From *Current Issues in the Philosophy of Science* (Proceedings of Section L of the American Association for the Advancement of Science, 1959), pp. 237–241. Edited by Herbert Feigl and Grover Maxwell, Minnesota Centre for Philosophy of Science, University of Minnesota. New York: Holt, Rinehart and Winston

Comments on Rescher's "On the Probability of Nonrecurring Events"

First of all, let me show where my sympathies lie by stating that I feel, perhaps even more strongly than Mr. Rescher, that frequency interpretations of probability have an uncommonly difficult time of it under any sort of critical analysis, and that I look with some sympathy upon Mr. Rescher's belief in a plurality of probability concepts. However, I have also become convinced that further insights into the nature of probability are going to come only at the price of meticulous attention to the fine logical details of probability statements, and here I fear that Mr. Rescher's desire to wrest a major conclusion from so necessarily brief an argument has led him to sprint right by those points which seem to me to constitute the real working front of inquiry in this area.

Consider, for example, Mr. Rescher's comparison of events which are "unique" with those which are not. Taken literally, this contrast is empty, for as Rescher himself recognizes, every event is unique, whether it be the assassination of Archduke Ferdinand or the sort that insurance companies reckon with so effectively. It turns out that what Rescher is after is whether or not the event can be "informatively" classified; but, unfortunately, he leaves distressingly vague the difference between events which can and those which cannot be so classified. For reasons which will appear shortly, I, for one, doubt that the distinction can be sustained. In fact, one might wonder whether there is complete consistency in Mr. Rescher's proposal that the probabilities of "unique" events are to be assessed by polling the experts, for how does an expert make such judgement except by using his experience to classify the event in question and drawing upon past knowledge about other similarly classifiable events?

Actually, it seems to me that Mr. Rescher has confounded two quite different issues in the theory of probability and that when these are separated, the place at which his problem embarrasses a frequency interpretation is not the one to which he has directed our attention. First of all, we must draw clearly the methodological distinction between the concept of probability which applies to propositions and the one which applies to predicates-that is, in Carnap's terms, the difference between the probability of an *event* on the one hand, and the probability of an event *type* on the other. In the latter case, we are asking about the probability of a certain property within some reference class, such as the probability of a forty-year old American housewife surviving her eightieth birthday, or the probability of a successful revolutionist himself being overthrown. In the case of *event* probability, on the other hand, we are concerned with the likelihood of a certain hypothesis, such as whether Mrs. Smith will survive her eightieth birthday, or whether Castro will be overthrown, relative to the available evidence.

Given this distinction, two questions now become particularly germane. The first is whether the *probability* of property P within class R can be identified with the statistical *frequency* of P in R. The second is whether the event probability that a given entity has property P can be analyzed as the event-type probability of P in an appropriate reference class *irrespective* of whatever interpretation may ultimately be given to event-type probability. Mr. Rescher's failure to distinguish these issues has led him to challenge the thesis that event probabilities are always derivable from event-type probabilities by drawing a dubious distinction between historically momentous events such as political coups and everyday trivia like the death of our neighbor, Mrs. Smith. Actually, what the sort of case he has in mind puts the screws on is the frequency interpretation of event-*type* probabilities.

As has been stressed repeatedly by such eminent practicing statisticians as R. A. Fisher, when we attempt to assign the probability that an entity has property P by assigning it to some reference class and judging the probability of P therein, the reference class must be the most restricted one indicated by the available information. For example, taking the probability of a 40-year old American housewife surviving her eightieth birthday as the probability that Mrs. Smith will survive her eightieth birthday is invalid if we also happen to know that Mrs. Smith is an inveterate jaywalker and a three-pack-a-day smoker. This means, however, that if we have a great deal of informationt about an entity x, *the reference class to which x must be assigned may be so restricted as to contain x as its only member*—and this can happen as easily when x is Mrs. Smith as when it is King Hussein or the Archduke Ferdinand. Now in such a case, of course, the *frequency* of P in this reference class is of no use in deciding the probability that x has P. But this in no way casts doubt that the probability that x has P, in light of the available evidence, is the same as the probability of event-type P in the appropriate reference class, so long as the latter probability is not construed as the frequency of P in this class.

In short, Mr. Rescher's real problem is not the probability of a "unique" event, but the probability of an event type within a reference class of only one member. But this is merely a special instance of event-type probabilities within reference classes containing only a finite number of members (including the frequently important case where the reference class contains *no* members—for example, when a certain vaccine is never used on human subjects because it has been concluded that the likelihood of fatality among humans so treated is prohibitively high), a problem which has already received a great deal of attention from probability theorists. It is well know that according to the standard probability calculus, any *probability* of property P is compatible with any *frequency* of P within a finite reference class, which is why few frequency theorists today would dare to identify probability with frequency in *finite* classes. But does this mean that the frequency theorist is cut off from any sort of frequency interpretation of probabilities within reference classes which are, in fact, finite? Of course not. All he has to do is to insist that event-type probabilities are *dispositional* frequencies, rather than actual ones. That is, he can assert that the probability of property P in class R is the limiting frequency that P *would* reach in R if R *were* to be developed into a denumerably infinite class. Of course, a question still remains as to how the probability of P in R can be assessed when R is very small, since in this case, a frequency count *cum* sampling theory will not help much; but this is only an instance of more general problems about the confirmation of counterfactual conditionals and is no more embarrassing to a dispositional frequency approach, whatever other objections this may sustain (and I assure you, there *are* other objections), than are the difficulties in actually computing probabilities under any alternative theory.

Let me close with a word (actually, several dozen words) about my strategy in this critique. When Mr. Rescher is given his chance to reply, I suspect that he will accuse me of completely overlooking his main point; for I have been concerned with what he has to say about the interpretation of probability statements, whereas in the course of his paper, Mr. Rescher becomes increasingly explicit that what he wants to talk about is how probability numbers get assigned, and *not* about their meaning. On this, I have two comments, one general and the other specific.

My general comment is that I certainly agree that analysis of how we *do* assign probability numbers is important, but why this is important is *because* it sheds light upon what probability statements signify. When assessing the merits of various techniques for assigning probability numbers, however—especially when evaluating new proposals such as the one here offered by Mr. Rescher—I do not see how either a method or a critique thereof could be anything but gratuitous unless we are given some indication of what the assigned probability numbers are supposed to be saying. In other words, even if we grant that probability concepts come in several meanings, it is still not the case that all techniques for generating probability statements are equally good (unless, of course, we stipulate that anyone who feels like introducing still another concept under the blanket title "probability" by introducing a new computational algorithm defining this concept has the license to do so). Consequently, it is necessary to have some idea of what it is our probability statements are supposed to be saying before there can be any criterion by which a proposal for how these statements are to be generated can be evaluated. And, of course, Mr. Rescher *does* give us *some* indication of what he intends the probability numbers obtained by polling the experts to mean. If it were not for this, everything else in his paper would be irrelevant for this recommendation, for no matter what interpretation of probability one holds, it could still be suggested that polling the experts might be a good way to get *estimates* of these probabilities.

My other remark is by way of a summary. Mr. Rescher has privately indicated to me that he feels the central point of his paper to lie in his remarks about personal probability, while its first part, about frequency interpretations and unique events, is only a side issue. Yet the latter portion of Mr. Rescher's paper is his proposed solution to the problem set forth in the first part—namely, the question of how probability numbers are to be assigned to events that cannot be informatively classified. What I have tried to show is that Rescher has not succeeded in making a case for the *existence* of such events. To categorize an event as belonging to reference-class R may still be an informative classification, even though R has only one member, so long as there are ways other than observing frequencies in R to assign probabilities to event types in R. To insist upon a special distinction between "unique" events on the one hand and common, or garden-variety, events on the other, does nothing to resolve the crucially important of estimating event-type probabilities of small N; whereas to the extent the latter issue becomes clarified—and I would insist that sampling sampling theory is only a part of the answer here—then Mr. Rescher's problem should also vanish with little or no residue.