Dear Professor Fisher,

As instructed, I returned those reprints to the Galton Laboratory, and am engaged at present in steadily reading through the others, with much pleasure and profit. Most of it is straightforward going, and such difficulties as I find are those, due to my own limitations, which it is better that I should clear up for myself without assistance. One of the feelings I have experienced is a sense of surprise that your notions, and many of your results, should have met with resistance on the part of statisticians. Particularly is this so with all that touches on "degrees of freedom" and the circle of ideas connected with $\chi^2$; one would have thought that, with the analogous situations presented in other and familiar domains of mathematics, the theory of vibrations under constraints in dynamics, or of matrices and quadratic forms of less than full rank (because of linear dependences) in algebra, the corresponding results in mathematical statistics, which has a very close relationship with dynamics, would have received an instant welcome, instead of the psychological repugnance which they still arouse, it would seem, here and there. The younger generation, of course, has no difficulty whatever in accepting your ideas; in fact a young German Jew who is studying group theory and algebra under me, and has
been attending my course on theoretical statistics from a desire
to widen his field, was astounded to learn that there had ever
been any discussion on these matters.

My principal difficulty during these last several years has
been to come to a settled agreement with myself, so that I could
be honest, without gloss or suppression of difficulties, with my
students, concerning the basis of probability. I read Keynes
several times and ended by discarding him entirely; he gives an
academic exercise, and no more; and that business of springing
suddenly the addition and the multiplication theorems as defini-
tions, not as deductions from his postulates, is very shaky, for
his "+" and his "×" signs have up to that point in the book been
logical, not quantitative symbols, and even as logical symbols are
not well defined. Also many statements in the later part of the
work are at complete variance with statements in the earlier part,
giving me the impression that the book was written at two different
epochs, and that what was begun under a fresh urge of enthusiasm
was completed under the incubus of fulfilling a duty long ago
incurred.

Other treatises one discards at an earlier stage than Keynes,
especially those that talk about subjective degrees of belief. In
the end I adopted an entirely objective standpoint, using the
notions of sets of points, measure and so on; keeping in mind
always the endeavour to do for an unsymmetrical, non-homogeneous
and biased system such as a badly made die tossed in a resisting
medium and landing on an uneven floor, what is done in elementary
treatments of symmetrical systems such as a cubical homogeneous
die. This approach, though it may seem sophisticated, gives a
good account of all that is happening, provides a derivation of
the fundamental laws of probability (which are exactly the laws
of measure in sets of points), and gives also a good explanation
of why relative frequency in iterated trials tends to conform to
probability. An interesting thing in the formulation under this
view of such a problem as the unsymmetrical system is that dyna-
mical determinism is presumed; indeed I do not understand the
statements of Jeans and other quantum mechanists, to the effect
that the substitution of probability for actuality in the deter-
mination, for example, of the position or momentum of an electron
implies that the physical universe is indeterministic. I should
prefer to substitute for the irresolution of an electron the
ignorance of the physicist. But I am open to further conviction
on this matter, which is outside my competence; and I do see
that the physicist, disturbing the state of his electrons and pho-
tons by the mere act of observing them, is in the same dilemma
as the experimentalist who wishes to study the properties of
living tissue and kills it in doing so, or of a psychologist who
tries to observe his own processes of thought and alters them by the very process of introspection.

It is a pity you had to turn aside and waste your time over the controversy with Jeffreys; though onlookers gained some advantage from reading the interchange. At first I xxxxxx it a major misfortune for this country that your opponent should be thought to have any standing whatever in the subject; but on second thoughts it does not seem to matter so much — his ideas are too obscure and too tenuous, they blow away and take no root, and I imagine few people read beyond the first page of his contribution. I was disappointed, however, when I read that he was to take part in the discussion on probability at the recent meeting of the London Mathematical Society, for the intrusion of loose mysticism weakens and demoralizes the salutary rigour of debate; the atmosphere becomes more like that of a meeting of theosophists, or at the very least of first year undergraduates in a philosophy class. When Wishart wrote and told me that you had been present at the debate but had left before the end without speaking, I imagined that my premonitions had been fulfilled.

It is possible that in May I may have occasion to visit London (to provide some diversion for the Lond. Math. Soc.) and I hope to be able then to call on you.

Yours sincerely,

A. C. Aitken
February 26th, 1936

My dear Aitkin,

Many thanks for your interesting letter of February 22nd. I do hope, whenever you are in London, you will come in and have lunch here, and a talk. As soon as I have them I am looking forward to sending you an offprint of a paper illustrating more fully than I have done before the power of the fiducial type of argument in resolving otherwise very troublesome problems.

Yours sincerely,