September 23rd, 1935

Dear Buchanan-Wollaston,

I rather doubt if there is anything to be gained at present by writing a second letter to "Nature", as the subject is an obscure one, and we cannot hope to lead people more than one step at a time.

With the aid of your letter to me, I think I understand something of what you are driving at in your letter to "Nature", but without the first I should have very little chance with the second, apart from the first two paragraphs.

What you raise in your letter to me is the question of what I have called 'problems of specification'. Logically, these must precede all questions of estimation and of goodness of fit; for, until the mathematical form of the population sampled is specified, both the number of unknowns and their meaning are undefined, and you cannot start estimating until you have a clear conception of what you are making an estimate of.

The test of goodness of fit, and other logically similar tests of significance provide the negative safeguard

\[ \text{[x]} \text{ See Nature 136, 192 and 296 (1935).} \]
of making sure that you are not interpreting the data on an hypothesis which the data themselves contradict, but does not exclude any hypothesis which might not improbably have yielded the whole body of observations in hand. Many such hypotheses will, of course, be rejected on other grounds as unreasonable or fantastic; but, even so, it is not the data themselves that reject them.

Practically the only rule of choice among hypotheses which are compatible with the observations, which is at all widely accepted, is to choose the simplest - and opinions will differ to some extent as to which is the simplest, according to the scientific ideas of each particular judge. The only case of scientific importance arises when two different hypotheses are preferred by different groups of workers for the interpretation of admittedly correct observations not incompatible with either hypothesis. In such a case, the wise course, I think, is to emphasise clearly that neither view is excluded by the observations so far made, and that other observations, probably of a different kind, are needed to discriminate between the two views. It would be unwise, I think, to attempt a discrimination on the data already available by saying that on one view $P$ is .1, while on the other $P$ is .5, for this fact provides only a slight ground for choice, since the
true hypotheses are not infrequently have a value of $P$
as low as .1, and the hypothesis which in fact yielded
15 was in any case lucky to do so.

I do not deny that there is here a slight ground
for preference, which I should express by saying that the
likelyhood of one hypothesis was slightly greater than that
of the other, though neither could be judged unlikely; but
it would, I think, be a mistake, as statistical methods
for much plainer issues are still so frequently misunder-
stood, to lay any emphasis on what is, at best, a rather
weak argument.

Many thanks for the offprint.

Yours sincerely,