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DISCUSSION

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Dawid and Stone make the useful point, already stated by Bunke, that those difficulties for the fiducial argument which arise from non-uniqueness of pivotals can be overcome by adopting the Bunkes' functional model, or some other model of similar type, such as Fraser's structural or this writer's pivotal model. Their formal investigation of the consequences of associating, with a functional model, a model they call "fiducial," is most useful in pointing to directions in which further supposed difficulties can be overcome, as well as to some directions in which difficulties remain. In particular it seems clear that we cannot apply to "fiducial probabilities" the same rules of conditioning which we apply to the probabilities we apply to observations.

They disclaim any attempt to give an account of Fisher's thinking on the subject; and they are, I think, right to do so in view of the revisions Fisher made between the 2nd and 3rd Editions of *Statistical Methods and Scientific Inference* (especially in the last few pages), and the views Fisher expressed in what was, perhaps, his last letter on the subject, quoted in the Royal Statistical Society's obituary notice (Barnard, 1963). These show that Fisher's views were continuing to evolve, and it is another tragedy of Fisher's life that it was cut short at the beginning of a decade and a half in which major new insights into foundational issues were gained as a result of the work of Birnbaum, Buehler, Robinson and many others.

Nearly a century of work on the much simpler foundations of mathematics has shown that attempts to incorporate a notion of "truth" into mathematical reasoning lead to the near paradoxes of Gödel's incompleteness results, and the complexities of Tarski's hierarchy of meta-languages. It is therefore not to be expected that rapid progress will be made with the formalisation of statistical reasoning, involving, as it necessarily must, not only a notion of "truth" but also a notion of "knowledge," or its absence.

Some of the issues not addressed by Dawid and Stone may be illustrated by reference to the simple situation in which we have observations x_i, $i = 1, 2, \cdots, n$, i.i.d. with density of known shape f, but with otherwise wholly unknown location λ and scale σ. Taking the pivotals $p_i = (x_i - \lambda)/\sigma$ with density $\Pi_i f(p_i)$ as basic, we transform to t_p, s_p and c_i with $p_i = s_p(t_p + c_i)$ and $\Sigma_i\, c_i = 0$, $\Sigma_i\, c_i^2 = n(n-1)$ so that

$$s_p = s_x/\sigma\sqrt{n}, \quad t_p = (\bar{x} - \lambda)\sqrt{n}/s_x, \quad \text{and } c_i = (x_i - \bar{x})\sqrt{n}/s_x.$$

Since the values c_{i0} of the c_i are completely known when the observations are known, we condition the density of s_p, t_p on these values, to get

$$\psi(s_p, t_p \mid c_0) = K s_p^{n-1} \textstyle\prod_i f(s_p(t_p + c_{i0})),$$

and then, if we are interested only in λ, we integrate out s_p to obtain

$$\xi(t_p \mid c_0) = \int_0^\infty \psi(s_p, t_p \mid c_0)\, ds_p.$$

We may now use this density to test hypotheses about λ, to obtain confidence sets, or families of such, or to obtain a fiducial distribution for λ. The step involved in going from $\psi(s_p, t_p \mid c_0)$ to $\xi(t_p \mid c_0)$ implies that we are regarding s_p, after the observations are known, as having the marginal distribution implied by $\psi(s_p, t_p \mid c_0)$; and to this extent, it may be argued, we are integrating out the nuisance parameter σ over its fiducial distribution. But if we use $\xi(t_p \mid c_0)$ to derive a P value for, say, the hypothesis $\lambda = 0$, this implied use of the

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fiducial distribution of σ involves much less far-reaching considerations than would be involved if we were to use the fiducial distribution of λ, derived from $\xi(t_p \mid c_0)$, to serve as a prior distribution in relation to further observations like x_i. There are very persuasive arguments (Barnard, 1982) why such P values should be seen as referring to a reference set in which σ follows its fiducial distribution, but such arguments do little to justify the use of a fiducial distribution, in general, as a prior.

Again, if we remove any complications associated with the unknown σ by supposing σ known $= 2$, say, so that we can further condition on now knowing $s_p = s_{x0}/2\sqrt{n}$, and so obtain a fully conditioned density $\eta(d \mid c_0, s_{x0})$ for the pivotal $d = \bar{x} - \lambda$, it seems that no difficulties arise in connection with a fiducial density $\eta^*(\bar{x}_0 - \lambda)$ for λ and the corresponding density for any $1 - 1$ function of λ. But there are difficulties in dealing with functions of λ which are not $1 - 1$, such as λ^2. It seems, in fact, that the density for any function $\phi(\lambda)$ must be thought of as attached to the pivotal $g(d)$, say, which directly generates the fiducial density of $\phi(\lambda)$, and unless we can find a function g such that $g(d)$ is a function of $\bar{x}$ and $\phi(\lambda)$ only, then we have no fiducial distribution for $\phi(\lambda)$.

Another issue not addressed by Dawid and Stone arises in connection with the fiducial distribution of the correlation coefficient. The arguments given in the paper are quite appropriate once it is agreed that the sample value r contains all the available information about ρ. But this will be the case only if the distribution of the observations is known to be exactly bivariate normal; whereas the arguments leading to fiducial distributions for location and scale require no specific assumption about distribution shape, so that any uncertainty as to this can be dealt with by corresponding adjustments to the fiducial distributions, the arguments in the case of r and ρ fail to have this property. Furthermore, even if we are prepared to assume exact bivariate normality, we still have to say why we begin by ignoring the information about location and scale. It was to deal with this question, among other things, that Fisher introduced the ideas expressed in the letter already referred to.

An indication of how Fisher might have dealt with some of the issues raised here is to be found on pages 46–47 of his book. He writes, concerning the reluctance we feel towards accepting that an event of low probability will occur: "The psychological resistance has been, I think wrongly, ascribed to the fact that the event in question *has* ... the low probability assigned to it, rather than to the fact, very near in this case, that the correctness of the assertion would *entail* an event of this low probability.... Disbelief is equally justified when the probability is hypothetical." He is here discussing tests of significance. But in the fiducial argument, not only do we have that a proposition H about (say) λ entails a proposition P about the associated pivotal; we also have, that not-H entails not-P. And the probabilities concerned are maximally conditioned. Such a close connection between H and the probability of P deserves a special name, distinct from the "confidence" we attach to a confidence set; to call it a fiducial probability would seem to do no harm, provided it is understood that the probability proper belongs to the pivotal, not to the parameter.

Faced with Burali-Forti's paradox concerning ordinal numbers, Hilbert defiantly proclaimed that no one was going to drive mathematicians out of the paradise which Cantor had created for them. The attitude to the fiducial argument indicated in Dawid and Stone's quotation from Hacking is very much to be welcomed. We may have a certain amount of reorganization to do in the paradise Fisher created, but that by no means requires us to quit.

REFERENCES

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