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Probability and Evidence. By A. J. Ayer. (London : Macmillan. 1972. Pp. x + 144. Price £3.50.)

Professor Ayer writes as well as ever but, by his own high standards, this book is rather slight and slipshod. The first, main and title essay is less than ninety pages. In so short a span one would not expect justice to be done to this vast topic, and it is not. Twenty pages more are spent in discussing and rejecting Harrod's attempted refutation, in his *Foundations of Inductive Logic* (1956), of Hume's sceptical arguments on induction. The last thirty go on a discussion of conditionals, especially of the non-truth-functional kind usually called " subjunctive " or " counterfactual ", which are involved in characterizing the generalizations that sustain inductive inference.

The topic of the second essay is slight, to my mind. Harrod's argument never was more than specious ; it does not call for such attention as Ayer gives it, especially in the light of what Ayer leaves out of his first essay. The last essay is good, clear and subtle, but neither very original, nor conclusive, nor well integrated with the rest of the book. Now Ayer does not claim these qualities for it but, like the Harrod piece, it is not *just* there " to bring this book up to a respectable size ", as Ayer engagingly puts it in the preface. It is also supposed to bear, and clearly does bear, on the theme of the first essay ; what is not clear in detail is how it does so. Ayer admits it leaves gaps ; another draft of the whole book might have plugged a number of them to our greater profit.

The meat of the book as it stands, however, is in the first essay, and here it must be complained that Ayer shows himself seriously ill-informed on his chosen subject. An extensive and pertinent literature on probability and statistical inference exists to which Ayer does not refer at all. Such names as those of Bayes, Fisher, Neyman, Pearson, de Finetti, Wald, Savage, Hacking, Jeffrey, Levi, Kyburg, do not occur at all, and some who are referred to are misrepresented, as we shall see.

Ayer's starting point is the traditional problem of induction, in an exposition and assessment of Hume's arguments and the various attempts made to resist his sceptical conclusions. To this end he then considers accounts of probability that might be appealed to, and the various problems encountered by confirmation theory. The first part of Ayer's essay calls for little comment. He invokes (p. 6) the concept of an " intrinsic description " of a thing at a time, from which nothing is to follow about the state of that thing at any other time or about the state of other things. Many descriptions of course fail to be intrinsic in this sense—' is a smoker ', ' is a younger son ', ' is mortal ' are obvious cases in point. It seems to me, however, that Ayer exaggerates the problem of finding intrinsic descriptions. He says they must lack causal implications and hence be " purely phenomenal ". And even his exemplars, colour and shape descriptions, he recognizes to have dispositional implications. What I don't see is how this prevents them being intrinsic in Ayer's sense. From a thing's being green now nothing follows about its state at any other time, nor about the state of any other thing. Something follows about how it *would* look *now*, *if* suitably observed—but Ayer's view of these conditionals (in his third essay) is that no *facts* correspond to their truth or acceptability (pp. 124-5). Certainly nothing follows about the *actual* state of anyone who merely *might* observe the thing, since it does not even follow that such a person exists. So Hume's crucial point, with which Ayer is here concerned, about the independence of distinct descriptions, can be made more directly of a lot more of our ordinary vocabulary than Ayer allows.

This point of course does not affect Ayer's conclusions. Nor does the protest I feel constrained to make about Ayer's dated scientific examples, which I fear may discredit his work in quarters where it could very profitably be read. He ought to know by now that Newton's theory of gravitation is incompatible with Kepler's laws, and is in no straightforward sense a special case of Einstein's theory (*pace* p. 19). To talk of successive generalizations here, as Ayer does, without argument, is to beg many of the most hotly debated questions of the last twenty years in the philosophy of science. And *à propos* of " our standard of temporal equality " (p. 25), the possibility he envisages has been realized for some time : the standard *is* no longer set by the earth's rotation.

These points are trivial, if irritating. The first part of the essay is unexceptionable, by and large, if hardly news. And the discussion of confirmation in the third part is at least adequate in its coverage of most of the literature. The really serious flaws come in the second part, on theories of probability that might be appealed to in answer to Hume. Ayer considers (p. 27) three kinds of probability judgment—of what he calls " *a priori* " probability, of statistical probability, and of credibility. His examples of the first kind show he has essentially mathematical judgments in mind, albeit with apparent empirical reference, as in " the probability of throwing heads three times in succession with a true coin is 1/8 ", where ' true ' is so understood as to make this analytic. Now ' true ' here does not mean anything so simple, as I have argued elsewhere, but the main criticism must be of Ayer's Laplacean interpretation of the probability calculus, i.e., in terms of numbers of equi-possible alternatives. What this amounts to, in the standard mathematical terminology, is that Ayer takes every " sample space " to contain only " sample points " that are all equally probable, whereas no such assumption need be, or normally is, made. Still, Ayer's main thesis stands—in empirical applications of the calculus, no assignment of probabilities to events is going to follow just from the mathematics. And that is no news, either.

Next Ayer criticizes Keynes' use of the " multiplication axiom " to show how favourable instances can raise a generalization's finite prior probability. There is nothing wrong with Keynes' example, as Ayer admits. But Ayer's example, designed to show the platitudinous nature of the inference, unfortunately does just the opposite. It isn't true in general that " as the number of possibilities grows less, the proportion of the possibilities which are adverse to any given distribution of values is bound to lessen also " (p. 32 ; in less Laplacean terms : " as sample points are removed from the sample space, the probability of events that remain possible is bound to increase "). If it *were* true, evidence which did not refute a hypothesis could only increase its probability, never reduce it. That is plainly false, as may be illustrated from Ayer's own example of hypotheses about the outcome of three tosses of an unbiased coin. Take the hypothesis that the last two tosses will be heads. Then elementary calculation shows that the evidence that either the first or last toss is tails lowers the probability of the hypothesis from 1/4 to 1/6. There is nothing platitudinous about situations in which the evidence raises the prior probability of an hypothesis. The difficulty with Keynes' example, as emerges in Ayer's later discussion of Carnap's confirmation theory, is the well-known one of crediting generalizations with a finite prior probability capable of being raised at all. Ayer's example goes wrong because he thinks only of hypotheses verified by so-called " simple " events, i.e., containing only one sample point, like his hypothesis that heads comes up on each of the three tosses. If the hypothesis can be verified in more than one way, as in my example, then its probability can be lowered by the evidence eliminating some but not all of these ways. Now most hypotheses are like this ; indeed where the possibilities are infinite in number (which can not only happen, *pace* Ayer, p. 34, but is the most common case), as in measuring a continuously variable quantity like length or temperature, the hypotheses are almost invariably of this kind, e.g., that a length lies in a certain interval of values.

One could go on at some length citing errors of this kind, like the implicit assumption on page 41 that the law of large numbers has to do with sampling *without* replacement from a finite population ; the claim on page 45 that all frequency theories applying to infinite classes do so " by introducing the notion of a limit " (Braithwaite's, to which Ayer refers approvingly on page 49, does not) ; the claim on page 47 that Popper " is committed to holding . . . that statements of probability, as they occur in science, are to be construed in accordance with the frequency theory ". One would never guess that Popper's conversion from a frequency to a propensity view was announced in a paper given to the very conference, fifteen years ago, at which Ayer first gave *his* well-known paper on " the concept of probability as a logical relation ". But then one would never guess from this book that there is such a thing as the propensity theory of statistical probability at all. Just as Ayer takes without question a stone dead Laplacean view of the probability calculus, so he takes without question an increasingly contested frequency view of such statistical probability judgments as " that the probability that a man will die of lung cancer is increased if he is a heavy smoker ", and *a fortiori* of statistical laws. And from the fact that frequency theories of statistical probability cannot account for its predictive consequences (e.g., that it affords reasons for a single individual not to start smoking if he wants to avoid lung cancer), it is no longer reasonable to conclude, as some subjectivists have done in the past, that no objective theory of statistical probability can account for such consequences. How Ayer would react to this controversy, which is surely central to his theme, we cannot tell, for he does not mention the subjective, or " personalist ", theory of probability either. He says nothing of the work on decision theory, on the foundations of statistical inference, and especially the personalists' Bayesian model of learning from experience. Perhaps their work is not a pertinent response to Hume's challenge, in Ayer's eyes. If so, far more people need to be told that, and why, than need to be told what is wrong with Harrod's argument.

It seems to me that the authors Ayer overlooks realize, as Ayer seems not to do, that there is more to be done in this field than the equivalent of proving yet again that perpetual motion is impossible. There is the job of explicating, in serious professional detail (i.e., that of the practice of applied statisticians, who have to decide such things as when the evidence for its safety warrants the general release of a new drug), the principles of inductive inference that are in fact relied on in situations in which the answer is uncertain, as well as in those, classically considered by philosophers, in which it is certain (or at least, where we are certain of it). Hence the concern both with theories of what consequences for rational expectation statistical laws in science have (e.g., propensity theory), and with theories about the evidence needed to warrant use, acceptance, or choice between scientific theories, whether statistical or not. It is a great disappointment that under the title " probability and evidence " so fine a philosopher as Professor Ayer has not chosen to enter on this current debate, to which his contribution would be as welcome as it has been in so many other areas of philosophy.

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Scientific Knowledge and its Social Problems. By JEROME R. RAVETZ. (Clarendon Press : Oxford University Press. 1971. Pp. xi + 449. Price £5.00.)

This is a good book that contains some poor philosophy. If we interpret ' philosophy ' in a wide sense, then it also contains some good philosophy. Ravetz describes the growth of science from what he calls the " academic " to the " industrialized ", tracing the social causes and effects of this process. This part of his work is scholarly, sensible, sensitive and humane, with generous references to what he finds excellent in the work of others, while ignoring what might be criticized. Academic science, says Ravetz, could be the work of gentlemen, with gentlemanly standards : industrialization goes hand in hand with capital-intensive research and development, with the economic pressures and reliance on power elites that this implies. The changed technical character of science inevitably produces changes in social institutions and practices " as radical as that which occurred in the productive economy when independent artisan producers were displaced by capital-intensive factory production employing hired labour " (p. 44) :