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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

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

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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