*Personal*

2 Sycamore Terrace,

Corstorphine,

Midlothian.

April 17, 1931.

Dear Dr Fisher,

Many thanks for your letter of this morning, which calls
for acknowledgment and a few small explanations in concluding the matter
I was really quite in the dark regarding all the controversial issues
involved ( Wishart's attitude having been extremely non-committal and
correct ) but now in a kind of way I do see them. As a matter of fact I
did not even know until your letter that it was Comrie's committee; I
only ~~inferred~~ inferred that you and Wishart must belong to it; and I do
not yet know, nor wish to know, who else belongs. I now see the impor-
tance of the question of the cost of printing differences and certain of
the other points involved, but the controversy seems unfortunate, and I
have no wish to add to acrimony.

Let me say at once that I have come to like Jordan's method very much,
and have no desire that any results of mine should be interpreted as
belittling it. ( I am writing to Wishart to insist on this. ) In its
favour I shall add this to my other remarks. You mentioned the psycho-
logical aspect, which of course must be borne in mind as well as the
times. In working by Jordan it is most heartening to see three good
linear approximations growing under you, and to know that the Jordan
differences will certainly be small. Personally I think this excellent
fun.

The committee, you remark, was ignorant of Jordan when you first men-
tioned it. So was I, I fully admit, and when Professor Whittaker first
handed me Wishart's letter in February the simplicity of the idea
struck me so forcibly that I felt the bottom must be knocked out of all
the other extant formulae. Wishart has just now given me the "Metron"
reference, for which I wrote asking him, but I have not yet been able
to consult it, though I see at once how the formula may be deduced from

Everett. In my own lecture-notes I had deduced it simply from Bessel, and from Stirling, by a similar line, a second formula which Jordan doubtless gives, useful for 5- or 7-point interpolation,

$$f_\theta = f_0 + \theta g_2 + \frac{(\theta+1)\theta(\theta-1)}{3!}(g_4 - 2g_2) + \frac{(\theta+2)(\theta+1)\theta(\theta-1)(\theta-2)}{5!}(g_6 - 4g_4 + 5g_2) + \cdots ,$$

where
$$g_{2r} = \frac{1}{2r}\{(r+\theta)(f_r - f_0) + (r-\theta)(f_0 - f_{-r})\},$$
$$= \frac{1}{2r}\{(r+\theta)f_r - 2\theta f_0 + (r-\theta)f_{-r}\},$$

a linear interpolation on first differences of $f$ at various symmetrical ranges. Indeed many other interesting possibilities are opened up.

By the way, my work, though done at high speed, was not hurried. The strain was ~~purely~~ largely due to being out of form. It is only that, as you will readily understand, being, as I know, adept in mental arithmetic, the more rapidly one does a mental calculation the more accurate it is; and this habit, to one not in the way of doing jog-trot routine, is a possibly bad habit that transfers itself to machine-work. Probably a relaxation of speed would have put 20 per cent on times all round. The truth is, when Jordan began to show up so favourably, I was keenly excited, and a certain spirit of competition made me put the two of them on their mettle. And I do not mind it being pointed out that I am not a trained or professional computer; indeed, when I examined the "Millionaire" again, I found that for years I had overlooked one of its most valuable features. What that was I do not care to confess, — you would be too highly amused.

By "the remarks of an isolated individual", a phrase I let slip, but might well have spared, in my letter of the 13th, I meant my first remarks to Wishart for March 5. They were written, I now see, under only partial comprehension of the issue, and could have been withheld without loss and perhaps with advantage. But my present tests I do regard as a contribution to experimental decision, though possibly more factual than statistical. The onus of testing, if it was to be done in Edinburgh at all, devolved on me; for our students, under my colleague Gibb, do not

use machines, except one or two individuals occasionally, but Crelle, logarithms and Barlow; and Gibb himself, a much more experienced computer than I, with a training in calculation for ballistics in war-time, was exceptionally busy with other things. Lidstone advised me, since, *then and* and has insisted on it, not to take it up again, saying that the only satisfactory criterion would be a statistical comparison of a large number of times recorded by professional computers trained in both methods, with full regard to different types of machine. There is no doubt he is quite right, and I leave it. But it was very interesting.

The principal reason why I took it on again against his advice was this: it would not be the first time I had received salutary correction in an erroneous view at your hands, either indirectly or from your writings, and I was fully prepared once again to find myself wrong. Once, in the matter of an asymptotic series, I felt I must be wrong, yet the machine seemed to bely it, and so I adopted a false view which you summarily disposed of. Doubtless you have forgotten it; but one can only be grateful for so quick a removal of error.

I am,

Yours sincerely,

A. C. Aitken.