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COMMENTS ON JOSEPH AGASSI

Dr. Agassi's complaint against Bayesian accounts of scientific inference is that they are 'bogged down in subjectivism'. He seems to say that only an 'objectivist vindication' of some sort can save the Bayesian from the excesses of Berkeleyan solipsism and Fichtean megalomania. He mentions what he takes to be one such attempted vindication, the argument that under certain conditions one can rationally expect that as evidence accumulates one will assign high probability to true statements and low probability to false statements, but comments that this argument 'does not deliver the goods'. Finally, he suggests that if the subjective element could somehow be eliminated from the Bayesian approach, it would actually show that personal probability is redundant. His implication is that a Bayesian cured of chronic subjectivism would become a healthy Popperian!

The charge of subjectivism, although it is often made against Bayesians, is more rhetorical than substantial. Agassi's indictment is particularly excessive. There is nothing inconsistent in a Bayesian holding an objective theory of truth and a realist account of scientific theories. He need be neither an empiricist nor a verificationist. Nor is there anything inconsistent in his holding that whether or not an experimental result corroborates a theory is a matter of objective fact. This can be contrasted with some 'objectivist' accounts of statistical inference and some of Popper's comments on corroboration. Popper remarks that the outcome of an experiment corroborates a theory only if the experimenter has made a 'sincere effort' to overthrow the theory.[1] In Neyman Pearson theory, which in many respects seems to be the statistical embodiment of Popperian methodology, the evidential import of an experimental result depends on the experimenter's *intentions* concerning when to stop sampling. If the experimenter should die without communicating his intentions or demonstrating his sincerity then, presumably, on these views his results are worthless. A Bayesian can objectively measure the severity if not the sincerity of an experiment for a particular hypothesis by the

Synthese **30** (1975) 25–30.

improbability of obtaining a result which confirms the hypothesis when it is false. In this respect Bayesian methodology appears to be more objective than its 'objectivist' competitors.

The usual objectivist objection to Bayesian methodology is that one of the inputs in Bayes' theorem, the prior probability distribution, represents personal subjective opinion and so the Bayesian approach lacks objectivity and is therefore unscientific. The subjectivity of the prior distribution is sometimes contrasted with the objectivity of likelihoods. Actually in many applications likelihoods are tautological. The probability of obtaining three heads on five flips of a coin conditional on the assumption of a Bernoulli model with parameter p is a mathematical consequence of the assumptions of the model. But the assumption of a particular probability model is itself a probability judgement. Such a judgement may appear to be more 'objective' than judgements concerning the possible values of the parameters of the model but only because there is wide agreement concerning the applicability of the model. The objectivist who assumes that a Bernoulli model holds and calculates direct probabilities in terms of it is making probabilistic judgements just as the Bayesian who assigns a probability distribution over the parameter space is.

Agassi says that prior probabilities are 'arbitrary and irrefutable'. By this I think he means that a prior probability cannot be criticized and is not subject to check against objective fact. Of course, what personal probability someone assigns to a proposition is a matter of his psychology. But this doesn't mean that he cannot be criticized for assigning the probability he does assign. Perhaps because Bayesians have taken such pains to argue that coherence, conditionalization, and maximizing expected utility are necessary conditions for ideal rationality, Agassi has mistakenly concluded that they hold that these are sufficient conditions as well. But in spite of a tolerance for divergent opinions, a feature that one would think Popperians would endorse, Bayesians are not committed to holding that personal probability distributions are immune from criticism. In fact, conditionalization itself can be viewed as a way of assimilating criticism.

There are a number of respects in which personal probabilities might be criticized. One is that a person's probabilities might be criticized for not reflecting all of the information which he has at his disposal. Another

is that they might be criticized for going beyond the information which he actually has. As an example of this, consider someone who is told that a particular urn contains four balls which may be red or blue. Suppose that this person assigns prior probabilities of 0.64 to the hypothesis that all the balls in the urn are red and 0.09 to the remaining four hypotheses. When we ask him why he has chosen this probability distribution he says that he has no particular reason but that this distribution does reflect his beliefs. By way of criticism we can point out to him that his beliefs are the same as one would have were he to begin with a uniform prior distribution over the hypotheses and, sampling at random with replacement, obtain a sample of three red balls. Our believer is pretending to have information which he does not possess. If he persists in choosing his prior distribution in this arbitrary manner he could be 'fleeced' by a more rational Bayesian gambler.

Actually, there is a Bayesian justification for subjecting one's opinions to criticism. The reason is that the expected utility of experimentation is never negative. We can view obtaining criticism as a kind of experiment. Even the cost of arguing with a Popperian may not be too high for it to be rational for a Bayesian to engage with him in mutual criticism.

Bayesians differ among themselves concerning the question of whether or not two people who possess the same information can rationally choose different personal probability distributions to represent their beliefs. Savage, for example, holds that they can while Jeffrey and Jaynes hold that, at least in some circumstances, a body of information determines a unique prior. Agassi's charge of subjectivism is directed mainly against Savage's view. He may feel that a probability distribution might be the result of prejudice, wishful thinking, or sheer whim, and so can play no useful role in an account of scientific inference. If Savage literally held that any prior distribution is compatible with a given body of information then there would be something to Agassi's criticism. But Savage's reluctance to accept the principle that two people with the 'same information' ought to have the same distribution may be due, at least in part, to a conviction that it is impossible for a person to articulate all the information which he brings to bear on a problem. After two people exchange all the information which they can articulate, differences between their probabilities may remain. But even these may be due not to subjective whim but to information which each cannot express. Such

information is subjective in the sense that it is private to its owner but it still may be reliable and rational for a person to act on it. Of course, if you cannot articulate the reasons on which you base your prior distribution, you provide me no reason for agreeing with you. Another consideration in favor of Savage's tolerant view is that a person's information may be so scanty as not to determine a unique probability distribution. The problem of finding an appropriate probability distribution to represent a state of ignorance or scanty belief has been a thorn in the sides of Bayesians. One response is simply to admit that in the face of scanty information many different distributions may be appropriate.

In any case, Savage has argued,[2] when two people start out with non-dogmatic prior distributions over a set of alternative hypotheses, they can rationally expect that as evidence accumulates their posteriors will converge toward one another, and toward the truth. The import of this argument is taken to be that differences in people's prior distributions can be expected to be cancelled out as evidence accumulates and so one need not be overly worried about the subjectivity of prior probabilities. Agassi takes this argument to be an attempt at objectivist vindication of subjectivism and makes a number of remarks in criticism of it. One point is that a person's prior distribution might not get better as he obtains information. Presumably what Agassi has in mind is that someone might start with a distribution over alternatives, $P(B_1) \ldots P(B_n)$, and even when B_k is true obtain an experimental result E for which $P(B_k/E) \leqslant P(B_k)$. Of course this can occur, although it is improbable and grows increasingly improbable as the experiment is repeated, provided that these are independent. But this has nothing to do with the alleged subjectivity of the prior distribution. Supposedly objective statistical methodologies also result in mistakes. In fact, Lindley's paradox shows that, for large samples, experimental results which fall on the boundary of the critical region of an orthodox test may actually yield a high likelihood in favor of the rejected alternative. By striving after objectivity Neyman Pearson testing can lead one to reject an alternative that one is almost sure is true.

It seems to me that Savage's argument does provide a measure of 'objective vindication' for the Bayesian approach although it is not the sort of vindication Agassi has in mind. Since we can reasonably expect that continued experimentation will bring our beliefs into harmony with each other and with the truth we have a compelling motivation to ex-

periment. So even those Bayesians who hold that there is room for variation in the probabilities assigned by rational men can argue that to be rational one must subject one's beliefs to experimentation and conditionalize on the results. Furthermore one can be almost sure that by following this procedure one's beliefs will get closer to the truth. All of this has a distinctly Popperian ring!

Savage's argument shows that one can expect that two investigators, starting with divergent priors, can be brought into close agreement by a sufficiently definitive experiment. But conversely, for any experiment there are pairs of prior distributions which are so divergent that they will remain far apart after the experiment. To put the point slightly differently, for any experiment there is a prior distribution that is so perverse that the posterior distribution conditional on the experimental results will remain far from the truth. I think that this lies behind Agassi's objection to personalism. He is afraid that someone may come to a problem with a prior which is so perverse that his posterior will also be perverse after any reasonable amount of experimentation. Before squarely facing this objection, which I think is a serious one, we can point out that it would be just as perverse to banish prior information from statistical problems, as orthodox methodologies attempt to do. Consider how an orthodox investigator deals with the following problem: Someone claims that by singing to plants he can get them to grow taller. Twenty pairs of randomly chosen seedlings are planted with the man singing to one of each pair. It is found that seventeen of the serenaded plants do grow taller than their counterparts. According to an orthodox test this is fairly good evidence against the null hypothesis of no effect. But our prior information is such that we would be very skeptical of there being a connection between song and plant growth. I think that few people would be willing to bet on there being such a connection on the basis of this experiment.

In order to answer Agassi's objection we have to find some way to legislate against perverse probability distributions. One attempt in this direction is due to E. T. Jaynes.[3] Jaynes argues that for a probability distribution to be of more than 'psychological' interest it must be based solely on testable information. He makes this idea precise by claiming that the appropriate distribution is the one which maximizes entropy subject to the constraints imposed by prior testable information. The

entropy of a discrete distribution is given by

$$H = -\sum_{i=1}^{n} P_i \log P_i$$

The idea is that the maximum entropy distribution subject to constraints imposed by prior information goes beyond that information as little as possible. Although Jaynes' suggestion is reminiscent of the principle of insufficient reason, it avoids some of the objections which are commonly lodged against it. Furthermore, it provides a way of translating information which may not be expressed in terms of probabilities into a probability distribution. Two investigators who approach the same problem can share their testable information and then choose the appropriate maximum entropy distribution. They can then work out an experimental program based on this prior distribution.

Whether or not Jaynes' rule is satisfactory is not a question I can discuss here. My point, however, is that it is perfectly consistent with personalism to hold that personal probabilities are subject to interpersonal criticism. The maximum entropy rule provides a general method for criticizing a prior probability distribution. In so far as that distribution goes beyond the testable information available it is colored by subjectivity. The result is an objective Bayesianism which escapes Agassi's charges. In fact, one might venture that a Popperian cured of his phobia towards subjectivism would become a healthy Bayesian.

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NOTES

[1] Popper, Karl R.: 1959, *The Logic of Scientific Discovery*, Hutchinson's, London, p. 428.
[2] Savage, L. J.: 1954, *The Foundations of Statistics*, John Wiley, New York.
[3] Jaynes: 1968, 'Prior Probabilities', *Transactions on Systems Science and Cybernetics*, SSc-4, No. 3, September 1968.