*The Means of Determining Causation in Insanity.** By WILLIAM R. HUGGARD, M.A., M.D., M.R.C.P. Lond.

At the last meeting of this Association, it was seen that great discrepancy of opinion existed as to the value of tables of causation in insanity. On the one hand they were regarded as of the first importance; and on this ground a plea was put forward for unanimity in the mode of tabulation. On the other hand they were looked on as worthless, or almost so; and some applause followed when they were characterised as rubbish heaps.

Such widely divergent opinions on a subject of this nature should have no place amongst us. The matter should be sifted. If the tables at present in use are untrustworthy, the grounds on which they are discredited should be fully set out. To know where the fault lies often is enough to indicate the remedy.

The object of this paper is to point out some of the sources of fallacy that vitiate our tables of causation, and to make a suggestion or two towards putting the matter on a better footing.

The subject is a complicated and abstruse one, requiring not merely a large amount of practical knowledge, but some acquaintance with the most difficult portion of the theory of inference, Probability or the Logic of Chance. And it is not an easy matter to cast a glow of interest over a subject consisting chiefly of abstract questions of calculation, and to explain clearly problems so heavily burdened with inherent difficulty. But its importance, I think, warrants its being laid before you.

The sources of fallacy in statistics, as in every other department of knowledge, are twofold—errors in observation and errors in inference. The source of the distrust in the Commissioners' tables of causation, that was displayed here on the last occasion, appeared mainly to be the imperfect and haphazard way in which statements of causation are made in the certificates on which the tables are, to some extent, based. Another fault was alleged—that the plurality of causes was overlooked.

There is no question that the statement of the "supposed cause" in the certificate is of the most untrustworthy char-

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acter. Persons who by lack of education and by lack of the knowledge of the causes of insanity in general are quite incompetent to form a sound judgment, are called upon to determine the cause in some particular instance. It is obvious that in some cases their opinion will be formed according to a preconceived theory of what should cause insanity. I know a lady, for example, who traces her son's insanity to his not having said his prayers; while she makes little account of heredity and a strongly marked neurotic predisposition. Even in published statistics I have seen tight lacing set down as a cause. Moreover, the friends of the patient generally consult appearances. Reluctance is shown to ascribing the insanity to vice or to anything that might reflect discredit on the patient or his family; and if there are two or three causes to choose from only the most respectable is put down.

Another source of inaccuracy, not less serious, is carelessness. That errors frequently spring from this source, does not, I think, admit of reasonable doubt. Apart from ignorance of the causes of insanity generally, preconceived theory, and the bias of respectability, there will be found many cases where the only reason that can be assigned for putting down the wrong cause instead of the right one, is negligence or carelessness. The people by whom the statement is made often seem to look on it only as a matter of form. Hence it happens that in a large number of cases the cause is put down either in a haphazard way or is returned as unknown. It is true that the Commissioners' tables are framed from the reports of the asylum physicians. But it is equally true that in a large number of cases these reports are based on the statement that accompanies the certificate.

But in order to get accurate facts, and to draw just conclusions, it is necessary not merely to get rid of the sources of error that have been enumerated. Another radical fault pervades the present mode of obtaining our knowledge of the subject. The person that gives us our information about the patient is not merely required to state a fact; he is asked to make an inference, or rather to guess an inference, to state what he supposes to be the cause.

It may be just as well to mention now, by way of reminder, how causation may be proved.

Dr. Savage, in depreciating our present method of determining causation, considered that the only sound course was to select examples where some definite cause could be shown to have produced the disease; cases for instance where gout was

followed by insanity. In simple subjects of investigation such a method is undoubtedly the correct one. The one thing needful to the successful application of it is that there should not be present any other element capable of bringing about the given condition of things. We should be able to show, not merely that the supposed cause was followed by the effect, but that no other change than the introduction of the supposed cause was made in the series of events.

It is easy to see that in so complicated a machine as the human body we can rarely indeed have the required assurance. Even in cases where there is well marked alternation between the mental symptoms and the bodily ailment, we cannot with certainty affirm causal relation. Such alternation may, as in the case of the alternation of day and night, be dependent on other causes.

This method can, indeed, do little more than suggest causes; the value of the suggestion to be afterwards tested by a more trustworthy plan. In fact, it may be said that it is to the use of this method in matters of unsuitable complexity that we owe so many crude opinions in therapeutics. One man finds a disease do well under one drug; another finds it do well under another drug; a third man, perhaps, finds it does better without any drug at all.

The mere fact that one event is followed by another event does not show that the one is the cause of the other. According to the Commissioners' tables, only about 14 per cent. of the insanity of this country is caused by intemperance in drink. This statement, I think, we may really regard as meaning that insanity was preceded by intemperance in 14 per cent. of the cases; leaving upwards of 85 per cent. where it was preceded by temperance. Temperance, on this showing, is a much more frequent precursor of insanity than is intemperance. On what grounds do we regard the least frequent antecedent as a cause, and deny causation in the case of an almost constant antecedent? The grounds, though palpable enough, are, as a matter of fact, altogether dropped out of view in all the calculations hitherto in use. If we wish to know the proportion of cases that are caused by intemperance, it is obvious that we must compare the proportion of cases of insanity occurring in the intemperate with the proportion among the temperate; the excess displaying the influence.

Our procedure, then, to establish causation in complicated cases, is much as follows. We first, by observation or by

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guesswork, find certain agencies that appear to be causes. We then must compare the cases in which the supposed cause is present with the cases in which it is absent, and find whether the phenomenon whose causation we are investigating, is more frequent in the one group than in the other. If the proportion be the same in both groups, the supposed cause is not a cause; it is inoperative. An excess in one of the groups betokens an influence positive or negative either in the supposed cause itself or in something on which it depends. It is necessary to observe that we cannot always say whether a given factor in the series of events is a cause or an effect of insanity. But I shall say a word or two on this point by-and-bye.

It is seen then that the question of causation is a question of statistics. One cannot in any individual case point to some prominent element in the series of events that culminate in insanity, and say with certainty, "That is the cause." The chain of causation—an apter expression would be the "web of causation"—is too complex to admit of our tracing with accuracy the individual strands. We can only follow the general direction.

For these reasons it is obviously unwise to put the task of making the inference of causation on the shoulders of the people who are in a position only to observe the facts. The work of inference should be the duty of the central authority. The patient's friends should merely be asked to supply information as to the facts preceding the outbreak. And, in order to secure comprehensive and trustworthy information, it would be necessary that a document, setting forth all the presumed causes of insanity, such for example as the table of causes in use by the Commissioners, should be sent to one of the friends of the patient to note the presence or absence of each item in the individual case. One or two other points in reference to such a document will be noticed presently.

But suppose we are assured of the correctness of our facts. Our most difficult task is as yet uncommenced. We may know that intemperance preceded insanity in 14 per cent. of the cases; but we should still be ignorant of the proportion of cases it caused. And even if we knew the proportion of cases it caused we should have knowledge of merely a trivial and unimportant character. One of the radical faults that Mr. Mill points out in Bacon's view of induction is that he adheres to the ancient opinion that the prime office of philosophy is *rerum cognoscere causas*, to find out the causes of things; instead of *cognoscere effectus*, to know the effects. In practical

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life it is more important to foretell the effects of a given cause than to be able to declare the causes of a given effect. This is the ruling principle of experimental science, and is specially exemplified in what logicians term the Method of Difference and its subordinates. To illustrate the nature and the importance of the distinction between knowing the causes and knowing the effects, let us for a moment recur to our old example. Suppose we find that 14 per cent. of the insanity is due to alcoholic intemperance, what does our knowledge profit us? We do not know the proportion of insanity amongst the intemperate; and we are no wiser than before as to the likelihood of hard drinking to eventuate in madness. And yet that is the really important point. Again in the case of heredity, what a bootless calculation to discover that about 19 per cent. of the annual insanity is caused by hereditary influence! Such knowledge is wholly worthless as regards application to any useful purpose. What would be really useful would be to know the proportion of cases in which heredity failed or succeeded in bringing about the insanity of the offspring.

But I have reason to believe that a fallacious interpretation is sometimes put on the present tables of causation; and that they are read as tables of effects rather than as tables of causes. For example, when it is stated that 14 per cent. of the insanity of the country is due to intemperance, I fancy that some people transpose the terms in their imagination, and understand that 14 per cent. of intemperate persons become insane. This error would appear to be the mere stupid blunder of an uncultivated intellect; but I imagine it is not quite so rare as the general diffusion of culture would lead one to suppose.

From what has been said it is evident that in order to draw any inferences of value we require a basis of comparison. It is not enough to know how many cases were due to one cause and how many to another. We must have the entire number of cases in which the supposed cause was present, and the number in which insanity followed. We must know for example not merely the number of cases in which drink is followed by insanity, but the number of cases in which it is not followed by insanity. This knowledge may be hard to get; in many instances it may be impossible. But there is no use in disguising from ourselves the fact that without it we cannot arrive at any comprehensive and trustworthy conclusions.

It is not within our province as medical men to collect the

data necessary for determining the prevalence of the causes. The knowledge must be supplied to us; we cannot acquire it by our own labours. The task of obtaining it is more nearly akin to the work of the census than to any other department of investigation. I may, however, be permitted to say that the undertaking appears on consideration to be of a less arduous nature than at first sight one would suppose.

Well now, supposing we have before us the whole array of cases where the reputed cause is present, we can compare the instances where it has been followed by insanity with the cases where it has not been so followed. After deducting the cases that may be ascribed to chance (more strictly the cases where the effect was due to other causes), it might be thought we should at last have reached the wished-for knowledge; and that we could state the proportion of cases where a given cause would lead to insanity. A little reflection will show that such a supposition would be erroneous, and that an important element has still to be taken into account.

This element, which I believe has hitherto escaped notice, is the duration of the cause. Let me illustrate my meaning by a fictitious example, as the facts requisite for a real example are not to be had. We wish to know for instance the efficacy of hereditary influence in producing insanity. Suppose we find that out of every 30 people with hereditary influence one becomes insane each year, it might be hastily concluded that the chance of heredity producing insanity was only 1 in 30. Let us now make an extreme supposition, and assume first, that every person with hereditary tendency becomes insane; and secondly, that they become insane at 30 years of age. Now if they vary in age from 1 to 30, it is evident that only one becomes insane each year; and if one is born each year to supply his place the proportion between the asylum admissions from this cause and the people with hereditary tendency will always remain 1 in 30; though, by our hypothesis, they all go mad in good time.

We thus see that a simple comparison of the annual admissions from any cause with the numbers of the unadmitted where the same agency is present, would lead to the most erroneous inferences. In the example just given, it would be necessary to compare the admissions during a period of thirty years with the number of the uncertified heirs of insanity in order to form a correct opinion.

On what grounds, now let us ask, do we base our calculations on yearly statistics? The fact is, that it is purely for

convenience that the data are collected annually. The arrangement is quite arbitrary, and without reference to any rational principle. The appearance of uniformity for comparison is altogether delusive. If we took the number of admissions from any cause, say drink, every three months, the numbers would appear insignificant; if we took them at remote intervals, say every twenty years, the numbers would grow to an alarming magnitude. Is there any reason why we should choose one period in preference to another? Some causes act slowly, taking years to accomplish their result. In such cases if we make merely an annual comparison we place for several successive years amongst the cases where the cause is inoperative cases where really the cause has not yet had its period. On the other hand, in the case of quickly acting causes, say causes that accomplish their result in three months, the admissions from such a cause would appear fourfold greater than they should be to admit of a just comparison with the cases where the same agency had not been followed by insanity.

There is one other element of importance that should be taken into consideration, but which, in the present state of the subject, need not be here presented in detail. I mean the expectation of life in people in whom any presumed cause of insanity is at work. This is really an essential point in the calculation, as without attending to it we should count some people more than once.

I will now attempt to work out an example in illustration of the foregoing exposition. It must be borne in mind that as no authentic facts are to be had in regard to some of the essential points, I have been obliged to lay down some of the figures merely from guess work and my own rough observation.

Suppose now we wish to evaluate the influence of drink in causing insanity, how shall we proceed? We must first determine the prevalence of the agency in the general population. In the present case, women and children, being comparatively little exposed to the agency in question, will be left out of consideration. Speaking roughly, the adult male population is about 6,000,000, and about 1 out of every 1,000 is yearly put under certificate. About 21 per cent. of all the males put under certificate, or 22 per cent. of the males over 20, have their insanity preceded by intemperance. From my own observation I estimate the proportion of intemperance amongst adult males at about 1 in 12.

Now let us take these figures as our data. Out of every 120,000 adult males we should find 110,000 sober and 10,000 intemperate. Moreover in a year out of the 120,000, 120 would become insane. Of that hundred and twenty we should naturally expect 10 to be intemperate, even if drink had no influence whatever in causing insanity. But as a matter of fact we should find that 26·4 were intemperate, thus observing a surplus of 16·4 in which drink was either the cause or the effect of the disease. In other words, out of 10,000 drinkers, 26·4 go mad each year; and of the 26·4 drink is the direct cause or effect in 16·4.

But now it is necessary to apply some of the principles previously established. The result we have just arrived at shows merely the numbers for a year. It does not show what would be the result if 10,000 people were watched from the time they began to be intemperate until they died. To obtain this knowledge there are three points on which we still require information—information which, with a properly devised system of inquiry, I think we should manage to possess ourselves of. These points are the mean, or perhaps the average, age at which insanity from a given cause occurs, the mean or average duration of the cause before the outbreak; and in the case of agencies that tend to shorten life, the mean or average expectation of life after the appearance of the given agency.

Now suppose we have the knowledge we want, and that the age at which intemperance generally begins is about 20; that when insanity is due to this cause it usually shows itself about 40; and that the average age at death of intemperate people is 50. In a stationary population of intemperates, two-thirds, according to these data, die in each period of twenty years, and the loss is made up by recruits. In making our calculations, it is obvious that we must compare the admissions (excluding re-admissions) in each period, not with the entire body of topers, but only with the recruits; in other words, only with those who have not been previously counted. The number of recruits then, in each twenty years, would be roughly 6,600; and the admissions of topers would be 520. But, making allowance for re-admissions, we find that only 330, or 64 per cent. of these, are new cases. Out of 6,600 topers, then, we have 330 new cases, or 50 in every 1,000; and, according to our previous calculation, of this 50 about 30 have a direct connexion with intemperance; the remaining 20 can be accounted for without reference to drink.

By way of comparison, let us see the rate at which people go mad from all causes. The expectation of life for males at 20 is about 43 years. Assuming again, what is roughly true, that 12 out of every 12,000 males over 20 become insane yearly, we should have 516 admissions during the entire period of life of each 12,000. But of the 516 only 64 per cent. or 330 would be new cases; and of the 330, 22 per cent. *i.e.* 72, or making allowance for the different expectation of life in tipplers, only about 50 would be due to alcohol, which is the same result that we arrived at by a different process.

We arrive then at these results. Out of every 1,000 people about 27·5, of every 1,000 non-drinkers 25·5, and of every 1,000 drinking people about 50 go mad before they die; and of the 50, in 30 cases the drink has a direct connexion with the disease.

In all these calculations it is assumed that the causes are independent, that is, without influence, hostile or otherwise, towards each other; and that the re-admissions are distributed equally over all cases, that they are not more frequent proportionally from one cause than from another. These points, of course, require to be determined.

It must, I need hardly say, be understood that I do not for a moment defend the accuracy of the figures; I wish merely to show the correct mode of reasoning.

The calculations I have laid before you have necessarily been intricate and tedious. But let me say one word more, and then I shall have done with this thorny and uninviting portion of the subject.

It might be said that we need not go to such trouble to determine the proportion between the cases in which the agency was followed by insanity, and the cases in which it was not; that a readier method is at hand—to compare the numbers in asylums with the numbers without. Such a plan, however, would be fallacious for two reasons. First, many, being only a short time under certificate, would count amongst the "outs." Secondly, the cause might be of such a nature that people who became insane from it died sooner than those who did not become insane from it; and thus they would not accumulate in the asylum.

In accordance with the views I have laid before you, I will make one or two suggestions.

First. It would be desirable, as previously mentioned, to replace or to supplement the present statement that accompanies the certificate, by a tabular enumeration of all the

supposed causes of insanity; and one of the friends should be required to note the presence or absence of each item; and if present, the length of time it had been at work should be put down. It would be important also, in view of determining the influence of heredity, to know how many children married lunatics had. In order to secure the careful filling up of such a document, it should not, I think, be made to accompany the certificate; but should be required within a week of the patient's admission. Such a method would go a long way towards checking the carelessness and negligence that spring from the hurry, confusion, and bother that sometimes occur in sending a patient to an asylum.

The second suggestion I have to make is that it would be useful if from the annual returns a table were compiled, showing the mean or the average age at which people went mad from each cause.

It hardly lies within our present scope to consider the means of getting information on the other points, such as the general prevalence of each cause, and the usual time of death from the various causes.

A few minutes ago I mentioned that it was not always easy to say whether a given symptom was a cause or an effect of the insanity. If the proposed suggestion of stating the duration of the cause were carried out, it is not unlikely that some light would be thrown on the difficulty. I think it is probable that we should then find that cases in which the alleged cause was present would divide themselves into two groups. In the one group where the agency had caused the insanity, it would have long preceded the outbreak; in the other group, where it was the effect of the insanity, it would have been present only a little time before the attack.

In conclusion, I will say that the course of inquiry I have indicated is an arduous one; but it has the merit of leading to useful knowledge. I am by no means unaware of the difficulties that meet us at every turn in attempting an investigation of the kind; but I am firmly persuaded that like all other difficulties they may be overcome by forethought and address. And if such be the case the result to be attained is surely of sufficient worth to call for a trial.