COPY

19th September, 1956.

My dear Alf,

For various reasons this summer I felt I should like to have the next couple of orders of magnitude in the Cornish and Fisher formulae, and I thought you ought to be the first to have the new terms, so that they could be recovered, if they were lost before printing. The four orders we gave can be written

			Divisor
I	c	x^1-1	6
III	bc	$-(x^3-1)$	6
	e	$1-6+3$	120
	cd	$-(1-5+2)$	24
	c^3	$12-53+14$	324

			Divisor
II	b	x	2
	d	x^3-3x	24
	c^2	$-(2x^3-5x)$	36
IV	b^2	$-x$	8
	bd	$-(x^3-3x)$	16
	bc^2	$5(2x^3-5x)$	72
	f	$1-10+15x$	720
	ce	$-(2-17+21)$	180
	d^2	$-(3-24+29)$	384
	c^2d	$14-103+104$	288
	c^4	$-(252-1688+1511$	7776

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ITEM —

The signs depend on the number of factors, and the power of the polynomial goes down by 2 for each factor b. In fact terms involving b seem to be easily derived from those of lower order. The others, so far as I know, have to be worked out the hard way, as we did it. Overleaf are the 5th and 6th corrections.

One reason why I wanted these was to take a step further the curious formula I give (p. 63) in my new book (of which Henry Bennett has an advanced copy) in connection with the fiducial argument.

If one puts in the cumulants of the binomial up to the seventh, one gets the expansion for a binomial frequency:

$$a = pN + x\sqrt{pqN} + (q-p)(x^2-1)/6$$

$$+\frac{1}{72\sqrt{pqN}}\begin{Bmatrix} x^3 & x \\ -1 & 1 \\ -2 & 14 \end{Bmatrix} pq$$

$$\frac{q-p}{3240pqN}\begin{Bmatrix} x^4 & x^2 & 1 \\ 12 & -17 & -19 \\ 6 & 14 & -32 \end{Bmatrix} pq$$

$$\frac{1}{3240\times 48(pqN)^{3/2}}\begin{Bmatrix} x^5 & x^3 & x \\ -207 & 388 & 911 \\ 468 & -728 & -1996 \\ 36 & 1024 & -1732 \end{Bmatrix} \begin{matrix} pq \\ p^2q^2 \end{matrix}$$

$$\frac{q-p}{3240\times 504(pqN)^2}\begin{Bmatrix} x^6 & x^4 & x^2 & 1 \\ 912 & -2151 & -7883 & 656 \\ -960 & 1044 & 8236 & 3032 \\ 48 & -972 & -3692 & 5888 \end{Bmatrix} \begin{matrix} p \\ p^2 \end{matrix}$$

V

b^3c	x^3-1	6
b^2e	$-(1-6+3)$	60
b^2cd	$1-5+2$	8
b^2c^3	$-(12-53+17)$	81
bg	$1-15+45-15$	5040
bcf	$-(1-13+33-9)$	432
bde	$-(1-12+29-8)$	240
bc^2e	$16-181+393-90$	1080
bcd^2	$12-129+291-64$	576
bc^3d	$-(80-803+1513-304)$	1296
bc^5	$960-8937+15062-2651$	3240×9

VI

b^3	x	16
b^2d	$5(x^3-3x)$	64
b^2c^2	$-35(2x^3-5x)$	288
bf	$-(1-10+15)$	288
bce	$7(2-17+21)$	360
bd^2	$7(3-24+29)$	768
bc^2d	$-(14-103+107)$	64
bc^4	$11(252-1688+1511)$	324×48
h	$1-21+105-105$	40320
cg	$-(2-37+160-135)$	5040
df	$-(1-17+69-57)$	1152
e^2	$-(2-33+132-108)$	3600
c^4f	$18-293+1100-795$	324×16
cde	$18-273+974-695$	1440
d^3	$9-131+451-321$	3072
c^3e	$-(396-5708+18755-11811)$	324×60
c^2d^2	$-(594-8193+26006-16367)$	576×24
c^4d	$5148-67004+195259-109553$	1296×48
c^6	$-(154440-1,887684+5,073714-2,542637)$	1296×3240

The expansion in the book is the result of using this as an equation in $\underline{p}$ to express $\underline{p}$ in terms of the observation $\underline{a}$,$\underline{b}$ out of N as below:

$$\mu_N = \frac{a}{N} - \sqrt{\frac{ab}{N^3}}\,x + \frac{b-a}{6N}(2x^2+1)$$

$$+\frac{1}{72N^{3/2}\sqrt{ab}}\begin{Bmatrix} x^3 & x & \\ -2 & -7 & N^2 \\ 26 & 34 & ab \end{Bmatrix}$$

$$\frac{b-a}{3240N^2ab}\begin{Bmatrix} x^4 & x^2 & 1 & \\ -12 & 17 & 19 & N^2 \\ -276 & -644 & -148 & ab \end{Bmatrix}$$

$$\frac{1}{48\times3240N^{5/2}(ab)^{1/2}}\begin{Bmatrix} x^5 & x^3 & x & \\ -36 & 470 & 265 & N^4 \\ 936 & 5564 & 5188 & N^2ab \\ -11268 & -39712 & -23804 & a^2b^2 \end{Bmatrix}$$

$$\frac{b-a}{126\times3240N^3a^2b^2}\begin{Bmatrix} x^6 & x^4 & x^2 & 1 & \\ 24 & 207 & -418 & -563 & N^4 \\ 240 & -261 & -2059 & -758 & N^2ab \\ 5784 & 28026 & 29882 & 3400 & a^2b^2 \end{Bmatrix}$$

where $\underline{x}$ is a normal deviate, so that

$$\bar{\mu} = \frac{a}{N} + \frac{b-a}{2N}\left(\frac{1}{N} - \frac{1}{N^2} + \frac{1}{N^3} - \cdots\right)$$

so far as these terms go. I have not yet reworked the variance.

Remember me kindly to Mrs. Cornish and to the family, of whom I have the nicest memories. Henry thinks I ought to make another visit, when it can be fixed.

Sincerely yours,

(signed) Ronald. (RAF)

COMMONWEALTH OF AUSTRALIA

COMMONWEALTH SCIENTIFIC AND INDUSTRIAL RESEARCH ORGANIZATION

DIVISION OF MATHEMATICAL STATISTICS

TELEPHONE: W 2778

UNIVERSITY OF ADELAIDE,
ADELAIDE

21 : 11 : 56.

Dear Sir Ronald,

I must apologise for the long delay in answering your letter of 19:9:56. I had hoped to thoroughly read 'Statistical Methods and Scientific Inference' before replying, but the arrival of your latest note of Nov. 16th calls for a change of plan.

Many thanks indeed for the details of the two new terms of the expansion which will no doubt find many uses. As for your new book, I have a copy which only came into my hands recently and is labelled "Rough Proof". I really haven't had the opportunity to read it thoroughly, but from what I have seen, it should finally put the skids under all those people who cannot, or will not, appreciate the significance of what you have done. I should like to write again when I have read the book through once more and have seen your recent paper on the errors-in-variables problem published by Pearson and Stanley.

Enclosed herewith is the list of publications which you requested; I have added 5 which will be ready for the press within the next few months; the main hold up is due to the preparation

of diagrams. I have appended models to the list to give you some indication of what these papers are about.

I hope to spend some time in Britain and N. America in 1958. Nothing definite has been fixed yet, but a rough outline of what I had in mind is to leave here during February or March and go direct to England, spending until the end of the summer there, then proceeding to Ottawa for the Biometric Conference and after that to the States. While in England I would like to spend as much time as possible with you and Jones, I only hope you will see your way clear to have me.

I have two items which I don't think I've told you about. Almost exactly 12 months after your visit, the Executive Committee established my Group with full Divisional status in C.S.I.R.O. and gave me the job as Chief. Our work now covers practically the whole range of C.S.I.R.O. activities and with the nucleus of very good statisticians we can proceed with a worthwhile programme of research. At about the same time the Australian Academy of Science was founded, and I was fortunate enough to be elected to a Fellowship.

To date we haven't seen much of Henry Bennett. He has been pretty well occupied organising courses and the department generally to suit his ideas, but I think he now has all this well under control, so we should soon see more of him.

Henry didn't mention to me his proposals about your second visit, but quite independently I have given this some consideration. It seems to me

that it would be much more preferable to arrange for you to stay longer than you did in 1953. What is your reaction to the suggestion that you take a temporary appointment as a visiting Research Fellow and spend from 3 to 12 months here in C.S.I.R.O.? I have not made any formal proposal to the Executive but I am quite confident that it could be satisfactorily arranged.

DIXA

With best wishes and kindest regards,

Yours

Alfred Russel Wallace

30th November, 1956.

My dear Alf,

Many thanks for your letter and list of publications. I am sorry you had trouble in getting a copy of my book. I had heard of some copies, bound up from rough proofs, having got into circulation and being snapped up in advance of the release to the general public, but that was quite a long while ago now. If you are in doubt about alterations or inclusion of new matter, I sent Henry Bennett an authentic copy as soon as I had my first batch. I am sure you can borrow that, as he was very familiar with most of the material before publication.

As you surmise, there are a good many people who will not like it at all, and I imagine, by every trick they know, will try to bawl it down. This might even succeed, for as long as I shall live, in the United States, but I do not expect it to be successful in other English-speaking countries.

I should like to make the effort to get to Ottawa in 1958, but commitments seem to get thicker, rather than thinner, after retirement. I should greatly like to take advantage of your

proposal for something like six months in the Commonwealth, which would have to be ~~something like~~ its half-year September to March, if I am to look after a growing season of Lythrum over here, though I imagine the other half of the year is better for those not acclimatized to your sunshine.

I am writing at once in order not to delay my reply, but before I have fully perused your long letter, so excuse me if I seem to ignore much news that should have interested me greatly.

Sincerely yours,

(R.A.F.)

I sent a cable about India, as I thought you might have to decide quickly.

18th December, 1950.

My dear Alf,

I am sending herewith two sheets:

(a) Smoothed data of Median Winter Rainfall for Adelaide, as I thought you might like to see the effect of using a better smoothing formula.

(b) A short discussion of what such a formula does, and demonstration that what I have used has a sharper cut-off than the 10-year mean.

I have written to Stoy at Cape Town, so far without reply.

Wishing you all the best of time at Christmas,

Yours,

(R.A.F.)

Encs.

Smoothed date of median winter rain — Adelaide

	JUNE	JULY	AUG
1844	.	12	.
1845	.	20	.
1846	.	24	.
1847	.	13	.
1848	.	19	.
1849	.	13	.
1850	.	10	.
1851	.	11	.
1852	.	9	.
1853	.	3	.
1854	.	7	.
1855	.	1	.
1856	16	.	.
1857	10	.	.
1858	12	.	.
1859	11	.	.
1860	19	.	.
1861	29	.	.
1862	.	2	.
1863	.	15	.
1864	.	21	.
1865	.	14	.
1866	.	11	.
1867	.	14	.
1868	.	17	.
1869	.	21	.
1870	.	23	.
1871	.	17	.
1872	.	13	.
1873	.	11	.
1874	27	.	.
1875	16	.	.
1876	16	.	.
1877	14	.	.
1878	16	.	.
1879	21	.	.
1880	24	.	.
1881	20	.	.
1882	.	1	.
1883	.	1	.
1884	.	5	.
1885	.	9	.
1886	.	4	.
1887	.	8	.
1888	.	15	.
1889	.	20	.
1890	.	21	.
1891	.	25	.
1892	.	25	.
1893	.	16	.
1894	.	11	.
1895	26	.	.
1896	12	.	.
1897	7	.	.
1898	7	.	.
1899	13	.	.
1900	23	.	.
1901	28	.	.
1902	29	.	.
1903	.	6	.
1904	.	7	.
1905	.	8	.
1906	.	7	.
1907	.	4	.
1908	.	2	.
1909	.	21	.
1910	.	28	.
1911	.	28	.
1912	.	.	2
1913	.	.	2
1914	.	24	.
1915	.	21	.
1916	.	19	.
1917	.	9	.
1918	.	1	.
1919	.	19	.
1920	.	17	.
1921	.	8	.
1922	.	4	.
1923	.	5	.
1924	29	.	.
1925	.	2	.
1926	.	13	.
1927	.	17	.
1928	.	22	.
1929	.	17	.
1930	.	16	.
1931	.	7	.
1932	.	13	.
1933	.	.	2
1934	.	27	.
1935	.	26	.
1936	.	28	.
1937	.	26	.
1938	.	17	.
1939	.	2	.
1940	14	.	.
1941	22	.	.
1942	28	.	.
1943	.	12	.
1944	.	2	.
1945	.	12	.
1946	.	25	.
1947	.	.	8
1948	.	.	9
1949	.	.	12
1950	.	21	.
1951	.	23	.
1952			
1953			
1954			
1955			

	A	CT
L	46	56
E	58	64
L	67	74
E	78	86
L	91	96
E	98	05
L	12	12
E	24	22
L	33	28
E	41	41
L	48	48

Any smoothing formula may be compared with an optical instrument nearly opaque to short wave lengths, and less so to others.

If T is the period and $\alpha = 360^\circ/T$, the angular change for one year, the ratio (r) of reduction of amplitude can be calculated, and thence the opacity, $1-r^2$.

For ten-year means

$$r = \frac{1}{5}\left(\cos\frac{\alpha}{2} + \cos\frac{3\alpha}{2} + \cos\frac{5\alpha}{2} + \cos\frac{7\alpha}{2} + \cos\frac{9\alpha}{2}\right).$$

In order to conserve the wave form better, the smoothing formula

$$\left.\begin{aligned} &-36u_{n-5} + 9u_{n-4} + 44u_{n-3} + 69u_{n-2} + 84u_{n-1} + 89u_n \\ &-36u_{n+5} + 9u_{n+4} + 44u_{n+3} + 69u_{n+2} + 84u_{n+1} \end{aligned}\right\} + 429$$

was used; in this case the ratio of reduction is

$$(89 + 168\cos\alpha + 138\cos 2\alpha + 88\cos 3\alpha + 18\cos 4\alpha - 72\cos 5\alpha)/429$$

Some typical wave lengths are compared below:

	Amplitude ratio		Opacity %	
Wave length	10 year mean	Smoothing formula	10 year mean	Smoothing formula
30	.82851	.99449	31	1.1 %
24	.74002	.98689	45	2.6
20	.63925	.97368	59	5.2
18	.56713	.96086	68	7.7
15	.41654	.92337	83	15
12	.19319	.83181	96	31
10	0	.69418	100	52
8	-.18478	~~.41604~~ .29077	97	~~83~~ 92
6	-.17321	~~.24809~~ -.06760	97	~~94~~ 100
4	.14142	-.07226	98	99
3	-.10000	.11888	99	99
2	0	.14219	100	98

Any smoothing formula may be compared with an optical instrument nearly of zero
It is best wave length, and has its other

If T is the period in $d = 360°/T$, the angular change for one year, the ratio (r)
of reduction of amplitude can be calculated, and thence the opacity, $1 - r'$.

For ten-year means

$$r = \frac{1}{5}\left(\cos\frac{d}{2} + \cos\frac{3d}{2} + \cos\frac{5d}{2} + \cos\frac{7d}{2} + \cos\frac{9d}{2}\right).$$

In order to conserve the wave form better, the smoothing formula

$$\left.\begin{aligned} &-36u_{n-5} + 9u_{n-4} + 44u_{n-3} + 69u_{n-2} + 84u_{n-1} + 89u_n \\ &-36u_{n+5} + 9u_{n+4} + 44u_{n+3} + 69u_{n+2} + 84u_{n+1} \end{aligned}\right\} \div 429$$

was used; in this case the ratio of reduction is

$$\left(89 + 168\cos d + 138\cos 2d + 88\cos 3d + 18\cos 4d - 72\cos 5d\right) / 429$$

Some typical wave lengths are compared below

Wave length	Amplitude ratios: 10 year mean	Amplitude ratios: Smoothed formula	Opacity %: 10 year mean	Opacity %: Smoothed formula
30	·82851	·99449	31	1·1
24	·74002	·98689	45	2·6
20	·63925	·97368	59	5·2
18	·56713	·96086	68	7·7
15	·41654	·92337	83	15
12	·19319	·83181	96	31
10	0	·69418	100	52
8	−·18478	·24077	97	8392
6	−·17321	−·06760	94	100
4	·14142	−·07226	98	99
3	−·10000	·11888 ✓	99	99
2	0	·14214 ✓	100	98

·38
51 −11·7
54 − 1
−36
·19

2nd January, 1959

Professor Sir Ronald Fisher, F.R.S.,
Dept. of Genetics,
University of Cambridge,
Whittingehame Lodge,
44 Storey's Way,
CAMBRIDGE.

Dear Sir Ronald,

I have received Henry Bennett's suggestions for your lecture course in genetics at Adelaide, but I judge from Otto Frankel's letter that he has not yet let you know about subjects for Canberra lectures. Frankel has suggested a short course of three lectures in the field of the genetical theory of natural selection, but does not explain why he did not write to you directly, as I requested him to do, so I hope this last-minute advice does not inconvenience you.

Basten, the new Vice-Chancellor at Adelaide, has just told me that you have agreed to accept an honorary degree, and I may add that I find it very gratifying to learn that the University has had the good sense to do the right thing. The degree will be conferred at the Commemoration ceremony on April 8th. Usually these ceremonies are attended by about a thousand members of the general public, mainly people with a direct interest in the proceedings, and the Vice-Chancellor put it to me that I write, asking whether you would be prepared to address the assembly, for about thirty minutes, after receiving the degree. You will appreciate that only very rarely is the University presented with an opportunity like this of honouring a world scientific authority, so their request for the oration is understandable; but I emphasize that you are perfectly free in making your decision, and that there is no hint of your being called upon to sing for your supper. On the contrary, the University will be honoured if you choose to give the address. Naturally, also, you are free to select your subject. Incidentally, Marston will receive an honorary degree on the same day.

Many thanks for your letter of Dec. 18th, which was not received here until the 30th; the oscillations seem very clearly defined, but I have not yet had the opportunity to look closely at what you have done.

Looking forward to seeing you, and with kind regards,

Yours sincerely,

Alf

7th January, 1959.

Dear Alf,

I will be considering lectures and collecting printed matter, some of which I had better post to you in Adelaide so as not to pay for excess weight in inter-continental flying.

I am enclosing a note of 10 smoothing formulae which you might like to have as they are fairly laborious to work out afresh. The column of opacity refers to the percentage of random variance, which the formula excludes. I also give the equivalent formulae in central differences, of which the first term supplies the asymptotic value of the opacity for long waves. I.e. by doubling the first coefficient we get $6\alpha^4$ for the first 11-point formula (which is the one I have used), where α is the angle corresponding with one year, or 360° divided by the periodic time, and for the 15-point formula of the second series (which is the only one I could be tempted to use), $\frac{100}{11}\alpha^6$.

For what it is worth, the second series gives a sharper cut-off than the first series. For example, this 15-point formula has at an 8-year period 83% opacity, at 10 years only 28%, and at 12 years less than 12%, so it should preserve the wave form rather well, though it would be tedious enough to apply and I doubt if it will give appreciably better results than the one I have used.

Much as I should like to oblige Mr Basten, I do not think it a good idea for me to give a discourse on receiving an honorary degree. The trouble is that though it is no trouble to me to talk for half an hour on a technical subject where there is some reason to think that the audience will be interested, I should feel quite at a loss in addressing a general University congregation who would expect perhaps to hear about politics or education from the administrative standpoint; and of course if I spoke about that I might be quite rude!

Sincerely yours,

(RAF)

Enc.

Dr E. A. Cornish.

19th January, 1959.

My dear Alf,

This is a very brief note to let you know that I have secured Cape Town data. It shows a periodicity which is clearly the same as that you have discovered at Adelaide, with, I should judge, a smaller amplitude and remarkable, though perhaps not reliable, agreement in phase. I will send you the actual figures as soon as I can. In the end you will probably have to check them in Australia.

Sincerely yours,

(RAF)

? A short note from us both in Nature fairly soon, leaving fuller publication and discussion to you in the Phil. Trans.

J.G.C.

17th February, 1959

Professor Sir Ronald Fisher, F.R.S.,
Dept. of Genetics,
University of Cambridge,
Whittingehame Lodge,
44 Storey's Way, CAMBRIDGE.

Dear Sir Ronald,

Many thanks for your letter of 3rd inst. Since receiving it, the full details of your outward flight have been forwarded from Africa House, so we are now right up to date. I shall be in Sydney to meet you, and am looking forward to it with keen anticipation.

Your news about the Cape Town rainfall was very exciting, and certainly indicative of a major climatic effect. I am now chasing the Santiago (Chile) record, which I believe goes back as far as 1867. Complete analysis of both these sets of data should settle the question one way or the other, and reconsideration of Perth may also be very helpful.

As opportunity has been presented, I have gone ahead to use the two additional terms of our expansion. The formulae for t, χ^2 and z have been extended, and also the tables of the original paper. This should provide a good start for the new paper giving the additional terms.

I haven't heard anything further about the Academy meeting in May, but shall learn the latest developments when I go to Canberra later this week. I shall keep all the remaining news until I see you.

With kind regards,

Yours sincerely,

J.G.

P.S. Have been tracking down references to multiple t-distributions and have a strong feeling that M.R. NANDI may have published one or more papers on this topic. Have a reference to one of his papers on compound decision procedures —

Calcutta Statistical Association Bulletin 7. (1957) pp. 87–100

and this may refer to his earlier work.

This journal is unavailable in Australia; it is possible that you have off prints of Nandi's papers at Whittingehame Lodge so would you be so good as to look them over and see if there is anything likely to be useful here, and bring them with you? If you haven't anything in Cambridge would you make enquiries while in Calcutta and bring any offprints on from there? Sorry to bother you with this chore but I see no other way of tracing this work in a reasonable time. My previous experiences with Indians is that they ignore requests of this kind. With my anticipated thanks

J.G.

25th February, 1959.

My dear Alf,

We have been searching for Nandi in my offprint collection here, and the only clue is a paper dated 1936 on the chromosomes of rice. This is by B.K. Nandi, who may be the man you are thinking of, but who seems to have been a cytogeneticist 20 odd years ago, and I do not know whether he ever wrote on statistics.

I have all the Cape Town data now, but not quite fully reduced. I will bring the whole boiling with me.

Sincerely yours,

(RAF)