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 χ^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

* an exile of Hitler

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 "$\times$" 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 biassed 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 dynamical 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 determination, 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 photons 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 ~~thought~~ felt 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 contributions. 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 Aitkfn,

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,