" that the frequency would be approximately equal to the true chance. If x is approximately equal to y, then y is approximately equal to x. So, after a large number of trials, we can take the true chances to be approximately equal to the observed frequencies.

This informal argument gains an air of plausibility by concealing difficulties behind a cloak of moral certainty and approximate equality. It is no proof, as is apparent if the argument is stated carefully. As Richard Price put it:

> Mr. De Moivre . . . has, after Bernoulli, and to a greater degree of exactness, given rules to find the probability there is, that if a very great number of trials be made concerning any event, the proportion of the number of times it will happen to the number of times it will fail, in those trials, should differ less by small assigned limits from the proportion of the probability of its happening to the probability of its failing in one single trial. But I know of no person who has shown how to deduce the solution to the converse problem to this; namely, "the number of times an unknown event has happened and failed being given, to find the chance that the probability of its happening should lie somewhere between two named degrees of probability." (1763, 372–73)

In the section of the *Inquiry* devoted to probability, David Hume wrote:

> But where different effects have been found to follow from causes, which are to *appearance* exactly similar, all these various effects must occur to the mind in transferring the past to the future, and enter into our consideration, when we determine the probability of the event. Though we give the preference to that which has been found most usual, and believe that this effect will exist, we must not overlook the other effects, but must assign to each of them a particular weight and authority, in proportion as we have found it to be more or less frequent. . . . Let any one try to account for this operation of the mind upon any of the received systems of philosophy, and he will be sensible of the difficulty. For my part, I shall think it sufficient, if the present hints excite the curiosity of philosophers, and make them sensible how defective all common theories are in treating of such curious and such sublime subjects. (1748/1777, Sec. VI, 47)

Neither Bernoulli nor de Moivre had given an answer to Hume.

An answer was supplied by Thomas Bayes, in an essay that Price is introducing. It was written around 1749 (see Zabell 1989) but only published posthumously (Bayes 1763) in the *Philosophical Transactions of the Royal*

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Society. Bayes's target, at least as seen by Price, was general Humean skepticism about inductive inference.[4] This is most evident in the title page affixed to reprints of Bayes's essay, which reads: "A Method of Calculating the Exact Probability of All Conclusions based on Induction" (Stigler 2013, 283). Bayes's goal was to calculate the probability that the true chance of a dichotomous event fell within a certain interval, given frequencies in a finite number of trials. For the question to make sense at all, chance must be a random variable—there must be a probability distribution over the possible chances[5]—something missing in Bernoulli and de Moivre. This raises the question of what to take as the probability distribution over the chances before any trials—the question of the proper quantification of ignorance.

Bayes assumed the uniform prior, in which intervals of equal length are given equal probability of containing the true chance. On this basis, he shows by a clever geometrical argument that on this assumption the probability of m successes in n trials is $1/(n + 1)$, for any m.[6] In a scholium Bayes remarks that this result itself might be taken as a proper quantification of ignorance—each number of successes has equal probability. This is ignorance about observables—and in particular about frequencies.[7]

Bayes's analysis was extended (perhaps independently) by Laplace in a remarkable essay of 1774.[8] Assuming the uniform prior, Laplace proves his famous rule of succession. Given p successes in $p + q$ trials, the probability of a success in the next trial is

$$\frac{p + 1}{p + q + 2}.$$

More generally, he considered the predictive distribution, for m successes in $m + n$ additional trials given the evidence of p successes in $p + q$ trials. Laplace shows that if the datum, $p + q$, is large and $m + n$ is small, the result is close to taking the observed frequency as giving the chances. But he also feels bound to point out that this is not the case if the number of

4. See also Gillies (1987) and Zabell (1997). Price was a friend of both Bayes and Hume.

5. Bayes's terminology reverses the modern one, which I use.

6. This is Bayes's "billiard table" argument, which uses geometry and symmetry to find the value of an integral. See Stigler (1990) for a discussion of Bayes's use of a physical example and for further references.

7. This implies the uniform prior, although Bayes did not have a proof of this.

8. The essay was published when Laplace was 25. Quotes are from the translation by Stigler (Laplace 1774/1986).

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trials predicted is also large: "and it seems to me essential to note this."[9] What is more, Laplace showed what is now called Bayesian consistency:[10] "One can suppose that the numbers p and q are so large that it becomes as close to certainty as one wishes that the ratio of the number of white tickets to the total number of tickets contained in the urn is included between the two limits p/(p+q−w) and p/(p+q+w). w can be supposed less than any given quantity" (Laplace 1774/1986, 366). The Bayes-Laplace inference converges to the true chances.

Given their assumptions, Bayes and Laplace show that Bernoulli's conclusion was right. We can infer the approximate chances a posteriori. And, in this setting, they do give an answer to Hume. They show when, and in what sense, it is rational to believe that the future is like the past.

A more radical skeptic will not be at a loss to find assumptions to question in the Bayes-Laplace analysis. Bayes himself found it necessary to buttress the assumption of the uniform prior with the argument of the scholium. Throughout the history of Bayesian inference, some have thought it necessary to defend a unique quantification of ignorance.

I think that this is a mistake. Ignorance is the opposite of knowledge. So an ignorance prior should be a prior that does not presume knowledge. I might know the composition of the urn or the bias of the coin exactly. I might know less and still know something. I might know that the urn contains more black tickets than white or that the bias toward heads is greater than 1/2. But suppose that I know no such thing.

Then my ignorance prior should put some positive probability on the true chance being in any open interval between 0 and 1. Specification of an ignorance prior is not unique. There are lots of them. If you do not like calling these ignorance priors on the ground that they may be sharply peaked, call them *nondogmatic* priors or *skeptical* priors, because these priors are quite in the spirit of ancient skepticism.

9. Compare Hans Reichenbach. He defends a rule that guesses, or "posits," the sample frequencies to be the limiting frequencies. Posits are treated as true until revised. Treating them as true means treating them as if they were a known chance. "We posit h^n (i.e. the sample frequency) as the value of the limit, i.e. we wager on h^n just as we wager on the side of a die" (Reichenbach 1938, 352). Taken literally, this is an intemperate remark. If the datum, $p + q$, is small and the number of future trials, $m + n$, is large, Reichenbach would be led to bizarre betting behavior. To make the point in the extreme, consider bets on limiting relative frequencies. After six heads in 10 tosses, Reichenbach would be led to bet his fortune against a penny that the limiting relative frequency would be .6. After one more trial, he would be sure of another limiting relative frequency. The same sort of thing happens, only to a lesser extent, if betting is on a large number of future trials.

10. In fact, he proves something stronger, the Bernstein-von Mises phenomenon. The posterior is asymptotically normal.

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What Laplace showed for the uniform prior holds for all skeptical priors. Given enough experience, priors lead a Bayesian to predict tomorrow using something close to the observed frequency. With chance one, priors lead the Bayesian to converge to the true chances.[11] Skeptical priors defeat skepticism.

What about the dogmatist? Suppose, for example, that this person is convinced that the bias toward heads is greater than 1/2 and has a uniform prior on it being between 1/2 and 1. If the true bias is 1/4, he will never learn it. Nevertheless, he believes that he will learn the true chances, because he is sure that they are between 1/2 and 1. You may not believe that he will learn the true chances, but he does. This holds quite generally (Doob 1948). With his degree-of-belief one, he will converge to the true chances. Neither the dogmatic prior nor the open-minded prior is consistent with inductive skepticism

* * *

The foregoing all takes place within a specific chance model. Perhaps, with Hume, we may believe that "there is no such thing as chance in the world" (Hume 1748/1777, Sec. VI, 46).

Suppose there is a potentially infinite sequence of yes-no events. And suppose that you are a frequentist in the following weak sense: for you, the only thing that matters for the probability of a finite outcome sequence of a given length is the relative frequency of successes in that sequence. That is to say that for you, two sequences of the same length having the same relative frequencies have the same probability. Then Bruno de Finetti proves that you behave like Bayes, with his chance model and some (not necessarily flat) prior. Furthermore, the prior is uniquely determined by your degrees of belief over the outcome sequences.[12]

De Finetti, like Hume, believes that there is no such thing as chance in the world and shows that we can have the virtues of Bayes's analysis without the baggage. If you are skeptical about the existence of chances, the chance model, and the prior over the chances, de Finetti shows how to get them all from your degrees of belief, provided that they satisfy the foregoing condition of exchangeability. Furthermore, you must believe with probability 1 that a limiting relative frequency exists and that with repeated

11. In the weak-star topology, a sequence of probability measures, P_n, converges weak star to P if for all bounded continuous functions, f, the associated expectations converge: $E_n \geq E$. This is not to say that these results hold generally. With an infinite number of categories, things are more complicated; see Diaconis and Freedman (1988).

12. De Finetti's theorem has been proved in a considerably more general form; see Hewitt and Savage (1955).

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experience you will converge to it.[13] If your degrees of belief are exchangeable, you cannot be an inductive skeptic.

What if your degrees of belief are not exchangeable? Short of exchangeability there may be other symmetries that have inductive consequences. De Finetti himself initiated this line of thought in 1938 (de Finetti 1938/1980), and there are many subsequent developments (see Diaconis and Freedman 1980b). We consider the problem at a very general, abstract level.

We suppose that you have a measurable space that encapsulates the problem that you are thinking about.[14] You bring to this problem your degrees of belief: a probability measure on the measurable sets that is invariant under some measurable transformation T (or group of transformations) of the space into itself. Transformation T represents your conception of a repetition of an experiment.[15] Invariance means that the transformation (or group of transformations) leaves the probabilistic structure unchanged.

For example, suppose the points in the probability space are doubly infinite sequences of experimental outcomes, indexed by discrete time.[16] If your probability is invariant under the shift transformation, that means that the probabilistic structure is not affected by the passage of time: that is to say that the stochastic process is stationary.

Fixing the transformation, the set of invariant probability measures is convex. Your degrees of belief are one member of this set. The extreme points of this set are probabilities that are, in a certain sense, resilient under conditioning on the invariant sets.[17] In these extreme probabilities, invariant sets have probability 1 or 0. (The measure cannot be decomposed into two or more invariant measures by conditioning on invariant sets.) These are the ergodic probability measures.

Starting at some point, x, in your probability space, you contemplate a series of experiments, x, Tx, TTx, . . . T$^n x$. You keep track of the relative frequency of the points being in some measurable set, A.[18] Given the foregoing, you believe with probability 1 that the limiting relative frequency

13. You may be skeptical about infinite sequences being part of the world, as was de Finetti. If so, consider finite exchangeable sequences that can be extended to longer sequences that remain exchangeable; see Diaconis and Freedman (1980a). On de Finetti's finitist views of his theorem, see Cifarelli and Regazzini (1996) and Zabell (2009).

14. The measurable space is $\langle \mathrm{W}, \mathrm{F} \rangle$ where W is a set of points and F is a sigma algebra of subsets of W.

15. As suggested in Billingsley (1965).

16. That is, from any point extending infinitely in the past and in the future.

17. A set, A, is invariant under transformation T, if its probability is equal to the probability of its inverse image under T: $P(\mathrm{A}) = P(\mathrm{T}^{-1}(\mathrm{A}))$.

18. More generally, of the average values of a measurable function.

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will exist.[19] In this sense, you cannot be an inductive skeptic. You cannot be a skeptic in the sense of Reichenbach. This is a consequence of invariance.[20]

The limiting relative frequency of A is a random variable. Your expectation of this limiting relative frequency is your probability of A. In the special case in which your degrees of belief are ergodic, you are sure that your probability of A is equal to the limiting relative frequency. This is Birkhoff's ergodic theorem.

The foregoing has all been at such an abstract level that it is impossible to say much about the extremal ergodic measures. Further specification of the transformations under which your probabilities are invariant can give more information. De Finetti's theorem is a special case with the invariant measures being the exchangeable ones and the ergodic ones being independent and identically distributed. A version of de Finetti's theorem for Markov chains, proved by David Freedman, is another (Freedman 1962). As emphasized at the onset, your probabilities and your conception of repetition of the same experiment are up to you. You and I may differ. We may be skeptical about each other but not about ourselves

So far, the envisioned learning experiences have been modeled as conditioning on the evidence. A more radical skeptic may well call this into question. This is the stance taken in Richard Jeffrey's Radical Probabilism (Jeffrey 1965, 1968). Must a radical probabilist perforce be a radical inductive skeptic?

One learns through some sort of black-box interaction that updates one's probabilities. Then we need some way of distinguishing interactions that are viewed as learning experiences and those that are viewed as mind-worms, brainwashing, drug-induced hallucinations, the Sirens singing to Ulysses, and so on. A plausible candidate is diachronic coherence (Goldstein 1983; van Fraassen 1984).

If one contemplates a sequence of such experiences stretching off into the future and regards them as learning experiences, coherence requires that they form a martingale in your degrees of belief, as I have previously pointed out (Skyrms 1990, 2006). That means that the martingale convergence theorem comes into play. As an added twist, one who is skeptical of countable additivity need not worry. This can all be done with finitely additive martingales (Zabell 2002). Even in this austere setting, one cannot be a complete inductive skeptic.

Hume remarks that it is psychologically impossible to be a consistent skeptic: "since reason is incapable of dispelling these clouds, nature herself

19. That is, for almost every point in the space—with "almost every" determined by your probabilities—the limiting relative frequencies exist.

20. If, in addition, your probabilities are ergodic, you believe that the relative frequency of being in a set will converge to your probability of that set.

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suffices to that purpose" (Hume 1896, Bk. I, Pt. IV, Sec. VII). One is not logically compelled to believe in a prospective sequence of learning experiments. One may not be coherent or believe that one will remain coherent in the future. One need not believe that there will be a future. Absolute skepticism is unanswerable.

But short of absolute skepticism, there are various grades of inductive skepticism, differing in what the skeptic brings to the table and what he calls into doubt. Some kinds of skeptics may call into question things to which they are implicitly committed. In such a case, reason is capable of dispelling doubts. It is remarkable the extent to which the logic of coherent belief itself constrains inductive skepticism.

REFERENCES

Bayes, Thomas. 1763. "An Essay towards Solving a Problem in the *Doctrine of Chances*." *Proceedings of the Royal Society of London* 53:376–418.

Bernoulli, Jacob. 1713/2005. *Ars Conjectandi*. Basil: Thurnisiorum. Trans. Edith Dudley Sylla as *The Art of Conjecturing*. Baltimore: Johns Hopkins University Press.

Billingsley, Patrick. 1965. *Ergodic Theory and Information*. New York: Wiley.

Cifarelli, Donato, and Eugenio Regazzini. 1996. "De Finetti's Contribution to Probability and Statistics." *Statistical Science* 11:2253–82.

de Finetti, Bruno. 1937/1980. "La Prevision: Ses lois logiques, ses sources subjectives." *Annales de l'Institut Henri Poincare* 1:1–68. Trans. as "Foresight: Its Logical Laws, Its Subjective Sources." In *Studies in Subjective Probability*, ed. Henry Kyburg Jr. and Howard Smokler. Huntington, NY: Kreiger.

———. 1938/1980. *Sur la condition d'équivalence partielle*. Actualités Scientifiques et Industrialles 739. Paris: Hermann & Cie. Trans. Paul Benacerraf and Richard Jeffrey as "On the Condition of Partial Exchangeability." In *Studies in Inductive Logic and Probability*, vol. 2, ed. Richard Jeffrey, 193–205. Berkeley: University of California Press.

———. 1972. *Probability, Induction and Statistics*. New York: Wiley.

———. 1974. *Theory of Probability*. Vol. 1. Trans. Antonio Machi and Adrian Smith. New York: Wiley.

———. 1975. *Theory of Probability*. Vol. 2. Trans. Antonio Machi and Adrian Smith. New York: Wiley.

de Moivre, Abraham. 1718. *The Doctrine of Chances; or, A Method of Calculating the Probability of Events in Play*. London: Pearson.

Diaconis, Persi, and David Freedman. 1980a. "De Finetti's Generalizations of Exchangeability." In *Studies in Inductive Logic and Probability*, vol. 2, ed. Richard Jeffrey, 232–49. Berkeley: University of California Press.

———. 1980b. "Finite Exchangeable Sequences." *Annals of Probability* 8:745–64.

———. 1988. "On the Consistency of Bayes Estimates." *Annals of Statistics* 14:1–67.

Diogenes, Laërtius. 1925. *Lives of the Eminent Philosophers*. Trans. Robert D. Hicks. Loeb Classical Library. Cambridge, MA: Harvard University Press.

Doob, Joseph L. 1948. "Application of the Theory of Martingales." In *Actes du Colloque International Le Calcul des Probabilités et ses applications*, 23–27. Paris: CNRS.

Freedman, David. 1962. "Mixtures of Markov Processes." *Annals of Mathematical Statistics* 33 (1): 114–18.

Gillies, Donald A. 1987. "Was Bayes a Bayesian?" *Historia Mathematica* 14:325–46.

Goldstein, Michael. 1983. "The Prevision of a Prevision." *Journal of the American Statistical Association* 78:817–19.

Hewitt, Edwin, and Leonard J. Savage. 1955. "Symmetric Measures on Cartesian Products." *Transactions of the American Mathematical Society* 80:470–501.

https://doi.org/10.1086/676637 Published online by Cambridge University Press

Hume, David 1748/1777. *An Inquiry Concerning Human Understanding*. London: Millar.
———. 1896. *A Treatise of Human Nature*. Ed. L. A. Selby-Bigge. Oxford: Clarendon.
Huygens, Christiaan. 1656/1657. *Van Rekeningh in Speelen van Geluck*. Trans. F. van Schooten as "Libellus de Ratiociniis in Ludo Aleae." In *Exercitationum Mathematicarum*, appendix. Leiden: Elsevirii.
———. 1656/1692. *Van Rekeningh in Speelen van Geluck*. Trans. John Arbuthnot as *Of the Laws of Chance*. London: Motte.
———. 1656/1714. *Van Rekeningh in Speelen van Geluck*. Trans. W. Browne as *The Value of All Chances in Games of Fortune; Cards, Dice Lotteries &c. Mathematically Demonstrated*. London: Keimer & Woodward.
Jeffrey, Richard. 1965. *The Logic of Decision*. New York: McGraw-Hill (2nd ed.; Chicago: University of Chicago Press, 1990).
———. 1968. "Probable Knowledge." In *The Problem of Inductive Logic*, ed. I. Lakatos, 166–80. Amsterdam: North-Holland.
Joyce, James M. 1998. "A Nonpragmatic Vindication of Probabilism." *Philosophy of Science* 65:575–603.
Kolmogorov, Andrey Nikolaevitch. 1933/1950. *Grundbegriffe der Wahrscheinlichkeitsrechnung*. Berlin: Springer. Trans. Nathan Morrison as *Foundations of the Theory of Probability*. New York: Chelsea.
Laplace, Pierre Simon. 1774/1986. "Mémoire sur la probabilité des causes par les événemens." *Savants étranges* 6:621–56. Trans. Stephen Stigler as "Memoir on the Probability of the Causes of Events." *Statistical Science* 1:359–78.
Popper, Karl. 1934/1968. *Logik der Forshung*. Vienna: Springer. Trans. as *The Logic of Scientific Discovery*. 2nd ed. New York: Harper.
Price, Richard. 1763. Preface to "An Essay towards Solving a Problem in the *Doctrine of Chances*." *Proceedings of the Royal Society of London* 53:370–76.
Reichenbach, Hans. 1938. *Experience and Prediction*. Chicago: University of Chicago Press.
Skyrms, Brian. 1990. *The Dynamics of Rational Deliberation*. Cambridge, MA: Harvard University Press.
———. 2006. "Diachronic Coherence and Radical Probabilism." *Philosophy of Science* 73:959–68.
Stigler, Stephen. 1990. *The History of Statistics*. Cambridge, MA: Belknap.
———. 2013. "The True Title of Bayes' Essay." *Statistical Science* 28 (3): 283–88.
van Fraassen, Bas. 1984. "Belief and the Will." *Journal of Philosophy* 81:235–56.
Zabell, Sandy L. 1989. "The Rule of Succession." *Erkenntnis* 31:283–321.
———. 1997. "The Continuum of Inductive Methods Revisited." In *The Cosmos of Science*, ed. J. Norton and J. Earman, 351–85. Pittsburgh: University of Pittsburgh Press.
———. 2002. "It All Adds Up: The Dynamic Coherence of Radical Probabilism." *Philosophy of Science* 69 (Proceedings): S98–S103.
———. 2009. "De Finetti, Chance and Quantum Physics." In *Bruno de Finetti: Radical Probabilist*, ed. Maria Carla Galavotti, 59–83. London: College.

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