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# REPLY

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Professor Barnard has raised a number of interesting issues that go beyond our limited objective which was "to integrate under a single mathematical structure the primary ideas that may be developed with functional models". A full understanding of his comments could well be a starting point for the definitive Fisherian hermeneutic itself.

With regard to the problem of inference for the correlation coefficient, we dispute the necessity of normality. Consider the model for a data matrix $\mathbf{X} = (X_{ij}: i = 1, 2; j = 1, \cdots, n): X_{1j} = \Gamma_1 U_{1j}, X_{2j} = \Lambda X_{1j} + \Gamma_2 U_{2j}$, where, independently for each $j$, $U_{1j}$ and $U_{2j}$ are uncorrelated with mean 0 and variance 1 but not necessarily normal. (It would cause no difficulty to include location parameters). This is a realistic model for data in which $X_1$ is considered as "causing" $X_2$. It may be reexpressed as a partitionable SFM: $X_{1j} = \Gamma_1 U_{1j}, X_{2j} = \Gamma_3 U_{1j} + \Gamma_2 U_{2j}$, $(j = 1, \cdots, n)$, or $\mathbf{X} = \mathbf{\Gamma U}$, where $\Gamma_3 = \Lambda\Gamma_1$. The correlation between $X_1$ and $X_2$ is again $\Phi = \Theta/(1 + \Theta^2)^{1/2}$, with $\Theta = \Gamma_3/\Gamma_2$. The arguments of Section 5 now yield, as the conditional SFM, the model $\mathbf{Y} = \mathbf{\Gamma E}$ discussed in Section 3, where $\mathbf{YY}' = \mathbf{XX}'$, $\mathbf{EE}' = \mathbf{UU}'$ and the distribution now assigned to $\mathbf{E}$ is that obtained by conditioning on the functional ancillary information $\mathbf{E}^{-1}\mathbf{U} = \mathbf{Y}^{-1}\mathbf{X}$. From this starting point, inference about $\Theta$ can proceed, exactly as before, in the reduced model of Example 2.2 using the above conditional distribution of $\mathbf{E}$. The resulting fiducial distribution for $\Phi$ will now be consistent with that found by differentiating the *conditional* distribution function of $R$ given $\mathbf{Y}^{-1}\mathbf{X}$, which will depend on $\Phi$ alone.

We respect the breadth of Professor Fraser's discussion. However, we wish to plead that our paper be first read and judged as it stands and in its own terms, uncoloured by the interpretations put on it by Fraser. Important "ingredients" that he detects in it are not among those that we put into it, and we are unwilling to believe that what we have written can countenance transsubstantiation on the scale that would be needed to support some of Fraser's comments.

Consider, for example, his remarks about our Lemma 3.1. Since one of our objectives was to produce, if possible, a powerful mathematical formalism, we ought to have been pleased with his observation that this lemma is trivial (which it is). However, its triviality is not a consequence of the question-begging argument with which Fraser prefaces his observation. His related comment on our treatment of the "Stein-Wilkinson" example suggests, moreover, that he prefers a definition of an SFM very different from our own. Quite simply, $Z = (\Theta_1 + E_1)^2 + (\Theta_2 + E_2)^2$ is *not* an SFM: given $Z$ and $(E_1, E_2)$, $\Theta$ is confined to a circle from a set of circles that do not form a partition—so that the given information is not expressible as a condition on some fixed function $\Lambda$ of $\Theta$.

We would like to sidestep the question of whether or not we have formulated our version of the "fiducial argument" with proper respect for the subtleties of the often conflicting viewpoints and terminology of previous workers. We merely wish to claim that our development touches significantly on what would be widely agreed to be core elements of fiducial inference. We would be only too happy to incorporate Fraser's ideas on "validity", given a clear enough exposition of them for this to be attempted.