5
History and Philosophy of Science

Fisher to W.E. Le G. Clark: 17 *November* 1952

. . . The point and policy of this little group[1] is indeed to make such real contact between practitioners, e.g. ecologists, pharmacologists, etc., and more theoretical types so that the methodological work done shall really be fit to cohere with problems arising in the real world. It would be no bad thing if people like yourself, concerned largely with forming just conclusions on morphological grounds involving some element of subjective judgement, should come to think of biometricians not so much as 'natural enemies', but rather as auxiliary specialists having had some practice in the difficult art of weighing numerical evidence. . . .

[1] The Biometric Society. Fisher's letter led to Clark joining the Society.

Fisher to R.F. Harrod: 17 *July* 1942

I have just read your paper on 'Population and the future', which I thought put the true state of the case quite excellently. I can only excuse your depreciation of it by my own frequent experience. A year or two after I have finished a piece of work I find myself remembering my efforts with a most uncomfortable distaste. But then, if a few years later I have occasion to reread what I actually wrote, I am usually astonished to find my own bad opinion to be very largely ill-founded. Try rereading your own article as if it were someone else's new contribution to the subject. It is a most encouraging experience.

The worst of trying to move the world is that you have to stick at it so long to move it even just a little. But I suppose I ought to have thought of that before starting.

Fisher to H.W. Heckstall-Smith: 30 *July* 1957

I like your quotation from Wittgenstein,[1] and to me the phrase 'because we prefer our hypotheses simple' is unanswerable, but 'we prefer to believe that the truth is simple' is a highly metaphysical statement. However, we must use what words we have with even less pedantry than we use what ideas or hypotheses we can form, so I am against a general ban on the words 'truth', 'proof', and 'real', but in favour of exposing their ineptitude in particular cases.

[1] Heckstall-Smith had written quoting a colleague's justification for choosing a particular hypothesis: 'Because we prefer our hypotheses simple. We prefer to believe that the truth is simple.' and added that he disagreed with this and would like to exclude the words *true, proof,* and *real* from scientific and statistical theory. He said he was 'deeply impressed' by the following quotation from L. Wittgenstein's *Remarks on the foundations of mathematics* (IV, 48, page 156): ' "*Mathematical logic*" has completely deformed the thinking of mathematicians and of philosophers, by setting up a superficial interpretation of the forms of our everyday language as an analysis of the structures of facts. Of course in this it has only continued to build on the Aristotelian logic.'

D. McKie to Fisher: 2 *June* 1943

I have been meaning to send the enclosed to you for some time — but you know how things are in these days. . . . I expect you have read Brewster's *Newton*: I find it a very irritating book. The new biography by L.T. More [1934] is nearly as bad. My own reaction, for instance, to the calculus controversy — purely a historian's reaction — is that Newton fares badly at the hands of both biographers. This reaction, of course, is due to what I hold to be the proper plying of my trade of historian — due heed to dates and documents and contacts, not too much stress on men's foibles and touchiness. However, what I feel is really needed is for a clear opinion by a mathematician on whether certain mathematical results used have been independently derived — no one seems to have put that right, not even Rouse Ball. I do get the very clear impression (as a historian/not a mathematician) that Newton's complaint against Leibnitz was just (altho' his method of dealing with it was unfortunate).

Fisher to D. McKie: 4 *June* 1943

Many thanks for sending me your notes about Newton's Chemical Philosophy which seem to me a much more worthy tercentenary notice than some of those I listened to at the Royal Institution and the subsequent dinner of the Royal Society Dining Club. Lord Keynes tried hard on that occasion to make all our fleshes creep with his stories of the mysterious contents of the big black box which Newton packed up when he moved to the Mint. To me this was very unconvincing.

There is one point often overlooked which of course only comes into question if on occasion Newton, like some other mathematicians, ever did show himself to be credulous, and that is that some degree of this weakness would seem naturally to be associated with any remarkable precocity of intellectual development, for the simple reason that the child in such cases is perfectly able to understand abstract distinctions and to follow long trains of reasoning, so satisfying his curiosity with apparent finality on a number of problems, using as unquestioned data the kind of over-simplified statements thought suitable for immature minds, which in later life are reinterpreted with a great deal of qualification. I mean such statements as that the Bible is the 'Word of God', which to any simple-minded child, not suspecting the adult world of a conspiracy of hypocrisy, seems to mean the devil of a lot more than it does to any bishop.

I think it very probable that Newton showed a credulity of this kind in that he had thought out, or at least satisfied himself when quite young as to the importance of different kinds of human endeavour, and would not easily be persuaded in later life that what he could do in mathematics was really more important than clearing up obscurities in the chronology of the Bible.

Thanks again for sending your note.

Fisher to J. Maclean: 11 *October* 1940

Thanks for your letter of September 1st which has just reached me with your enclosure.[1] Have you ever thought how eccentric mankind is in the application of his *attention* or *interest*? On almost any point on which public interest is lively and sustained, mankind, with or without using the organisations we call States, can do apparently almost what it likes. By some geometrical accident, for 50 years or so, most of mankind was interested in the discovery of, or rather access to, the poles of the earth's rotation. The difficulties were enormous and organised States did not do very much to help, but one shot followed another until what had seemed impossible has become quite easy, and there may be Cook's tours to the North Pole when our aviators and machines are looking for jobs after the next demobilisation.

When something difficult, remote and unprofitable like this can be done, it would seem easier to reorganise our internal affairs to bring about any possible clearly conceived and generally desired object. The difficulty I feel is that since the French Revolution (I mean the one about 150 years ago) the programme has been (1) abuse, (2) destroy, and then, perhaps, (3) reform, that is to say that distrust, hostility and hatred towards existing institutions and persons is quite taken for granted as desirable, honourable and effectual in most groups which are at all impatiently concerned with getting anything put right.

In fact, I suggest that this agitator's mentality is by far the most serious obstacle to the internal reform of Society and, in fact, to the very reforms which the agitator and his supporters would like to bring about.

Devotion to France is genuinely and effectively traditional among modern Frenchmen; yet this year the majority of Frenchmen acquiesced in a fairly obvious betrayal of their own country. The reason, of course, lies in the internal dissensions of French politics. The France to which that loyalty was pledged was threatened both from Berlin and from Moscow. The Communist danger developed first, and the *front populaire* with the Spanish Civil war showed that it was really dangerous, and gave not only die-hards but all law-abiding Frenchmen a feeling that there were worse things than Fascism. Consequently, on the collapse of the French army, it was alarmed extremists of the type of Laval who were able to seize power and, as they had perhaps some reason to believe, to save France from revolution.

I merely mention this as an example of how completely human effort can be frustrated where opinion is divided as to social aims. Hitherto, in the past, unity as to the fundamentals of social aims seems only to have come from religion, through religious control of education. I think it is noteworthy that the secularisation of education in Europe has only lasted two or three generations before collapsing, especially where it had been logically and thoroughly carried out, as in Italy and Germany, where the totalitarian parties have attempted to find an artificial substitute for the previous influence of the Church; the first example of this was, of course, provided by Russia. So far as I can judge, the substitute is of a very inferior brand in all three countries but its immediate aim is quite clearly to give to a people something of the unity of social aims and interests which I have been talking of.

I am afraid this is not very much to your point. You must take it merely as my reaction to your stimulating letter and enclosure.

[1] Maclean had written to Fisher from Wilson College, Bombay, referring to the world situation and saying he believed there was an urgent and challenging intellectual problem in economic relations — 'one well worthy of your powers' — and that 'it might make all the difference to mankind if you could arrange to put your unique strength into straightening things out in this respect'.

Fisher to J. Needham: 3 *November* 1956

I am returning the galley proof,[1] and I think your correspondent, or whoever wrote the marginal notes in red ink, has made some useful comments. Of course, in this sort of field the difficulty of a conscientious historian is to know what was taken for granted as common knowledge, as certain widely taught arithmetical processes may be, and what was novel and for some purposes effectual. I imagine one of the outstanding qualities of Chinese science is that they kept their eye on specific purposes, and though they may have used general or abstract considerations in ordering their thoughts, did not so much elaborate these in theoretical disquisitions, as the Greeks often did very elegantly and modern mathematicians often with intolerable prolixity, as write down a specific formulation for a particular job. I do not think that there is a higher and a lower in all this so much as a traditional difference in mode

and style. The difficulty I feel must bother the historian is that to the modern European mathematician the theory of finite differences can only be said to be introduced or used from the time of the first generalized and systematic treatises, and not from the time of such effectual private usage as undoubtedly Newton was doing in his early twenties. I understand that in fact most of the named theorems in this subject up to about 1850 should be called 'Newton's theorem', though only one of them bears his name.

In this case, and because it was Newton, I believe it would be admitted that finite differences were fully developed in the sixties of the seventeenth century, but if a nameless mathematical schoolmaster had used the same methods very adroitly for astronomical observations it might be 200 years before printed material and textbooks showed an acquaintance with the methods mastered long before.

[1] Presumably the proof of an extract from Needham's *Science and civilization in China*.

R.N. Salaman to Fisher: 20 *March* 1944

Probably you have seen in *Nature*, March 11th, p. 298, a short article by Dingwall on Telepathic Phenomena.

I would like to know whether, in your opinion, it is to be taken as serious evidence that such occur, because as far as I understand it the man with the cards is not actively wishing the guesser the answer, nor is the guesser making any wishful effort to learn what the other sees, but would simply seem to be making guesses which for some reason are influenced by the fact that another man at a distance has the cards in his hands.

I find it very difficult to attune one's mind to accept the interpretation that either party is influencing the other, but I gather the statistical evidence almost proves that such occurs.

If you have a spare moment, I would like very much to know what you think.

Fisher to R.N. Salaman: 22 *March* 1944

The question you ask me is one which appeals to me a great deal, and though I have constantly seen these claims to immensely high significance made on behalf of work with which I personally have had nothing to do, yet in all of the good many cases in which I have been asked to look at data actually secured with a view to demonstrating extra-sensory perception there has never been any such decisive evidence.

I suppose what gives rise to these confident statements is something like this case within my own experience, and of which I remember the details clearly:

A very rational and entirely honest girl who had become interested in supposed telepathy had formed the conclusion that one could not investigate evidence of supposed telepathy unless one had first a clear basis of knowledge as to the possibility or impossibility of clairvoyance. She therefore designed a test to see whether any considerable number of ordinary people possessed a clairvoyant faculty, at least in a slight degree. I think she grasped, and this also seemed to me enlightened, that for evidential value a slight but constant measure of successes among a large number of subjects was much more important than evidence of a knock-out star conjurer's performance done once under special conditions and not in any sense reproducible.

What she asked people to do was to turn up five cards in succession from a well-shuffled pack, noting in each case what they thought the card was going to be, and after turning it up, what it actually was. She thought that five cards at a time was the right amount, but asked each person to do this five times so as to provide the results of five guesses or attempted clairvues. I made out for her a system of scoring partial successes, as when you guess the King of Clubs and the Knave of Clubs turns up, framing the system so that the average score was zero, and the standard deviation for random guessing, I think, ten points, so that the total of 25 guesses had a standard error of 50, and the mean of 25 scores had standard error of 2.

She got to work and persuaded no less than 240 people to co-operate, finally producing 6000 records for examination. Before I saw them she had scored the whole lot and found positive scores well in excess of negative, and in the aggregate very significantly so.

Of course, some of her subjects may have cheated, and possibly those who reported the most improbable successes should be discounted, but even if one sets these aside the preponderance of small positive scores among the remainder requires an explanation. I was led to think by my own experience that though all but one or two of the collaborators may have been entirely honest, yet they may have vitiated the results just as much as if they had sent in false returns. I had myself received a set of forms to fill in, and when first I had them, sat down, did 5 guesses, and filled in one form. I was not successful, no interest was evoked, the forms went into a drawer and were forgotten. Later, further correspondence induced me to fill in a second form, but I never completed the five. What I think is that if a chance success had come my way in the first five trials, I should have been interested, completed the set (unless later experience was too disappointing), and sent in my return to be included with the others.

I was able to show that in fact the large body of data collected had been vitiated in just this sort of way, and the confirmation is important, for of course it is just as easy to make a hypothetical and unfounded objection to an experimental result as it is to make a false experimental claim. It happens that when people guess cards in numbers, certain [general?] preferences begin to show themselves quite strongly; red cards are guessed more often than black, odd numbers are guessed more often than even, and so on. In 6000 guesses, each card of the pack should have appeared about 120 times, but actually the frequencies had a very wide range, from about half of this amount nearly to 200. This of course proves nothing except that you could make money in the right company by offering slightly better than the calculated odds to anyone who will guess the card in your hand, provided you make sure that, on the whole, unpopular cards are the ones you hold. What is really informative about the card-guessing data that I'm speaking of is that, after tabulating frequency of choice of the cards *guessed*, one could quite independently tabulate the frequency with which each of these same cards was *drawn*. Here at least one might expect the frequencies to be in accordance with chance, namely equal numbers of Black and Red, equal numbers of Odd and Even, and so on; but the frequencies of cards drawn were also disturbed by some factor other than chance, and what I think was enlightening was that these frequencies were in fact a pale reflection of the frequencies appropriate to cards chosen. The differences were not so much as half as great, but they were regularly in the same direction. Consequently I think that one must infer that a large number of the cases in which unpopular cards were drawn have somehow been eliminated from the record in just such a way as would be brought about the suppression, by non-completion, of trials started inauspiciously.

It seems to be one of the ways in which faith moves mountains!

Fisher to E.B. Wilson: 5 *February* 1957

I see that I have not answered yet, as I had hoped to do, your letter of November 24th, in which you refer to Bridgman's contribution in *Science*[1] to the discussion on 'Science and the Supernatural' which that journal organized.

I believe a great deal is done towards clarifying this tangled situation by the realization that however complicated some of the cases may be, in which uncertain inductive inference is useful, there is yet an underlying simplicity recognizable in the simple disjunction: Either the hypothesis is false (including the milder faults of being incomplete, or imperfectly true), Or a very extraordinary coincidence has taken place. I do not believe that the practical use of this simple type of disjunction is misunderstood by practical experimenters, unless, and until, they come under the influence of attempts, originating in mathematical economists, to reinterpret this simple disjunction as an 'acceptance procedure' or 'decision function'.

In relation to parapsychology, I have never had the least hesitation in admitting that the data as reported do present the dilemma: Either there is something here which you do not understand, or a very remarkable coincidence has occurred, but it only presents this dilemma to those who are already satisfied (a) that they are not being hoaxed, (b) that the observation and recording is of scientific standard, and (c) that the reporting is unbiassed, i.e. that there has been no exaggeration, or the omission of mitigating circumstances. I do not at all believe, in the light of the attitude exhibited by those who report parapsychological work, that anyone has reason to be satisfied with these three preliminaries.

It is, to my mind, a very ominous sign that a parapsychologist should exhibit himself as insulted, and throw off the gestures of moral indignation, at the thought that he is not strictly and habitually truthful, even when his reputation and career is largely at stake. I think the psalmist was more nearly right than the parapsychologist. In genetics I do not think that touchy dignity should prevent one from saying, 'Here is some material which I have bred, and which I can hand over to you so that you can verify the remarkable phenomena which I believe it will exhibit'. Without being able to take some such attitude to a critic, it seems to me sheer impudence for Soal and others to claim that they have provided anything like a scientific demonstration. Years ago in my book on *The Design of Experiments*, in Section 7 headed 'The Test of Significance', I tried to express this idea by saying:

'In order to assert that a natural phenomen is experimentally demonstrable we need, not an isolated record, but a reliable method of procedure. In relation to the test of significance, we may say that a phenomenon is experimentally demonstrable when we know how to conduct an experiment which will rarely fail to give us a statistically significant result.'

I believe experimenters in the Natural Sciences will take that as not too hard a saying, but it appears to be fatal to the parapsychologists. I suggest, therefore, that the weakness lies not with tests of significance as applied to their own purposes in the Natural Sciences, as Bridgman seems to suggest, but with the peculiar use that psychologists have tried to make of them.

[1] Bridgman, P.W. (1956). Probability, logic and ESP. *Science* **123**, 15–17.

Click here for next section