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Whole Article

REPORT ON CERTAIN ENTERIC FEVER INOCULATION STATISTICS.

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THE statistics in question were of two classes: (A) Incidence (B) Mortality Statistics. Under each of these headings the data belonged to two groups: (i) Indian experience; (ii) South African War experience. These two experiences were of a somewhat different character. That for India covered apparently the European army, of whatever branch and wherever distributed; that for South Africa was given partly by locality, partly by column, and partly by special hospital. Thus the Indian and South African experiences seem hardly comparable. Many of the groups in the South African experience are far too small to allow of any definite opinion being formed at all, having regard to the size of the probable error involved. Accordingly, it was needful to group them into larger series. Even thus the material appears to be so heterogeneous, and the results so irregular, that it must be doubtful how much weight is to be attributed to the different results.

The following groups were made on the basis of Lieutenant-Colonel Simpson's table (see Appendix A).

A. *Incidence.*

South Africa:

I. Hospital Staffs. See Appendix B, Table (1).
II. Garrison of Ladysmith. See Appendix B, Table (2).
III. Methuen's Column. See Appendix B, Table (3).
IV. Group of three special Regiments: 7th Hussars, C.I.V.'s, and 5th Battalion Manchesters. See Appendix B, Table (4).

India:

V. Army in India. Here I have taken only the data for years 1900 and 1901, as I am told that the statistics for 1899 are not fully comparable with those for the two later years. See Appendix B, Table (5).

B. *Mortality.*

South Africa:

I. Hospital Staffs. See Appendix B, Table (6).
II. Garrison of Ladysmith.* See Appendix B, Table (7).
III. Special Regiments. See Appendix B, Table (8).
IV. Group of special Hospitals, South Africa. See Appendix B, Table (9).
V. Various military Hospitals. See Appendix B, Table (10).

India:

VI. Army in India, 1900-1. See Appendix B, Table (11).

* No mortality experience was given in the case of Methuen's column.

After some consideration, it seemed best to reduce the above data by measuring (1) the association of inoculation and escape; (2) the association of inoculation and recovery by aid of the coefficient of correlation[1] extracted from the fourfold tables given in Appendix B, by the method treated in a memoir published in the *Philosophical Transactions*, vol. 195, A, pp. 1 to 47, 1900. The chief reason for using this method is that it enables us at once to compare the enteric fever inoculation returns with the results for vaccination and antitoxin treatment in the case of diphtheria, which have already been dealt with in this manner.

For example, taking the relation between deaths and recoveries, and presence and absence of vaccination scar in cases of small-pox, we have:[2]

	Correlation.
Metropolitan Asylums Board Returns,	
Epidemic 1893	0.595 ± 0.027
Epidemics for six towns	[illegible]

Epidemics for six towns	... 0.636 ± 0.009
Sheffield, 1887-8	... 0.769 ± 0.012
Homerton and Fulham, 1873-85	... 0.576 ± 0.009
London: Epidemic 1901 ...	... 0.578 ± 0.031
Glasgow: Epidemic 1900-1 ...	... 0.629 ± 0.030

We may safely say that the protective character of vaccination as against mortality after incurring small-pox is very substantial, and numerically it is represented by the value 0.6, which is fairly closely the actual result for the various epidemics which have at present been dealt with.

The correlation between recovery and the administration of antitoxin in laryngeal cases of diphtheria is 0.47, and between need for tracheotomy and the administration of antitoxin is 0.24.[3] We may therefore state that when the correlation is as high as 0.6 it is admitted that inoculation or vaccination is a distinct advantage; that, even when it is 0.24 to 0.47, it has been universally adopted, as in the case of the antitoxic serum treatment of diphtheria, by the medical profession. If, therefore, with enteric inoculation we find values as high as 0.5, we may rest satisfied with its very considerable effectiveness; if between 0.2 and 0.5, its justification stands on the same statistical footing as the antitoxin treatment of diphtheria; but, if we find the value below 0.2, or even 0.3, we ought to be rather doubtful of its efficiency.[4] Before working out the data the impression formed on my mind was that if the correlations turned out to be fairly constant and greater than 0.4, we might consider the case for inoculation against enteric proven; if irregular and less than 0.2, unproven—that is, it must be of very small value, possibly more than balanced by concomitant ills; while, if lying between 0.2 and 0.4, further cautious investigation by experimental inoculation seemed desirable.

The following table gives the results of calculating the correlation coefficients of the tables in Appendix B:

INOCULATION AGAINST ENTERIC FEVER:

Correlation between Immunity and Inoculation.

I.	Hospital Staffs	... + 0.373 ± 0.021
II.	Ladysmith Garrison ...	... + 0.445 ± 0.017
III.	Methuen's Column ...	... + 0.191 ± 0.026
IV.	Single Regiments ...	... + 0.021 ± 0.033
V.	Army in India	... + 0.100 ± 0.013
	Mean value	... + 0.226

Correlation between Mortality and Inoculation.

VI.	Hospital Staffs	... + 0.307 ± 0.128
VII.	Ladysmith Garrison ...	... − 0.010 ± 0.081
VIII.	Single Regiments ...	... + 0.300 ± 0.093
IX.	Special Hospitals ...	... + 0.119 ± 0.022
X.	Various military Hospitals	... + 0.194 ± 0.022
XI.	Army in India	... + 0.248 ± 0.050
	Mean value	... + 0.193

If we except IV and VII, the values of the correlations are at least twice (in the very sparse data of VI) and generally four, five, or more times their probable errors. From this standpoint we might say that they are all significant, but we are at once struck with the extreme irregularity and the lowness of the values reached. They are absolutely incomparable with the fairly steady and large values of the vaccination correlations obtained for different epidemics and towns. The effect of enteric inoculation is evidently largely influenced by difference of environment or of treatment.

Considering the correlation between immunity inoculation, the order of decreasing intensity is:

Ladysmith Garrison.
Hospital Staffs.
Methuen's Column.
Army in India.
Single Regiments.

Indeed, while the correlation is sensibly zero in the latter case, it is actually negative for the C.I.V.'s.

Now, the one thing that appears in broad outlines to corres. ond with the above arrangement is the extent to which the environment was changeable in the gronps referred to. The single regiments were very rapidly changing their environment—the C.I.V.'s, perhaps, more than any other group—the Ladysmith garrison least, and Methuen's column more mobile than the Ladysmith garrison, but far less than the single regiments. I am unaware how far the inoculated portions of the Indian army were seeing actual service of any kind during the years 1900 to 1901. But, so far as these statistics can guide us at all, they would seem to suggest that inoculation provided more immunity under constant than changing environment. It is difficult to explain this on the basis of any real theory of inoculation.[5] It would be easier to account for it if the apparent correlation between inoculation and immunity arose from the more cautious and careful men volunteering for inoculation. It is far more easy to be cautious under a constant environment than when the environment is nightly changing. The order of intensities is not inconsistent with a spurious correlation between immunity and inoculation arising from a real correlation between immunity and caution.

Putting on one side, however, the mysterious difference between the 0.445 of Ladysmith and the practically zero result for the single regiments, and assuming that the average correlation is real and 0.23 about, we may doubt whether this is sufficiently large to justify the treatment being adopted as routine. I think we may safely say that the 0.1 of the Indian army is not. Of course, if the Ladysmith garrison result could be practically reached for all units, whatever their environment, it would be worth doing, almost as effective as the diphtheria antitoxin treatment, although still far below vaccination efficiency. But can any differentiation be discovered, except the stationary nature of the troops between the treatment and environment at Ladysmith and those, say, of the C.I.V.'s? The matter is at least worth inquiring into.

If we turn from immunity to mortality we see that the results are even less conclusive. The order of *increasing* intensity is now:

Ladysmith Garrison.
Special Hospitals.
Military Hospitals.
Army in India.
Single Regiments.
Hospital Staffs.

The last item is of small value, as the mortality experience was very small. Generally this order is the same as that of decreasing immunity, or, roughly, the environment which causes inoculation to give the greatest immunity also causes, when immunity has broken down, inoculation to have relatively greater mortality. In fact, the inoculated in Ladysmith, once they had the disease, were more likely to die than the non-inoculated.[6]

Again, if any stress could be laid on the result, it is most mysterious that an inoculated man, if he caught enteric, profited less by his inoculation in Ladysmith than if he had been with one of the single mobile regiments; while he would be much less likely, relatively to the non-inoculated, to catch it in Ladysmith than in a mobile group.

Need for Further Investigation.

To sum up, it seems that, while most of the correlations both for immunity and recovery are distinctly sensible, having regard to their probable errors, yet they are so irregular that little reliance can be placed upon them as representing any definite uniform effect. They certainly appear to fall into that range of intensity which would justify suspension of the operation as a routine method. But the fact that the correlation, while sensible—and, in the case of the Ladysmith garrison, moderately substantial—is subject to remarkable variations, seems to urgently suggest the need for further investigation.

This might take a two-fold form :

1. An inquiry as to what happened at Ladysmith. Did the

serum or the inoculation differ in any way from that usual in India or that administered to the C.I.V.'s. On the whole, were the inoculated and uninoculated at Ladysmith in different units, and, if so, was the exposure to risk the same in both cases?

2. An experimental inquiry. If further experimental inoculations were made, which seem fairly justified by the Ladysmith if not by the other returns, the greatest care ought to be taken to get homogeneous material, that is, men of like

caution, subjected to the same environment. Assuming that the inoculation is not more than a temporary inconvenience, it would seem to be possible to call for volunteers, but while keeping a register of all men who volunteered, only to inoculate every second volunteer. In this way any spurious effect really resulting from a correlation between immunity and caution would be got rid of.

But admitting that such an experiment might conclusively answer whether there is a correlation between immunity and inoculation, and, further, that inquiry might suggest a means of thrusting up the general mean result nearer the Ladysmith figure, it seems to me that the present statistics show that with the existing serum or the method of administration it is extremely unlikely that the effect of enteric inoculation will be as good as the antitoxin diphtheria treatment and will certainly fall far short of the mortality[7] result in the case of vaccination.

If it were not presumption for me with no special knowledge whatever on the subject to give an opinion, I should say that the data indicate that a more effective serum or effective method of administration must be found before inoculation ought to become a routine practice in the army. The statistics suggest the possibility of the discovery of a more perfect process, rather than indicate that the existing method, even if it be shown to give a slight correlation between immunity and inoculation, is really worth continuing as a universal rule of the service. Hence such an experiment as that indicated above should be made in the first place with a view to ascertaining whether any inoculation is likely to prove useful. If it showed a definite correlation in homogeneous material of only 0.2 or 0.3, I think the right conclusion to draw would be not that it was desirable to inoculate the whole army, but that improvement of the serum and method of dosing, with a view to a far higher correlation, should be attempted. In other words, the "experiment" might demonstrate that this *first* step to a reasonably effective preventive treatment was not a false one.

APPENDIX A.

INOCULATION.

Group.	Inoculated.			Non-Inoculated.		
	Strength.	Cases.	Deaths.	Strength.	Cases.	Deaths.
Staff Portland Hospital	28	7	—	13	3	1
" I. Y. Deelfort	59	4	—	25	4	—
" I. Y. Pa. (1)	12	2	—	38	3	—
" I. Y. Pa. (2)	20	1	—	34	4	—
" No. 8 General Hospital	21	5	—	110	44	8
" No. 9 " "	87	11	1	47	13	2
" 2nd Sc. Red Cross	70	2	1	12	4	1
	297	32	2	279	75	12
Garrison, Ladysmith	1,705	35	8	10,529	1,489	329
Methuen's Column...	2,535	26	?	10,981	257	?
	4,240	61		21,510	1,746	
5th Battalion Manchesters	200	3	—	547	23	7

C.I.V.'s	700	60	9	494	39	11
7th Hussars	307	9	—	244	20	3
	1,207	72	9	1,285	82	21
India, 1899	4,502	44	9	25,851	657	146
,, 1900	5,999	52	8	54,554	731	224
,, 1901	4,883	32	3	55,955	744	199
	15,384	128	20	136,360	2,132	569
Tintown Hospital	—	30	2	—	265	5
No. 7 General Hospital	—	137	3	—	1,017	58
Im. Yeo. Hospital	—	47	4	—	301	26
No. 9 General Hospital	—	387	32	—	586	64
Irish Hospital	—	80	5	—	592	74
No. 6 General Hospital	—	50	5	—	477	58
No. 1 ,, ,, ...	—	32	6	—	194	22
Portland Hospital	—	54	4	—	178	25
Station. Hospital, Harrismith... ...	—	263	18	—	947	135
Scot. Nat. Red Cross	—	15	1	—	70	10
No. 5 Station. Hospital	—	24	2	—	53	8
No. 2 General Hospital	—	28	3	—	202	31
Orange River Hospital	—	27	1	—	109	22
	—	1,174	86	—	4,991	538
Various Military Hospitals	—	764	63	—	3,374	510

APPENDIX B.

A.—Incidence Experience.

	Inoculated.	Non-inoculated.	Totals.
	1. Hospital Staffs in South Africa.		
Escaped	265	204	469
Cases	32	75	107
Totals	297	279	576
	2. Garrison of Ladysmith in South Africa.		
Escaped	1,670	9,040	10,710
Cases	35	1,489	1,524
Totals	1,705	10,529	12,234
	3. Methuen's Column in South Africa.		
Escaped	2,509	10,724	13,233
Cases	26	257	283
Totals	2,535	10,981	13,516
	4. Single Regiments in South Africa.		
Escaped	1,135	1,203	2,338
Cases	72	82	154
Totals	1,207	1,285	2,492
	5. Army in India.		
Escaped	10,798	109,034	119,832
Cases	84	1,475	1,559
Totals	10,882	110,509	121,391

B.—Mortality Experience.

	6. Hospital Staffs in South Africa.		
Recovered	30	63	93
Died	2	12	14
Totals	32	75	107
	7. Garrison of Ladysmith.		
Recovered	27	1,160	1,187
Died	8	329	337
Totals	35	1,489	1,524
	8. Special Regiments in South Africa.		
Recovered	63	61	124
Died	9	21	30
Totals	72	82	154
	9. Special Hospitals in South Africa.		
Recovered	1,088	4,453	5,541
Died	86	538	624
Totals	1,174	4,991	6,165
	10. Various Military Hospitals of South Africa.		
Recovered	701	2,864	3,565
Died	63	510	573

Totals	704	3,374	4,130

11. Army in India, 1900-1.

Recovered	73	1,052	1,125
Died	11	423	434
Totals	84	1,475	1,559

NOTES AND REFERENCES.

[1] Those who have not had occasion to study the Theory of Correlation may possibly find some difficulty in attaching a definite meaning to the expression "coefficient of correlation," usually denoted by the letter "r." Without going into the theory of the subject, I offer a few explanatory remarks which may to some extent clear up the difficulty. When two characters have no correlation at all then $r = 0$; when they are completely correlated, or when the idea of correlation passes over into that of cause and effect, $r = 1$. The extreme limits between no relationship and cause and effect being 0 and 1, r may take any value between the limits, the value depending on the intensity of the relationship between the two characters. We may calculate r for a pair of characters in one class—for example, for right and left femur in man, or for two classes with respect to a selected character—for example, for English middle-class fathers and sons with respect to stature. Many such calculations have been made, and from the mass of results already obtained I select the following:

High correlation 0.75 to 1.	Right and left femur in man	$r = 0.96$
	Stature and femur in man	$r = 0.80$
Considerable correlation 0.50 to 0.75.	Weight and length of new-born infants ...	$r = 0.63$
	Head breadth and face breadth in criminals...	$r = 0.62$
	Vaccination and recovery in cases of small-pox	$r = 0.60$
	English fathers and sons (stature)	$r = 0.51$
Moderate correlation 0.25 to 0.50.	English fathers and daughters (eye colour) ...	$r = 0.44$
	Low barometer (Norway) and high barometer (Lisbon)	$r = 0.30$
Low correlation 0 to 0.25.	Strength of pull and stature in women... ...	$r = 0.22$
	Size of family for mother and daughter (in man)	$r = 0.11$

[2] Macdonell, *Biometrika*, vol. i, p. 375. et seq., and vol. ii, et seq. [3] Pearson, *Phil. Trans.*, vol. cxcv, A. p. 45. [4] Writing in 1900 of the diphtheria antitoxin statistics, I said: The correlation "between administration of antitoxin and recovery in laryngeal cases is substantial. But the relationship is by no means so great as in the case of vaccination, and if its magnitude justifies the use of antitoxin, even when balanced against other ills which may follow in its train, it does not justify the sweeping statements of its effectiveness, which I have heard made by medical friends It seems, until wider statistics are forthcoming, a case for cautiously feeling the

Key Passage(s) Whole Article/Book JLL Article(s) Acknowledgements Context

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