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THE RELATION OF INFANTILE MORTALITY TO MORTALITY IN SUBSEQUENT LIFE.

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[Read before the Royal Statistical Society, February 20, 1917,
the President, Sir BERNARD MALLET, K.C.B., in the Chair.]

THE subject of infantile mortality is one of the very greatest importance. In the last ten years the problem of the waste of child life has come more and more prominently to the front. At the present moment administrative measures of all kinds are being taken with a view to save infantile lives. There are three points of view. Among many there is a strong opinion that every child's life saved adds another potentially fit member to the working power of the state. The holders of this opinion are at the present moment working for all they are worth to make proselytes. Such publicist action is not confined to those who have some right to speak in the matter, but it is a present-day fashionable fad for ladies of position and others who either have no children themselves or have had their children brought up by strangers in their houses to start societies to raise funds from the public with a view to popularize and extend some specious formula of quackery. Now it must be quite obvious that such persons, whether informed persons or enthusiastic ignorant persons, at best represent but one side of the truth.

There is secondly, as is only to be expected, an opposing group. The chief of these is Professor Karl Pearson, who seems to take quite definitely the view—I am sorry if I misinterpret—that measures such as those taken to save infantile life can only lead to an increase in the number of unfit persons whom the nation has ultimately to provide for. Such a view, if correctly interpreted, is as obviously onesided.

There is yet a third party, and this party maintains that, given moderately healthy conditions of life, the number of unfit is very much smaller than is assumed by Professor Pearson, and that the unfitness which develops is to a very large extent due to the environmental conditions to which the child is subjected during the early years of its life. To this party belong many of the most able medical officers of health in the country. One of these lately told me that if his Health Committee would give into his hands the

whole money spent on health administration and allow him to allocate the spending, he would do more good in ten years than the present methods would do in thirty. Others temporize, and, though they feel that the best results by far will follow the systematic improvement of the general conditions of life, get caught in the vortex, and spend much of their energy in directions which their better judgment tells them give but a second best method of attack.

It does not concern us in this place, but the manner in which energy is being wasted brings a very grave feeling of misgiving regarding present-day tendencies. Hundreds of slums exist which should be cleared away, as there is no possibility of healthy human life in such places. Human death is the best incentive to sanitary reform, and if these present-day workers whose chief desire is to save infantile life, prove that a lower death-rate can be obtained in the worst slums, one of the most effective engines of progress will be put out of action for a generation, and many innocent children, unfortunately born under conditions which should not exist, will have to spend their early years in circumstances which are a disgrace not only to Christianity but even to the purely pagan deities of science.

It seems advisable, in view of these differences of opinion, that such statistical evidence as exists should be gathered together and criticized so as to determine how far any of these opinions are justified.

Now there are many documents relating to health conditions published between 1840 and 1900 which contain nothing to which modern statistics can offer a word of objection, documents which go to the root of the matter. Unfortunately with the beginning of the present century much loose thinking was introduced. The health officers were many of them men of narrow outlook, and in addition there was a new school of critics, a school useful at the present moment, but not required had the health statistics been in the hands of the giants of last century.

The first work published on death-rates as indicative of natural selection from the pure statistical side was by Professor Karl Pearson. (1.) In this he showed from certain definite statistics that long life and short life are to a certain extent hereditary, giving statistical corroboration to a fact which had hitherto been assumed as so well known as not to need any proof, though not necessarily true on that account. Whatever the result of this statistical inquiry, however, has been, whether positive or negative, is immaterial for the purposes of the present inquiry, as the result has no bearing one way or another on the special problems at issue.

The next contribution was by Dr. Newsholme and Mr. Yule. Mr. Yule's contribution to this subject is, I think, much the soundest which has been made up to the present moment. (4.) Unfortunately, however, Mr. Yule felt compelled to say that the evidence at his disposal did not seem to justify any definite conclusion, and in this matter Mr. Yule's opinion must, I think, be taken as correct.

Following this, Dr. Snow published an investigation in which he came to the quite definite conclusion that the more children who die, below the ages of three years, the healthier the next five years of life. (7.) This communication will be discussed later. A point, which must be specially mentioned however in this place, is that he criticized Mr. Yule's statistics in that they applied to England and Wales as a whole. Not once but many times he returns to this point, and maintains that such a method obscures seriously the conclusions which might be ascertained from the evidence. In this he is right, but not I think for the reasons he alleges. In his next contribution to the subject Professor Pearson however takes his data from the life tables for England and Wales as a whole. In this communication Professor Pearson tries to show that infantile mortality exercises a definite selective effect. (6.) This paper was criticized by Dr. Newsholme, on the ground of the methods of measurement of the death-rates used, but he himself, however, did not put his criticism to the only useful proof, namely, that of arithmetical calculation. (5.) Later Miss Elderton and Professor Pearson, by applying the new method of Variate Differences, claim to have proved that the conclusion before arrived at by the latter is well founded. (3.)

It seems to me that the best method of proceeding will be to discuss the purely statistical evidence in the first place. The article of Professor Pearson in the Royal Society's *Proceedings* of 1912 will be taken as the text, the choosing of a text being clearly legitimately open to any author who has been born north of the Tweed.

A discursus, however, is necessary. It must be remembered that there is at present no sound method of measuring the mortality below 5 years of age. All are alike purely empirical, and much work will be required before any results which can be called scientific are arrived at. It is an undiscovered country from whose bourne if any traveller has returned it is quite certain that he has brought no exact information. This is usually not sufficiently realized by writers on vital statistics, and by the statisticians who determine to discuss the problem.

This uncertainty of measure, however, was quite well known to the

founders of vital statistics in the middle of last century. They saw quite clearly the great difficulty in obtaining a method which was exact. All they could hope for was a measure which would serve their purposes, and realizing perfectly well that the figures they used were mere approximations, used them as such, not trying to draw conclusions unless they were satisfied that the statistics were such as to give a sufficient basis to enable them to hazard a guess. It is always a guess with Dr. Farr in England and with Dr. Russell in Glasgow. Both being men of great intuition, and the latter at least mathematically trained, their guesses are mostly correct. It is one thing, however, for such men to make approximations. It is quite another for their successors to take these approximations as exact results, and this is what has happened.

The measures which were chosen by these older writers were two : (1) the infantile mortality rate, which they defined as the ratio of the number of deaths occurring in each year to the number of births in the same year, expressing the results in parts per thousand, and (2) the childhood mortality rate, which was defined as the ratio between the number of deaths per thousand persons living between the ages of 0 and 5 years. More rarely they use the same ratio for the ages between 1 and 5 years. Both of these are very rough measures.

With these provisos the methods of measuring used or suggested for use at the present day will be defined.

Professor Pearson chooses the life-table figures as his basis, and he takes for the infantile mortality the number observed to die in these tables in the first year of life : this will be denoted by r_i . For the mortality between 1 and 5 years he takes the number dying during these years per thousand survivors to the age of 1 year : this will be denoted by r_c .

Dr. Newsholme objects, and calculates the infantile mortality by the usual method, namely, the number of deaths occurring in each year per thousand born. This being the method used by the Registrar-General will be denoted by R_i .

For the measures of the mortality between 1 and 5 years Dr. Newsholme takes the number of deaths between the ages of 1 and 5 years per thousand persons living at the age of 1 year, a figure obtained by subtracting the number of deaths under 1 year from the corresponding number of births : this will be denoted by N_c . One other measure of the mortality between the ages of 1 and 5 is the ratio of the number of deaths to the number of the population living at these ages expressed in parts per thousand. This being the usual method adopted by the Registrar-General it will be denoted by R_c .

In all the above a suffix *i* signifies the infantile mortality and *c* the child mortality, the capital letter denoting the authority.

As there is no essential difference between the results obtained by the different methods for the two sexes, the figures which follow refer solely to the male sex. This has been done to save labour in calculation.

To return now to Professor Pearson's paper. He, as we have seen, takes first, from the life-table, as the infantile mortality, the number of those born who die in the first year per thousand born : this will be termed P_i . Secondly, the number of the same population who die between the years 1 and 5 years of age in proportion to the number living at 1 year of age, this is termed P_c ; and thirdly, the expectation of life at the age of 6 years, termed hereafter E_6 . He calculates the correlation between each of these pairs for the four English life-tables 1838-54, 1870-80, 1880-90 and 1890-1900, and then proceeds to calculate the partial correlation coefficient between the infantile mortality and child mortality on the assumption that after the age of 6 the expectation of life is constant. He arrives at the conclusion that there is a high negative correlation between the infantile death-rate and death-rate in childhood—in other words, that the infantile death-rate exercises high selective influence. Dr. Newsholme has criticized this paper on the general ground that the method of estimating the mortalities is not sound, but before discussing his objection it is necessary to eliminate another possible source of fallacy. The life-table of 1838-54 represents different conditions. The population of England was much less urbanized; further there were great epidemics of diseases attacking adults: cholera, typhus, &c. This table cannot, therefore, be placed in rigid comparison with the later three tables; this objection, however, can be removed by replacing this life-table by a later life-table published since Professor Pearson's paper was written—namely, the life-table referring to the ten years 1900-10. It is found, using Professor Pearson's method, that this change introduces practically no difference, the value of the partial correlation obtained by Professor Pearson being $-.86$ and that obtained by the same method with the different data being $-.92$. This, therefore, does not furnish any source of error, as the difference found is hardly worth considering.

Taking, then, the four life-tables 1870-80 . . . 1901-10 as a basis, we have the following results:—

$$\begin{array}{llll} E_6^r P_i P_c & = & -.92. & E_6^r P_i R_c & = & -.33. \\ E_6^r R_i R_c & = & -.59. & E_6^r R_i N_c & = & -.01. \end{array}$$

The difference between the correlations found by Professor Pearson's method of measurement, namely $-.92$, and that obtained

by the Registrar-General's method of measurement is not great, the latter being $-.59$, quite sufficient for all practical purposes. When, however, the methods are mixed and P_i and R_c are calculated, the partial correlation falls from $-.92$ to $-.33$, while when the measures suggested by Dr. Newsholme are used the resulting correlation is $-.01$. In other words, there is no selective influence in the infantile death-rate. Dr. Newsholme distinctly scores, but the ground is more than treacherous. The complete figures are shown in Table I.

Positive correlations can, however, also be obtained by using the same data in a different manner.

Before showing how this is possible, however, a note on a point in the methods of construction of a life-table is necessary. In calculating infantile mortality and the mortality between 1 and 5 years, the data used in the construction of a life-table do not refer wholly to the same persons. Those children born between 1891-95 inclusive have their life history completed in the data of the life-table for the first five years of life, but there is in addition a series of children born in the previous decade who die in the epoch of the life-table and a series of children born in the epoch in question who die in the next decade. The data are, therefore, not based on the same persons. It is necessary to mention this because I propose to use as a check to Professor Pearson's method the data for the whole of the county districts of England and Wales for the two decades 1881-90 and 1891-1900, and in doing so am using figures open to the same objections.

England and Wales are divided by the Registrar-General into forty-five county districts. The infantile mortality for these is known, while the mortality occurring between the years of 1 and 5 can be calculated either by Dr. Newsholme's method on the 1,000 survivors at 1 year or by the method ordinarily employed by health authorities. It is, in addition, comparatively easy, adopting the method I have previously given, to obtain the expectation of life at 5 years. (29.) This is not quite the same quantity as that used by Professor Pearson, but the correlation between the two is so nearly unity that no error is introduced, the value obtained on the life-table data being $r_{P_i P_c} = -.92$ and $r_{P_i P_c} = -.87$. Employing Professor Pearson's method with the figures for the counties, it is found that instead of obtaining negative partial correlation coefficients, coefficients are obtained which are positive and nearly as great in quantity apart from sign as those found by Professor Pearson.

We have for the decade 1881-90 :—(cf. Table II.)

$$r_{P_i P_c} = +.69.$$

$$r_{R_i R_c} = +.67.$$

And for decade 1891–1900 :—

$$E_5' P_i P_c = + \cdot 71.$$

$$E_5' R_i R_c = + \cdot 73.$$

One other example will be given to illustrate the variations found. Glasgow is the only city in which the statistical districts were based on sanitary considerations. The range of mortality in these districts was in the seventies and eighties of last century very great. The data are not given in complete detail but in sufficient detail to allow the same correlations to be calculated. The result in this case is that the partial correlation is zero.

Where then lie the sources of error ?

Firstly, as already stated, there is no known method of measuring the mortality in infancy and childhood, with any of the accuracy which is required in investigations of this kind. I have worked at this subject on and off for many years, and while I have obtained approximations for the period of childhood, I have hitherto failed to obtain even a working measure of the infantile mortality. I doubt if the data in existence are sufficient. There is, further, absolutely no doubt in my mind that the method is fundamentally fallacious. There is no possibility, as far as I see, in calculating partial correlation coefficients on the assumption that the expectation of life at the age of 6 can be taken as constant. Life is a very complex phenomenon, and the death-rates at different ages are so closely related organically that different factors cannot be separated. A child brought up in a slum till over the age of 7 before it is boarded out in the country has been found in the experience of the parochial authorities of Glasgow not to maintain the same health as a child of 3 taken from the same environment. A child in a slum thus, whose parents die when it is young, has a better chance in after life than if its parents survived until it reached the age of 7. This accords with my own clinical experience. It will be noted by reference to the table that the correlation between the death-rate at the ages of 1 to 5 years as given by any of the methods suggested and the expectation of life at 6 years of age is practically absolute. In the various examples given in the tables associated with this part of the paper the values obtained by different methods range from .992 to .997. This degree of association is almost as absolute as the association between the phenomena of the solar system and the law of gravity, when the difficulty of knowing exactly how to measure the death-rates in childhood is allowed for. Partial correlation coefficients thus cannot be computed by considering the expectation at 6 years of age constant. I think such a method as arbitrary as trying to correlate the perturbations of Saturn and Jupiter after eliminating the law of gravity. How the problem will

ultimately be solved is yet unknown, but I have no hesitation in saying that the application of such an extremely crude method as the method of partial correlation coefficients will never enable us to unravel the complexity.

This, however, is not all; there is a further serious defect in this contribution which must be mentioned. Professor Pearson wholly neglects the epidemiological aspect of the statistics. The data on which the arguments are based were data referring to a period in which mortality in childhood was subject to special conditions. There seems no doubt that a great epidemic of infantile diarrhoea occurred in the years 1891-1900. A great many more infants died of the disease during the period of this epidemic than in either of the other three decades. A study of the causes of death as issued by the Registrar-General leads me to believe that this is a genuine phenomenon, and not due to a change in the nomenclature of disease. This introduces a factor of which so far there have not been any serious attempts to measure the effect. Guesses have been made by myself and others, but no evidence of any great value has hitherto been presented as to the effect infantile diarrhoea exercises in eliminating the unfit. We clearly have no right to use figures which contain a large element of uncertainty of this kind without special care. Some estimate of the effect which this epidemic produced is necessary. Again, there is no statistical method of any importance. It is clearly not legitimate to deduct the infantile mortality due to diarrhoea from the total infantile mortality and recalculate the correlations, but it is permissible to inquire into what the effect of this procedure would be. With the notation hitherto adopted, it is found that $E_6 r_{R_i R_c}$ in place of being equal to $-.59$, assumes the value $-.10$, and $E_6 r_{R_i N_c}$ changes from $-.01$ to $+.37$. All evidence of selection thus disappears, but it must be remembered that no trust can be placed on this result. The calculations show, however, that the introduction of this epidemiological feature must be specially considered in any future attempts to measure the influence of selection.

From this maze of partial correlation coefficients the defender of their use may escape as he will. It is a veritable labyrinth of Knossos, and there seems to be no Ariadne to supply a clue.

The next important paper upon the subject is by Miss Elderton and Professor Pearson. In this paper the subject is attacked by a new method, termed the method of variate differences.

The reason of the introduction of this method is that there are many groups of phenomena which may possibly be correlated but which are subject to some influence which causes changes as time

progresses. This is well illustrated by the example which is at present being considered, as the death-rates at nearly all ages and especially under 5 years of age have been decreasing steadily for many years. The death-rates under 1 year of age were the last to show evidence of this change, but during the last fifteen years a marked decline in this rate has also taken place. Consequently if we seek to correlate the death-rate between 1 and 2 years with that between 2 and 3 years a high positive value of the correlation coefficient is found, the steady fall in all the death-rates bringing about a false correlation which need mean nothing more than that the causes which have produced the phenomena act more or less equally in both cases. It is, therefore, important to have some method by which this kind of influence can be eliminated. Various methods have been suggested, but all are more or less open to fallacy. If a new method, not subject to fallacy, could be introduced it might be extremely useful. With this end in view the method termed that of variate differences has been suggested and used, but I think I shall be able to show that this method is open to even greater doubt than some rougher and less difficult tests. The advantage of a rough method is that the whole mechanism of the process is open to inspection, so that a fallacy, if not perceived by the author, is easily found out by the critics. When, however, an elaborate machinery, such as that of the method of variate differences, is introduced, it is often quite impossible to determine whether the assumptions on which the method is based are fulfilled in any particular case, or whether some hidden factor may not be in action which invalidates the use of the method. For myself, for reasons presently to be stated, I place no trust in any result obtained by this process. It belongs to the type of reasoning of which Poincaré has said—"mathematics are sometimes a nuisance."

Briefly described, the method is as follows. Let there be two series of terms $x_1, x_2, x_3, \&c.$, and $y_1, y_2, y_3, \&c.$ Let to both of these a time factor be added capable of being expressed in a few terms of Maclaurin's series. The additional variation introduced by this time factor can be obviously eliminated by taking the successive differences of the terms. It is evident from this that the time factor added to each series need not be the same. It can also easily be proved that if the correlations between the series x_1 and y_2 , and between the series x_2 and $y_1, \&c.$, are zero the correlations of the successive differences of the two series will be equal to that between the series of terms x_1 and y_1, x_2 and $y_2, \&c.$ It is also obvious that if there be any periodic factor the process will not hold, inasmuch as correlations will exist between terms assumed uncorrelated in the theory. The

method is not, therefore, applicable in such cases. The assumptions made further are very extensive. In general, it will be impossible to prove that no correlation exists between x_1 , y_2 , &c., and if such correlation does exist the results obtained by the method will be quite fallacious. These points will be illustrated presently.

The application of the method made by Professor Pearson in this instance has been to take the death-rates in England and Wales for each yearly age period from birth to 5 years of age and tabulate these so as to compare the death-rates at each age belonging to the same group of births as age advances. Thus taking the number of births he subtracts the number of deaths occurring under 1 year, thereby obtaining the number of those who start the second year of life. From this number the deaths between the age of 1 and 2 years after birth found in the tables of the Registrar-General are next subtracted. This process is continued. The death-rates are obtained by dividing the number of deaths occurring in each year by the number of individuals living at the beginning of that year. The correlations between the death-rates themselves and between their successive differences up to the sixth difference have next been calculated. The correlations between the rates themselves are positive, but with each successive difference a larger negative value is obtained till the fourth difference is reached, after which the correlations remain constant. The time factor thus seems to be eliminated. High negative correlations are uniformly found. Such agreement need not, however, mean anything if the method by which the results have been obtained is untrustworthy. It is assumed that these correlations show that a higher death-rate in one year of life is associated with a lower death-rate in the next year of life.

Professor Pearson makes a short note on the question of periodicity. He says his results are not invalidated on that account because he has not found any evidence of such periods. I have no doubt that periodicities in the neighbourhood of two years might be found if the data were sufficient, but the figures relate to whole years and it is very difficult to determine the length of a period with such statistics. A biennial periodicity, however, can be observed between the years 1870-90. Adding the death-rates in columns in alternate years and observing the differences in the sums relative to the mean value, a small but quite definite amplitude is found in each series of death-rates. If such a periodicity is due to biennial epidemics it should alternate with each year of age. Thus if between 0-1 years the maximum was in the first year, it should be in the second between 1-2 years and again in the first between 2 and 3 years, &c.

This is exactly what occurs, and it is found in spite of the fact that as all the death-rates have been steadily declining a constant excess would be expected in one column of sums over the other.

Special criticism of the method will now be made. In the first place some of the components of the death-rates will be examined. There are three infectious diseases in childhood which cause very considerable mortality in successive years of age, zymotic diarrhoea, measles and whooping cough. With zymotic diarrhoea the mortality is chiefly at the age periods 0-1 and 1-2 years; with whooping cough the same holds; with measles the chief period of mortality is in the age period 1-2 years, the age periods 0-1 and 2-3 years being of practically equal importance though of less importance than the period between. The working of the method can be tested against the figures for these diseases, and in this way some of the results obtained interpreted. As the calculation of the correlations of all the differences is very laborious and as the fourth differences were found by Professor Pearson to give a satisfactory approximation, the correlations of the death-rates themselves and of their fourth differences have alone been worked out. Probable errors have not been calculated as I have no belief in the processes by which they have been obtained.

The statistics relating to zymotic diarrhoea will first be discussed. This is a disease which occurs every summer or autumn in greater or less amount, the amount of the disease depending in general on the warmth of the early part of the summer. No definite periodicity other than this has hitherto been demonstrated, but it seems probable in view of the fact that I have hitherto found periodicities in all the cases examined that, when I have a clerk at liberty to undertake the work, true diarrhoeic periodicities independent of the solar year will be found. The outbreaks, however, occur sufficiently often in large epidemics in alternate years on account of climatic variation to introduce false periodicity which must have exactly the same effect in bringing about a false selective result as a true periodicity would.

The disease is about equally common at the two age periods 0-1 and 1-2 years, though very much less fatal in the latter age period. If 10 per cent. of the infants are affected, assuming that an attack one year establishes some immunity for which there is some evidence though the fact is doubtful, then with the rough periodicity of 2 years the selective correlation should be in the neighbourhood of $-\cdot 10$. That found for the whole of England and Wales is $-\cdot 16$, a value not out of the way, but the correlation between the fourth differences is equal to $-\cdot 91$, a value which cannot be conceived, apart

from some correlated variation in the occurrence of epidemics. It would seem that zymotic diarrhœa furnishes a large component to the selective figures as estimated by Professor Pearson between the infantile mortality and the mortality in the year of life following. When the deaths from diarrhœa are subtracted from the total deaths at the first two age periods the correlation between the actual death-rates becomes $+ \cdot 80$ and that between the fourth differences $- \cdot 22$, in place of $+ \cdot 40$ and $- \cdot 66$ found by Professor Pearson. During the years 1850-89, years preceding the great epidemic of zymotic diarrhœa and years in which quite marked biennial periodicity is observed in the diarrhœa deaths, the correlation between the same quantities is even higher. The correlation between the actual death-rates is $- \cdot 36$ and between their fourth differences $- \cdot 93$, while the corresponding values for the total death-rate when the death-rate from diarrhœa is subtracted are respectively $+ \cdot 52$ and $- \cdot 11$. The selective influence of infantile mortality thus very largely disappears. This process of subtraction as used in this place is not legitimate, but the fact that it produces so profound a difference in the results shows that no mere statistical process unless accompanied by an adequate discussion of the epidemiological concomitants can be relied on to furnish results of any validity. The complete failure of the method, however, will be more definitely seen when two measures which cannot possibly have any connection are correlated, namely, the death-rate between the years of 1 and 2 years and the death-rate under the age of 1 year for the epoch for 2 years later. The correlation between the actual death-rates is $- \cdot 06$, *i.e.*, the two are independent as would be expected, but the correlation between the fourth differences is $- \cdot 76$.

Finally, making some calculation for London specially it is found that the correlation coefficient between the fourth differences of the death-rates for diarrhœa at ages under 1, and that between the ages of 1 and 2 years for the year following, is equal to $- \cdot 83$, while when the death-rates under the age of 1 year of age are correlated for successive years the value found is $- \cdot 87$ which does not differ from the preceding. Yet in this latter case there can be no question of selection. The periodicities due to weather and as yet undiscovered causes are thus quite capable of explaining the values of the correlation coefficient found.

The same phenomena are found when the death-rates of measles are studied. The correlation coefficients have been calculated for the age periods of 1-2 and 2-3 years on the same basis as that used by Professor Pearson. In the actual figures the correlation between the number of deaths between the ages 1 and 2 years in one year and

that between the ages of 2 and 3 in the next year is $-.16$. The figures which would result if epidemics of different size were random phenomena distributed according to the normal curve of error would be in the neighbourhood of, though in all probability less than, $-.1$. The coefficient thus tells little. When the fourth differences, however, are correlated the correlation is found to be $-.83$; that is a value is found quite impossible unless very considerable periodicities exist or unless there is correlation among the terms which are presumed by the theory to be uncorrelated. This is a specially important case, as the time factor here consists of two large long waves. There is at present no evidence that the death-rate from measles is permanently declining.

With whooping cough the correlations between the death-rates at 0-1 years and 1-2 years show similar values, that between the actual death-rates being $+.15$ and that between the fourth differences $-.67$.

A further check on the method is afforded by the statistics of London and for this city I have determined the main periodicity of two of the chief epidemic diseases, namely, measles and whooping cough. The statistics of the death-rates under 5 years of age have been reduced by exactly the same method as that employed by Professor Pearson for the whole of England and Wales. It is found that the values of the correlation coefficients both for the actual death-rates and for the fourth differences are practically identical with those for all England. Now a large part of the death-rate in childhood in London is due to two diseases, measles and whooping cough. The main periodicity of measles is 97 weeks and the mean range of variation from the minimum to the maximum is in proportion of 6 to 14 so that the average maximum in the epidemic period is more than twice that of the minimum in the inter-epidemic period. This is, therefore, a sufficiently definite period. With whooping cough the period is somewhere in the neighbourhood of 100.3 weeks; the amplitude in this case is not so great, the range of variation being expressed by the proportions of 74 to 126 an amplitude of considerable magnitude. Both of these periods in the sum are very closely sine functions, and on the hypothesis that the epidemics themselves can be represented by such a formula, it is easy to estimate the correlation between the number of deaths occurring in one year and those of the same generation in the succeeding year. The approximate value of this correlation is $-.98$. Now such a periodicity cannot wholly be overshadowed by the mass of deaths from other causes, though the effect may be obscured to some extent. The effect will be best seen in the differences. The negative values of the

correlation coefficients found seem to have an explanation in the ordinary variation in the amount of the infectious diseases of childhood. It may, however, be stated that the epidemic constitution of the death-rate has a large biennial component. If we correlate the death-rate between birth and 1 and 1 and 2 years and 1 and 2 and 2 and 3 years for the same years we get in the first case a correlation for the actual death-rates of $+ \cdot 6$ and for the fourth differences of $+ \cdot 5$ and in the second case correlations of $+ \cdot 96$ and $+ \cdot 93$ respectively. Where the correlations are calculated with an interval of 2 years between the same age periods the corresponding values are $+ \cdot 40$ and $+ \cdot 16$ and $+ \cdot 86$ and $+ \cdot 42$. There is thus a very marked tendency, especially in the second group of age periods, for the deaths to show a biennial variation. This effect had hitherto been assumed as well known. The statistics of London thus give strong verification to the view that the results obtained by the method of variate differences are quite sufficiently explained by the epidemic constitution of the year.

I have discussed the results of the method of variate differences at some length because I fundamentally distrust it. I know of no phenomenon in vital statistics which can in the least be said to fulfil the canons implied in the definitions of validity, which require that the correlation between all the terms except those directly correlated is zero and that the time function is additive. With reference to the latter point the case of scarlet fever may be taken. In this disease the death-rate has very markedly fallen in the last forty years, the rate of fall being closely described by a geometrical progression. The range of swing above and below the mean value at any time has also greatly declined, being now but a fraction of what it was in 1870, so that not only has the mortality fallen but the proportionate swing has also fallen. Such a condition obviously cannot be described by a variable factor added to a time factor. The same holds in all the other cases which I have examined.

The question of the validity of the method is of much importance to this Society. It is being applied to railway statistics, imports, &c., and it will, if not thoroughly scrutinised, probably lead to conclusions being pressed for which there is no adequate evidence.

It is now possible to consider Dr. Snow's Paper. The chief part of Dr. Snow's Paper is taken up with a discussion of the effect which mortality under 3 years of age has upon the mortality during the fourth and fifth year of age. For this purpose he divides the country up into urban and rural districts. He finds in the rural divisions that a very marked negative correlation between the earlier and later period exists. He explains the absence of correlation in

urban districts by the emigration and immigration which is constantly taking place. The data he chiefly examines are those referring to children born in 1870. These he follows through the first five years of life. He, however, does not consider at all the epidemiological aspect of the matter, and thus fails to clear his evidence of an important source of fallacy. The results he obtains are results such as might be easily anticipated. In the country districts epidemics of measles and whooping cough occur at much longer intervals than in towns. In general, for a country district to be infected a new source of disease must be imported, while in towns measles and whooping cough are always present and occur roughly at intervals of two years. In correlating the mortality under 3 years with that during the fourth and fifth years of age Dr. Snow thus arranges the data in a form which greatly increases the difficulties of disentangling the effects produced by the presence of epidemic children's diseases. As these are among the most important causes of child mortality it is evident that a large part of the correlation observed must be due to these diseases. If the epidemics occur in the country at intervals of 3 years or longer, it is quite obvious that a high mortality under 3 years of age will be associated with a low mortality in the fourth and fifth years, and *vice versâ*. The negative correlation obtained from the statistics of the country divisions is, therefore, what might be expected, and the absence of correlation found in the statistics of the towns might also be explained on the same hypothesis of more frequent epidemics. Dr. Snow's treatment of the statistics of the Prussian rural districts is open to exactly the same objection.

These methods hitherto discussed seem to give no kind of trustworthy answer. The question therefore remains, whether the subject can be approached in any other way. This I attempted in a previous paper by comparing the death-rates in the different life-tables which relate to districts possessing more or less similar health conditions. (26.) I hoped in this way to be able to compare the mortalities at all ages of life in different places by taking some such standard as a Healthy District Life-Table. The method seemed promising, but on re-consideration of the subject I can only come to the conclusion that what I wrote cannot be justified. In the accompanying diagram the death-rates in the life-tables for Scotland and for England and Wales for the decade 1891-1900 are compared. In Scotland there was during this decade a much lower mortality under 1 year of age than in England. The mortality at 2 years of age was greater than that in England, while during the long period between 10 and 35 years of age the mortality was very much higher in Scotland

than in England. It looked as if the large saving of infantile life was followed by a period of higher death-rates reaching to early adult years. This conclusion is fallacious. When I wrote I had not studied the statistics of phthisis in any adequate manner, I did not know that phthisis was not one disease but a mixture of two or probably three diseases, as I am now certain it is. Epidemics of these two diseases affect different districts of the country in different proportions. One type of phthisis has its highest mortality between 20 and 25 years of age. This is the type which is most prevalent in Scotland. The other type, which is typical of London, causes the maximum number of deaths in the neighbourhood of 40 years of age. The two, therefore, vary in a manner which is quite distinct. This introduces a complicated factor. I do not think it could be urged, except as a matter of special pleading, that a low infantile mortality conduces to a high death-rate from that form of phthisis which attacks at the earlier years of life and not from the form which affects the middle ages of life.

The second instance, which I thought even more convincing, must also be rejected, its use being due to a mistake. The data of the life-tables referring to the mortalities in Salford and Manchester at different ages were compared. These data are illustrated in the diagram (page 238). It will be noted that Salford has a much higher death-rate in infancy than Manchester and in compensation thereafter the Manchester death-rate is very much higher between the years of 5 and 15. Unfortunately, my memory played me traitor, and I forgot to notice that these two life-tables referred to different decades, that of Manchester referring to the decade 1881-90, and that of Salford to the decade 1891-1900. Had this not been so an almost typical experiment would have been observed in progress, Salford and Manchester being essentially the same town. The differences found seem to be due to epidemiological causes. In Manchester in the decade 1881-91, there were 4,011 deaths between the ages of 5 and 10 of which 692 were due to scarlet fever, in the next decade the corresponding figures are 3,154 and 344, giving a net fall in the number of deaths of 857, of which nearly half, 348, were due to scarlet fever. Secondly, the great epidemic of zymotic diarrhoea occurred between the years 1890-1900 and affected Salford like the rest of the country. There are thus two completely different epidemic constitutions in the two decades. Comparison of figures without reference to epidemiological variations can thus only lead to results of little significance. Different epidemics occur according to their own laws and cannot be assumed to offer protection or the reverse against one another. There is at present no evidence at all

DIAGRAM I.

Illustrating the ratios of the death-rates in the life-tables of Manchester, 1881-90, Salford, England, and Scotland, 1891-1900, to the corresponding rates in the Healthy District Life-Table, England, 1891-1900.

that any epidemics are continually associated. What looks like an association in one place is often due to the existence of periodicities of similar lengths with two diseases, and if sufficient investigation is made it will in many cases be found that the association observed in one place for a number of years has no parallel elsewhere. The origin of periodicities in disease is at present not settled. In my belief it is due to variations in the infectivity of the organism

which attacks, though it can also be argued that decline of the disease is due to the exhaustion of the susceptible persons, and its return to the fact that a new susceptible population has grown up. Whatever the causes it is none the less necessary to note that the phenomenon is independent of selection. The same conclusion must, I think, apply to any methods that can be suggested. If we could make a direct experiment some information might be obtained, but as long as we are dependent upon the statistics of deaths and as long as the epidemiology of diseases is a subject so little worked as at present, so long any conclusions advanced must remain doubtful. For instance, if phthisis is more than one disease it follows that any statistical work upon phthisis which does not recognize this may be open to fundamental fallacy.

The most important work on this subject has now been discussed. The result of the enquiry has been to throw great doubt on every method which has been proposed to measure the influence of the death-rate as a selective agent. Personally I do not see how any accurate information is to be obtained. Experiment, were it possible, might solve certain of the questions, but even where something analogous to experiment has taken place the data are rarely presented so as to be statistically useful. For my own part I am very strongly of opinion that the improvement of environment is the most important measure with a view both to the procreation and the maintenance of a healthy race and that no effort can be too great to accomplish this improvement. The desirability of saving infantile life when that life must be lived under conditions which are incompatible with healthy human life seems very doubtful. It is a waste of effort to save life when the individuals are brought up so as to be incapable of exercising the useful functions of citizenship. Improvement of housing and the provision of open spaces should be the main line of action. It is quite true that during the war not much can be done in this direction, but I am quite sure that as soon as the war is over the older sanitation will again come into its own.

Finally, it is my strong opinion, having examined all the different methods in use, that unless a very careful investigation is made into the epidemiology of any series of health statistics no estimate of selective influence can be of the least value.

NOTE.—*On the Value of the Infantile Death-rate as a Mathematical Measure.*

This note is added to make clear that I have no objections to the infantile death-rate as a measure of healthiness or unhealthiness of an environment as it is commonly used in public health statistics, but that my objection is to its use as a mathematical measure forming the basis of rigid statistical deduction. For practical purposes it is a most convenient figure, and with reasonable care furnishes a sound guide into sanitary conditions. It has probably been of more direct importance in determining municipal measures and general legislation than any other figure. There is, however, a great difference between a guide for practical purposes and a mathematical measure. For the latter a much more rigid method of deduction is obviously necessary.

The reason of the difficulty is as follows. Though the methods by which death takes place under one year of age has not as yet been unravelled, there is one point which is well recognized, and that is that two completely different processes exist by which death takes place. In one of these the chance of dying from certain diseases decreases from birth, in the other it increases. Further, under the age of 3 months there is not much difference between the town and the country; a greater difference appears between 3 and 6 months and a still greater in the last six months of the first year of life. It is also to be noted that about the fifth or sixth month a marked change takes place in the physiology of the infant. Children under 5 months, for instance, even though exposed to infection, hardly develop measles; children over 6 months are just as susceptible to the disease as in the following year of life. This applies also to a number of other diseases. Not only is this the case but the mortality changes in the same striking manner. The case mortalities in the four quarters of the first year of life are respectively, in the figures of Dr. Hay of Aberdeen, 7·8, 9·8, 14·7 and 18·2. In my hospital experience the difference is even more striking, the corresponding figures being 10·0, 9·3, 21·3, 24·3. It also is to be specially noted that in the second six months of life diseases which are undoubtedly infective such as broncho-pneumonia, &c., play a more prominent part in the total mortality. It is in these months that an unhealthy environment tells most against the life of a child. This is illustrated in Table I, in which the proportions of the total deaths occurring at certain periods during the first year of life are shown for the urban and the rural districts of England—the figures are those of the Registrar-General for the year 1909.

TABLE I. *Showing the percentage of the total deaths under 1 year of age which occur in certain specified age-periods for the urban and rural counties for the year 1909.*

Age-period.	Urban.	Rural.
0- 1 month ...	34·4	41·5
1- 3 months ...	18·7	19·9
3- 6 „ ...	12·2	11·2
6- 9 „ ...	10·1	8·3
9-12 „ ...	9·1	6·4

In view of these facts it would seem that the death-rates at each month under 1 year of age may be represented as the sum of two functions. It seems obvious that when at least two different conditions determining the death-rate

are mingled in this way, no rough and ready criterion such as is obtained by dividing the total number of deaths by the total number of births can be taken as a mathematical measure. I have certainly never in a purely mathematical matter seen any such procedure suggested.

With regard to the death-rate between the years of 1 and 5 almost exactly the same remarks apply. When we consider the returns of the Registrar-General we find that the form of the curve which describes the fall of the death-rate in these years varies within such wide limits as to preclude any but rough comparisons. It is only after the age of 4 is reached that the slope of the curve giving the number of deaths changes sufficiently slowly to permit the death-rate as usually calculated to be taken as a basis of mathematical development.

TABLE II.—*Showing the correlations between each pair of the following three measures: the infantile death-rate, the childhood death-rate, and the expectation at 6 years of age taken from the life-tables for England and Wales, 1871-80—1901-10.*

Notation: Suffix *i* means infantile mortality; suffix *c* means childhood mortality; *E* expectation of life; suffix figure the age from which expectation taken; *P*, *N*, and *R* refer respectively to the methods of calculating the death-rates suggested by Professor Pearson, Sir A. Newsholme, and the Registrar-General.

(a.) ENGLAND AND WALES.—As calculated by the different measures used or suggested.

1838-54—1891-1900.	1871-80—1901-10.	1871-80—1901-10.
r_{PiPe} — ·6359	r_{PiPe} + ·4358655	r_{PiRc} + ·4997350
r_{PiE_6} + ·5620	r_{PiE_6} — ·5233236	r_{PiE_6} — ·5233236
r_{PeE_6} — ·9941	r_{PeE_6} — ·9942197	r_{RcE_6} — ·9969259
$E_6 r_{PiPe}$ — ·8605	$E_6 r_{PiPe}$ — ·92288	$E_6 r_{PiRc}$ — ·32921
1871-80—1901-10.	1871-80—1901-10.	
r_{RiRc} + ·6934197	r_{RiNc} + ·7202381	
r_{RiE_6} — ·7273973	r_{RiE_6} — ·7273973	
r_{RcE_6} — ·9969259	r_{NcE_6} — ·9917431	
$E_6 r_{RiRc}$ — ·59037	$E_6 r_{RiNc}$ — ·013104	

(b.) ENGLAND AND WALES.—Deaths from zymotic diarrhoea subtracted.

1871-80—1901-10.	
r_{RiRc} + ·8923319	r_{RiNc} + ·9173291
r_{RiE_6} — ·9038190	r_{RiE_6} — ·9038190
r_{RcE_6} — ·9930991	r_{NcE_6} — ·9848053
$E_6 r_{RiRc}$ — ·10461	$E_6 r_{RiNc}$ + ·36661

TABLE III.—*Showing the correlations calculated as in Table I for the decades 1881-90 and 1891-1900. The English county districts being the units.*

(a.) 1881-90 :—

r_{RiNc} = + ·8363815	r_{RiRc} = + ·8326501
r_{RiE_5} = — ·6768483	r_{RiE_5} = — ·6768483
r_{NcE_5} = — ·8774573	r_{RcE_5} = — ·8724798
$E_5 r_{RiNc}$ = + ·68674	$E_5 r_{RiRc}$ = + ·67309

(b.) 1891-1900 :—

r_{RiNc} = + ·8796644	r_{RiRc} = + ·8873039
r_{RiE_5} = — ·7447911	r_{RiE_5} = — ·7447911
r_{NcE_5} = — ·8778616	r_{RcE_5} = — ·8859062
$E_5 r_{RiNc}$ = + ·70668	$E_5 r_{RiRc}$ = + ·73494

TABLE IV.—*Showing the correlations between the actual death-rates (A.R.) and their fourth differences (F.D.).*

Age-period correlated.		England and Wales.								London.
		All causes. Professor Pearson. 1850-1908.	Measles. 1850-1908.	Whoop- ing cough. 1850-1908.	Zymotic diarrhoea.		All causes, less zymotic diarrhoea.		All causes. 1850-1908.	
					1850-1908.	1850-89.	1850-1908.	1850-89.		
0-1	A.R.	+ '40		+ '15	— '16	— '36	+ '80	+ '52	+ '44	
and 1-2	F.D.	— '66		— '67	— '91	— '93	— '22	— '11	— '36	
1-2	A.R.	+ '86	— '16						+ '88	
and 2-3	F.D.	— '64	— '83						— '73	
2-3	A.R.	+ '92								
and 3-4	F.D.	— '62								
3-4	A.R.	+ '91								
and 4-5	F.D.	— '62								

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DISCUSSION ON DR. JOHN BROWNLEE'S PAPER.

DR. W. H. HAMER, in moving a vote of thanks to the author, said that, if he might use a sporting *simile*, the author had played a strong defensive innings, and dealt with very great skill both with the "fast deliveries" from University College and with the "googlies" of Dr. Snow. But he would like to ease his mind by dissenting from him concerning three things: first of all, his first paragraph; secondly, his statement on page 237 that there were three kinds of phthisis, but that was a matter he (the speaker) had dealt with elsewhere, and he would not trouble them with it now; thirdly, with regard to the statement on page 239, he had thought Dr. Brownlee had been convinced by Sir Ronald Ross that exhaustion of susceptibles did play a part in determining periodicity of disease, but apparently Dr. Brownlee was unrepentant. Turning now to Dr. Brownlee's text, Professor Pearson's Paper, he rather wondered why, on page 229, Dr. Brownlee did not comment upon the fact that when Professor Pearson wrote the Paper of 1912 he was dealing with figures of the last century, and with nothing later than 1900; thus, when Professor Pearson thought that the infantile mortality was rising, and that there was, therefore, an inverse relation between infant mortality and mortality at the ages of 1, 2 and 3, the infant mortality had, as a matter of fact, been rapidly falling for twelve years. The delay in the decline was a point which troubled Medical Officers of Health very much twenty years ago, and the late Dr. J. F. J. Sykes had written a Paper of great interest about it; they had all at that time waited and watched for the morning, but it was broad daylight when Professor Pearson wrote his Paper. He thought that the marked decline since 1900 was a point deserving of notice. With regard to the main contention, that "the greater the infantile death-rate the less is the death-rate among the survivors," he supposed there was something in that; and he supposed there was also something in the to some extent opposed view, which had been carefully set out by Dr. C. J. Thomas (Dr. Kerr's Annual Report for 1905), that the average heights and weights of children of school age who were born in years of high infant mortality were subnormal, indicating as Dr. Kerr had said, that "the conditions to which five or six of the children born are sacrificed have a maiming effect upon the other four or five." Neither of these results were obtained without very great labour, statistically speaking, and when obtained they were more or less in conflict with one another. He was reminded of the man who was asked why married men lived longer than single men. The answer was that they did not, it only seemed longer. Both the natural selectionists and Dr. Thomas must be right, and yet when they set about proving their contentions by careful statistical inquiry they could not

make very much of it. It was interesting to note that the year in which Professor Pearson was composing Dr. Brownlee's text, was also the year which marks a turning point in his career, for he then began, as he told them all in his Address at York, to venture to regard the reports of Medical Officers of Health as literature. Possibly as a result of this he made two years later the statement "Even in highly organized human communities the death-rate is "selective and physical fitness is the criterion for survival. To "assert the existence of this selection and to measure its intensity "must be distinguished from advocacy of high infantile mortality "as a factor of racial efficiency." So that everyone was now satisfied ; they on their part continued to admire the skill with which Professor Pearson obtained his high correlations where heredity was concerned, and Professor Pearson on his part, despite the comparative lowness of the correlations when environment is in question, was content to allow them to go on dealing with slums and ashpits, and generally with abatement of nuisances and overcrowding. As a matter of fact, the war had settled the differences between Professor Pearson and his critics. It was the rank and file that mattered now, and they were all agreed as to the value of the rank and file, for had they not been told that "the men are splendid." No amount of insistence upon high correlations would convince anyone that some of them ought never to have been born. The rank and file were what they were to no insignificant extent on account of the work of those whom Dr. Brownlee had referred to as the "giants of the last century." Dr. Brownlee, of course, was thinking of the men who had struggled to remove adverse environmental conditions, to improve water and food supplies, and to control the spread of epidemic diseases. They all owed a debt of gratitude to Dr. Brownlee for reminding them of the value of their work, and he hoped Dr. Brownlee was right in thinking that as soon as the war was over the older sanitation would again come into its own.

Captain M. GREENWOOD, Jr., in seconding the vote of thanks, said that he did not intend to discuss the general aspects of the controversy touching the operation of selection within the human race, a controversy which was in some danger of recalling to the mind of the historical student the voluminous literature initiated by a sermon preached almost exactly two hundred years ago and forming the corpus of that "Bangorian Controversy" said to have developed into "an intricate and bewildering mass of side "discussions in which the main issues of the dispute were concealed "almost beyond the possibility of discovery." He did not wish to add to this confusion, and would confine himself to purely technical matters, merely hinting a doubt whether Dr. Brownlee's reference to Miss Beeton and Professor Pearson's Paper on the inheritance of longevity was well adapted to eliminate the personal element from scientific discussion. Dr. Brownlee had criticized severely the method of variate difference correlation, but he (Captain Greenwood)

did not think that Dr. Brownlee's discussion of the subject was adequate. With respect to the first criticism, viz., that the correlations of variables free from the time factor were assumed to be zero when the subscripts were unequal, there was no serious difficulty, from the algebraical side, in dropping these restrictions provided the additive time effect could be represented by an n th order parabola. The *practical* difficulty was that the actual statistics could not from the nature of the case provide one with the values of the cross-correlations; but it would evidently be possible, after obtaining the general formula, to investigate the modifications of the final result consequent upon assuming various values for the cross-correlations. This, he thought, should be done. With regard to the much more serious difficulty of periodic factors, he thought that Dr. Brownlee had done less than justice to Miss Elderton and Professor Pearson. These authors were under no illusion as to the importance of the point and definitely expressed their dissent from Dr. Anderson, who had stated that periodic factors could be eliminated by the difference method. Neither did Miss Elderton and Professor Pearson base their conclusion that, in the case with which they dealt, the fallacy was not involved upon a mere inspection of graphs—as Dr. Brownlee's readers might suppose—but upon the sign and magnitude of the correlations at intervals of more than two years. He also thought that Dr. Brownlee's mathematical note was insufficient. All that note provided—taking for simplicity the case of one periodicity—was the correlation between successive terms of a simple Fourier series; the denominator contained the well-known mean square of error functions of such series, and the numerator necessarily took the form shown owing to the evanescence of all cosine product terms not reducible to $\cos \frac{2\pi}{p}$. Professor

Pearson might reasonably retort that Dr. Brownlee had only proved what he (Professor Pearson) regarded as a truism, and that what really needed investigation was the case where Dr. Brownlee's M and M' were not constant but variable, and also correlated one with another, while the phases of the periodic time factors were not arbitrarily assumed to be in the relationship given by Dr. Brownlee. He thought that this most interesting problem merited fuller consideration. With respect to Dr. Brownlee's criticism of the processes employed by Dr. Snow and others prior to the introduction of the variate difference method, he questioned whether the substitution for certain measures of others highly correlated therewith was satisfactory. In view of the very high values of the total correlations, small changes in them might lead to large changes in the deduced partial correlations. This, indeed, was one of the dangers of the method, and, although it had not been much commented upon in the earlier publications, Miss Elderton and Professor Pearson had given special attention to it in their recent Paper. Next, as to Dr. Brownlee's objection to the infant death-rate as a mathematical measure. He (Captain Greenwood) saw no reason for supposing

that Professor Pearson regarded the infant death-rate, however measured, as something sacrosanct; probably Professor Pearson regarded the measure as a rough one, but thought that no reason why further errors should be introduced by employing a rough and inaccurate method of analysis; hence Professor Pearson made use of what he regarded as the most accurate available process. Dr. Brownlee appeared to wish to have it both ways; he not only denied that Professor Pearson's methods were exact, but he also asserted that the data could not bear exact treatment. He (Captain Greenwood) did not see how data which would lead to correct conclusions when treated roughly must lead to incorrect conclusions when treated accurately. Lastly—and this perhaps was trespassing upon the forbidden field of generality—he did not understand how an epidemic disease which came to an end by the exhaustion of susceptible persons—a method of termination admitted by Dr. Brownlee to be possible—could fail to exercise, in greater or less measure, some selection. He begged to second the vote of thanks to Dr. Brownlee.

Dr. J. C. STAMP said that valuable as the Paper was in relation to the subject at the head of it, it seemed to have a still greater value in what it might lead to in the field of pure statistics. It was what he might call an *experimentum crucis* in the application of a new method. They all knew that when a Chemistry Professor failed in an experiment it was not because the scientific principle he wanted to show was not there to be shown, but that some little error always crept in in his working of it, so that he never achieved the actual result he wanted to achieve. It seemed to him they had something in the nature of a test of the new method of Variate Differences which did not work, and if it led, as he thought it might, in the hands of those able mathematicians who would follow the matter, to a further exploration of this method, it might be the beginning of the setting out of the actual conditions which must be present for that method to be valid. It was very well to experiment on paper with X's and Y's, and arrive at results, and then to pin their attention to any form of vital or other statistics produced by the Registrar-General, and expect that they were going to be as amenable to discipline as the X's and Y's were. Such statistics had a little habit of having a character of their own, and it was only by exploration of experiments that had not worked that one could set out those limiting conditions which were necessary for the full statement of any theoretical question. When the variate difference method was commenced, he thought its first application was to Professor Mortara's statistics of certain Italian industrial and vital statistics, productions of agriculture, and so on. The whole object of the method was then to eliminate a constant time factor, increasing population increasing all classes of products of activities. It seemed to him a very far cry from that to this class of variation where they had admittedly periodicity, but had not a constant rate of

growth that they wished to eliminate in order to keep the coefficient of correlation down. This might lead them to find what were the conditions of fallacy and what were the conditions of success. He quite agreed with the remarks of the last speaker, that it did not follow because they applied an exact method to things that were rough they were going to make the situation any worse. It was perfectly true that we wanted to discover what classes of roughness were critical. If there was something behind those figures, as there most evidently was, which caused the extraordinary differences between the negative correlations found in one way and much smaller negatives and positive correlations found by adopting another standard of vital statistics, there must be something behind which would really explain it other than the mere arithmetic change. There must be something in the relationship between the methods of ascertaining the death-rate for England and the mortality rate up to the age of 5—some connection which invalidated that application. So that while he thought their thanks were due to Dr. Brownlee for his application to that subject, he thought perhaps he was building more than he intended to do on this particular thing, in that they might have arise from it acutal contributions to the theory of statistics.

(The vote of thanks was put to the Meeting and carried unanimously.)

Dr. BROWNLEE, in reply, said he did not want to make many remarks. With regard first to the measures of the death-rate in children, he had been working at this subject on and off for many years without any great success ; he quite realized the point of view of those who criticized his statements regarding the measures of the death-rates, and also that the different methods of Dr. Newsholme, Professor Pearson and the Registrar-General might have some definite connection. His point was that by using a definite method of calculation and using the different values of the death-rates, as suggested by the several authorities, quite different results were obtained. It was thus quite clear that a part at least of this difference found must be due to the different methods of measurement. With regard to the criticism of Captain Greenwood that the method might not be wrong and that Dr. Brownlee wished to have it both ways, he submitted that he did wish to have it both ways as he thought the method fallacious. He did not think they could take the expectation at 6 years of age as a constant in a partial correlation coefficient as the variations in the death-rate were organically connected. If the death-rates at different ages in Salford were taken and contrasted with the Healthy District Life-Table death-rates for the same ages, certain relationships were found ; if the life-table for London was similarly treated exactly the same kind of relationships at different ages appeared : the difference was in quality not in quantity. These relationships seemed quite definite unless large amounts of epidemic diseases were present at a particular

time. With regard to the possibility that the positive correlations arose from the expectation at 5 years of age being taken in place of the expectation at 6 years, he said that was not the case because when Professor Pearson's correlations were re-calculated, taking the expectation at 5 in place of the expectation at 6, it was found that in the former case the value was $-.87$ as against $-.92$ in the latter, not a difference of moment. With regard to Captain Greenwood's point as to the selective death-rate caused by an epidemic, selective effect on the death-rate must be much larger on account of the cases of disease than on account of the deaths as epidemic diseases for the most part protect against second attacks. In measles epidemics, for instance, there are on an average at least twenty times the number of cases as deaths. Death from measles further is a matter of the age at which the child takes the disease, the death-rate between 1 and 2 years of age may be as high as 15 per cent., while between 4 and 5 years it is not more than 2 or 3 per cent. The selection, therefore, in epidemic disease cannot be considered as due to the number of deaths but to the number of cases. This selection gives a small correlation coefficient being approximately $-.1$ in the case of measles, a disease which about a tenth of the number living at the early ages on the average take each year, and this must have a much larger selective effect than any number of deaths. When, however, in this case the Variate Difference method is employed the correlation rises rapidly to $-.8$. Mr. Trachtenberg has worked out the mathematics of this point and is discussing it in a separate Paper.

The undermentioned Candidates were elected Fellows of the Society :—

Matthew Stevenson Birkett.
Joseph Davies.

Hugh Hastings.
Humfrey Mitchell.