St John's College
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1934 Feb. 24.

Dear Fisher,

Thanks for loan of your paper. I have glanced through it but of course have not yet had time to make up my mind about all the points. However my main reaction is that the difference between you and me on any questions that concern you directly is not great. Wrinch and I showed in our original Phil.Mag. paper that in sampling problems, provided the sample is large, that for a wide range of distributions of the prior probability the posterior probability is practically proportional to the p. of the sample given the whole class, and therefore to your 'likelihood', Consequently allowance for prior probability in these matters makes little difference except in freak cases, as when the sample consists of five white balls ~~that happen to be the only white balls in a bag~~ we knew to contain 100 in all, of which just 5 were known to be white. Your expressions in ordinary cases are good approximations to the posterior probability ; if Iclaim in one case to have got a slightly better one Idon't see that you need worry. If in your original Phil.Trans. you had referred to this result of ours as providing an alternative justification of your results you would have been on strong ground. I notice that in your paper you don't defend your statement that the ratio of two infinite numbers has a definite value ; and this is my crucial objection to your point of view.

My point is that we need a theory that will allow us to draw inferences in cases where sampling methods do not apply, and it must have two properties. It must make it possible to assimilate new experimental knowledge ; and it must give us ground for believing the laws that we do believe, or else say definitely that our inferences are fallacious. You would accept the first ; but for the second you must introduce a priori considerations of some kind. A quantitative physical law is on an entirely different footing from any type of sampling, for reasons given in the Quantitative Laws chapter of my Scientific Inference. It would be a quite logical position to maintain that all quantitative laws are merely interpolations and that all values of y for other than the observed values of x are equally likely; e.g., that the predictions in the Nautical Almanac are meaningless ; but if you really mean that Ithink you ought to say so.

My quarrel is not with you, but with Eddington and similar people. E. would say that the law of gravitation is determined by purely a priori considerations ; Isay that it is partly a priori and partly experimental. You would deny the a priori considerations altogether. So it is rather anomalous that in a recent Phys. Soc. paper on measurement E. throws as much mud as he can at probability (while introducing probability considerations at every turn without noticing them)and at the same time quotes some of your work with approval. Such an attitude means a complete a priori theory and an experimental one, with no possible point of contact. I prefer to have just enough a priori material to give experiment a chance to decide what is true. You give too little ; Eddington gives too much.

I cannot follow your objection to the generalization to all probabilities of the laws of probability obtained for samples. You do not deal with my argument for the need for an a priori postulate, and your argument would say equally well that we cannot determine the distance of a star from its parallax.

Yours sincerely,

Harold Jeffreys