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METHODS OF INVESTIGATION IN SOCIAL AND HEALTH PROBLEMS

PAPERS BY

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FRANZ SCHNEIDER, JR.

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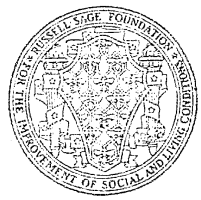

READ AT CINCINNATI MEETING OF THE AMERICAN PUBLIC HEALTH ASSOCIATION OCTOBER 24-27, 1916. REPRINTED FROM AMERICAN JOURNAL OF PUBLIC HEALTH, VOL. VII, NO. 1, JANUARY 1917

DEPARTMENT OF SURVEYS AND EXHIBITS
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THREE PAPERS

The Necessity for Health Standards

Donald B. Armstrong

Some Shortcomings of Socio-Sanitary Investigations

Franz Schneider, Jr.

The Application of the Statistical Method to Public Health Research

Louis I. Dublin

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THE NECESSITY FOR HEALTH STANDARDS.

DONALD B. ARMSTRONG, M. D.,
Executive Officer of the Community Health and Tuberculosis Demonstration, and Assistant Secretary of the National Association for the Study and Prevention of Tuberculosis.

AT THE Rochester conference of the American Public Health Association, during the closing meeting of the general sessions, a strong appeal was made by Dr. E. C. Levy of Richmond for a satisfactory formula to be used by health officers in the standardization of death-rates. Doctor Levy pointed out that while this would be an instrument to be employed by health officers, it was up to the statistician rather than to the health officer himself to provide the mechanism.

Undoubtedly the average health officer, because of the deficiency which Doctor Levy emphasized, is in a more or less serious plight today. It also happens that the socio-health investigator finds himself in much the same plight, and for a somewhat similar reason. In the average instance this type of health worker is not a statistician and does not have professional statistical training. Consequently, though his intentions may be of the best, he occasionally blunders into errors in his efforts to collect and present research data largely because he doesn't always know to what extent his subject is adaptable for a statistical analysis or because he is not familiar with the methods and limitations of accurate mathematical presentation.

It is true, of course, that socio-sanitary studies are carried out to increase knowledge and not to make statistics. The statistician, of course, must be the servant of the investigator. At the same time, much research work along these lines in invalidated, because of inaccuracies, hazardous generalities, the acceptance of assertion for proof, etc., on the one hand, or, on the other hand, because of lack of care in defining the scope of the inquiry, in classifying and tabulating data, in analyzing and interpreting results, etc. Is it not possible for the

statistician to emphasize at least the basic requirements in the health investigative field? It is not to be supposed that socio-health research can ever be made absolutely foolproof. Can it not, however, with the assistance of the statistician and the standardization expert, be made a field less productive of unreliable and irresponsible findings, and less hazardous for the reputation of the investigator?

It would seem, to quote a recent communication from an American statistician of note, that "the usual socio-health investigator is willing to adopt in his search for facts, the method of the shot-gun rather than the rifle." He hopes that out of a poorly planned, but enthusiastically manned investigation, something useful will come. Sometimes something useful does result, but very frequently, too, there is promulgated an excessive amount of misinformation under the guise of pseudo-statistical accuracy. This is one reason, perhaps, why reformers so radically disagree, and may also have something to do with the fact that health administrators seldom get entrusted with adequate funds for their work. If the public judges us by our differences, and our inaccuracies, might it not be that we are receiving, for certain phases of public health work at least, about all the financial support our programme deserves?

Now, it seemed to the officers of the Sociological Section that this matter was of importance not to that section alone, but to the Association as a whole, as well. It seemed, further, that the most effective assistance might be expected from the Statistical Section. While the solution of these problems is vital to the balance and sense of the socio-health workers and to the validity and accuracy of their conclusions, it is, at the same time, essential to the good reputation of statistics, if not of the statisticians. Consequently an informal committee representing the two sections was created to prepare the present joint programme.

As many of you know, the first step after the preliminary consideration by this committee, was to send out a questionnaire soliciting information and suggestions along the lines indicated by the foregoing discussion. As a result of this work, it seemed that there were several approaches to the subject as a whole. It seemed, further, that it would be best to attempt to treat it, even though in a preliminary and somewhat superficial way, through at least two quite obvious channels. Consequently, the subsequent papers on the programme will attempt, first, to point out, from a general point of view, the chief defects of value judgment in modern socio-health investigations, and second, to discuss the commoner statistical errors in health work, together with methods for the application of corrective statistical devices. It is expected that the subsequent discussion, initiated by those whose names appear on the programme, will be, perhaps, the most illuminating part of the programme.

SOME SHORTCOMINGS OF SOCIO-SANITARY INVESTIGATIONS.

FRANZ SCHNEIDER, JR.,
Sanitarian, Department of Surveys and Exhibits, Russell Sage Foundation.

IN EXAMINING reports of investigations in the field where sociology and sanitation overlap, one is appalled by the frequency of slovenly technique, unsupported argument, and logical fallacy. Many of these reports, presumably presenting the results of scientific investigations, are hardly more than expressions of the authors' personal opinions. The figures presented are often of very questionable significance. The supporting evidence gleaned from other sources frequently takes the form of expressions of opinion. Yet these things are put forward as incontrovertible evidence, and, for the most part, with excellent and honest intentions. Examination of such reports produces a feeling of discouragement—almost of despair. There is so much poor work aggressively asserted, and so little good work. It is all very human, but it is all very far removed from science.

The cause of this state of affairs is not far to seek. Public health is not as yet an exact science; sociology is hardly a science at all. If we add that very few of the persons carrying on socio-sanitary investigations have had statistical training or experience, or, in fact, scientific training or experience, I think the situation is easily understood.

One's feelings on examining many of the reports in this field are well expressed by Professor A. G. Keller in his searching essay, "Sociology and Science."* Professor Keller inquires, with regard to the run of sociological treatises, " 'What would a genuine scientist, say, Professor———, think of this?' " His answer is, "An opulence of pronouncements; a dearth of evidence—with the result that the former are unverifiable, unless, perchance, they represent 'painful elaborations of the obvious.' "

I would like to draw attention to a few examples of this sort of thing in our own field, and try to point out the more important types of defect. In taking my first example from the field of housing betterment I wish particularly to emphasize the fact that I am heartily in sympathy with housing reform. Good housing is a matter of essential decency that can stand unshakable on the foundation of its own inherent desirability. What I wish to discuss is the scientific question of the effect of housing on health.

HOUSING AND HEALTH.

The investigation which I have examined is one of the more recent and more carefully made studies of housing conditions. I am prepared to believe that the part setting forth the actual conditions found in the houses (which

* *The Nation*, New York City, Vol. 102, No. 2653, page 475, May 4, 1916.

occupies the major part of the report), is beyond criticism; the discussion of the relation between housing and public health starts, however, with the following words:

"It seems almost useless repetition to refer at this time to such a well-established fact as the intimate relation existing between housing and the public health. The only excuse one can offer for referring to the subject is that, in spite of the fact that it is so evident and has been so definitely proved time and again by numerous observers, this aspect of the public health question has not received the attention from the public officials that it deserves.

"Without attempting to show in detail in just what manner the various common defects in housing affect the health of the tenants, we will merely call attention to the importance attached to housing by several national bodies and health officers."

The author then cites the facts that at the 1911 meeting of the American Association for the Study and Prevention of Infant Mortality there was a housing section, and that this section considered housing important; and that the British national tuberculosis association included the subject for discussion in the program for its meeting of 1914. He then quotes the opinions of five persons to the effect that housing is of predominant importance in tuberculosis; gives a few death-rate figures for Edinburgh and Liverpool; and concludes with a quotation which relates that in a certain city pin maps showing cases of infectious diseases corresponded to those showing bad housing conditions. The final paragraph of the section reads:

"The value of the opinions of these physicians and health officers who have given the subject considerable study and the experiences of these cities must convince even the most prejudiced that, from the standpoint of public health, the housing of the population is a matter which deserves the most careful consideration."

No references to sources are given.

I wish to analyze this example for a few moments as it impresses me as a striking instance of most positive statements backed up by the slightest scientific evidence.

In the first place, I think every one will agree that the fact that two national associations have admitted a subject to their programs and that certain members of these associations think the subject of predominating importance, amounts to nothing in the way of scientific evidence. Of no more convincing value is the citation of the similarity of the pin maps; it disregards the elementary principle of rates. In those parts of a city where population is densest it is but natural to expect a clustering of pins on the pin map—whether the latter represents cases of diseases, bad housing, or any other social manifestation.

With regard to his Edinburgh figures our author admits his doubts in the following words:

"It has been contended that tuberculosis was more prevalent in certain districts, not on account of the bad housing, but because, where bad housing was most prevalent, there one also

found poverty, with all its associated evils."

He states, however, that: "The results obtained in Liverpool show the fallacy of this contention.

"The city of Liverpool in several districts tore down large sections of defective houses (from the standpoint of health) and erected suitable sanitary dwellings *in which practically the same people were housed who had occupied the former dwellings*. The result was that the phthisis death-rate fell in these districts from 4 per 1,000 to 1.9 per 1,000. The death-rate from consumption was more than cut in half by the improvement in *housing alone*."*

Liverpool is accordingly his clinching evidence. He avers that practically the same people were rehoused and that the death-rate from phthisis was more than cut in half by the improvement in housing alone. This is evidence that, so our author says, must convince the most prejudiced. It certainly is important, if true. Let us see how it stands the test.

No reference is given to the paper or papers setting forth the Liverpool experience, so that the author may have had before him sources of information unavailable to me. However, I have examined the reports for 1912 and 1913 of the Housing Committee which has had this rehousing work in charge,† as well as the reports of the Medical Officer of Health for the years covered by the rehousing work.

The references to the health effects of the rehousing which I have found are as follows. In the report of the housing committee for 1912 there is the following paragraph (page 12):

"The improvement in the condition of the people rehoused by the Committee is very striking. Reliable statistics were available at the time the insanitary areas were dealt with, to show that the death-rate on these areas ranged from forty to sixty per thousand per annum, mainly due to the insanitary condition of the property, although squalor, bad habits, and the extreme poverty of the tenants were powerful factors in contributing to cause this high rate of mortality. The incidence of phthisis amongst these people, under their old conditions, resulted in an average annual death-rate of approximately 4 per 1,000. Under the new conditions, the general death-rate has been halved, and the average annual death-rate from phthisis has fallen to 1.9 per 1,000. A very careful investigation has been made into the incidence of typhoid fever, and leaves very little room for doubt that the defective and insanitary closet arrangements in the courts was one fruitful factor in the dissemination of this malady. It is a singular coincidence that year after year as the number of courts have diminished, so, also, the cases of typhoid have diminished; no doubt other factors were operative also, but that this was one, there is no reason whatever to doubt."

A little further on (page 15) we come to the following:

"It is, however, still to be regretted that the Infantile Mortality is exceptionally high. Based on the present

* The italics appear in the original.

† City of Liverpool, Artizans and Labourers Dwellings and Insanitary Property, Reports of the Housing Committee, for 1912 and 1913; Liverpool, 1913 and 1914.

population, and excluding the unrestricted tenements, the Infantile Mortality for the years 1909, 1910 and 1911 indicates a death-rate of 245 per 1,000, as compared with 139 per 1,000 for the entire city. Apart from other considerations, this fact alone emphasizes the necessity for special training on the part of those who supervise the Dwellings."

This is all the committee reports, and it is very little. In the report for 1913 these statements are reiterated in slightly different form (page 5); in addition (page 98) a table appears giving the average death-rate from all causes and the average phthisis death-rate for 1904–1906 of six of the unhealthy areas dealt with. The average population of these areas was 536; their general death-rates varied from 26.2 per 1,000 to 54.9, and averaged 44.4; their phthisis death-rates ranged from 1.1 to 7.0 per 1,000, and averaged 3.9. The significance of this table is doubtful, unless it is as a basis for the statements regarding rates of mortality before rehousing. A chart is given which shows decline in the city's typhoid fever rate, which parallels not only the number of courts and alleys but percentage of cases treated at home.

To sum up the evidence from the Housing Committee reports, it appears that the corporation has been rehousing displaced people since 1896, the rate of demolition since 1902 being something like 500 houses per year. By 1912 the corporation had under its control 2,747 dwellings, of which 79.7 per cent. were reserved for persons who had been dispossessed. It is stated that the phthisis death-rate has fallen from 4 per 1,000 under the old conditions to 1.9 per 1,000 under the new; but no specification is made as to the period covered by this fall or the area and population to which it applies, and no supporting evidence is adduced. Evidently the reports of the committee are not an adequate original source.

Turning to the annual reports of the Medical Officer of Health little more definite information is to be found. The same general assertion regarding the fall in the phthisis rate appears,* this evidently being the source of the Housing Committee's statement. I have been unable to find any definite elaboration of this general statement. In looking back through the reports it is evident that the process of rehousing was a gradual one extending over a period of about fifteen years, a period during which the general and phthisis death-rates of the whole of Liverpool and of all England underwent marked decline. We are not dealing with a clear-cut experiment of which a definite record has been kept and published. The data regarding the fall in phthisis among the rehoused is most fragmentary.

One's belief in the sweeping statement regarding the effect of housing on health in Liverpool is already shaken. But how about the statement that "*practically the same people were housed who had occupied the former dwellings?*" Well, the report of the Medical Officer for 1913 (page 299) states that the population of these

* Report of the Medical Officer of Health for 1911, 239.

corporation tenements was 10,223, of which 1,799, or 18 per cent., were "unrestricted"; that is, other than dispossessed. For six specific rehousing schemes 73 per cent. were rehoused in corporation dwellings; of all persons dispossessed since 1896 only some 64 per cent. were rehoused.* Thus a very considerable proportion of those dispossessed were not rehoused.

In addition, those rehoused were a carefully chosen group. Listen to the Medical Officer's report for 1907 (page 231); "The committee are already aware that in accordance with their instructions applicants of drunken habits and of dissipated character are excluded from the dwellings, and that a selection of the better class of dispossessed is being made. This is an additional reason why better results may be looked for." In other words, the poorer stock was weeded out. So much for practically the same persons being rehoused!

There remains the assertion that the death-rate from consumption was reduced "by the improvement in *housing alone*." The selection of the best tenants for rehousing of course vitiates the force of this statement, but there are other grounds for objection. For example, when rebuilding an area the corporation bought out and removed a large proportion of the saloons. The Medical Officer repeatedly emphasizes the importance of this policy and dwells on its beneficial results.† In addition we must reckon with an actively developing health department which inaugurated in 1897 a staff of Female Inspectors to do infant hygiene work in homes and which "little by little, was added to";* which initiated a system of milk depots in 1901;† which instituted a systematic medical inspection of school children in 1908;‡ and which in 1901 requested the reporting of tuberculosis and has since progressively developed a well-rounded campaign against the disease.§ Now when we find that there is reason to believe that the health department exercised especially close supervision over the corporation's own dwellings,|| that playgrounds and gymnasia were included in the rebuilding schemes,¶ and that old age pensions went into effect during the rehousing process, it becomes very evident that there was a great deal going on besides the improvement in housing alone.

The conclusion of the whole matter is something like this. The Liverpool rehousing work was not conceived as a demonstration of the effect of rehousing on health and was not carried on in such a manner as to permit a clean-cut answer to that question. The actual fall in disease rates is uncertain, because the time periods and populations involved are uncertain. Only two-thirds of the persons dispossessed were rehoused, and these were selected

* Report of the Housing Committee, 1913, page 4.

† See Annual Reports: 1909, page xix; 1912, page 246; 1913, page 294.

* Annual Report of the Medical Officer of Health for 1913, page 64.

† *Ibid.* page 72.

‡ Annual Report of the Medical Officer of Health for 1908, page xv.

§ Annual Report of the Medical Officer of Health for 1913, page 101.

|| Annual Report of the Medical Officer of Health for 1911, page 252.

¶ Annual Report of the Medical Officer of Health for 1913, page 294.

with reference to their habits and desirability as tenants. Finally, many important health measures were undertaken simultaneously. The Liverpool experience is a splendid example of civic regeneration, but it has not contributed any convincing scientific evidence to our knowledge of housing and health.

I have gone into this reference to the Liverpool experience because it is a striking example of the hasty generalizations dogmatically uttered that are so frequent in reports dealing with social and health conditions, and because it occurred in one of the better reports. Before leaving the question of housing and health I would like to call attention to some of the other dangers that abound in this perilous field of investigation. These are well exposed in Prof. Karl Pearson's paper. "Eugenics and Public Health.* The ease and clearness with which Professor Pearson can lay bare the fallacies underlying apparently significant statistics will be a revelation to those unfamiliar with the publications of the Galton Eugenics Laboratory. In the paper mentioned he shows how the prevalence of phthisis among persons occupying different sized tenements may be associated with questions of age distribution, economic conditions, and the physique in the stock. In another place he takes up the oft quoted Glasgow data, published by the British Local Government Board, which represent that by virtue of living in four room tenements boys weighed 11.7 pounds more than boys living in one room tenements, and shows that of this difference 6.2 pounds was due simply to the fact that the boys living in one room houses were younger.*

And Pearson does more than subject the existing data regarding the social causes of tuberculosis to destructive criticism, he produces evidence regarding the importance of inherited physique that cannot be ignored. In considering the effect of environment we must take into consideration his investigations showing that whereas correlation between husband and wife with regard to tuberculosis is about that which they exhibit with regard to "sexually selected" physical, psychical, and pathological characteristics, the correlation between parents and children with regard to the disease is twice as great, or an amount about equal to that exhibited between parents and children with regard to such inheritance factors as stature, eye color, deaf-mutism, and insanity.†

Altogether, it seems to me that we have very little scientific evidence regarding the effect of housing on health. The situation is aptly put by Dr. Charles J. Hastings in the following words:

"We naturally ask ourselves then,

* Pearson, Karl: "Eugenics and Public Health, "Questions of the Day and of the Fray, No. VI; Cambridge University Press, London, 1912.

* Pearson, Karl: "Social Problems: Their Treatment, Past, Present, and Future," Questions of the Day and of the Fray, No. V; Dulau & Co., London, 1912.

† Pearson, Karl: "Tuberculosis, Heredity and Environment"; Dulau & Co., London, 1912.

For a discussion of the causes of the decline in the death-rate from tuberculosis see the same author's, "The Fight Against Tuberculosis and the Death-Rate from Phthisis," Questions of the Day and of the Fray, No. IV, Cambridge University Press, London, 1911.

how much of the misery in our back streets is due to one thing and how much to another? How much is due to worry? How much to dark, dusty factories? How much to improper clothing? How much to improper, insufficient, or badly cooked food? How much to general malnutrition? How much to over-crowding? How much to unsanitary privy pits? How much to drunkenness and dissipation?

* * * * *

"Have we not taken too much for granted in the deductions we have made in the past in our investigations, and has not the time now come in the march of social progress when we must be more specific and debit to the various items of our list of social ills, the proportion of maladjustment due to each?

"While I do not wish for a moment to underestimate the significance of good housing conditions, yet I wish strongly to urge the necessity of a more careful analysis of the various causes of the greater prevalence of tuberculosis under bad housing conditions."*

Other Examples.

Let no one think that unsatisfactory investigation is limited to the field of housing and health. What are we to say of a report entitled "Facts About ———," which devotes three printed pages to an attempt "to obtain some information at first hand" regarding privies as a cause of an epidemic of diphtheria? Among other things the investigator reports the following choice bit of evidence:

"A very intelligent appearing woman, an old resident of North ———, says that undoubtedly condition of privies has something to do with spread of diphtheria, for as she counts the cases she finds most of them lived in houses with privies."

And further on:

"House found on Broadway, privy filled absolutely to the brim. One case of diphtheria on top floor."

Of course this is simply an example of a well-meaning person dabbling in something in ingenuous and almost total ignorance.

From another report, a "survey or social inventory" comes the following example of elegant vital statistics:

"There were in ———burg in 1913 ten deaths; of these five were male and five were female. They were distributed according to age as follows:

"1 under 1 year,
1 under 5 years,
3 between 50 and 60 years,
3 between 70 and 80 years,
2 between 80 and 90 years.

"Taking the population of ——— burg at 683, the number given by the census of 1910, the death-rate would be 14.6 per 1,000 of the population. The death-rate of the United States is given as 16.3 per 1,000. H. N. Ogden, in his book, 'Rural Hygiene,' page 3, under the caption 'Ideal Death-rates,' says, in a comparison of the rates of different nations,—'Norway, Denmark and Sweden have

* Hastings, Charles J.: "Relative Prevalence of Tuberculosis Under Good and Bad Housing Conditions"; Transactions of Eleventh Annual Meeting of the National Association for the Study and Prevention of Tuberculosis, page 335, Baltimore, 1915 (105 East 22nd Street, New York City).

rates of 14.5, 14.8 and 15.5, respectively; rates which may be considered as good as any country can attain at the present time.' Thus the health of ———burg would seem to be 'ideal.' "

It is all very simple; if the crude death-rate of your city is under 15.5 its health conditions are ideal! It seems a pity that our statistician did not use the population estimate for 1913, which would have given ——— burg 41 more inhabitants and would have reduced its death-rate from 14.6 to 13.8; but then, why try to improve on the "ideal?"

After all, these bulls by untrained investigators are not so remarkable when such a statement as the following can pass the eye of the editor of a recent text-book of educational hygiene. Speaking of the importance of sunshine in schools the contributor alludes to an experiment showing that tadpoles will not grow without sunlight, and continues:

"Just such an experiment society tried with babies in the dark rooms of New York's tenements, and every baby, almost without exception, born and kept in those rooms died. But when Jacob Riis and others tore down the windowless walls and the light of heaven streamed in, the babies began to live."*

Now I would like to have a reference to the source of that statement. Doesn't it come pretty close to being pure emotionalism?

Almost any number of instances of this sort of thing could be produced, but I shall not bother you with them further. Interested persons can refer to Pearson's papers, and there see shattered a number of widely-circulated and much referred-to investigations. The supposed blazing-up in the conception of idiots at the time of the vintage in wine-growing countries*; the figures reported as showing that unfitness for hard work is the result of alcoholism†; figures regarding the effect of employment of mothers, back-to-back houses, and other factors on infant mortality‡; and the question of "cancer houses" in Dublin§; all these are subjected to analysis with surprising results.

Association not Causation.

Is it possible to classify the shortcomings of socio-sanitary investigations and to say which are the most frequent and most important? Doctor Dublin will in his paper¶ treat this question in a systematic way; I cannot refrain, however, from emphasizing one or two of the defects that seem of particular importance. The mistaking of association for causation is certainly one of the most serious. Two variables, such as a death-rate and an environmental condition, are measured; a variation in one is found to be associated with a corresponding variation in the other; from this the conclusion is jumped at all too frequently that the one is the cause of the other.

* Educational Hygiene, Scribner, N. Y., 1915; page 389.

* Social Problems, etc., page 23.

† Eugenics and Public Health; page 22.

‡ *Ibid.* page 24.

§ *Ibid*, page 16.

¶ Dublin, L. I.: "The Application of the Statistical Method to the Field of Public Health Research;" American Journal of Public Health, VII, 1, Jan., 1917, p. 14.

The logical fallacy involved is the same one we are familiar with in *post hoc* reasoning; after this, therefore, because of this. The avoidance of the error involves the application of the elementary principle: all other things being equal. The point seems too obvious to urge, yet it is overlooked with astounding ingenuousness.

The danger of mistaking association for causation is greatest when but a few of the possible factors are measured; as the number of associated factors is increased it becomes more obvious that the matter of causation is complicated. Thus in a recent investigation of infant mortality* the variation in mortality is given in association with some 32 possible causes, such as dryness of home, yard clean or dirty, number of others sleeping in the baby's room, age of mother, number of pregnancies, and earnings of the father. Now it is obvious that some of these associations are accidental and that others are not equally important, or not of importance equal to the variation in the mortality rate with which they are associated. For example, it would not be contended because the infant death-rate with water supply inside the house is 118 and 198 when outside, as compared with 132 with city water available and 148 without city water, that it is much more important to have the water supply inside the house than to have city water, although this is what the figures indicate. Or, as the figures indicate, that it is more important to have a bath-tub than a water-closet. But many an uninitiated investigator is capable of measuring association with a single variable, such as cleanliness of yard, and of announcing then that dirty yards are a main cause of infant mortality

The determination of true causation is a difficult matter, calling for much patient elimination of other factors, or the giving to them of due weight. Pearson puts the matter in this wise:

"Here we reach the great rule of modern statistics: 'When investigating the relation of two characters which you find associated, test whether they still remain related after you have given all other characters likely to be influential constant values. Before you have done this you certainly must not treat the relation as a *causative* one.'

"The full theory of this method is what in modern statistics we term the treatment by partial correlation."*

To many, such a painstaking mode of investigation before the acquirement of "results" may seem an intolerable hardship, but it is the price that must be paid if we are to acquire that degree of certainty in our conclusions, and the popular respect for the latter, which prevails in the case of the recognized sciences.

Definition of Terms.

Another of the major causes of unsatisfactory investigations is lack of precision in the use of language—lack of definition in the terms used. Anyone who has attempted to compare

* Duke, Emma: "Infant Mortality: Johnstown, Pa."; Children's Bureau, Washington, 1915.

* Eugenics and Public Health; page 24.

the results obtained in different investigations of the same general subject knows how fatal this defect is to the general usefulness of a piece of work. An indefinite term may mean one thing to the person who plans the investigation; another to one enumerator and something else to some other; and it may have an appreciably different meaning to the editor of the data. Results obtained under such conditions are frequently worse than useless. Part of the trouble consists in ignoring standardized terms when they exist; and this offense is the worse when the indefinite term is used so as to give the impression that a well-known term is meant. Even when definite terms are used in the investigation it sometimes happens that the published report fails to give the necessary specifications. An example of this is such a statement as "the contagious disease rate was 25," where no inkling is given as to the diseases included in the contagious rate, or as to whether the rate is figured per 10,000 or per 100,000 of population. Indefiniteness is certainly a most effective way of destroying the value of one's labors.

The Scientific Spirit.

Many other important points to be observed in making a statistical investigation are touched on in Doctor Dublin's paper.* Accordingly, I shall pass over the questions of an adequate plan, preparation of schedules and questionnaires, standardization of enumerators, collection, tabulation, and analysis of data, precision of measurements, and errors arising from paucity of data.

In closing I wish to enter a strong plea that we carry on our investigations in the scientific spirit rather than that of propagandism, or if on occasion we do carry on propaganda that we be frank about it, and not try to pass it off on an unsuspecting public as science. Otherwise we will be plunged back into the verbalism of the middle ages, and be ineffective as well, for, after all, I fancy that the public does suspect. Have not observant journalists taken the claims of various groups of social enthusiasts as to the percentage of a given social disease, such as poverty, caused by their particular evil and demonstrated that the sum is several hundred per cent. of importance?

In this connection interest attaches to the observations of Professor Keller on the failure of sociology to win the recognition accorded other sciences.* The reason is, he says:

"In part, certainly, because the phenomena are far more complex than those of natural science; certainly also because, in this field, we may not experiment. But it cannot be denied, either, that observation has been carried on in anything but a dispassionate and objective manner, that generalizations have been formed on entirely insufficient and often incorrectly observed data, and that such hasty generalizations have commonly been erected at once into dogmas, which have then served as a basis for endless

* *Loc. cit.*

* *Loc. cit.*

and often grotesque deduction. Too many minds have conceived themselves to be of the unifying type, and have set themselves to harmonize a series of guesses, dreams, and utopias upon the basis of some 'principle' happily occurring to them.

"The phenomena invite all this. Observation is made under bias, because the facts under review are those of human life, which touch human interests. A man can count the legs of a fly and report his findings without having his heart wrung because he thinks there are too many or too few. But when he observes the life of the society within which he himself lives, moves, and has his being, or some other human society near by, it is the rule that he shall approve or disapprove, be edified or horrified, by what he observes—that is, that he shall pass a moral judgment."

I think it is clear that investigations in the socio-sanitary field have suffered from these causes. The remedy is the application of the scientific spirit. We want less assertion and more evidence; less emotionalism and more reason; less faith and more science. Let us use standardized terms and methods when they exist, and let us be explicit as to terms and method when they do not. Let us have a well defined plan before commencing work, and let us beware of association masquerading as causation.

Let us above all preserve our work from the influence of our emotions—our bias. The opportunity for investigations in the socio-sanitary field to benefit the race is an extraordinary one. Let us not waste this opportunity by indulging ourselves in emotionalism. The latter is very human, and often very winning; but it has nothing to do with science, and will not get us very far in the right direction in the long run.

THE APPLICATION OF THE STATISTICAL METHOD TO PUBLIC HEALTH RESEARCH.

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THE statistical method is today an indispensable tool for conducting scientific investigations. It is especially important in the fields of public health and applied sociology, where the data are often not quantitative but qualitative in character and where conclusions drawn are more likely to be subject to error than in the field of the more exact sciences. The statistical method, however, is valuable only in so far as it permits certain deductions to be drawn with precision, thus replacing impression or opinion. It is worse than useless if it does not accomplish this end; for its use gives the appearance of accuracy and reliability to the conclusions which are justified only by the soundness of the method itself. Too many fallacies are mistaken for truth because they are stated in statistical terms or are deduced through apparently accurate statistical procedures. This, indeed, has been the cloak of many an error which has obstructed the progress of good public health work in America.

I propose in my part of the symposium to discuss the errors which are most important in themselves or are most frequently met with in the literature of the public health worker. In doing this I shall follow the sequence of the usual investigation. I shall consider, first, those errors which mar the investigation at the very outset, that is, those which arise in the planning of the inquiry, the preparation of the schedule and the collection of the data. Second, I shall point out those errors which are commonly made in the editing, classifying and tabulating of data. Finally, I shall consider some errors which occur in the analysis or interpretation of tabulated facts. My text and illustrations will be drawn from the current literature of our field.

1. DEFINING THE SCOPE OF THE INQUIRY, PLANNING THE INQUIRY SCHEDULE AND COLLECTING THE DATA.

A successful investigation must be carefully planned. The problem in mind must be definite and should be stated, if possible, in the general form of a question. This is the method of the worker in the physical, biological and chemical sciences who answers his questions through the medium of the well organized experiment. It should be the ideal of the health investigator to organize his work after the manner of the experimentalist, putting his problems so clearly that only what bears directly on the point at issue will be

considered. Yet, it is the rule, rather than the exception, for investigations carried out in the field of public health to be diffuse. Many points are aimed at and nothing hit. Often there is but little concentration of effort on the one matter of paramount importance. Out of the mass of material collected it is hoped that somehow, and at some time, a useful project will be furthered.

The scope of the inquiry having been definitely outlined, the next step consists in planning the inquiry schedule. Many an investigation is ruined in advance because of lack of care in determining upon the form and content of the questionnaire. Nothing can come out of a study that has not first been put into it as a subject for inquiry, and no data will be received unless definitely called for. It is important, however, that the schedule should include only such items as are absolutely necessary to bring out the essential facts of the inquiry. It is better to ask a few pointed questions than to defeat the ends of the investigation by asking too much. Schedules should be well constructed as to form, printing, etc., and should, if possible, not exceed in size the standard letter sheet. The order of items upon the sheet should be arranged so as to economize the time and attention of the persons from whom the facts are to be obtained. Wherever possible, the questions should be so worded as to call for numerical replies or for statements of "yes" or "no." Qualitative replies such as "good," "fair" and "bad" should be avoided, if possible, for such returns involve too much the personal equation of the correspondent or field agent to insure reliability. Wherever indispensable, such qualitative distinctions should not exceed three in number and the limits should be defined and kept clearly in mind. Questions should be worded to present no difficulty of interpretation. For investigations of wide scope it is often necessary to prepare a set of instructions to guide the staff, both in the collection of the data and in its editing and classifying in the office. Such instructions should include the definitions of all important terms used.

Errors in the collection of data are of many types. I shall consider only those that arise out of incomplete registration of the facts. All reports and special studies in public health work are vitally affected by incomplete registration. Thus, death-rates, infant mortality rates and lethal rates for diseases are reliable or unreliable depending upon the accuracy of the registration. A death-rate will be too high if the population is undercounted. It will be too low, if either the population is overestimated or the deaths not all recorded. An infant mortality rate has no meaning if the registration of births is not complete. The lethal rate, say, for a disease like measles, is of little or no value if all the cases are not registered or if the deaths reported are disguised under such terminal conditions as "pneumonia." It is not necessary to give further illustration of this evident source of error. The literature of public health work teems with discussions which are unreliable for the reason that the registration of the primary data was incomplete.

2. Editing, Classifying and Tabulating the Data.

Editing the data is a process of preparing the original statements in the schedules for classification and tabulation. If data are incorrectly or carelessly edited they will not be representative of the original facts. In editing matter for statistical tabulations we must be on the lookout for incomplete, inconsistent, improbable and impossible statements. "Pneumonia," for example, is an incomplete statement of cause of death or of sickness for it may have been "bronchial" or "lobar" pneumonia or it may have been only a terminal condition of some other primary disease to which the case of sickness or death should be charged. "Poland" is an incomplete statement of nativity because the important item of whether German, Austrian or Russian Poland is meant, is not in evidence. "Laborer" is an incomplete statement of occupation because the important item of the industry in which the laborer worked is missing. "Prostatitis" is an example of an inconsistent and impossible statement when the decedent or patient was a female; "endometritis" when male; "married" or "widowed" when statement of age indicates clearly that the subject must have been single. As examples of improbable statements we may take those of extremely rare diseases in localities where it is more than likely that such diseases were not in evidence. It is the practice of the best statistical offices not to accept such statements without further query and such queries often clear up the errors in the original records. The schedule having thus been edited, the material is ready for classification—with one preliminary, however. The material must be divided first into two classes, that which is nominally satisfactory and that which is defective, the latter to be entirely discarded or returned to the original source for correction. Facts in the inquiry schedule which are stated in numerical terms are classified by magnitude. Statements such as occupation, birthplace, cause of death and similar facts are classified by separating the material into as many qualitative groups as are called for by the system of classification used. The classification system should be standard, that is, such as is agreed upon in advance by representative workers in the special field. If the material be statements of cause of death the International List of Causes of Sickness and Death should be used; if it be statements of occupation, the system should be that of the Population Division of the United States Census Bureau; if of accidents, the list recently evolved by the International Association of Industrial Accident Boards and the Bureau of Labor Statistics.

Indifference to the use of standard classification systems virtually destroys the value of much of our social and medical records. Hospital records are notoriously bad in this respect. In the reports of one of the largest hospitals of the country a tabulation of the diseases and conditions treated is presented, not in accordance with the International List, but in ac-

cordance with a scheme gotten up in this individual hospital. The cases were classified under several broad anatomical groups and under each of these groups by single terms as given in the original diagnoses. No uniform system of nomenclature or arrangement of titles and terms seems to have been followed. It is clear that a comparison of the records of this hospital with the records of Bellevue and Allied hospitals of New York City which follow standard practice cannot possibly be made. Much the same condition prevails at the present time in the reports of other institutions such as those for the care of the insane, reformatories, prisons, charitable organizations, etc. The chief statistical need of these institutions at the present time is a system of classification for the major items with which they are concerned.

The first essential of a system of classification is that it shall consist of mutually exclusive terms, the limitations of which are definitely established. Yet how often do we find classification systems, usually invented on the spot by isolated workers, presenting categories which overlap. Thus, a recent publication on the causes of street accidents issued by one of the largest cities of the country gave in one table the number of cases due to "carelessness of individual" and the number of cases due to "automobiles and to other conveyances." Other examples of mixed classifications in the field of vital statistics will occur to every one of you.

Tabulation in its simplest form consists in summing up the elements of the several classes into which the material has already been grouped. It is thus the first step in the investigation at which definite numerical answers to the questions of the investigation are provided. For the most part the processes involved are those of elementary arithmetic and I shall take no time to refer to errors which arise in the course of such work, although clerical errors are not at all infrequent or inconsequential. I shall refer rather to table structure as an important element in statistical method. A table is primarily a visualization of the relation between coördinate and subordinate facts. A source of error and confusion in tabulations lies in obscuring such relations. It is very common, for example, to pick up a table and find it impossible to make the several columns sum up to the totals indicated. The difficulty in such cases is not so much that an error in calculation has been made but rather that in the arrangement of the items no effort has been made to distinguish between subordinate and coördinate items. It is also important, in order to avoid misinterpretation, that the form of the table should be as simple as possible. At the utmost, not more than two or three primary classifications of the data should be displayed at one time. Thus, the amount of income per family classified according to income scale may be still further qualified by the characteristic of "number of persons in family." Such complicating factors as "nationality of head of family," "occupation of head of family" or other elements which have a decided effect on the size

of income, should be arrayed in other simple tables, where these factors alone are considered. Tables, like classification systems, should follow standard practice.

Under "tabulation" we shall consider a series of derivative concepts which are employed as aids to analysis. Such are "percentages," "averages," including medians and modes, devices for measuring variation from type (the standard deviation) and the degree of correspondence between related groups (the coefficient of correlation). It will take us too far afield to consider each of these devices in detail. I shall refer only to the simplest of them and shall indicate only the commonest errors in their computation and use.

The arithmetical average is the easiest to compute and is the one most often used. It is the ratio between the sum of a series and the number of items in the series. Its utility lies in giving a concise picture of a large group for purposes of comparison with other similar groups. The most frequent error in computing this unit consists in averaging averages. Thus, a prominent writer in the field of public health computed the average lethal rate of scarlet fever for three states by combining the three lethal rates for these states and then dividing by three. This is, of course, a very serious error. According to the definition, the true average lethal rate would be the sum of all the deaths divided by the sum of all the cases reported in all the states. The figure thus obtained is ordinarily very different from the supposed average obtained by the other method. This illustration suggests the use of weighted averages in place of simple arithmetical averages. Thus, a comparison of the cost of living at the present time with that, say, of ten years ago cannot accurately be made unless the increase in the cost of trifling commodities is discounted and the increase in the vital necessities of life is duly weighted. An increase of 50 per cent. in the cost of pepper is of but small importance while an increase of 50 per cent. in the cost of clothing is a matter of serious concern. An index of the cost of living must give due regard to the relative importance of the items which make up such a composite measure. This is accomplished by using the weighted arithmetical average.

The mode must be distinguished from the simple arithmetical average. It points out which item in a series occurs most frequently. It is useful whenever we wish to obtain a picture of the series disregarding the extremes of variation. It is often more useful, for example, to know that workers in a certain industry have a modal wage, let us say, of two dollars per day than that the average wage in the industry is three dollars, when the latter figure results from the inclusion of higher salaries obtained by a small number of persons in managerial or official capacities.

Another statistical form, which serves a purpose in tabulation that cannot be accomplished by either the average or the mode, is the median. Let me illustrate: in 1913, the average age at death for broncho-pneumonia was 19.7 years. Practical ex-

perience tells us, however, that this disease is not characteristic of the ages around 20 but that it is found most frequently in infancy and in lesser degree in old age. The disease has two modal points. The average, therefore, is not a good unit to use in this case. The median is used instead; for broncho-pneumonia it is 1.5 years. This tells us that one-half of the deaths occur under age $1\frac{1}{2}$ and that the other half occur over age $1\frac{1}{2}$ years. This agrees more nearly with practical experience. The median may then be defined as the point which divides the total number of cases of a series into two equal parts. It takes into consideration the tendency of groups of cases to congregate at the extremes. Where a series is fairly regularly distributed, following the law of error or chance, the median, the mode and the average have practically the same value.

3. Interpretation and Analysis of Results.

The community is not primarily interested in the methods of science but in its results. The conclusions and interpretations found in reports and investigations in the field of public health are, therefore, of the greatest practical importance, since it is these which determine the direction of definite programmes of action. For this reason, errors in interpretation and analysis of otherwise satisfactory statistical data are most to be avoided. Yet, the gravest errors in the use of the statistical method in public health work are made at this stage. Those who have little or no acquaintance with scientific methods of investigation show considerable modesty in preparing tabulations but few have any hesitation when it comes to drawing conclusions. Almost any one can do that. As a result, as one very competent statistician puts it, "a great deal of our statistical analysis is of a very low grade."

Most errors in interpretation and analysis of statistics are errors of simple logic. There is failure to distinguish whether phenomena are connected, indirectly connected, or apparently or absolutely unconnected. Because events happen to be related in time and space it is often promptly inferred that they bear to one another the relationship of cause and effect. Again, nothing is commoner than that one possible cause is singled out as a sufficient explanation for a whole phenomenon when there are doubtless many other causes at work to determine the effect. A fruitful source of error lies in making comparisons between materials which are not comparable. The saving clause "other things being equal" is only too often overworked; for other things are not equal. Finally, conclusions of the greatest importance are drawn from insufficient data, the element of error involved in small numbers being forgotten.

Karl Pearson, in his now classic essays on how to conduct social investigations, has presented, with a wealth of illustration, the commoner errors of interpretation and analysis in the field of sociological discussion. Mr. Schneider has drawn largely from this and other sources. He has al-

ready given excellent examples of fallacies which result from confusing association with causation and also from ascribing to one single cause what is undoubtedly the result of the action of many causes. In the little time at my disposal I shall cite only a very few additional examples of what should be avoided in our reports and discussions. Other examples will probably seem more illustrative to you but those that I quote have arisen in my own field of work.

Serious errors arise from the comparison of tabulations that are essentially imcomparable. There is a constant temptation, for example, to compare the death-rates of cities and of states without regard to the differences in the composition of their populations. A higher crude death-rate in a southern city does not necessarily indicate lower standards of sanitation than prevail in a northern community with which it is being compared; but may result entirely from the high proportion of colored population. The age factor should play an important part in all comparisons between the death-rates of northwestern and eastern cities. I believe that the time is not far distant when we shall be compelled likewise to take into consideration the differences in the race stocks of populations in making comparisons of mortality since this element plays a large part in determining the true death-rate. Similar reservations must be made in comparing rates for certain diseases in different parts of the country. For example, colored persons, while not immune from scarlet fever, are not as susceptible to it as white persons. Consequently, the general scarlet fever death-rate is likely to be low in the registration cities in which the proportion of colored persons is large, although its white population may have a general death-rate for this disease close to that in the registration area as a whole. With whooping-cough the situation is reversed, as colored persons are more susceptible to this disease than whites. Comparisons of this nature, therefore, which do not take into consideration fundamental differences in the composition of the population, are worse than useless, since they give only a distorted picture of the true facts. This inevitably results in misunderstanding and controversy.

Errors arise from a confused idea that proportions based upon deaths from all causes, are somehow equivalent to rates based upon populations. Writers on occupational hygiene have been especially guilty of this fallacy. One writer, for example, deplores the high incidence of tuberculosis among school teachers, intimating that this disease is even more prevalent in this occupation than among stone cutters. His evidence was the Census Bureau tabulation showing a high proportion of tuberculosis in deaths from all causes in this profession. No consideration was given, however, to the important item of the death-rate in relation to the number exposed, this rate being exceptionally low among school teachers. A high proportionate mortality may, therefore, be entirely consistent with a low absolute death-rate from tuberculosis and this is exactly the case in the oc-

cupation in question. To state the case more emphatically, we may say that even if all the deaths of school teachers in the age period 25–34 were from tuberculosis, the death-rate from this disease would still be lower than that found among stone cutters.

Another example of this same fallacy appeared in a recent publication of the health department of one of the largest of our cities. In this article, the attempt was made to compare the mortality of children under two years of age according to the nationality of their mothers. The method was very simple indeed. It consisted in comparing the percentage of deaths of the children according to the nationality of their mothers and the percentage of the total population on the same basis. Thus, the American born population constituted 64 per cent. of the total population, whereas only 25 per cent. of the total deaths were babies of American born mothers. The conclusion was, of course, drawn that the mortality of children of foreign parentage was very much higher than that of children of native parentage. No consideration whatever was given in this discussion to the important factor of the birth-rate for the several nationalities. The birth-rate for native Americans is decidedly lower than for Russians, Poles and Italians who are heavily represented in the population of this city. If American born mothers had carried their birth restriction to its logical conclusion, there would have been no deaths at all among their offspring for there would have been no offspring to die. In such event, the numerical disadvantage of the babies of foreign born parentage would have been even more marked.

Finally, I would call your attention to an element of error which arises out of drawing conclusions from relative statistical numbers based upon too few cases. The very definition of statistics predicates reasonably large groups; otherwise the result obtained may be due only to a chance variation with little inherent probability. Averages, rates and other derivative figures are subject to the error of sampling and such errors form a part of their value and indicate the limits between which the true value lies. When the number of cases involved is small, the error is large. Indeed, it may be larger than the value of the average or rate itself. Let me repeat an oft-quoted illustration. We desire to learn the lethal rate of a disease and have the results for only a small number of cases, let us say ten, of which seven recover. The lethal rate (0.30) is not trustworthy as an index of the lethality of this disease. It is undoubtedly true for this particular group of ten, but one cannot generalize from it as to any larger group of cases because it may be only a chance result. The possible error for the ten cases is 0.41 and this is larger than the rate; this is, of course, absurd. If the results in 100 cases rather than ten were tabulated and 70 recoveries followed, the error would be 0.13. If 1,000 cases were studied and the recovery rate were the same, the error would be only 0.04, which is fairly negligible as a corrective, for the lethal rate would range from 0.26 to 0.34. This is suffi-

ciently exact for all practical purposes. In other words, it would be necessary to have 1,000 instances in this particular case before we could draw a conclusion which would be generally true. The accuracy of the result increases directly with the square root of the number of cases. A simple test which is often useful to determine whether the number of cases is adequate to permit general conclusions to be drawn is given by Poisson's formula, which will be found sufficiently illustrated in Newsholme's useful work on Vital Statistics.*

4. Conclusion.

In conclusion, permit me to point out the necessity for extreme caution in the collection, preparation and interpretation of data in public health work. It is important that the future development of the public health movement shall be based only upon sound premises and upon thoroughly trustworthy and competent analysis of the facts. The general public will not support health work or any other social welfare programme which is based upon snap judgment or upon conclusions drawn from incomplete, biased or otherwise defective statistics. We must continually keep in mind the possible reaction of a community which forgets the urgency and efficacy of a health programme and remembers only the errors which have already been exposed in the basic data used to support previous health and welfare enterprises.

*Newsholme, Arthur. Elements of Vital Statistics. Lond., Sonnenschein, 1889, p. 323.

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