Care-of-Children Committees, but the Care-of-Children Committee and the School Attendance Nurse are not mutually exclusive,—on the contrary, the sphere of the School Attendance Nurse can properly be regarded as complementary to that of the Care-of-Children Committee.

INSANITARY AREAS IN GLASGOW.—In representations touching areas in Blackfriars, Hutchesontown, Cowcaddens, Townhead, and Whitevale, Dr. Chalmers submitted the following statistics:—“These ‘death areas’ contain many foreigners, and are very similar in character, varying from two-and-a-half to three-and-a-half acres in extent. The population on each is about 1,500 persons, occupying for the most part one and two-roomed houses (*anglice*, tenements). In the city, as a whole, the number of persons per acre is about 60, in the combined areas referred to it is 460. The average number of square yards per person over the city is 80; in these areas this is reduced to 10.5. The average death-rate in the city from all causes is 16.4 per 1,000 living; in the combined areas it is 22.7. From respiratory diseases the deaths in the city are equal to 3.1 per 1,000 of the population living; in the areas under review the rate is exactly double (6.2 per 1,000). The average infantile death-rate is 136 per 1,000 births; for the combined areas it is 180 per 1,000; while in Hutchesontown area 282 children out of every 1,000 die.”

APPOINTMENTS.

DR. JAMES P. CULLEN has been appointed on the staff of the King Edward VII. Welsh National Memorial.

DR. DENNEHY, of Lismore, has been appointed by the Waterford County Council as medical officer for the Abbey-side tuberculosis dispensary.

J. BELL FERGUSON, M.B., Ch. B. (Edinburgh), D.P.H. (Manchester), appointed chief tuberculosis officer, City and County of York, formerly assistant medical officer of health and medical officer-in-charge Tuberculosis Dispensary County Borough of Leicester.

DR. E. A. FREEAR WILKES has been appointed assistant medical officer of Hampshire.

DR. J. R. GILLESPIE, Southampton, has been appointed by the Down County Council as medical superintendent of the tuberculosis dispensary.

DR. M. HALFPENNY, J.P., medical officer of Scotstown dispensary district, has been appointed tuberculosis officer by the County Council of Monaghan.

J. KING PATRICK, M.B., Ch. B., B. Sc. (Public Health) Glasgow, D.P.H. Dublin, resident medical officer Borough Isolation Hospital and Groby Road Sanatorium for Consumptives, Leicester, and formerly assistant medical officer of health and school medical officer, County of Ayr, has been appointed assistant medical officer of health and medical officer-in-charge of tuberculosis dispensary, County Borough of Leicester, *vice* Dr. J. Bell Ferguson.

DR. THOMAS ROBERTS, of Wrexham, has been appointed county medical officer of health for Denbighshire.

SOME OBSERVATIONS BEARING ON PROF. PEARSON'S RESEARCHES ON TUBERCULOSIS.

By R. J. EWART, M.D., F.R.C.S., M.Sc., D.P.H.

LET me state at the outset that I do not approve of destructive criticism. If a stone in the structure of our science be faulty it is hardly advisable to remove it, unless we have a better to put in its place. Thus it seems to me that the popular articles Professor Pearson has lately presented to us are lacking in that constructive attribute. Still I think we owe a great debt to the work of the Eugénics Laboratory for the able way in which the necessity of number has been demonstrated.

From my own experience, however, of the public health branch of the medical profession I certainly am not of the opinion that the members of that branch err on the side of credulity, nor do I think there could be found a single instance where a medical officer of health has blindly applied to his district data collected in another. In this way a healthy suspicion and repetition of observation largely remove the element of “chance,” against which Professor Pearson has warned us. It seems to me, moreover, that the possibility of such a source of error is already appreciated, though we owe to him its mathematical expression, and that a further stage has been reached, one which he at times overlooks, namely that of homogeneity of material.

If the data be pure but scanty there is an odd chance that it may show a correct sequence, but if it is not homogeneous even that possibility disappears. There is no difficulty in collecting vast masses of material; it is tedious and irksome perhaps, but to get a large series of observations that show biological similarity except in respect to the quality under consideration is a matter in which judgment and considerable care are necessary. Let us always keep in mind a saying, which I believe comes from the lips of Charles Darwin, “Nature is a subtle dame; she will tell you a direct lie if she can.”

To exemplify this point I will give a few examples. If we take a general survey of the work on Phthisis that has emanated from the University College, London, we must be struck with the comprehensive way in which the term “Tuberculosis” is used.

I cannot help calling to mind a warning given to me by one of my teachers in my

student days, namely, that scientifically the designation "Pulmonary Tuberculosis" should be limited to those cases in which the bacillus has been found in the sputum, irrespective of the condition of the patient, and that all other cases should be called "Phthisis." This criterion would, of course, exclude a large number of early cases, which in all probability were tuberculous, but still it also excludes such as are not of that nature. Thus the error becomes a negative one only, and not positive and negative, which, from a statistical point of view, simplifies the problem.

Within recent years, other physiological tests have come into use in the diagnosis of Pulmonary Tuberculosis. Nevertheless, in dealing with such a problem as the association of consumption in husband and wife it surely would have been helpful if it had been stated whether the bacillus was present or not in the ejecta.

When we consider the grounds on which an opinion is formed, as to whether a patient is, or is not, suffering from this disease, we realise the necessity of a simple test; thus it is a well known fact that such characters as the height-weight ratio, texture of skin, facial appearances, shape of chest and temperament, all influence the diagnosis, and, more than that, a knowledge of the family history is very apt to make a difference in our decision. All these characters are admittedly of a hereditary nature, and it is just as well that they should be excluded in scientific work. Personally, I find it almost impossible to limit one's self purely to physical signs, as detected in the chest, when examining healthy and diseased persons indiscriminately. The sub-conscious suggestion from a general survey is very strong, and it must be a factor influencing the Physician or Specialist.

So that if we are investigating the strength of the transmission of the phthisical tendency, and we, to some extent, base our diagnosis on qualities that are transmissible, it is not surprising that we should obtain high correlations, and I must admit that I fail to see how any manipulation of data would remove a possible source of error of this kind. Statistically, as bacteriologically, the criteria should be the presence of the bacillus in the ejecta during life, its demonstration in the tissues after death, or one of the lately introduced tests, but in questions involving the transference of infection, the bacillus itself should be found in such a situation as would render its introduction into a second person beyond

question. I do not think these ideals are pedantic; they are, in my opinion, simple precautions necessary to ensure the biological homogeneity of material, and as far as I can see in none of the publications of the Eugenics Laboratory has any attempt been made to purify the original data on which such elaborate precautions were taken to avoid chance factors. To take another point:—In his *First Study on Pulmonary Tuberculosis* (page 21) Professor Pearson definitely states that first children are more susceptible, and last children less, to this condition. To criticise the handling of the material, would be an impertinence on my part, but I am, perhaps, in a better position than he to judge the biological nature of the data. Is it fair to say that a woman who reproduces 14 children is similar in character to one who reproduces one or two, and thus totally ignore a possible hereditary factor? Would it not have been better to have given the type of parent some consideration?

I have data to show that women who reproduce big families, are, in many respects, different from those who reproduce small ones, and I suggest that had this factor been considered the sequence would have been reversed after the fourth child.

Take the following figures from page 20, just the page before that on which the above statement appears.

Both parents tuberculous ...	mean size of family	3.50
One parent " ... "	" "	5.42
Neither parent " ... "	" "	5.82

If heredity counts for anything we should expect the early child, in a general population, to possess a greater phthisical incidence from this observation alone. Again, take Dr. Buchan's figures, which I have corroborated quite independently, showing that the general death-rate amongst children steadily increases as they progress in sequence from the second onwards.

How can we be sure that, had these children lived, they would not have developed, in course of time, tuberculosis in a ratio exceeding that of those surviving? and I think I am justified in saying that as the family increases, the tendency to tuberculosis in the parents diminishes, though this must not be taken to mean that child-bearing of itself reduces the phthisical diathesis. Hence, I have no doubt, that if the material were purified in this way, the influence of birth sequence would be found to agree with that demonstrated by Mr. Heron for Insanity.

At the International Congress on Eugenics last August, Mr. Macauley, President of the Society of Actuaries of America, drew attention to an even more serious statistical fallacy, one which could easily have been avoided had the precaution been taken to estimate the mean ages of each group of siblings. He pointed out that the method of selection was not a random one, and that the Professor had included units of all ages, many of which would of course be infants or children. The point may be expressed in another way—If the basis of selection is that of families taken at random, a greater proportion of first births will have reached the phthisical age than late births, and hence the greater incidence.

In the *Journal of the Royal Sanitary Institute* (August, 1912) the Professor has ridiculed some suggestions of mine based on 79 cases of Pulmonary Tuberculosis. His objection would be perfectly valid had I stated that my figures were beyond chance error, but I did not. I had, however, taken the precautions necessary to secure some degree of similarity in my comparisons. Thus, the Tubercle Bacillus was found in all cases, the mean ages of my tuberculous and normal populations were the same, the sex distribution was similar and the social grades from which they were taken were alike.

How many of these requirements were satisfied in the 400 cases, that Professor Pearson used? If my surmise be correct he compared persons with a mean age of about 28 years with a population whose mean age was under one year. We are, I think, justified in refusing to accept such unscientific work. It is interesting to note that an increase of the number in my own data has verified the original observation, namely, that this disease is more frequently met with amongst those born at the extremes of life than those born at maturity.

Though I do not wish to labour these queries, let me, however, take two other examples of error arising from this lack of biological homogeneity.

I take, in this case, "*A Second Study in Pulmonary Tuberculosis—Marital infection*," and from this I turn to a popular pamphlet—"*Tuberculosis, Heredity, and Environment* (1912)"—and the first thing that strikes me is the difference in attitude that has taken place in the four years, 1908-12.

In the first and more elaborate memoir the data are discussed, and some of their peculiarities are noted, and the possible fallacies set out.

The later pamphlet largely draws from the former, though some work by Dr. Goring is added, but the main difference is the alteration in tone. Where error was admitted as likely before, now it is excluded and facts are given us to be swallowed whole. It seems to me that the possibility, that Phthisis may shorten life, has been overlooked, and that such units would hardly be met with in a random selection as often as might be expected.

The natural reply to this is, that we can go to the churchyard as well and there complete our investigation. It is surely not necessary for me to state that people do not reproduce under such conditions.

This would not be a source of error, however, were the point under investigation one other than hereditary transmission of character. That is to say, a character that tends to shorten life, before or during the reproductive period, will be found less frequently than it should be in a random selection.

This contention is applicable to the data relative to marital infection, which Professor Pearson has collected from his own records. Nowhere, so far as I can see, does he give the mean ages of the three groups—healthy, one mate phthisical, and both phthisical, and thereby allowing a correction to be made.

To put the point shortly, I take the following from his own work:—

PEARSON.—PROFESSIONAL CLASSES.
TUBERCULOSIS.

HUSBAND.				
Actual.	Wife.	Not T.	T.	Totals.
	Not T.	992	43	945
	T.	49	6	55
		951	49	1,000
				Correlation 0.24

HUSBAND.				
Chance.	Wife.	Not T.	T.	Totals.
	Not T.	899	46	945
	T.	52	3	55
	Totals	951	49	1,000
				Transfer + 3 - 3 - 3 + 3

INSANITY.				
Actual.	Not I.	I.	Totals.	Correlation 0.24
Not I.	856	71	927	
I.	60	13	73	
Totals	916	84	1,000	
Chance.	Not I.	I.	Totals.	$\begin{array}{c c} +7 & -7 \\ \hline -7 & +7 \end{array}$
Not I.	849	78	927	
I.	67	6	73	
Totals	916	84	1,000	

The conclusion deduced from the above is, that since there is as much association between husband and wife with reference to Insanity as Tuberculosis, the infection factor cannot be very strong. The question I ask is, what were the mean ages of the classes given? Should the mean age of the group tuberculosis in husband and wife be less than the others, as it probably was, then the requirements of biological homogeneity are not satisfied.

I cannot, however, allow this example to pass without expressing some appreciation of the aptness and subtlety not only of the illustration, but of the whole line of argument pursued in that paper.

Then again, take Dr. Pope's data with reference to the presence or absence of Phthisis in the parents of offspring suffering from that condition and marital infection. The basis of selection, it has to be remembered, is parents of tuberculous offspring. Turning to the quotation already given, we see that where both parents were tuberculous, the mean family is 3.5, and where one only 5.4. Hence on this basis, the number of occasions where this disease was present in both father and mother of a tuberculous offspring, should be multiplied by two, or more accurately, $\frac{5.4}{3.5} = 1.5$. Surely this would increase the correlation between husband and wife for this condition so that a most important step in the subtle chain of reasoning, with which the Professor entranced us last March, may be most fallacious.

In dealing with alterations in death-rate from phthisis, in the latter part of the same paper, he seems to me to be obsessed with the law of natural selection and survival of the fittest. I have suggested other explanations, as a possibility, and have brought his ridicule and cutting scorn upon my unpretentious efforts. I have collected some data which seem to show some connexion between the alteration in the reproductive habit, and the lowered death rate, and I hope to obtain further evidence in favour of this hypothesis later.

Again, in the generalisations in "The Fight Against Tuberculosis, 1908," he has blindly assumed that the virulence of the bacillus is a constant, and has not varied in the last hundred years, yet, from laboratory experiments, I think we should all agree that of the variables—man and tubercle bacillus—the latter is the less constant factor.

Yet another rather disturbing possibility arises, concerning which investigation is necessary, namely, that if we replaced the word Tuberculosis by Scarlet Fever, in the memoirs already referred to, they would read equally well with perhaps some slight alterations, due to the different nature of the diseases as regards duration. These conditions appear to have two characteristics in common. Firstly, the infection is much more widely diffused than is supposed; and secondly, the reaction, as we mean it—that is, discomfort to ourselves; a different state from reaction in the sense of simple infection, which includes a condition of symbiosis as well as parasitism—is dependent on inherited characters.

Lest the reader may dismiss the foregoing suggestion as fanciful, let me give one series of observations out of a number of such, which he can verify in his own district should he care to take the trouble. I have not as yet calculated the probable errors, but in any case they will be small.

The incidence of Scarlet Fever has been found to be as follows:—

Up to and including	5 to 6 years	... 4 to 5 per cent. attacked.
" "	7 to 9 "	... 7 to 8 "
" "	13 to 14 "	... 13 to 14 "
Reproducing women	... 17	" "

Thus if we take the number per hundred of mothers who have had scarlet fever as 17, and of their offsprings as 14, how often would we expect in 100 trials to meet a mother and child both of whom have had Scarlet Fever?—obviously the number is $100 \times \frac{17}{100} \times \frac{14}{100} = 2.7$, or about once in 50 times.

Now in 446 children 60 had been attacked with this disease, the mothers had been similarly affected in 28, or approximately 45 per cent., and out of another series of 58 mothers who had had Scarlet Fever, and possessed a child of 6 years of age, 28 or 46 per cent. had already been attacked. It is to be observed the number attacked in the general population is 4 per cent. at this age. Lastly, out of four hundred pairs taken at random, the children being of constant age, Scarlet Fever was found in both parent and children, 7 per cent. The expected number being 2.7 per cent.

It is obvious, I think, that the tendency to re-act to the virus upon which this disease depends is a transmissible character, and further, for such a strong correlation to exist, the infection must be practically everywhere, as very few susceptible units seem to escape.

Another way by means of which the same point may be shown is to take the number of houses in which two or more cases occur. In the town under consideration there are 15,000 households approximately, and close on 200 cases of Scarlet Fever are notified per annum. Now as a matter of chance two cases in one house should be met with about once or twice each year. It is found, however, that 20 per cent. of the cases occur in households in which someone has been infected within the previous three months.

If the micro-organism were the sole factor, and the soil counted for nothing, the percentage should rise to 75 per cent., for in that number of households are there children of susceptible ages. In measles, for example, 75 per cent. of children brought into contact with a previous case eventually develop the disease.

I do not wish in the present instance to carry this line of inquiry any further. All I desire to indicate is that a comparison between Tuberculosis and Scarlet Fever would have been most helpful, and I fancy would have led to some modification in the dogmatic statements made by Professor Pearson.

Further, it should not be assumed that all the research that he has done on this subject is unreliable owing to its faulty biological basis. In the main, I believe it to be correct and to agree with those somewhat vague impressions that slowly grow in the minds of those who handle considerable quantities of material and who are in any way observant.

If I may dare to generalise I would say that organisms are freely transmitted from individual to individual; but whether they

become symbiotic or parasitic depends on the nature of the soil. In the case of Scarlet Fever we have isolated all those upon whom this organism becomes parasitic—probably a small proportion only of those upon whom it is symbiotic—with the result that, while the gross amount of infection amongst us has not been materially altered, we have prevented the organism from becoming enhanced in its virulence, and hence we get a fall in the death-rate, but not in the attack-rate.

It is, therefore, possible that the control of those upon whom the tubercular organism becomes parasitic may have the same effect as isolation has had on the death-rate from scarlet fever.

I do not suggest this as proved, but simply to justify the doubt I feel as to the truth of the little facts that Professor Pearson thinks he has adequately demonstrated, and also to draw some attention to the importance of biological homogeneity rather than number of observations as an ideal.

If the data be pure, and the number adequate, I do not think elaborate mathematical methods are necessary to draw an accurate conclusion as to which way the bias tends, though to measure it we must hand our work on to those specially trained in the handling of figures.

Let me, in conclusion, say that I have not made these observations in any carping spirit, but simply to try to correct a tendency which seems to me to be leading to error.

NOTE.—The papers and memoirs referred to are as follows:—

- I. First Study of the Statistics of Pulmonary Tuberculosis (Inheritance). By Prof. Pearson (Dulau and Co.).
- II. Second Study of the Statistics of Pulmonary Tuberculosis (Marital Infection). By G. Pope, revised by Karl Pearson (Dulau & Co.).
- III. Tuberculosis—Heredity and Environment. By Karl Pearson (Dulau & Co.).
- IV. The Fight against Tuberculosis and the Death-rate from Phthisis. By Karl Pearson (Dulau & Co.).
- V. Eugenics and Public Health. By Karl Pearson. *Journal of the Royal Sanitary Institute*, September, 1912.

TOWN OR SUBURB?—Is there any indication of the present craze of getting out of London to the suburbs—supposed to be more or less rural(?)—passing away? According to the medical officer of health of Islington there are signs that the lure of suburban rurality is found to be too heavily handicapped by the longer train journey, with its inherent waste of time and money, and the household difficulties associated with absence from centres of distribution.