THE
JOURNAL OF MENTAL SCIENCE

[*Published by Authority of the Medico-Psychological Association of Great Britain and Ireland.*]

No. 257 [NEW SERIES No. 221.] APRIL, 1916. VOL. LXII.

Part I.—Original Articles.

ON CAUSATION

WITH A CHAPTER ON BELIEF

BY

CHARLES A. MERCIER, M.D., F.R.C.P., F.R.C.S.

(*Continued from p.* 108.)

III. SIMILARITY.

Unquestionably the most usual and frequent ground for assuming a causal relation which is not immediately apparent is the similarity of the case in hand to other cases in which the causation has been ascertained. As it is the most frequent, so it is the most direct application of the fundamental Axiom of Causation, that *Like causes in like conditions produce like effects*, from which we obtain, by a logical process that is unknown to logicians, the immediate inference that *Like effects in like conditions are due to like causes*. It is by the application of this method not only that causation is most often established, but also that some of the most important discoveries of causes in the various sciences have been made. It is in perpetual use, both in the most recondite problems of science, and in the commonest affairs of daily life.

It is asserted in nearly every book on Logic that the planet

Neptune was discovered by Mill's Method of Residues. The planet Neptune was not discovered by the Method of Residues. The very descriptions of the discovery that are given to show that it was discovered by the Method of Residues show that it was not discovered by the Method of Residues, and the same is true of every other instance in which the books assert that a cause has been discovered by this method. No cause of anything has ever yet been discovered by the Method of Residues, and it is extremely unlikely that any cause of anything ever will be discovered by it. What was discovered by the Method of Residues was that there were certain movements of the planet Uranus that were not accounted for by known causes. The Method of Residues did not discover the cause, nor point to the cause. All it discovered, and all it pointed to, was that there was something for which an additional cause was required. The additional cause was discovered by the Method of Similarity. It was found by applying the Axiom *Like effects in like conditions are due to like causes.* After all the perturbations of Uranus that are due to the attraction of known planets had been reckoned, it was found that there was a residue of perturbation unaccounted for; and this led astronomers to guess that there must be some other cause of perturbation, yet unknown, and to look for it. The astronomer said 'This residual effect must be due to some extra cause that I have not reckoned on. But though it is a new effect, it is not a new kind of effect. I am familiar with perturbations of planets, and I know how they are produced. They are produced by the attraction of other planets. Now, *Like effects in like conditions are produced by like causes;* therefore this perturbation must be due to the attraction of some undiscovered planet, and I must proceed to discover it. In order to produce this effect, the causal agent must have been in a certain place at a certain time.' Then he investigates, and finds that at that time Neptune was in that place.

Precisely the same method is employed by the cook when she finds herself short of a pot of jam. This also is a residual phenomenon. After accounting by known causes for the absence of most of her jam, she finds there is a residue of loss that cannot be so accounted for. This is all she can learn from the Method of Residues. She learns from it that there is something for which a cause is required. She then sets to work to discover the cause. She says 'This loss must be produced

https://doi.org/10.1192/bjp.62.257.241 Published online by Cambridge University Press

by some cause that I have not reckoned on; but though it is a new effect, it is not a new kind of effect. I am familiar with the abstraction of pots of jam from my cupboard, and I know how it is produced. It is produced by the action of human hands. Now, *Like effects in like conditions are produced by like causes;* therefore the abstraction of this pot must be due to the hands of some undiscovered person. In order to produce this effect, the causal agent must have been in a certain place at a certain time.' Then she investigates, and finds that at that time the page-boy was in that place.

It is the same with every other application of the Method of Residues. What is found by it is not the cause of anything, but something unaccounted for, something requiring explanation, something for which a cause must be found; but in finding the cause the Method of Residues is never employed, and would be useless if it were employed. The cause is found by one of the methods here described, and very often by the Method of Similarity.

When physicians desired to know the cause of yellow fever, did they proceed by the Method of Agreement, or the Method of Difference, or the Joint Method of Agreement and Difference, or the Method of Concomitant Variation, or the Method of Residues? They did not. They were not so foolish. The way they went to work was to assume that the cause of this disease is like the cause of a similar disease occurring in similar conditions. There is no disease exactly like yellow fever: such a disease would be yellow fever itself; but there is a disease, ague, which is like enough to yellow fever for the purpose of the argument; and the cause of ague is known. Ague is caused by the injection, by the bite of a mosquito, of a parasite into the blood; therefore, it was argued, on the ground of the Axiom of Causation, that yellow fever also is caused by the bite of a mosquito; and suitable investigations being made, the conclusion was verified in this case and in that. But it was not verified in every case, and it cannot be verified in every case. In the cases that now come under care, we do not and cannot satisfy ourselves by observation or experiment that they have been caused by the bites of mosquitoes; but for all that we do not doubt for a moment that they have been so caused. What, then, gives us our assurance? The same variant of the Axiom of Causation, that *Like effects in like conditions are due to like causes.*

https://doi.org/10.1192/bjp.62.257.241 Published online by Cambridge University Press

When a chemist wishes to determine whether lead is present in certain water, he applies certain reagents; and if he obtains certain results, he concludes at once that lead is present; and so sure is he, that he is prepared to go into a court of law and swear to it. By what method has he ascertained that the cause of the reactions that he obtained was the action of lead in the water? By the same method that leads the cook to conclude that the disappearance of her jam was due to the action of the page. The chemist knows that on every previous occasion on which he or anyone else has ever tried it, lead has had this effect, and nothing else has; and he assumes at once that since the effect and the conditions are similar, the cause is similar.

When the photographer finds that directly he pours his developer on the plate, the image flashes up, he knows that the plate has been grossly over-exposed; and he discovers the cause of this effect by the Method of Similarity. The effect is like the effect that has in like conditions been produced by a certain cause; therefore, he concludes, the cause in this instance is like the cause in that. Is his plate fogged? Then he concludes that diffused light has fallen on it, and his reason is the same. Is his result brilliant? Then he determines that on future occasions he will repeat the conditions as closely as possible; and is confident that the more closely he can get them like the conditions in this case, the more closely similar will be the result.

When the horticulturist finds his tomatoes suffering from disease displaying certain symptoms, does he apply any of Mill's Canons? Not if he knows his business. He looks round for similar diseases in similar plants, confident that if he finds such a disease, and the cause of it is known, he may assume a similar cause for the disease of his tomatoes. He has not far to look. On his potatoes, plants belonging to the same natural order as the tomato, he finds a very similar disease; and he knows that this potato disease is due to a fungus of a certain kind. He concludes at once that the disease of his tomatoes is due to a fungus, and to a similar fungus; and more, he concludes that whatever treatment effectually cures the disease of his potatoes is likely to relieve the disease of his tomatoes. He does not look for two or more instances which have nothing in common but the occurrence of the phenomenon, and two or more instances which have

https://doi.org/10.1192/bjp.62.257.241 Published online by Cambridge University Press

nothing in common but the absence of the phenomenon: he looks for a single instance as like as possible; and having found an instance that is like enough for the purpose of the argument, he looks no further, for he knows that *Like effects in like conditions are due to like causes.*

A remarkable instance of the application of this method has recently divided with the war itself the interest of this country. Four women in four different parts of the country were found drowned in baths under conditions that were closely similar; and the similar conditions were not only closely similar, but were numerous. In each case the woman was recently married; in each case she either possessed money or her life had been recently insured; in each case she had made a will in favour of her husband; in each case the husband reported the death on his return from going out to buy food; in each case the woman had been said by the husband to have fits, though she was not otherwise known to have them; in each case the funeral was hurried, and was carried out as cheaply as possible. Such closely similar effects in such numerous closely similar conditions pointed conclusively to closely similar causes and closely similar agents. When it was discovered that in all the cases the husband was the same man, the similarity became merged in identity. This one circumstance was antecedent in every case, and was the only common antecedent; and it was impossible to doubt that he was the agent that had produced all the effects. But the Method of Similarity, though by itself it was sufficient, was not the only method employed in discovering the agent. The sixth method also, the Method of Common Rarity, was employed. It is, in fact, not usual for the discovery of a cause or of an agent to be made by the employment of one method only; and here we may give an anticipatory instance of the Method of Common Rarity. Death in a bath is rare. Death in a bath of a newly married woman, under all the conditions enumerated, is extraordinarily rare. The rarity of the effect pointed in each case to a cause equally rare; the common rarity of all the effects pointed not merely to rarity, but to actual uniqueness of the cause and of the agent. In all the cases there was but one common factor that alone could possibly be the agent, and this was the husband; who was accordingly charged with murder, tried, convicted, and executed.

Instances of the application of the Method of Similarity

might be multiplied indefinitely. It is the ordinary common method of discovering those causes that are not forced upon our attention by the Method of Instant Sequence; it is used by everyone many times every day, and is more frequently employed in scientific investigations than any other method; but logicians, though in common with other people they are constantly using it, have never described it, and never discovered it.

IV. Association.

The mere association between an action upon a thing and a following change or accompanying unchange in that thing points to a causal connection between the action and the effect, and is often taken to establish the causal connection. It does not necessarily establish the connection, but in certain circumstances it may do so, and our task is to discover and state these circumstances.

This is the method so clumsily expressed, and so erroneously expressed, by the first three of Mill's Canons, which we may now examine. The first thing that strikes us upon reading them is the extraordinary cumbrousness, the elephantine ponderosity, of their expression. A statement is not necessarily erroneous because it is badly expressed; but cumbrous and awkward expression is a sign of confusion of thought; and when we find such portentous circumlocution as these Canons display, we may be quite sure that the writer is trying to convey some thought that he has not thoroughly worked out; that it is certainly no more than an approximation to the truth; and that it is very likely to be erroneous. Elegance of expression is no guarantee of accuracy, but it is an indication of care; and clumsiness of expression is an almost certain sign of confusion and want of thoroughness in thought.

The first of the Canons runs: 'If two or more instances of the phenomenon under investigation have only one circumstance in common, the circumstance in which alone all the instances agree [why not 'this circumstance'?] is the cause (or the effect) of the phenomenon.'

Apply this to a concrete case, and let the 'phenomenon under investigation' be green colour. Two or more instances of green colour (a bucket, an armchair, and a pool ball) have only one circumstance (that they are green) in common; this circumstance is the cause (or the effect) of the green colour.

So obvious is this booby-trap that some of Mill's followers have noticed it, and have modified the Canon so that it reads 'have only one *other* circumstance in common.' Let us see how the amendment works out in practice, and let the 'phenomenon' still be green colour.

If two or more instances (a bucket, an armchair, and a pool ball) of the phenomenon under investigation (green colour) have only one other circumstance (that they are in the same house) in common, this circumstance (being in the same house) is the cause (or the effect) of the green colour.

Of course, according to my nomenclature, the green colour of these objects, since it is neither a change nor an unchange, is not an effect but a result; but it is certainly a phenomenon, and according to Mill's nomenclature it is an effect; and out of his own mouth must he be judged. If he had recognised that an effect means a change or an unchange, and that a cause means an action, and had expressed his Canon accordingly, it would have at least been true, though even then it would not have been much use. It would then have run as follows:—

If two or more instances of an effect are preceded or accompanied by only one mode of action on the thing changed or unchanged, that mode of action is the cause of the effect in each case.

This of course would be true, but when was there ever such an effect? Events in this world are not thus isolated, and we have no experience, and are never likely to have any experience, of an effect that is preceded or accompanied by one action and no more on the thing in which the effect is produced.

Mill's second Canon runs thus:—'If an instance in which the phenomenon under investigation occurs, and an instance in which it does not occur, have every circumstance in common save one, that one occurring only in the former; the circumstance in which alone the two instances differ, is the effect, or the cause, or an indispensable part of the cause, of the phenomenon.'

Again let us clothe these dry bones in flesh and skin, and let the phenomenon still be green colour. If an instance (a pool ball) in which the phenomenon under investigation (green colour) occurs, and an instance (another pool ball) in which it does not occur, have every circumstance in common save one (touching the cushion) that one occurring only in the former; the circumstance (touching the cushion) in which alone the

two instances differ, is the cause, or the effect, or an indispensable part of the cause of the phenomenon (the green colour).

In terms of action and effect, this Canon would run as follows:—'If an action and an effect in the thing acted on are associated both in presence and in absence, everything else being the same, the action is the cause of the effect.' This of course is true, but in practice the Canon, even in this form, is of no value, for everything else never is the same. In order to give it any value the Canon should run:—'every other material circumstance remaining the same.' In this form the Canon is true, and is valuable, but it is a very different Canon from Mill's.

Mill calls his third Canon the Joint Method of Agreement and Difference, and puts it thus:—

'If two or more instances in which the phenomenon occurs have only one circumstance in common, while two or more instances in which it does not occur have nothing in common save the absence of that circumstance; the circumstance in which alone the two sets of instances differ is the effect, or the cause, or an indispensable part of the cause, of the phenomenon.'

In a concrete instance, If two or more instances (say a blade of grass, a garden seat, and a park gate) in which the phenomenon (green colour) occurs have only one circumstance (that they are out of doors) in common, while two or more instances (say a reel of cotton and a frying-pan) in which it does not occur have nothing in common save the absence of this circumstance (being out of doors) the circumstance (being out of doors) in which alone the two sets of instances differ is the cause, or the effect, or an indispensable part of the cause, of the phenomenon.

The qualifications of this Canon are grotesque. When were there ever two instances of any 'phenomenon' that had only one circumstance in common? It is impossible to find such instances, and impossible to imagine such instances. The supposition is outrageous. If the 'phenomenon' is a material thing, or a change in a material thing, the instances must have at least the common circumstance that they are all subject to the action of gravity. If the 'phenomenon' is a mental state or a mental change, the instances must have at least in common the circumstance that they are in some mind or other. And

how is it possible to find two other instances that have nothing in common but the absence of the 'phenomenon'? Instances of what? Of the 'phenomenon'? No, for that is to be absent. Of the 'circumstance', then? No, for that also is to be absent. And these instances of nothing are to have nothing in common but the absence of the 'circumstance', yet they are to have also in common the absence of the 'phenomenon'! Was there ever such a farrago or nonsense? And yet this precious Canon was not only gravely stated by Mill, but has been gravely accepted by every writer of his school ever since, and in seventy years not one of them has discovered its tomfoolery; nor has even any one of his critics, and they are numerous enough, discovered its tomfoolery. Had its author been anyone else, I should have suspected him of perpetrating a huge joke, and laying an elaborate trap for his worshippers; but Mill was as destitute of humour as Herbert Spencer himself, so that hypothesis will not stand. No. The only explanation is that Mill, and everyone else who has accepted or criticised the Canons, have had their minds so bemused and bemuddled by the study of Traditional Logic, that they are no longer capable of distinguishing sense from nonsense.

As with the previous Canons, I have tried to make sense of this by translating the terms 'circumstance' and 'phenomenon' into action and effect, but no such amendment, and no amendment of any kind, can make sense of it. Its ineptitude is hopeless and incurable, enormous and incredible; and no tinkering or patching can amend it.

Preposterous as these Canons are, both in sense and in expression, they are nevertheless blind gropings after a meaning that is both true and valuable; that is to say, that there are circumstances in which the association of an action on a thing and an effect in that thing indicate a causal connection between the action and the effect, and that these circumstances may be formulated. We have already seen that this is true in one set of cases—in those cases in which the effect is associated in instant sequence with the action—and have now to show what other cases there are. It must first be insisted that the mere association of an action on a thing with an effect in that thing does not necessarily imply causation. The sun may shine on a house when it falls down; or on a river when it overflows; the birds may be singing in the hearing of two pugilists; the train may be late when the rain is falling on it;

the wind may be blowing on the corn when it is falling in swathes; all these actions may be associated in time with effects in the things acted on, and yet the association does not justify us in concluding that the action is the cause of the effect. Nor can we draw this conclusion from an association in space. Grooming the horse is not the cause of its casting its shoe; painting the gate is not the cause of its being out of plumb: putting the kettle on the fire is not the cause of the fire burning up, or of the kettle being full; crossing the swing bridge is not the cause of its opening.

Yet there are cases in which we may properly argue from association to causation, and it is important to distinguish the cases in which we are warranted in so arguing from those in which we are not. There are four such cases, that is to say—

Causal connection between an action on a thing and an effect in that thing may safely be argued from their association

A. When other material action can be excluded;
B. When the association is of proved constancy;
C. When, though inconstant, the association is more frequent than casual concurrence will account for;
D. When, though itself inconstant, the associated effect has constant peculiarities.

A. If a certain action on a thing is associated with a certain effect in that thing, and all other material action can be excluded, then that action is the cause of that effect.

This is indubitable. It needs no proof. It is axiomatic; and the method is unassailably valid whenever it can be employed; but the occasions on which it can be employed are restricted. Of course, if it were necessary to exclude all other action, the method could never be employed at all, since such exclusion is impossible. In material things, for instance, it would often be impossible to exclude the pressure of the air, and always impossible to exclude the action of gravity. But there are few cases in which causation needs to be investigated and in which these actions are material. A greater difficulty is to know what actions are material to the effect and what are not: and even if we do know this, it may be difficult to exclude all the material actions but one; and often there may be a material action at work of which we know nothing. If we suspect an action of being the cause, and can isolate it, the method is easy, and the result, positive or negative, is certain;

but in many cases in which we have to depend on the method of association the inquiry is a fishing one. There may be no single action that can be plausibly suspected, and the number of actions that may, for aught we know, be material, may be indefinitely great. Take the case, for instance, of a disease. It occurs among men and women whose course of life brings upon them the action of innumerable agents, some of which we know; some of which, without knowing, we suspect; and many others of which we are altogether ignorant, and of whose very existence we entertain no suspicion. Yet any of these may, for aught we know, be material. In such a case it is inevitable that the method of association, employed loosely and without rigour, as it always is at first, should lead us astray. In such cases we are apt to choose, pretty much at random, an action or an agent that may or may not exist, and assign to this action or agent, real or imaginary, a causal influence. We assign the causation of disease, or of a disease, to the planets; to the air; to some food, or ingredient in food, such as purin; to some drink, or ingredient in drink, such as port wine; to anything in the heavens above, or in the earth beneath, or in the waters under the earth. These are mere random speculations; it is not until we submit our speculation to the test of one of the twelve methods here described that any reasonable assignment of cause begins; and the method that first suggests itself is usually the method of association. The first step towards accuracy is made when we establish an association in time or space between the agent or action that we have tentatively fixed upon and the effect or result whose cause we are seeking.

It is not enough, however, to establish an association in time or space between them, for, in such an effect as disease, innumerable actions on the body of the patient are associated with the disease. It is necessary to pick out one particular action, and prove that it is associated with the disease in one of the four ways that have been enumerated above; and the most obviously conclusive association is that now under consideration, *viz.*, association in isolation; that is to say:—

If, in given conditions, other material things remaining the same, the addition alone of an action is attended by an effect, or the withdrawal alone of an action is attended by the disappearance of an effect, that action is the cause of that effect in those conditions. The obverse also is true:—

If, in certain conditions, other material things remaining the same, the addition of an action is not attended by an effect, or the withdrawal of an action is not attended by the disappearance of an effect, that action is not the cause of that effect in those conditions. Both these maxims are easily derivable from the Axiom of Causation.

Unlike Mill's so-called Experimental Methods, these methods are almost of necessity experimental. The isolated addition or withdrawal of an action does not often take place unless it is artificially produced. If, however, the action can be isolated, and added or withdrawn without disturbing other material actions or conditions, then a single instance is all that is necessary to establish causation, not only for that instance, but generally for all cases that are similar in material respects.

Is the pressure of the air the cause of the maintenance of the mercury in a Torricellian barometer? If we place the barometer in a chamber, and exhaust the air from that chamber, we can determine the question with certainty, for by so doing we withdraw the single action of the air-pressure, and leave all other material actions and conditions unaltered.

What is the cause of the baby's crying? Is a pin pricking it? The nurse undresses the baby and finds a pin in such a position that it may perhaps have pricked the baby. She removes the pin, and the crying ceases. Was the pricking of the pin the cause of the crying? We cannot be sure. We are not sure that there was any such action on the baby as we supposed, and therefore cannot be sure that any such action was withdrawn. Nor can we be sure that other material things have remained the same. In undressing the baby some other source of pain or discomfort may have been removed.

What is the cause of this cutting in my greenhouse wilting? Is it drought? I water it, and after the lapse of an hour I can discern no difference: the cause is not drought, therefore. Is it the scorching of the sun? I move it into the shade, and in due time it recovers. There is little doubt the cause was scorching; but in moving it, I may have altered other conditions. If, however, instead of moving it, I screen it from the sun, and find that it recovers, I can have no doubt that scorching was the cause.

A certain milk or water supply is suspected of being the cause of an epidemic of disease. If, upon cutting off that supply, the epidemic ceases to extend, the suspicion is con-

firmed. If the spread of the epidemic is unaffected, the suspicion is removed. In this case the conditions are complex, and it is difficult to be sure that all other material circumstances remain the same. Even if the suspected supply is the cause of the disease, the epidemic may still spread after the supply is cut off, for persons who were infected before the supply ceased may not exhibit the disease until a week or a fortnight afterwards. Again, suspicion of the supply may lead many people not to use it, or to boil the milk or the water before using it, and in such a case other material circumstances will not be the same, and again the effect will be obscured. If, however, the conditions of the test can be observed, and are observed, then the test is infallible.

Is the fogging of the photographic plates due to leakage of light into the camera? Expose the next plates in another camera, and observe the result. If they are not fogged, the fault is probably in the camera, but it is not certainly so unless we can be sure that all the other operations were carried out in the same conditions. If the plates are still fogged, the fault is probably not in the camera, but this is not certain, for the second camera also may not be light tight. The method requires care and strictness in its application, but, properly applied, it is thoroughly trustworthy.

Is the discontent in the regiment due to the incompetence or lack of judgement in the colonel? Remove the colonel, and see if it subsides. In this case, again, there are sources of fallacy. A regiment that has once got out of hand cannot be restored to discipline in a day, or a week. The evil that men do lives after them; and it may be that no ordinary man, and no ordinary measures, will cure the regimental defect. Even in so simple a matter as altering the pendulum of a clock we may be deceived, unless we take precautions to observe that all other things remain the same. It may be that the very day we lengthen the pendulum a severe frost sets in and counteracts our action by shortening it. In short, the sources of error in the application of this method are numerous, and are often difficult to guard against; but none the less is the method perfectly efficient if we can and do eliminate errors in its application.

By these instances we may see that the method requires great care in its application; that it is often difficult, and often even impossible to isolate the action, and to be sure that in

adding or withdrawing it, no other material action has been added or withdrawn; nevertheless these instances also show that when the method can be employed, and when it is employed with care, it yields results which are perfectly trustworthy.

B. When the association of an action with an effect, though not isolable, is yet of proved constancy, causal connection between the action and the effect may be presumed. By proved constancy is meant constancy without exception in cases that are numerous and diverse.

Constant association between an action and an effect may be association in presence, that is to say, that if one is present the other also is present; or it may be association in absence, that is to say, that if one is absent the other also is absent. In practice these amount to the same thing.

Constant association in presence may mean that whenever in given conditions the action occurs, the effect occurs; which is the same thing as saying that whenever the effect is absent the action is absent. In this case, the more numerous and diverse the instances in which the association is observed, the more surely we may presume that the action is a cause of the effect; but we have no reason to assume that it is the sole cause.

Or it may mean that whenever in given conditions the effect is present, the action is present; which is the same as saying that whenever the action is absent the effect is absent. In this case, the more numerous and diverse the instances, the more surely we may presume that the action is the sole cause of the effect.

The removal of a queen bee from the hive is always followed by the rearing of a new queen by the bees; and this association has been so frequently observed without any exception, that we may now confidently presume that the removal of the queen is a cause of a new queen being reared. We may not, however, presume on the ground of this association, constant though it is, that the removal of the queen is the sole cause of a new queen being reared; and in fact bees at a certain time of year will always rear new queens, even if the old queen remains. A severe frost when fruit trees are in flower is always followed by failure of the crop, and the association is so constant that we may conclusively presume that the frost is a cause of the failure. We may not, however, presume from this mode of constant association that frost is the only cause of failure of

the crop, and in fact it is well known that it may fail from other causes. The warrant for the presumption, and the justice of it, are so manifest that no further illustrations are needed.

If the effect never occurs unless the action occurs, this mode of constancy in association warrants us in concluding, and if the cases are numerous and diverse compels us to conclude, not merely that the action is a cause of the effect, but that it is the sole cause. A watch never goes unless it is wound: we are compelled to conclude that the winding is the sole cause of the going. Eggs never hatch unless they are incubated: we are compelled to conclude that incubation is the cause, and the sole cause, of the hatching. This man is never quarrelsome unless he is drunk: we are justified in concluding, and compelled to conclude, that his drinking is the sole cause of his quarrelsomeness. Certain flowers are never fertilised unless they are visited by insects: we are justified in concluding, and compelled to conclude, that the visits of insects are the sole cause of fertilisation. Cancer of a certain kind is never found except among chimney-sweeps; chimney sweeping is the sole cause of that kind of cancer. Instances could be added in indefinite numbers. It is important to appreciate that the constancy of association is quite a sufficient warrant for concluding causation, even though we may not know, and may not be able to surmise, how the effect is brought about by the action, or what intermediate steps there may be between the action and the effect. Though we may not know anything of the mechanism of a watch, how the action of winding affects the mainspring, or even that it has a mainspring, yet the constant association, both in presence and in absence, of winding and going compels us to conclude that there is a causal connection between them. It is not material to the conclusion, and does not affect the validity of the conclusion, whether or not we know how the removal of the queen bee influences the bees to rear another queen; how the frost causes failure of the crop of fruit; how incubation promotes the chick in the egg; how insects contrive to fertilise flowers; how chimney-sweeping causes cancer; and so forth. These are, no doubt, useful and valuable things to know, and until we know them our knowledge of the chain of causation is not complete: we know a cause, but not the immediate cause. Nevertheless, we do gain from observing association a very valuable knowledge of

causation, and a knowledge that, though it may not be complete, is none the less certain as far as it goes.

The method of establishing constant association is the method that Mill had confusedly in his mind when he formulated his ridiculous Canons of Agreement and of Difference.

C. If the association is inconstant, it may be that the action is sometimes attended by the effect and sometimes not, or it may be that the effect is sometimes attended by the action and sometimes not. For the sake of brevity we will consider those effects only that are changes.

If, on the action occurring, the effect sometimes follows and sometimes does not, the action may be a cause of the effect, but can be so in certain conditions only.

If the effect is sometimes preceded by the action and sometimes not, the action may be a cause of the effect, but cannot be the sole cause.

If, however, the association of the action with the effect, although inconstant, is yet more frequent than casual concurrence will account for, the action must be the cause in some cases.

No housekeeper has any doubt, or need have any doubt, that thunder is causally connected with the beer turning sour. The association is not constant. Beer does not always turn sour in thundery weather, and sometimes turns sour when the weather is not thundery; but still, considering how relatively rare thundery weather is, and how relatively rare it is for the beer to turn sour, the relative frequency of the conjunction is much greater than mere casual concurrence will account for on the Doctrine of Probability. The excess of cases of the association over the number that casual concurrence will account for justifies the presumption, in that excessive number of cases, of a causal connection.

The presumption that fog is a cause of bronchitis is entirely justifiable, and is justified by the same principle. Not everyone who is exposed to fog has bronchitis; not everyone who has bronchitis has been exposed to fog. Clearly, therefore, fog is not a necessary cause of bronchitis: it can be a cause, if at all, in certain conditions only; and clearly, fog cannot be the only cause of bronchitis. Nevertheless we may safely presume that in certain conditions fog is a cause of bronchitis, because, though the association is not constant, it is much more

frequent than mere casual concurrence will account for. In this instance the method of association grades off and merges into the method of concurrent and proportional variation, for not only is the number of cases of bronchitis increased whenever there is a fog, which exemplifies the first method, but also the number of cases of occurring bronchitis has a direct relation to the severity and duration of the fog, so that there is to some extent concurrent and proportional variation. The proportion is, however, but very vague, for on the one hand, though we can measure the duration of a fog, we cannot, or do not, measure its severity; and on the other, though we register the number of deaths from bronchitis, we do not register the number of cases that occur; and this vagueness in the proportion prevents us from applying Method V (Concurrent and Proportional Variation) with any strictness; and in fact our presumption, our valid and justifiable presumption, that fog is one cause of bronchitis rests in the main upon the observation that they occur in association much more often than a casual concurrence would account for.

Many of the assigned causes of disease, and most of the assigned causes of insanity, are assigned upon this principle when they are assigned on any principle at all. No alienist has any doubt that childbirth is a cause of insanity, nor need he have any doubt, although by far the greater number of childbirths are not followed by insanity, and by far the greater number of attacks of insanity are not preceded by childbirth: in fact, many cases of insanity occur in males, and could not own this cause. The reasons which justify us in presuming that childbirth is a cause of insanity are first, the rapidity with which the insanity follows the childbirth, which goes some way to bring the case under the first Method of ascertaining causes, the Method of Instant Sequence; and second, and mainly, the fact that insanity and childbirth are associated together more frequently than can be accounted for by casual concurrence. That they are more frequently associated is always taken for granted, and though it has never been avowed, or even discovered, that it is this more frequent association that is the warrant for our presumption of a causal connection, there is not the slightest doubt that this is our warrant. Now that the warrant is discovered, it will be easy to show how far it is valid. The aggregate number of the female population of child-bearing age in this country in any year is approximately

known. The number of child-births, and the number of women of child-bearing age who become insane, are also known for any one year. From these data it should be easy for any competent statistician to calculate the number of child-bearing women who would become insane, on the Doctrine of Probability, if child-bearing had no part in the causation of the insanity. Any excess over this number of cases of insanity at the puerperium must be due to child-bearing, provided, of course, the numbers in the calculation are large.

Most of the cases in which heredity is alleged as a cause of disease rest, though the assertors do not know it, upon the same principle. Gout, insanity, phthisis, leprosy, cancer, and other diseases, are found sometimes to occur in those whose one or more relatives have suffered from the same disease; and when this is the case it is usually assumed without hesitation that inheritance was the cause, or had a share in the causation, of the disease. On the principle now under discussion there is no warrant for such an assumption unless the number of cases occurring in one family is greater than would be normal on the Doctrine of Probability, and unless also causal influences proper to the families, and common to the several members of the families, can be excluded.

While this principle, if applied strictly, and with caution to ensure that the cases of association are actually more numerous than they would be on the Doctrine of Chances, is sound, and justifies the presumption that the association is causal in some at least, though probably in some only, of the cases in which it is found, yet, when this precaution is not taken, the method is extremely likely to mislead, and is more often the ground of false attribution of causes than perhaps any other method. Nothing is more frequent than to find an action assigned as the cause of an effect on no other ground than that of an association, which may have been merely casual, which may not be more frequent than casual concurrence will account for, and which may have been observed in but few cases, or even in but one. It is perhaps the most frequent source of the fallacy of arguing *post hoc, ergo propter hoc*.

D. Again, we may assume causal connection from association, even though the association of the action with the effect is not constant, if the associated effect has a constant peculiarity: if, that is to say, whenever that action has preceded, the effect

has a certain quality, which is absent when the effect is not preceded by that action.

Insanity often occurs in persons who have not drunk to excess, or have even been total abstainers; and often does not occur in those who have drunk to great excess for many years. The association between drinking to excess and insanity is very inconstant. But when insanity does occur in those who have long drunk to excess, it has certain features which are peculiar—which are alike in all such cases, and are never seen in the insanity of those who have not drunk to excess. This constant quality in the effect warrants a confident presumption that the cause in all such cases is similar; and as the only constant preceding action is excessive drinking, we assign this as the cause.

Similarly, there is no constant association between total abstinence from alcohol and self-righteousness. There are many total abstainers who are not self-righteous, and many self-righteous persons who are not abstainers; but when a total abstainer is self-righteous, there is a smugness in his self-righteousness that is so constant that it warrants us in attributing the self-righteousness to the total abstinence, or at least in presuming a causal connection between them.

The handling of *primula obconica*, *humea elegans*, *whitlavia grandiflora*, and certain other plants, is apt to be followed by the appearance of nettle-rash on those who handle them. The association is not constant: nettle-rash does not always follow the handling of these plants, and often occurs in people who have never been near any of them; but when nettle-rash does follow the handling of the plants, it has certain characters that are the same in each case, and do not appear in other cases of nettle-rash. Hence we may presume, from this constant character, a causal connection between the nettle-rash and the handling of the plants.

Rain often falls without the accompaniment of a thunderstorm: thunderstorms sometimes occur without the accompaniment of rainfall; but when rain does accompany a thunderstorm, it has, in the large size of the drops, a peculiar character by which it may be recognised, and which justifies us in presuming a causal connection between the thunderstorm and the rain.

This is as appropriate a place as any in which to examine Mill's fourth Canon, which runs as follows:—'Subduct from any

phenomenon such part as is known by previous inductions to be the effect of certain antecedents, and the residue of the phenomenon is the effect of the remaining antecedents.'

Why Mill should have invented the word 'subduct' when he had already to his hand the familiar words subtract and deduct, it is not easy to say. Used by a latter-day philosopher, one would surmise that it had been employed to conceal poverty of thought, to strike awe into the mind of the reader, and impress him with an expectation of the profundity of the wisdom and penetration of what follows; but Mill was too honest to have recourse to such a stratagem unless he had first deceived himself, and this was probably the case. Passing this, we may next notice that the method has no claim whatever to the title of Experimental. The instance given, not by Mill, but by every other authority, is the discovery of the planet Neptune, and Mill, though he does not give this particular illustration, gives others from the science of astronomy. But no experiment was employed in the discovery of Neptune, nor is it possible to experiment with the positions of the planets or the stars. This Experimental Method for the discovery of causes is therefore neither experimental, nor is it employed in the discovery of causes. We have already seen that it was not the method by which Neptune was discovered, and if we analyse the instances that are adduced by Mill and other writers, we shall find that in not one case has the cause of anything ever been discovered by the Method of Residues. I do not say that it is impossible to discover a cause by this method, though I think it very unlikely that it can be done; but it has certainly not been done yet. All that has ever been discovered by the method is that there is something new to be accounted for, something of which the cause is not yet known, and then the cause of this new 'phenomenon' is discovered by one of the methods set forth in this Chapter, but not by the Method of Residues.

V. Concurrent Variation.

Causal connection may be established by the discovery of concurrent and proportional variation of action and effect; and is the more warrantable the closer the concurrence and the more exact the proportion.

This is a very far-reaching method, and though its employment is seldom in comparison with some of the other methods,

https://doi.org/10.1192/bjp.62.257.241 Published online by Cambridge University Press

it gives results when their employment is impracticable. In some cases, as will be seen in the examples adduced hereunder, it is impossible to trace any action upon the thing changed, but the concurrent and proportional variation of the action and the change impels us irresistibly to conclude a causal connection between them.

The method, as stated above, replaces Mill's Method of Concomitant Variations, which, as he states it, is manifestly false. His fourth Canon runs:

'Whatever phenomenon varies in any manner whatever whenever another phenomenon varies in some particular manner, is either a cause or an effect of the phenomenon, or is connected with it through some fact of causation.'

This Canon is, if possible, more ludicrously inept than the others, but it has nevertheless been endorsed by every writer of the school of Mill since he first stated it. According to this Canon, if the weather varies in any manner whatever whenever a child is growing, then the weather is either a cause, or an effect of the child's growth, or is connected with the child's growth through some fact of causation. Similarly, if the tide varies in height when the corn is ripening; if the fashion in women's dress 'varies in any manner whatever' whenever icebergs are unusually numerous in the Atlantic; if slugs become very numerous when Halley's comet reappears; then these 'phenomena' are connected through some fact of causation. Manifestly, it is not enough that the one 'phenomenon' should vary in any manner whatever; such a stipulation is of no value, as any child can see. The one phenomenon must vary proportionally with the other. The proportion need not be exact, but some proportion there must be between the two occurrences or changes to enable us to presume a causal connection; and the more exactly the proportion is maintained, and the closer in time the one change to the other, the more confidently we may presume the connection.

The most familiar instance is the concurrent and proportional variation between the turning of a tap and the flow of water or the size of the gas flame. As the tap is turned more and more towards the straight position, so, concurrently and proportionally, does the flow of water increase in volume or the flame increase in size. As the tap is turned more and more towards the cross position, so, concurrently and proportionally, does the flow of water or the size of the flame

diminish. The variation is not exactly proportional throughout the whole range. When the tap is near the straight position, the additional effect produced by additional alteration is less than when it is near the cross position; and when it is straight, or nearly straight, slight alterations of position have no answering alterations in the flame or the stream of water; but still, on the whole, the variation in the size of the flame or the stream are so closely concurrent with the variations in the position of the tap, and generally observe so strict a proportion, that a bystander who had never before seen a tap or a gas flame would be compelled to presume the causal connection, and would feel his conclusion the more inescapable, the more often he saw the experiment repeated. Still more assured would his certainty become when he found that the more rapid or the slower the action, the more rapid or the slower was the effect, and that any interruption of the one was attended by interruption of the other. Concurrence so close, and generally so closely proportional, would carry to his mind the irresistible conviction of causal connection. It is true that in this case our conclusion is partly derived by the Method of Instant Sequence, but, as will be more fully shown hereafter, we usually employ more than one method.

The great importance of the method of concurrent and proportional variation is that it can be applied when no other method of ascertaining causation is applicable, when experimentation is impossible, and even when the means by which the effect is produced are beyond our knowledge and beyond conjecture. It is by this method that a causal connection has been established beyond all doubt between spots in the sun and magnetic storms on the earth, a causal connection that could not possibly have been established in any other way. It is by this method that a causal connection has been established beyond all doubt between the tendency of mankind to suicide and the length of the day. The number of suicides in Europe, and the proportion of suicides to the population, have been found to be subject year after year to seasonal variations. The number of suicides is lowest in December, when the days are shortest, and highest in June, when the days are longest. The proportional variation is not exact: if plotted on a curve, the curve would be irregular, and would vary from year to year and from country to country: but still, taken over many years and in many countries, the number of suicides increases with an

approach to regularity, month by month from the winter solstice, until, when the summer solstice is reached, the number is doubled, and it then declines again irregularly through the summer and autumn months to its minimum in November and December. Since the proportion is not exactly maintained, it is clear that other influences are at work; but since the proportion obtains generally year by year in every European country, we are compelled to presume a causal connection between the number of suicides and the length of the day, even though we are utterly unable to conjecture the manner in which the causal influence is exerted. It is clear that the number of suicides cannot affect the length of the day; and we cannot suppose that longer hours of daylight affect the mind of the potential suicide so as to confirm his purpose. Through what devious channels the causal influence travels we cannot conjecture; but that the length of the day is in some way causally connected with the number of suicides we cannot doubt.

In such a case as has just been examined, the facts are beyond doubt, and admit of no uncertainty; but the method requires care in its application, and is open to more opportunities for error than any other method, for this reason among others, that it is employed usually in cases that are complex and intricate; in cases in which many causes, some perhaps unsuspected, may be contributing to a result; in cases in which other methods cannot be employed to check and control our conclusions; and also because it usually depends on the collection of statistics, with all the numerous and inevitable errors to which the collection of statistics is liable. The manipulation of numbers is perhaps the most accurate process of which the human intellect is capable. Given a set of numbers to start with, every step in calculation can be checked with the most rigid exactness, so that it is scarcely possible for two competent calculators to arrive at different results; but the applicability of these results, and the correctness of the inferences to be drawn from them, depend entirely on the correctness of the original figures from which the start was made, and this is usually sadly to seek. It is easy, for instance, to establish a concurrent and proportional variation in the amount of drunkenness in a community and the number of crimes committed in that community, and hence to establish a causal connection between drunkenness and crime; but consider the methods in which the statistics of crime and of drunkenness are collected.

The statistics of crime are taken from the records of the police, but different chief constables have very different views of what should constitute an offence 'known to the police', and their statistics will vary accordingly. When loss of property is reported to one chief constable, he enters it at once as a theft. If it is subsequently discovered to have been an accidental loss, it is taken out of the class of thefts; but if the manner of the loss is never discovered, the loss remains recorded as a theft. Another chief constable will not enter a loss as a theft unless there is good reason to believe that the property has been stolen; and a third will not enter anything as a theft unless the thief has been caught and prosecuted, and a conviction obtained in a court of justice. It is clear that to compare with one another the statistics of theft in these three districts would be absurd. Again, in a district in which the Watch Committee contains a large proportion of teetotalers, and the magistrates take a stern view of drunkenness, the number of drunkards apprehended, or summoned, and convicted will be at a maximum. In an adjoining district, in which the amount of drunkennesss is not less, or may even be greater, but in which the police have instructions to look leniently on slight departures from sobriety, and rather to see a man home or to put him in care of a friend than to arrest him, and in which the magistrates are prone to give offenders the benefit of any doubts they may entertain, the statistics of drunkenness may be less by a third, or even a half. Again, 'serious' offences are those which are tried at assizes or quarter sessions: 'trivial' offences are those disposed of in courts of summary jurisdiction; but in many cases the offender has an option whether he will have his case disposed of by the magistrate, or whether he will elect to go for trial; and in exercising this option he will be influenced by the reputation of the magistrate for leniency or severity; and in this case again the statistics of 'serious' crime in the jurisdiction of one magistrate are not comparable with those of such crime in the jurisdiction of another. Differences such as these are seldom allowed for by the statistician. In his eagerness to have a set of figures to manipulate, and to produce a result that shall be 'mathematically accurate', he is too often blind to the initial errors of the figures that form the basis of his calculations.

In most cases, variation, when concurrent and proportional, is so within certain limits only, and unless these limits are observed the causal connection will be stated too absolutely, as

in fact it usually is. Within certain limits, the rate at which a plant grows is concurrent and proportional to the temperature; but there is a certain lower limit of temperature at which the plant will not grow at all, and however much this limit may be exceeded, the growth of the plant exhibits no proportional variation; and there is a certain higher limit at which the plant suffers damage, and will not grow, and however much this limit may be exceeded, the growth of the plant exhibits no proportional variation. Within certain limits, the consumption of a commodity varies in inverse proportion to the price; but there is a certain lower limit of price at which the consumption is at a maximum, and however much the price may be lowered beyond this limit, the consumption will not increase; and there is with many commodities a certain price at which the consumption of that commodity is at a minimum, and however much beyond this the price may be increased, the consumption of the commodity will not diminish. Within certain limits, the amount of work that a man can do varies concurrently and proportionally with the amount of food he eats; but there is in the quantity of that food a certain lower limit at which he can do no work, and no diminution of the food below this limit can diminish his work; and there is in the quantity of this food a certain upper limit at which he can do the maximum of work, and any increase beyond this does not increase, but diminishes, the quantity of his work. This limitation of the application of the method of concurrent and proportional variation, obvious as the limitation is, has never been noticed by any writer on the subject; but then no one but logicians have written on the subject, and, as I have said elsewhere, logicians are blind to the obvious, naturally blind; but they must have taken great pains not to see many of the things they neglect. Such an excess of unobservation is not in nature.

VI. Common Rarity.

If an unusual effect is associated with an unusual action, we are apt to assume a causal connection between them, and the assumption has the more justification the more unusual both the action and the effect are.

In the early '80's of the last century there was a terrific volcanic eruption at Krakatoa, in Java, a great part of the mountain being blown up and dissipated. An eruption of such

violence had not occurred in historic times. Weeks afterwards there occurred in this country, and indeed almost the world over, a prolonged series of most wonderfully coloured sunsets, such as no one living had ever witnessed before. This extremely unusual effect was connected by its very rarity with the extremely unusual volcanic action, far away as that action was; and it was argued, and the argument was generally accepted, that the gorgeous sunsets were due to the presence in the air of an unusual quantity of impalpably fine dust, which had been projected into the upper regions of the air by the explosion of the volcano, and had floated to distant parts of the earth. It was the common rarity of the action and the effect which suggested a causal connection between them.

In the great frost of 1686 many great trees suddenly split from top to bottom with a loud report like that of a cannon. Our ancestors did not know how the frost could produce this effect; but it is a very rare occurrence, and so intense a frost was a very rare occurrence; and the common rarity of the two events led to the assumption that they were causally connected, and that the frost was the cause of the splitting of the trees.

In sparsely populated countries the advent of a visitor is a rare occurrence. If, after such an occurrence an object is found to be missing, and this also is a rare occurrence, causal connection between the occurrences will be presumed on the ground of their common rarity.

In the very exceptionally severe winter of 1895, seagulls appeared for the first time as far inland as London Bridge. The common rarity of the two events pointed inevitably to a causal connection between them.

VII. Corresponding Qualities.

Any peculiar quality in an effect points to a corresponding quality in the agent that produces the effect.

This principle is very frequently employed in practice, so frequently that it is puzzling that writers on causation have overlooked it. Like several of the other methods here described, it jumps up and hits in the face anyone who gives a moment's consideration to the subject; and like others of the methods, it has been familiar to us from our earliest years. The leading case is that of Robinson Crusoe and the footprint. When he

https://doi.org/10.1192/bjp.62.257.241 Published online by Cambridge University Press

saw the footprint in the sand, did Crusoe wait until he had seen two or more instances of the phenomenon having only one circumstance in common, and two or more instances in which the phenomenon was absent having nothing in common but the absence of that circumstance? Not being a logician or a lunatic, he did nothing of the kind. He said at once 'A man has trodden here.' What was his justification for this conclusion? It was that he saw in the print certain peculiar qualities which pointed irresistibly to corresponding qualities in the agent that produced the print. These peculiar qualities in the print corresponded with peculiar qualities of the human foot. No other agent possesses them. The inference was inescapable that the human foot was the agent that produced the print.

This method is particularly valuable when it is desired to identify, not so much the cause, as the agent that has produced a certain effect. It is therefore especially used by the police in criminal investigations, in which the cause, human agency, is already known, and what is desired is to identify the agent. The modern method of criminal investigation, devised by Major Atcherley, the Chief Constable of the West Riding, is avowedly founded on this principle. He takes it as an accepted fact that no two men are exactly alike, and that the differences, small but easily distinguishable, that enable us to identify the face and figure of every man, and to distinguish him from his fellows, are paralleled by differences that, if small, may be distinguished by skilled and trained observation, between their modes of action. Thus it is found that each criminal has his own special department of crime, to which he confines himself wholly or mainly. One is a burglar, another a pickpocket, another a long firm swindler, another an area sneak, another a perpetrator of the confidence trick, and so on. More than this, each pickpocket, each burglar, each long firm swindler, and so on, has his own minor peculiarities of action, which leave their peculiar impress on the effects that he produces; so that, given all the details of the effects produced by a crime, it is usually possible to conclude which particular criminal known to the police has committed it.

In order to secure a conviction, however, it is not enough that the police should know what criminal has committed the crime; it is necessary in addition that they should have evidence to lay before the jury connecting the criminal, as

agent, with the crime as effect or result. This can only be done by proving some peculiar quality in the crime, or in some accompaniment, part, or condition of the crime, that corresponds with a peculiar quality, either in the agent himself, or in some instrument peculiar to him.

Thus, if a wound has such qualities as show that it was inflicted by the left hand, and the accused is left-handed, the conjunction is evidence against the accused; but since left-handedness, though unusual, is not peculiar to the accused, he should not be convicted on this evidence alone. If, however, the print of a bloody hand shows that the criminal had lost half the second finger and the whole of the third, and if the accused has lost these parts, then he must be convicted, for such qualities are peculiar to him. It is on this principle that the evidence of finger marks is conclusive of the presence of the person with whose fingers they correspond; for the finger markings of each individual person are peculiar to him alone.

If a jemmy found in the possession of the accused exactly fits marks on a door that has been prized open, the jemmy is evidence against the accused; but it is not proof, for many jemmies may be made of the same bar of steel, and many bars of steel by the same rollers, and therefore the quality of the jemmy would not be peculiar or proper to that jemmy; but if the edge of the jemmy is chipped and shows a notch, and if the mark on the door fits the edge of the jemmy, notch and all, then the identification of the jemmy, as the agent that produced the effect, is beyond doubt, for now the corresponding qualities of the effect and the agent are peculiar.

When Crippen was accused of the murder of his wife, certain human remains were found in his cellar wrapped in pyjamas. So far this was no evidence against Crippen; but it was subsequently proved that he had bought those very pyjamas; and thus an instrument of the crime was shown, by the possession of peculiar qualities, to have been in his ownership.

An anonymous letter, typewritten throughout, is received. The script is that of a common make of typewriter, and is not peculiar; but every impression of one of the letters exhibits a certain defect. If a typewriter can be found having that peculiar defect in that letter, then there is no doubt that this typewriter was the agent employed, and that the person who wrote the anonymous letter had access to that typewriter.

A gardener finds his seedlings gone, and on the soil on which

they grew he finds a shining track of dried slime. He concludes at once that the agent that has taken his seedlings is a slug, for the quality of the shining track is peculiar, and corresponds with the peculiar quality of slugs of leaving such a trail behind them. He knows, moreover, that slugs have an appetite for seedlings, having often lost seedlings by slugs before. Thus by a combination of the Method of Corresponding Qualities with the Method of Similarity he concludes that the criminal that stole his seedlings was a slug.

It is usual in English parks to see all the trees, however irregular the rest of their outline may be, present a flat surface towards the ground, at the same distance from the ground in every tree. The common effect points to a common cause: the peculiar quality of the effect points to a peculiar quality in the agent: the agent must be one that can reach to just the height from the ground at which the foliage terminates; and the only such agents that have access to the trees are the cattle or deer that are pastured in the park.

VIII. Coincidence in Area.

If an action has taken place on a certain area of a thing, and if subsequently a certain effect is found to be precisely limited to that area, then we may confidently presume that that action was the cause of that effect. It is more frequent, however, to infer from coincidence of area the influence of a condition than that of a cause, and in many cases the distinction is practically unimportant.

When a picture that has long been hanging on a wall is taken down, it is usual to find the area of wall paper that was behind the picture deeper in colour than that of the surrounding wall paper, and the area of the deeper colour coincides with the area of the picture. In such a case we are driven to the conclusion that the prolonged presence of the picture in that place was a condition of the retention of its colour by the paper behind.

If in summer a drain is laid across a lawn, and the ground is filled in, and the turf relaid, it may be found in the following winter that hoar frost is thick upon the ground over all the rest of the lawn, but that the line over the drain is free from frost. The coincidence in space compels us to presume that the altered state of the ground brought about by laying the

drain is a condition of the non-appearance of the frost, and that the action of laying the drain was an indirect cause of this unchange.

It sometimes happens that a rash appears on a person's legs exactly up to the level of the top of his stockings, and there ceases abruptly. Such coincidence in area compels us to presume that the wearing of the stockings is a condition of the effect, the putting of them on an indirect cause of the effect, and the action of something in the stockings the direct immediate cause of the effect.

In experimental agriculture it is a frequent practice to sow an area of soil uniformly with a certain kind of seed, after different portions of the area have been treated with different manures, and one portion of the area with none. Any difference in the crop which is uniform over one portion so treated, and coincides with the area treated, is presumed to be due to the presence of the manure in that area, which was a condition, as the manuring was an indirect cause, of the result.

It has been found that the vegetation of a meadow is different in two narrow parallel lines a few inches wide, extending from one gate across the meadow to another. When it was remembered that a cart was driven across the meadow from one gate to the other, and that the lines of different vegetation coincide with the cart track, it could not be doubted that the traverse of the cart was the cause of the difference in the vegetation.

The area over which the action extends, and to which the effect is limited, need not be continuous.

Every gall that grows on trees or plants is found to contain, or to have contained, the larva of an insect. It is therefore presumed that the presence of the larva in the gall is causally connected with the formation of the gall. From other sources of information we know that in each case the larva grows from an egg that has been inserted by the mother insect into the tissues of the plant. As galls do not grow on any part of a plant into which an egg has not been inserted by an insect, the coincidence in area, of the attachments of the galls with the places into which eggs have been inserted, compels us to presume that it is the operation of inserting the egg, or something accompanying that operation, which is the cause of the galls.

The same principle is constantly employed in the physiological and pathological laboratory. To find the physiological action of a food or a drug, two animals as nearly as possible

alike are taken, and placed under similar conditions. The food or drug is then administered to one, and not to the other; and any physiological change that is limited to the one to which the food or drug has been administered is presumed to be due to the administration.

Similarly, in experimenting on or with bacteria, two or more test-tubes or surfaces are taken, and are treated similarly in every respect but one. Whatever difference ultimately appears between them is held to be due to the one respect in which they were differently treated.

IX. Coincidence in Time.

As the method of Instant Sequence is limited in application to the discovery of those effects, or of the causes of those effects, that are changes, so the method of Coincidence in Time is limited to the discovery of the causation of those effects that are unchanges; with this exception, that by the latter method we may sometimes identify the agent that produces repeated instances of change. This we do by ascertaining the presence during the whole time these effects are being produced, of a certain agent, or of similar agents.

If, upon making a manure heap near a house, that house becomes infested by a plague of flies, and if, upon the removal of the manure heap, the plague is stayed, then we should presume a causal connection between the manure heap and the flies, even if we did not know that flies breed in manure.

How do we gain the belief that sea-sickness is due to the motion of the boat? The sole foundation for the belief is in the coincidence in time of the motion with the unpleasantness.

How do we know that the din of a factory is due to the motion of the machinery? Partly, no doubt, by Subsumption of the case under the law that all noise is due to motion; partly by the method of Concurrent and Proportional Variation, since the nearer we approach to the apartment in which the machinery is, the louder the noise becomes, and *vice versâ*; but mainly by the knowledge that when the machinery starts the noise begins; that the noise continues as long as the machinery is going; and subsides into silence the instant the machinery stops.

How do I know that the draught that is blowing my papers about comes from the open window? By observing that it

https://doi.org/10.1192/bjp.62.257.241 Published online by Cambridge University Press

began the moment the window was opened, continued as long as the window remained open, and ceased as soon as the window was shut.

It is necessary, I suppose, to adduce an instance from 'science,' and therefore I may here point out that the causation of magnetic storms by sunspots, which is ascertained partly, as already shown, by the method of Concurrent and Proportional Variation, receives corroboration from the method now under consideration, of Coincidence in Time.

If a number of thefts take place in a house, and if, upon one of the servants leaving the house, the pilferings cease, and especially if it is then remembered that the pilferings did not begin until after that servant entered the house, the presumption is very strong that that servant is the pilferer. In this case the coincidence in time is not between a cause and an effect, but between the presence of an agent and a series of effects.

If it is found that explosions in coal mines coincide in time with depression of the barometer, the presumption is raised that the lowness of the pressure of air has a causal influence on the explosions. It is evident that, while from one aspect this may be regarded as a case of Coincidence in Time, from another aspect it may be regarded as a case of Association.

These, then, are the nine or twelve circumstances that warrant us in presuming a causal connection between an action, an agent, or a condition, and an effect or result. Any one of them, if fully established, justifies the presumption of causation or of causal connection, but in practice we rarely limit ourselves to one method, and in practice, moreover, they are not as distinct as they are here made to appear by systematic description and somewhat artificial separation. When we seek to discover a cause, or a condition, or an agent, we use what means we can; and it is only after our reasonings are complete that we are able to analyse them, and to extricate from the various considerations that influenced us the separate elements that are here disentangled and separately displayed. In practice they are no more pursued in isolation from one another than deduction and induction, fundamentally different as they are, are employed in isolation from each other. Few of the methods of ascertaining causation can be employed quite separately, for as most of them have a common origin in the Axiom of Causation, they are not wholly different, but merge and blend

into one another; what separation they have being largely artificial, so that a given instance may often be ranked under one or another method according to the way in which we contemplate it, and according to the feature to which we give prominence. The only methods that are not derived from the Axiom of Causation are the Method of Instant Sequence, the Method of Coincidence in Time, and the first application of the Method of Association. It will be interesting to inquire what grounds we have for inferring causation by the use of these methods.

What warrant we have for concluding that a change in a thing that instantly follows upon an action on that thing is the effect of the action, is not immediately apparent. Few convictions are more firmly and deeply rooted in our minds, and at a very early age too, as we see when the baby in arms blows upon a watch. Having seen the change follow once, the child concludes that it is the effect, and that it does draw this conclusion is proved by the child repeating the action with the evident intention of seeing the change repeated. If the sequence, of a change in a thing occurring instantly upon an action on that thing, were constant in experience, the empirical ground of the conviction would be manifest and would be sure; but there is no such constancy in experience. We frequently witness actions that are not instantly followed by perceptible changes in the thing acted on, and we frequently witness changes in things that are not instantly preceded by perceptible action on the thing changed. The experience of instant sequence is no doubt frequent; but it is by no means constant in experience. The real ground of the inference is, I believe, in our experience of our own acts—in the changes in our own bodies that instantly follow the exertion of our wills, and in the changes instantly produced both in things around us and in ourselves by our own acts. The first sequence is strictly constant in experience. Our own movements instantly follow the action of our wills, and never in health take place except in instant sequence to volition. It is often objected that this cannot be the origin of our notion of causation, because we do not know how the mental operation of the will can produce a bodily movement; but this is beside the question. Such knowledge is quite unnecessary for the origin of the notion. It is enough for us that the exertion of the will is to us an action. It is an exertion of the activity of the self, and is not

only to us an action, but is, I believe, the ultimate source of our notion of action. And it is, to us, an action on our bodies and limbs. Whether the will does or can act upon the body, and if so by what means, is beside the question. It is indisputable that it seems to us to do so, and that, until our minds are sophisticated by the teaching of philosophers, it is to us as unquestionable a certainty as the existence of an external world, or as our own existence. The second sequence also, that of the instant changes that follow our own acts on things around us, is constant in experience. It is true that some of our actions on things around us are not instantly followed by perceptible changes in them, as when we hit a brick wall with the fist, but there is always an instant change either in them, or in ourselves, or in both. Even when we hit a brick wall with the fist, the action is instantly followed by the sound of the blow and by the pain of the blow. I think, therefore, that the ground of our belief in the causation of a change that instantly follows an action is empirical, and is based, as so many of our most certain convictions are based, upon the *enumeratio simplex*.

That we should argue causation from Isolated Action is more easily explained. We come to the instance with the conviction in our minds that a change in a thing must be due to an action on that thing; and if the change is preceded by one action only, or by but one material action, that action must be the cause of the change.

The method of Coincidence in Time rests upon the manifest connection that this coincidence establishes. A cause is an action connected with a change or unchange in the thing acted on. If we can establish a coincidence in time between the unchange and an action, we have gone far to identify the cause; for, as already shown, the action that causes an unchange is necessarily contemporaneous with the unchange, and begins, continues, and ends with the unchange.

All the other methods derive their validity from the fundamental Axiom of Causation, that like causes in like conditions produce like effects. The Method of Assimilation is the direct application of the principle. Subsumption under a general law is a direct, but a wider application of it, to cases fundamentally similar though superficially different. It is effected by establishing similarity in material features between the case in hand and the cases assembled under the law. Constant

Association of the action with the effect means the constant association of similar action with similar effects, so that if one pair is causally connected, the other pairs are causally connected.

Constant Association of an action with some quality in the effect comes under the same rule. An association that is more frequent than casual concurrence will account for again implies the comparison and assimilation of cases, and assumes that in similar conditions similar effects are produced by similar causes. The Method of Concurrent and Proportional Variation rests upon the assumption that not only do like causes in like conditions produce like effects, but also like differences in causes produce like differences in effects; and similarly, the other Methods manifestly obtain their validity from the same fundamental axiom, or from some derivative of it.

It follows that the methods, being founded upon the same principle, and being but different applications of the same principle, are not only fundamentally similar, but merge and blend into one another, so that not only may we employ more than one concurrently, but also the method that we employ in any individual case may often be relegated to one or another of the twelve methods, according as we choose to regard it, or according as we lay stress on this or that feature in our method. The Method of Coincident Areas, for instance, may be regarded as a case of the Method of Association. It may be called a case in which the addition alone of an action is followed by an effect, or the withdrawal alone of an action is followed by the disappearance of an effect. In this way of stating the matter, however, the time element is brought into prominence; but in applying the Method of Coincident Areas we drop the time element out of consideration, and found our conclusion directly upon the coincidence in space which is a guide or indication to the presence or absence of the action. The Method of Common Rarity is, in one aspect of it, another instance of the first Method of Association. Seeing that Like effects in like conditions are always owing to like causes, it follows that a rare effect must be due to a rare cause or to rare conditions; and when it is preceded by a rare action we are justified in associating the rare action with the rare effect, because common actions can be excluded if the conditions are common. It is possible, therefore, to diminish the number of

methods, but only at the cost of exercising a certain amount of ingenuity in bringing some under others; and it would be possible to increase the number, but only by making distinctions scarcely worth making, and at the cost of increasing the burden on the memory. As they are stated, they present a useful and practical compromise.

Summary.

The methods of ascertaining causation used by scientific men in scientific matters are precisely the same as those used by everyone else in the common affairs of daily life, and are nine in number, one of them including four distinct methods, so that there are twelve in all, as follows—

I. Instant Sequence.
II. Subsumption under a general law.
III. Assimilation.
IV. Association.
  A. When sole, or isolable.
  B. When constant.
  C. When too frequent to be casual.
  D. When attended by a constant peculiarity in the effect.
V. Concurrent and Proportional Variation.
VI. Common Rarity.
VII. Corresponding Qualities.
VIII. Coincidence of Area.
IX. Coincidence in Time.

These are here substituted for Mill's four Methods of Experimental Enquiry, which are not four, but five; some of which cannot be, and none need be, experimental, and none of which ever has been used or ever could be used. Mill's methods are examined and found to be all absurd, and one of them unintelligible.

Each of the methods above enumerated is examined, and shown by illustrative examples to be in use for the discovery of causes, both in scientific and in other matters. In practice it is usual for more than one method to be employed without discrimination in the same case; and as all but three of them are founded on the Axiom of Causation, separate discrimination of any but these three is to some extent artificial.

CHAPTER VII.

ERRORS IN ATTRIBUTING CAUSATION.

CAUSATION has been defined as the connection between action on a thing and the sequent change or accompanying unchange in the thing acted on. It follows that in order to prove causation we must prove

(1) Action on the thing changed or maintained unchanged.

(2) Sequence of the change on the action, or contemporaneous action and unchange.

(3) Connection between the action and the change or unchange.

It follows also that the following blunders in attributing causation are possible, and in fact they are often committed.

(1) An agent may be taken for a cause.

(2) The agent may not exist.

(3) The action may not exist.

(4) The action may not be on the thing in which the effect is produced.

(5) The action on the thing changed may not be connected with the change.

(6) The action may not precede the change or accompany the unchange.

(7) A condition may be taken for a cause.

(1) A cause is an action, and an action implies an agent. It would seem, therefore, that the first step in discovering a cause is to discover the agent; but this is not necessary. A cause is an action, and when we have identified the action that causes the effect, we know the cause, and need not go behind it to discover the agent. Before the discovery of gravitation, the action of the earth, in attracting bodies on its surface towards the centre, was as well known as it is now, but that action was attributed, not to the earth, which contributes immeasurably the greater part of the action, but to the heavy

body, which contributes but an infinitesimal part. When we have discovered that a man's death is due to the action upon him that we call typhoid fever, we know the cause of his death; and this cause was known long before the agent, the micro-organism, was discovered. When we find a window starred, we have no doubt that the starring is due to the impact of a hard body, though we may be quite unable to discover the body, the agent whose action was the cause of the damage.

An action is sometimes mistaken for an agent. Natural Selection, which is the action upon living organisms of destructive agents, is often spoken of as an agent, and taken to be an agent. Few expressions are more frequent in the writings of biologists than 'the action of Natural Selection', an expression that is quite correct if it means 'the action that is called Natural Selection', but that is mistaken if it means, as it often does, 'the action that is produced by Natural Selection.' Passing this error, which is something more than an error in nomenclature, we come to the first of the errors enumerated in our list, the taking of an agent for a cause. This is a very common error in popular speech. 'Thou art the cause of this anguish, my mother.' 'You are the cause of this disaster.' Mill even considered the earth to be the cause of the fall of a stone. It is, of course, the action of the mother, and of the other person accused, and of the earth, that were the causes. The persons were the agents, and not being actions, could not be causes. I think every one with a nice sense of the use of language, and of the meanings of words, will admit that to speak of a person, or indeed of any other agent, as a cause, is a perversion of language.

(2) In the search for causes we are not obliged to go back as far as the agent. The cause is already discovered when we have discovered the action connected with the change or unchange in the thing acted on; but it is often extremely useful to identify the agent, and some of our investigations into causation, such as those into the causation of crimes, have no other purpose. Still, as we have seen, the action and the agent are often identified, and very often indeed no sufficient distinction is drawn between them, and search is made for an agent instead of for an action. Nay, the fancied necessity for finding an agent is so urgent, that not only may that be taken

for an agent which exerts no action on the thing changed or unchanged, but also an agent that is purely imaginary may be invented *ad hoc*, and the cause may be identified, not only with an agent that is no agent for the purpose in view, but even with an agent that does not exist.

The attribution of causation to agents that have no existence except in the imagination of the searchers after cause appears *a priori* unlikely, but in experience it is frequent enough. Gardeners attribute canker in fruit trees to the action of sourness in the subsoil on the roots of the trees, but there is neither proof nor evidence that the subsoil is sour. I have myself tested the soil three feet below a badly cankered fruit tree, and found no acid reaction; but this is, I am pretty sure, the only attempt that has ever been made to test the subsoil for sourness. The spiritualistic medium accounts for the table rapping out a wrong answer, by the existence of a lying spirit *in the table*; but there is no proof and no evidence that the spirit of the medium has entered into the table. The Mendelian accounts for feeble-mindedness in other people by the transmission of a unit-character from the parents of the feeble-minded; but there is no proof and no evidence of the existence of a unit-character in either parents or child. Perhaps the most remarkable and the least justifiable of these imaginary agents is that of the psycho-analyst. He assumes that the cause of your forgetting a word is some unpleasant association of the word in your mind. In fact, in most cases there is no evidence of any such unpleasant association; but the psycho-analyst, like the spirit rapper, is equal to the occasion. He says the very fact, that you cannot remember any unpleasant experience connected with the word, is itself proof that you have had such an experience; for, being unpleasant, you have thrust it out of your mind. The less you remember it, in fact, the more certain it is that you are wilfully putting it out of your mind, and the more you wilfully put it out of your mind, the more certain it is that the remembrance is unpleasant. In short, the less evidence there is that you have had such an experience, the more certain it is that you must have had it. Deny that you have wilfully put out of your mind either the word you have forgotten or its unpleasant association, and still the psycho-analyst is ready for you. Your will was exercised unconsciously. Manifestly, by such means as this one could prove anything. What cannot be accounted for by uncon-

scious volition is accounted for by repressed sexual passion, the existence of which is assumed with a similar disregard of the necessity of evidence. It is another imaginary agent. It would be tedious to enumerate but a tithe of the imaginary agents that have been invoked as causes of phenomena. They range from the sour subsoil of the gardener, through the repressed complexes of the psycho-analyst, the Social Contract of Rousseau, and the archæus of Paracelsus, to the hypostatised Ideas of Plato.

The imaginary agent invoked as a cause was the *causa non vera* of the Scholastic writers.

(3) Next in gravity of error to imagining an agent that is imaginary is to take for a cause an action that is imaginary. Though not quite so grave or so gratuitous a blunder as the last, this is bad enough, and it is extremely frequent. It is the error that underlies judicial astrology, and the greater part of the bewildering lore of amulets, mascots, omens, talismans, phylacteries, and lucky and unlucky things of all descriptions. Astrologists declared, yes, and still declare, for there are still survivors of this queer class of believers, that the position of the planets at the moment of a man's birth determines the whole course of the subsequent life of the 'native.' The planets do really exist. They are not mere phantoms of the imagination, like the lying spirit of the table or the unconscious pain of the psycho-analyst; and having a real existence, they are agents in some respects and towards some things. They act, for instance, on their satellites, and on one another. But there is not a smidgeon of evidence that they act upon the course of human lives in the way the astrologers imagine. Similarly, charms and amulets, and the whole apparatus of popular superstitions, do exist as material objects; and having a real existence, they are capable of action of some sort, if only by their weight; but there is no evidence that they exert the action that is attributed to them by popular fancy.

It is common to find that people who go to warmer, damper, and more low-lying places sleep more and are less energetic than they were when at home; and it is common to find that people who go to colder, higher, and drier places appear to gain energy and to be capable of more exertion. These effects are always attributed to the action of the air in such places, which is said to be 'relaxing' in the one case,

and 'bracing' in the other. There is no evidence that the air has any such action, or that there is any difference in the air of the one place and the air of the other. Not seldom places of the two different qualities are near together, and the wind frequently blows from the relaxing place to the bracing place, and *vice versâ*. It is most improbable therefore that the air in the one place is appreciably different from the air in the other; and if a difference were found, it would still remain to be proved, by one of the twelve methods set forth in the last chapter, that this difference has or can have such an action on the human body as is attributed to it.

Many temporary and obscure ailments are attributed, not only by the laity, but by some medical practitioners, to 'a sluggish action of the liver,' or to 'a chill on the liver.' The actions of the liver are many, and are imperfectly known, but in the cases in question there is not a shadow of evidence that any one of them is being performed less actively than usual, nor is there any evidence that the liver has been chilled. The liver is deeply seated, and is covered by thick layers of muscle, bone, skin, and other structures, and could not possibly be chilled unless the temperature of the whole body were reduced; and if it were, there is no evidence whatever that such lowering of the temperature of the liver could produce the effects that are attributed to it. Many drugs are advertised and taken for the purpose of purifying or cooling the blood; but apart from the want of evidence that the blood of the person taking them is impure, or is unduly hot, there is no evidence whatever that these drugs exert any purifying or cooling action upon it.

Gardeners and rustics commonly attribute changes in the weather to changes in the moon, which are really changes in the relative positions of moon, earth, and sun; but that these relative positions have any influence upon the weather there is no evidence to show.

At a certain spiritualistic seance at which Dr. (now Sir James) Crichton Browne was present, 'manifestations' occurred until he so plugged the eyes and ears of the medium that the medium could neither see nor hear; then the manifestations ceased. At the end of the sitting, a believer who was present attributed the cessation of the manifestations to 'the offensive incredulity of Dr. Crichton Browne.' There was no evidence, however, that this mental attitude of the sceptic exerted any action upon the medium, or upon the spooks who were supposed to be in

relation with the medium; while there was another action of Sir James' upon the medium to which the effect might well have been attributed.

When the Hawke rammed the Olympic in the Solent, those on board the Olympic attributed the change in the course of the Hawke to the action of starboarding her helm; but it was proved at the trial that this action was imaginary: the Hawke had not starboarded her helm.

The mistake of attributing as a cause an action that is entirely imaginary is as old as humanity, and shows little sign of becoming less frequent, although the most impressive exposure of it that has ever been made is three thousand years old. It is to be found in the Wisdom of Solomon, XIII, 11, and runs as follows:—

'Now a carpenter that felleth timber, after he hath sawn down a tree meet for the purpose, and taken off all the bark skilfully round about, and hath wrought it handsomely, and made a vessel thereof fit for the service of man's life;

'And after spending the refuse of his work to dress his meat; hath filled himself;

'And taking the very refuse among those which served to no use, being a crooked piece of wood, and full of knots, and hath carved it diligently when he had nothing else to do, and formed it by the skill of his understanding, and fashioned it to the image of a man;

'Or made it like some vile beast, laying it over with vermilion, and with paint colouring it red, and covering every spot therein;

'And when he had made a convenient room for it, set it in a wall, and made it fast with iron;

'For he provided for it that it might not fall, knowing that it was unable to help itself; for it is an image, and hath need of help;

'Then maketh he prayer for his goods, for his wife and children, and is not ashamed to speak to that which hath no life.

'For health he calleth upon that which is weak; for life he prayeth to that which is dead; for aid humbly beseecheth that which hath least means to help; and for a good journey prayeth of that which cannot set a foot forward;

'And for gaining and getting, and for good success of his hands, asketh ability to do of him that is most unable to do anything.

'Again, one preparing himself to sail, and about to pass

through the raging waves, calleth upon a piece of wood more rotten than the vessel that carrieth him.'

No doubt it will startle the ecclesiastically minded ladies who throw some of the spilt salt over their shoulders to avoid disaster, to know that their attitude of mind is the same as that of the idolater.

(4) The action attributed as a cause may not be on the thing in which the effect is produced.

This is the fundamental error of witchcraft, of spells and charms, and many other superstitions. Witches undoubtedly existed: the agent was not imaginary. Nor was the action imaginary, for the witches did undoubtedly exercise their craft. They did cast spells and execute incantations, they did say the Lord's prayer backwards, they did make wax figures, and stick pins in them, and exercise in other ways the craft of the witch; and these things they did in order to influence the weather, to produce illness and misfortunes to their neighbours, to make their cattle slip their calves, their children have fits, and to cause other effects. But the gap in the chain of causation was that the action they exercised was not upon the thing they desired to change. Whatever incantations they uttered exercised no action on the weather. The pins which would have produced pain and injury if they had been stuck into the persons of the witches' enemies, were not stuck into their persons; they were stuck into images of them. The action was not on the thing in which the effect was to be produced. The spells that they cast upon the cattle or the children did not act upon the cattle or the children; and if any effects on the various objects followed the witchcraft, they could not have been due to the witchcraft, which did not act on the things in which the effects were produced.

It is currently believed that if you cut your nails on a Friday, or bring a peacock's feather into the house, or cross the knives, or spill the salt, or view the new moon through glass, or do any of a hundred other harmless acts, the action will bring misfortune upon you. In each of these cases there is an action; but in none of them is the action upon the thing in which any unfortunate effect that may follow is produced. You cut your nails on Friday, and on Sunday you put a sovereign instead of a shilling into the offertory. The misfortune happens right enough, but the action was on the nails, not on the sovereign

You bring a peacock's feather into the house, and in the following week your child at school is attacked by measles; but your action was on the feather, not on the child. You spill the salt, and next day your horse casts a shoe, or your motor tyre bursts at an inconvenient moment; but your action was on the salt and the tablecloth, not on the horseshoe or the tyre.

A certain Irish tenant tried to diminish what he considered his landlord's rapacity by shooting the landlord's agent; but the action, strenuous though it was, was not directed at the thing, the landlord, that the tenant desired to alter, and was therefore ineffectual; and so the landlord explained. 'If you think' said he 'that you can intimidate me by shooting my agent, you are very much mistaken.'

An old woman who had the reputation of a witch acquired a large practice by uttering a certain spell, to which immense efficacy was attributed by her neighbours, who willingly paid her for it the fee that she demanded, which consisted of a loaf and a penny. At length her practices reached the ears of the authorities, who seized her and threatened to tie her thumbs and great toes together, and to duck her in the horse-pond, *secundum artem*, unless she revealed the spell by which the wonders were worked. I trust I do them no injustice if I surmise that the authorities would not have been unwilling to have in their own hands an instrument of such power. Under this duress the poor woman consented to reveal the text of her spell, which ran, so she said, as follows:—

Thy loaf in my lap,
  Thy penny in my purse;
Thou art never the better,
  And I am never the worse.

It seems unlikely that the action of uttering this could have had the causal influence with which it was credited, and the same may be said of all spells and incantations, whether of witches or of psycho-analysts.

(5) The action on the thing changed may have no connection with the change.

To attribute an effect to an action with which it has no connection is a blunder, and a very frequent blunder, but it is a much more pardonable blunder than any that we have considered hitherto. As we have seen in the seasonal variations in

the frequency of suicide, it may be impossible to trace the nature of the connection, even when the facts render a connection of some kind certain; and experience of such cases might well lead us to suppose a connection when the nature of the connection is obscure. But the error we are now examining does not rest on experience of such cases, and does not consist in inferring a connection that is obscure: it consists in inferring a connection without sufficient evidence. The seasonal variation of suicides, the concurrent variations of sunspots and magnetic storms, and many other instances, show that to establish a connection it is by no means necessary to discover the nature of the connection; but it is necessary to establish, by one of the twelve methods described in the previous Chapter, that there is a connection, or causation cannot properly be inferred.

It is evident that the fallacy in all the previous cases that have been examined lies in the absence of any connection between an action and the change or unchange in the thing acted on. Such a connection is necessarily absent when the supposed action is that of an imaginary agent, such as acid in the subsoil, or unconscious pain, or a Social Contract; or is itself imaginary, such as the supposed action of the planets on human life, or that of a chill on the liver; nor can there be a connection between an action and an effect if the action is on something other than that in which the effect is produced, as when witches stick pins into the effigy of a person they desire to bewitch, or a tenant shoots the agent in order to affect the landlord; in all these the connection is wanting, but is not the only thing that is wanting. There remain still other cases in which an agent that actually does exist, exerts a real action upon the thing on which the effect is produced, and yet we are not justified in regarding it as a case of causation, for want of evidence, such as is required by the Methods described in the last Chapter, of connection between the action on the thing and the effect produced in that thing. In these cases, since so many more of the conditions of causation are satisfied, and the last link only is wanting, the error is less enormous, and may easily be committed by those who have sense enough to avoid the greater errors; while, on the other hand, those whose mental equipment is insufficient to save them from the greater blunders are scarcely likely to avoid the less.

The opportunities for committing the error now under con-

sideration are perhaps greater in medical practice than in any other range of observation. When a drug is administered to a person who is ill, and thereafter the symptoms change for better or for worse, it is difficult not to assume that the administration of the drug was the cause of the change, especially if the change is in the direction of improvement. In such a case all the gross errors are eliminated. The agent, the drug, does exist; it does exert action; its action is upon the thing, the body of the patient, that changes; and moreover the action definitely precedes the change. All these conditions are satisfied, but we are still in doubt, or ought to be in doubt, whether the action of the drug was the cause of the change in the symptoms; for connection between the action and the change is not established.

There is a widespread notion, dating from the battle of Waterloo, that the firing of heavy guns is a cause of rain. The firing of heavy guns does produce an action, and a powerful action of its kind, upon the thing, the atmosphere, in which a change occurs when it begins to rain; but no connection has been shown between the cause and the effect. At the battle of Waterloo, and no doubt many times before and since that battle, there was an association between the cause and the effect: but in the first place, the alleged cause did not precede the effect, for it had rained heavily for several days before; and in the second place it has never been shown, A, that the action was isolated—that it was the only action upon the atmosphere at that time—; nor, B, that the association is constant—that the firing of heavy guns is always followed by rain—; nor, C, that it is followed by rain more frequently than casual association would account for; nor, D, that there is any peculiarity in the rain that falls after the firing of heavy guns, that is constantly present in such rain, and absent from other rain. We may therefore confidently assert that the firing of heavy guns has not been proved to be a cause of rain.

The fallacy of arguing *post hoc, ergo propter hoc* is so frequent and so well recognised that further illustrations are not needed, but what is needful is to point out, what never has been pointed out, viz.: why it is a fallacy when it is fallacious. For it is not always fallacious. Quite the contrary. In every case in which a cause acts and produces a change, the effect follows the cause, and is both *post hoc* and *propter hoc;* and in those cases in which the effect immediately follows the cause we argue

https://doi.org/10.1192/bjp.62.257.241 Published online by Cambridge University Press

propter hoc because of the immediacy *post hoc*, and on no other ground. It is only when an interval of time elapses between the action and the effect that there is opportunity for fallacy to enter into the reasoning; and whenever the interval is short, the fallacy is extremely alluring and extremely frequent. Nevertheless, it has been recognised as a fallacy for two thousand years, and yet, in the face of this common knowledge, Hume and Mill, and all their followers down to the present hour, have taught that causation is nothing but sequence—invariable sequence it is true, but still, invariable sequence and no more.

What constitutes the argument *post hoc, ergo propter hoc* a fallacy when it is fallacious, is, of course, the absence of any proof of connection between the action that is *ante* and the effect that is *post*. This is the element that must be added to mere sequence in order to transform it into causal sequence; and this is the element that Hume perversely denied, and that Mill and all his followers have failed to appreciate, although in every case of causation that they witnessed throughout life it must have jumped at them and hit them in the face. When the sequence is instant and immediate, we argue connection from sequence alone: in other cases it must be proved by one of the methods set forth in the last Chapter, on the Methods of Ascertaining Causes, for each of these methods is a method of establishing connection between action and effect. Until a connection is established, that which is *post* can never safely be assumed to be *propter:* as soon as the connection is established, causation is proved. Of course, if causation were mere sequence, or invariable sequence, or unconditional sequence, whatever that may mean, the argument *post hoc, ergo propter hoc* would not be fallacious; but the very same writers who declare that causation is nothing but sequence insist in another chapter that to argue from *post hoc* to *propter hoc* is a notorious fallacy.

(6) The action may not precede the effect if it is a change, or be contemporaneous with it if it is an unchange.

Of all the errors in attributing causation this is the most difficult to avoid, and the most pardonable when it is incurred. In some cases it is so difficult to determine precedence that the only justifiable course is to suspend our judgement; but this course, always difficult, seems to be most difficult in attributing causation. In many cases the action, which is

the cause, arises so gradually that it is difficult to fix its position in time; and the change also that it effects may be spread over a considerable duration, so that the cause and the effect are for part of their duration contemporaneous, even when the effect is a change. When the effect is an unchange, contemporaneousness may be difficult to establish; and when the effect as well as the cause is an action, as it sometimes is in the case of an unchange, cause and effect are reciprocal, and which is to be called cause, and which is to be called effect, depend on the way in which they are contemplated.

Was his excessive drinking the cause of his insanity? Granted that the proper association is established, so that we may be sure there is a causal connection between the drinking and the insanity, then the answer to this question depends on which came first. If the drinking preceded the insanity by months or years, that settles the question; but supposing that he did drink heavily for a short time before the insanity was recognised, is it certain that the insanity was recognised as soon as it existed? One of the earliest symptoms of insanity is defect of self-control, and defect of self-control is a condition that favours excessive drinking. Insanity in the early stage is often difficult to detect, and to be sure of. Is it not possible then, that the excessive drinking was rather an early symptom than a cause of the insanity?

A certain game becomes popular, and about the same time a book upon it is published. It is said that the publication of the book is the cause of the game becoming popular, but may it not be the other way about? A book is not often published unless there is a public to which it appeals, and the existence of such a public is just the thing to stimulate an enterprising publisher. In such a case we must ask which came first, but this cannot be determined with certainty. The date of publication of the book can, indeed, be determined with accuracy, but how are we to determine when the game became popular? Attaining popularity is a gradual process, and may spread over months or years. In such a case we must suspend our judgement pending further information, and it may be that the matter cannot be determined.

Increase of population has been said to be a cause of taking inferior and hitherto uncultivated land into cultivation; and reversely, the taking of such land into cultivation has been said to be the cause of increase in the population. Which is

correct? It seems that the only way to determine is to discover which was first; but by the nature of the case this cannot be discovered, for both are slow and gradual processes, having no definite time of beginning.

Is the failure in the flow of the sap the cause of the death of the leaves in autumn? or is the death of the leaves the cause of the failure of the sap to rise? or are they not common effects of some other cause? In this case again, the causal connection is established; but again it is quite impossible to say whether the slackening of the sap-flow precedes the beginning of the death of the leaves first to die, or whether the gradual death of the leaves precedes the gradual failure of the sap-flow. But in this case we can call experiment to our aid. We can ring the tree, and so stop the flow of sap; and then we find that the leaves do in fact die, but they die in a very different manner, and the tree dies too. Or we may strip the tree of leaves and see if the sap ceases to flow; and when the experiment is tried, we find that the sap does not cease to flow, for the naked branches bud again. In this case, therefore, we may confidently assert that the death of the leaves and the failure of the sap-flow are common effects of some other cause.

Is the formation of the heavy rain-drops of a thunderstorm the cause or the effect of the electrical disturbance? If we could tell which change preceded the other we should have no doubt; but this we cannot tell.

Syphilis is said to have been introduced into Europe in April, 1494, by Pedro Boyle and Pedro de Margarit, the first a Benedictine monk and the second a Calabrian gentleman, both of whom accompanied Bartholomew Columbus, the brother of Christopher, in his voyage to and from New Hispaniola. On the other hand it is alleged that the disease had long existed in Europe, and even that Egyptian mummies have been found with the signs of the disease upon them. If the latter assertion is true, the former is a false attribution of causation.

It appears from the foregoing considerations that even when we employ our nine or twelve canons for ascertaining causation, we cannot always be successful; and when we can successfully establish a causal connection between two events, we cannot always determine which is cause and which is effect, or whether both may not be effects of some third action.

(7) The last error in attributing causation is that a condition may be mistaken for a cause.

This is a blunder that is very commonly made: it is perhaps the most frequent of all the blunders that are made in assigning causes; and fortunately it is the least important. If we discover that a certain percentage of potash in the soil is necessary to obtaining the maximum crop of potatoes, it does not greatly matter, from one point of view, whether we speak of the application to the soil of so much potash as the cause of a bumper crop, or of the presence in the soil of the potash as the cause; though of course the latter, as a passive state, is a condition, not a cause, of the crop being a bumper crop. On the other hand, to call a man's sex, or age, or the locality or climate in which he lives, a cause of his disease, is clearly a misnomer, and shows a confusion of mind: and it can never be as important, with reference to the causation of his disease, to discover his age or sex as to discover that which acted on him.

Though a condition is not a cause, and though the difference between condition and cause is often conspicuous and important, yet there are many cases in which the distinction is not important, and many in which it is quite as important to discover the conditions of an effect as to discover its causes. The external causes of the growth of plants are few, and are ascertained. They are the action of warmth and light upon the plant; but the conditions under which a given plant will thrive are often extremely difficult to ascertain. There are certain plants that seem to be animated by feminine caprice. Side by side in the same garden, in the same soil, in the same aspect, subject, as far as we can discover, to the same conditions in every ascertainable respect, one plant of *tropæolum speciosum* will thrive luxuriantly, and another will dwindle and perish. The sciences of agriculture and horticulture consist almost wholly in the study of conditions. Obviously, a passive state is by its very nature less conspicuous than an action, and therefore the discovery of a condition is almost necessarily more difficult than the discovery of a cause.

It is often as important to discover a condition as to discover a cause, and for this among other reasons, that the discovery of a condition often points to a cause, or enables us to eliminate an action or an agent that we have thought of as causally concerned. The researches of Wells into the cause of dew con-

sisted in identifying one after another the conditions under which dew is deposited, and those which interfere to prevent its deposition; and when these were ascertained they pointed straight to the causal action, namely refrigeration of the stratum of moist air in contact with the bedewed surface, the only action common to all the conditions. One of the conditions of the occurrence of a strong wind is a low pressure of air, as indicated by a low barometer; and this points straight to the cause—the action of the pressure of the air in a neighbouring region of higher pressure. A motor-car runs better after it has been running for some time, and again the condition points to the cause; for the only action that has taken place in the interval has been the action of the engine and moving parts on themselves and each other, and this action, whatever other effect it may have had, must have had the effect of warming up the engine and other moving parts; and it can be shown *a priori* that warming them up is likely to improve the running of the car.

On the other hand, the discovery of a condition may assist us in eliminating an action or an agent that we have thought of as possibly having a causal connection with the effect. A man is suspected of having committed a certain burglary, but it is found that one of the conditions of the burglary, the window through which the burglar is known to have entered, is incompatible with this man's action, for it is too small for him to get through. It is surmised that sourness in the subsoil is the agent that causes canker in fruit trees; but one of the conditions in which the tree grows is the presence of chalk in the subsoil, and chalk is incompatible with sourness. It is suspected that the ship was lost in obedience to the orders of the owners, that they might claim the insurance money; but it is discovered that the ship was under insured.

Again it is often important to discover a condition for its own sake. The cause may be well known, but the conditions under which it acts may be obscure, and in that case it is important to discover the governing conditions; and these are the cases in which it is usual to call the enabling condition the cause. No harm is done in practice by the confusion of nomenclature, but still, the confusion is there, and accurate thought expressed in accurate language would clear it up. It is, however, as prevalent in books on logic as among plumber's labourers. We speak of the absence of a damp-proof course in the walls

of a room as the cause of the room being damp. Strictly speaking the effect is not the room being damp, but the room becoming damp; and the cause of this is the action of the moisture from the soil, creeping, by capillary attraction and other forces, up the walls. One condition of this penetration of moisture into the wall is the absence of a damp-proof course; and so we speak of the absence of this course as a cause of the room being damp. It is not a cause. The absence of a thing cannot possibly be a cause. It is a condition. If there were a damp-proof course in the wall, the moisture could not penetrate that course, and could not rise above it; and the wall not becoming damp, the room would not become damp. Still, for practical purposes we call the absence of the course the cause, because we know now what prevention to apply, and where to apply it. What is the cause of the oven not getting hot? The cook will tell you it is the door or the window being open; but these are passive states, and therefore conditions, and not causes. The cause is the action of the draught of cold air; and this is not caused, but permitted, by the door or the window being open. If she said that the opening of the door or of the window was the cause of the oven being refractory, she would be punctually correct, for this, though not the proximate cause, was the cause of the proximate cause, and therefore a cause of the effect. A cause is an action, and an effect is a change or unchange. But when the result of an action upon a thing is to produce a change, the changed state or result may be a condition of further change in that or other things. In the instances just given, the confusion of cause with condition is not important for the purpose of the cook, or of the builder who is called in to remedy the dampness of the room, but it is important for the logician who is discussing the nature and relations of cause and effect, and the rules for discovering them. The importance of the distinction appears very plainly in the indescribable muddle that, for lack of it, appears in the books that discuss these subjects, *omnes libros canentes eandem cantilenam*, as Van Helmont says.

Summary.

In order to prove causation it is necessary to prove

(1) Action on the thing on which the effect is produced.

(2) Precedence of the action on the change, or accompaniment of the action with the unchange.

https://doi.org/10.1192/bjp.62.257.241 Published online by Cambridge University Press

(3) Connection between the action and the effect.

In thus endeavouring to prove causation, the following blunders are committed:—

(1) An agent may be taken for an action.

(2) The agent may be imaginary.

(3) The action may be imaginary.

(4) The action may be real, but not on the thing changed or unchanged.

(5) The action may be on the thing, but unconnected with the effect.

(6) The action may not precede the change or accompany the unchange.

(7) A condition may be taken for a cause.

The first blunder is frequent, but not often very important. The second is the worst of all, and is not made except by the most muddleheaded. The next is nearly as bad, and the rest decrease in importance in succession until the last is often practically unimportant, though it is one which a clear thinker would never make.

https://doi.org/10.1192/bjp.62.257.241 Published online by Cambridge University Press

CHAPTER VIII.

CAUSES OF DEATH. CAUSES OF INSANITY.

How great is the need of clear and correct concepts of cause and effect, and how great, too, the need of a knowledge of the proper methods of ascertaining and assigning them, is well shown by the official publications on the causes of death and of insanity. The Registrar General and the Board of Control annually publish elaborate Tables, from which it is evident that neither of these authorities has any clear notion of what is meant by a cause, or of the means that should be adopted to verify causation. Both authorities publish as causes what are not causes, and both authorities have altered from time to time the construction of their Tables without improving materially their illogical character. The Board of Control, the successor of the Lunacy Commission, has followed its dignified predecessor in frankly abandoning the attempt to distinguish causes of insanity, not only from its conditions, but even from its accompaniments. This seems to me a deplorable admission of incompetence. The old Table, that did at least purport and pretend to be a Table of Causes, is now superseded and replaced by a Table of Ætiological Factors and Associated Conditions. 'Ætiological Factors' would not be a bad term if it were intended to embrace causes, direct and indirect, immediate and remote, as well as conditions. It is a sound, logical, comprehensive term, which might properly be employed to include all these things; but I am sure that I do no injustice to the Committee of the Medico-Psychological Association which drew up the Table and gave to it its title, when I say they had no such meaning and no such intention. There is not the least doubt that their reason for giving to the Table this new title was to seek in vagueness a refuge from uncertainty. They did not know what constitutes a cause, nor did they know the rules or methods by which causes should be assigned; and small blame to them, for philosophers could not tell them, and if they knew, which is improbable, of the various definitions of

https://doi.org/10.1192/bjp.62.257.241 Published online by Cambridge University Press

cause given in the books, they had the good sense to disregard them. They collected a hotch-potch, whose constituents they were unable to discriminate from one another, and they selected a title that is a dignified name for a hotch-potch. If it is objected, as it well may be, that many of the items in the Table are not Causes nor Ætiological Factors, they can reply that at any rate they are Associated Conditions, and thus silence that criticism. It is true that they lay themselves open to the much more damaging criticism that such a hotch-potch is of no conceivable use to any human being; but this, perhaps, they did not foresee.

I. Causes of Death.

The Registrar General divides causes of death into Primary causes and Secondary causes; and it is significant of the validity of the distinction that at different times he has defined them in different ways. Originally, in 1845, the instruction of the Registrar General was: 'Write the causes of death in the order of their appearance, and not in the presumed order of their importance.' As he did not mean primary and secondary, that is to say, first and second in order of importance, it is a pity that he used these terms; and as he meant first and second in the order of time, it is a pity that he did not use terms, like first and second, or earlier and later, which would have expressed accurately what he did mean. However, some of the medical practitioners to whom the forms were issued persisted in assuming that the Registrar General meant what he said, and accordingly returned as primary cause of death that which they considered more important, and as secondary that which they considered less important. As this practice grew and increased, the statistics naturally lost in value, and became much confused, so that it might have been supposed that the Registrar General, who recognised and deplored the confusion, would have revised either his formula or his instructions. In fact he did neither. He allowed the terms to remain, and withdrew his instructions altogether, leaving the certifiers to interpret his terms as they pleased. This happened in 1902, and for the next nine years medical men who certified causes of death were left to their own discretion, to interpret primary and secondary as they pleased. The result, which is creditable to the doctors, was that most of them interpreted the terms

in their proper sense, as first and second in the order of importance.

In 1893, a Select Committee reported on the subject, and advised that if the terms primary and secondary were retained, they should be defined 'as meaning the order of the development of the diseases as they occurred,' that is to say that the Registrar General should revert to the former vicious practice of defining the terms in a sense that is false, and that they cannot properly bear. The Registrar General did not take this advice. As I have said, he withdrew the instructions, and left the doctors to do as they pleased; and then, after a decent interval of nine years, he directed that the primary cause of death was to be considered 'that cause of death which was of greatest importance and upon which any other related causes were dependent.'

It is unfortunate that the Registrar General, following the example of Mill, whose teaching has so long been dominant in the matter of causation, is not able to make up his mind about the meaning of his terms, and gives several definitions, which are not only unsatisfactory, but are inconsistent with each other, and even with themselves. In his Suggestions to Medical Practitioners, he defines primary cause of death (in the case of deaths from disease) as 'the disease, present at the time of death, which initiated the train of events leading thereto, and *not* a mere secondary, contributory, or immediate cause, or a terminal condition or mode of death.' In a footnote he adds: 'Acute specific diseases, if of recent occurrence, are to be considered the primary cause of death, even though the actual disease, as tested by the power of infection, be no longer present at the time of death.' Thus he warns us that his cardinal test of what is primary may be no test at all. He takes back with one hand what he has just given with the other, and leaves us in confusion. If we turn to the remainder of the definition for guidance we are no better off, for it does not help us much to understand what is meant by a primary cause of death to be told that it is *not* a mere secondary cause. But even in this he is not consistent, for though this contradictory footnote appears in his Suggestions, it is not embodied in the instructions to medical practitioners that appear on the face of the certificate of death. 'Secondary cause' he does not attempt to define, though he warns us that a terminal condition or mode of death should not be entered as a secondary (or con-

tributory) cause; but as he does not tell us what he means by a terminal condition or mode of death, this does not give us much assistance; and if he did, it would only tell us what a secondary cause is not: it would not tell us what it is.

In most ordinary cases of causation a cause is a cause; that is to say, it is one of a train of causes, and if it is omitted, if the train is broken at any point, the effect will not be produced. If the cat does not begin to eat the rat, or if the rat does not begin to gnaw the rope, or if the rope does not begin to hang the butcher, the rest of the effects will not be produced, and the old woman will never get home. The case of death, however, is peculiar. The death of every human being is inevitable, and the utmost that any cause of death can do is to hasten or precipitate a result that must take place some day. For practical purposes, however, we look upon the duration of life as indefinite, and call that a cause of death which is the cause that death, which otherwise would have been postponed, occurs at a particular time. In other words, the cause of death is that which hastens or precipitates an event that would in any case have occurred sooner or later. Now it is evident that the extent or degree to which life is shortened by any cause materially affects our estimation of the cause. If a man is already so ill that his life is despaired of, and he may die any hour, we scarcely regard as a serious or important 'cause of death' the dose of morphia that not only relieves his pain, but overpowers his enfeebled respiratory centre, and accelerates his death by a few hours at most. We should not in such a case enter poisoning by morphia as a cause of death. On the other hand, if a young man in robust health, whose expectation of life is thirty or forty years, were to die with symptoms of narcosis after a large dose of morphia, we should unhesitatingly enter, as the cause of his death, poisoning by morphia.

We may look upon the living animal as a clock, wound up at conception to go for a certain maximum time. When death occurs, the clock stops; but, apart from disease and accident, the clock will not stop until it runs down—until the spring has unwound itself and its resilience is exhausted. When this happens, the clock must stop. For the first years of life the spring has double work to do. It has not only to keep the clock going, but also to build it up in bulk and complexity. When this task slackens and ceases, the whole energy of the spring is

devoted to keeping life going, and therefore early adulthood is the time of greatest vigour, and the time when the attacks of disease are most easily repelled. As the tension of the unwinding spring diminishes, less and less serious interference suffices to stop the clock. When it is fully wound, the power of the spring will drive the clock even though the pivots are lubricated with cart-grease; when it is nearly run down, a slight thickening of the oil on a frosty night will arrest the action. So it is with human life. In early adulthood, the motive power is abundant, and it takes much interference to stop the clock of life; but as age advances, the power of living weakens and fails, until at length in extreme old age, which is to be measured not by years only, but rather by the amount remaining of the initial store of energy, a very trifling obstruction, an obstruction so trifling that we cannot identify it, is enough to be a 'cause of death.' It may be in some cases, such as that of the first Duke of Wellington, that the clock merely runs down, and there is no more to be said. The Registrar General deprecates the return of old age as a cause of death, but in such a case as that of the Iron Duke it is difficult to see what more accurate return could be made.

Properly considered, life is what I have called an unchange. It is the maintenance of a continuous state in spite of opposing forces which tend to terminate it. A cause of death is an action that removes one or more of the conditions maintaining the unchange, and allows it to be brought to an end. Life is maintained with effort and with striving, and subject to certain conditions. Any interference with any of these conditions increases the difficulty of maintaining life; interference with a second condition, or further interference with the same condition, further increases the difficulty; and the concurrence of two or more interferences may increase the difficulty to the point of impossibility. In this way there may be several causes simultaneously tending to bring life to an end, and it may be very difficult in a given case to say how much of the effect is due to one cause, and how much to another. The effect is death, and it is incongruous to speak of part of death being produced by one cause and part by another; nor is it much less incongruous to speak of death as being partly due to one cause and partly to another. In such a case it is the combination of causes that produced death, and if this is so, and if

neither of the causes acting singly would have produced it as and when it happened, can we rightly say that one was a more important cause than the other? Which is the more important cause of the discharge of a gun—the loading of it, or the pulling of the trigger? This case is scarcely on all fours, however, with the case of death. If one cause would have produced death sooner or later, and the cooperation of a second caused the death to take place sooner, then I think the former may be considered the more important, the less the anticipation produced by the latter.

The cause of death is always a function of two variables—the power acting to maintain the unchange that we call life, and the action or actions that increase the work that the power has to do. To recur to the simile of the clock, the time of death depends on the amount of resilience left in the spring and the amount of friction in the works that must be overcome. If this friction is materially increased at more than one place in the train, then each increase is a separate cause of the stopping of the clock. The less the power or means of living, the less interference with the processes of life necessary to bring life to an end; the greater the life-worthiness, the more powerful must be the interference necessary to cause death.

Again, the living body may be likened, and the likeness is more than a mere simile, to a besieged fortress. It is constantly subject to the assaults of microscopic enemies, who are trying to obtain a footing, but are repelled as long as the garrison is strong enough. If the fortress is attacked by a single foe strong enough to break down its defences and capture it, then the action of that foe singly is the cause of the fall of the fortress. But it may be that while engaged in repelling one invader, which is not strong enough alone to capture it, the fortress is attacked by another, and the combined assault succeeds. In such a case the cause of the capture is the combination of assaults. Or it may be that the garrison is completely successful in repelling one assailant, but at such a cost that it falls a prey to a second, of perhaps inferior power, which succeeds in consequence of the exhaustion of the defenders. In such a case the second attack was the cause of the capitulation, but the exhaustion left by the first was a necessary condition.

If we use the term Cause, with a capital, to include both

cause and condition, and cause, with lower case, to mean a true cause or action as distinguished from a condition, then I think the Causes that may combine to produce the death of any individual man may be combined in any of the four following ways :—

Case I. The first Cause is a cause of the second ; or, otherwise put, death is due to some particular manifestation of a disease, which, without that manifestation, might or might not have been fatal. A man suffers from typhoid fever, from which he might recover, but that the fever causes a perforation of the bowel, which kills him. He might recover from his rheumatism, but for endocarditis which is a manifestation of the rheumatism. He might recover from his endocarditis, but for an embolism which is caused by the endocarditis. He suffers from phthisis, which might endure for years but for an hæmoptysis, which is rapidly fatal. He suffers from diabetes, and the diabetes causes coma, which ends in death. He suffers from general paralysis, and dies in *status epilepticus*, which is a manifestation of the general paralysis.

The fatal manifestation of a disease is, I surmise, what the Registrar General means by a terminal condition or mode of death ; but as he gives no indication whatever as to what he does mean, this can be no more than a surmise. In such cases the disease may appropriately be called the Principal cause of death, and the manifestation the Precipitating or Subordinate cause of death.

Case II. The first Cause is not the cause, but is a necessary condition of the second. It is necessary in the sense that without it the cause could not have come into operation. A person suffers from a compound fracture, which becomes complicated with pyæmia, of which he dies. The fracture is not the cause of the pyæmia. The cause of this is infection with the appropriate coccus ; nevertheless, this infection would never have taken place but for the existence of the compound fracture, regarded as a continuing passive state—as a condition. It is not the occurrence or action of the fracture that produces the infection, and therefore the fracture is not the cause of the infection ; but without the existence of the fracture the infection could not have occurred. The fracture is a necessary condition of the pyæmia which is the cause of death. Or a man suffers a chill, which so diminishes his powers of resist-

ance that the pneumococci, that before were harmlessly present in his body, are now able to make an effectual attack, to invade his lungs, and to cause pneumonia, of which he dies. Regarded as an action on the body, and it is quite legitimate so to regard it, the chill is a cause of death; but it is not the cause of the pneumonia. The cause of the pneumonia is the invasion of the pneumococcus, and of this invasion the chill was a necessary condition. But when we regard the chill as a condition, we do not regard it as an action; we regard it as a passive state; and as a passive state it is a necessary condition of the attack of pneumonia, for without the existence of the chilled state of the body the infection of the pneumococcus would not have taken place. The chill by itself was not the cause of death. Death would not have occurred from the chill without the aid of the coccus. The pneumonia was the cause of death, but without the chill there would have been no pneumonia.

In such cases we may call the necessary condition the Preparatory cause of death, and the subsequent cause the Consummating cause of death.

Case III. The first Cause is a favouring, but not a necessary condition of the second. Persons who are already suffering from measles or typhoid fever are more obnoxious to the attack of broncho-pneumonia than those not so suffering; and broncho-pneumonia is more likely to be fatal to those who are already suffering from measles or typhoid fever than to those who are not. Yet measles and typhoid are neither of them a necessary condition of the pneumonia. They are not necessary either in the sense that pneumonia necessarily follows them, or in the sense that one of them must necessarily precede pneumonia in general. Nevertheless, it may be that in any particular case the precession is necessary, and that without it the pneumonia would not have occurred, or would not have been fatal. Still, since it is impossible to say that broncho-pneumonia cannot occur unless it is preceded by measles or typhoid, we cannot say that the specific fever is a necessary condition of the broncho-pneumonia. Persons suffering from diabetes are specially liable to be attacked by phthisis. Diabetes is no necessary condition of phthisis, either in the sense that diabetes is necessarily followed by phthisis, or that phthisis is necessarily preceded by diabetes; but the frequency with which diabetics are attacked by phthisis

https://doi.org/10.1192/bjp.62.257.241 Published online by Cambridge University Press

indicates that the existence of diabetes favours the occurrence of phthisis.

In this case again the condition may be called a Preparatory cause, and the subsequent disease the Consummating cause of death.

Case IV. The last case is that in which two causes, neither of which is in any way dependent on the other, combine to bring about a death that neither of them singly might have been able to produce. A man is suffering from heart disease, which does not menace his life as long as the heart is not subjected to extraordinary strain. He is attacked by bronchitis, which would not be fatal if his heart were sound; but the effect of the bronchitis is to put a strain upon the heart that, in its damaged condition, the heart is unable to overcome; and the combination of diseases is fatal. Or he suffers from ague, which by itself might leave him years of life, but that he is attacked by dysentery, which alone would not be fatal, and the combination of the two diseases carries him off.

In such cases one of the two diseases may be found to play a preponderant part in bringing about the fatal issue. In the first of the two instances given above, the heart disease may be regarded as preponderating, and in the second the dysentery. Thus viewed the causes may be called Preponderant and Adjuvant; but it is not easy in any case, and in many cases it is not possible, to assign to either of the diseases a preponderant part; and if it is not practicable, then we can only fall back upon the order in time, and speak of the causes as Earlier and Later.

There are here three pairs of terms that may be used to characterise, in appropriate cases, the several causes of death. They may be characterised as

Principal and Precipitating or Subordinate,
Preparatory and Consummating,
Preponderant and Adjuvant, or Earlier and Later.

If I am right in my surmise that what I have called a Precipitating or Subordinate cause of death is what the Registrar General means by a terminal condition or mode of death, then, as he advises the certifier not to insert the terminal condition or mode of death into the certificate, this cause is ruled out, and in cases in which the causes of death can be distinguished as principal and subordinate, the principal cause

only should appear in the certificate. I should have thought that it would be of value to know the number and proportion of cases in which the precipitating cause of death in typhoid fever, for instance, is perforation, those in which it is hæmorrhage, those in which it is hyperpyrexia, those in which it is exhaustion, and so forth; but no doubt the Registrar General knows best.

Excluding the pair just dealt with, in the very great majority of deaths in which more than one cause can be assigned, the causes are related in the way I have explained as Preparatory and Consummating, or as condition and cause. Most people, I think, would understand the term 'condition' in the sense in which it is here used, as a pre-existing state, either necessary or helpful to the occurrence of the fatal disease; and the term Preparatory cause would, I think, be allowed to be a substitute for condition, accurate enough for ordinary use. The term Consummating cause would perhaps scarcely be as readily accepted, but once accepted and become familiar, it would not give rise to difficulty. I do not think there is any other term that expresses the nature of the cause, and its relation to the preparatory cause or condition, with the same accuracy. Immediate cause is ambiguous, and might easily be misleading. It would be very apt to be confused with what I have called the Subordinate or Precipitating cause of death. The terms Primary and Secondary have been found in the experience of many years to be misleading and confusing, and I think they would be better abandoned; but if they are to be retained, then I think it should be explained that in these classes of cases, Primary means Preparatory, and Secondary means Consummating, in the senses here explained.

When the causes of death are two independent diseases, the difficulty is greatest. If it were possible always, or even frequently, to decide which of them took the greater share in bringing about the death, it would undoubtedly be better to distinguish them as Preponderant and Adjuvant; but this is unfortunately not often possible. The alternative is to distinguish them by the order in time of their occurrence, as Earlier and Later; but this distinction is ruled out by the instructions of the Registrar General that are now in force. In a considerable proportion of cases in which two diseases that appear to be independent co-operate to bring about death, we may sus-

pect that the earlier in time does in fact facilitate the attack of the later, and therefore many cases that appear *primâ facie* to belong to Class IV may be removed into Class III without doing violence to the facts; but when the case unmistakeably belongs to Class IV, and it is not possible to apportion the degrees of importance among the causes, I do not see how the terms Primary and Secondary can be made applicable except by taking them to mean first and second, which would not only be contrary to the instructions of the Registrar General, but would introduce inexcusable ambiguity and confusion into the meaning of the terms. It seems that there is no single sense in which the terms Primary and Secondary can be used that will cover all the cases of the relation between two causes of death when more than one cause has been in operation; nor is there any other pair of terms that can be used for the same purpose, for the relation is not the same in all cases.

In the tabulation of causes of death, one cause only is entered, and the Registrar selects for this purpose that cause which is 'most important' out of the two or more that are submitted to him by the certifier. Now, it seems from the language used by the Registrar General, and from the whole trend of his remarks, that he looks upon the 'importance' of a cause of death as in the first place an ascertainable quality, and in the second place a fixed quality, a quality that is present or absent, and if present at all, present in some fixed degree which does not vary. This, however, is not so. The importance of anything varies with the point of view from which we regard it. Regarded from the point of view of the hostess of a garden party, or of the farmer whose hay is cut but not carted, the state of the weather is of great importance; regarded from the point of view of the cook, who spends her life in the basement, or the prisoner, who spends his life under cover, the state of the weather is of no importance at all. The cause of death which is important to the doctor who has an hypothesis to test may be of no importance at all to the police; and the cause of death which is important to the police may not have any importance at all to the company in which the life of the deceased was insured. Before we can say that a cause of death is important or unimportant, or estimate the degree of its importance, we must settle the point of view from which the importance is to be regarded. It is more im-

https://doi.org/10.1192/bjp.62.257.241 Published online by Cambridge University Press

portant, says the Registrar General, that this death, which was caused by the combination of measles and bronchitis, should be registered as death from measles than as death from bronchitis; but why? From the point of view of the doctor who has views about bronchitis it may be very much more important that bronchitis was a cause of death than that measles was a cause of death. If the Registrar General considers that measles is a more important cause than bronchitis, it can only be because for some purpose it seems more important to ascertain the number of deaths in which measles had a share than to ascertain the number in which bronchitis had a share. It is impossible, therefore, to estimate the relative importance of the different causes of death in any given case for the purpose of registration, until we know what this purpose is; and as to the purpose of compiling tables of the causes of death, the Registrar General does not enlighten us. I do not know for certain what this purpose is. I do not even know whether the Registrar General has any one purpose distinctly and prominently before his mind, and I strongly suspect that he has more than one purpose, but does not distinctly formulate to himself what his purposes are. It is clear, I think, that it is impossible to estimate with any approach to accuracy the relative importance of different causes of death until we know for what purpose the information is required, and in what respect importance is to be estimated; and if more than one purpose is to be served by the estimation, it must often happen that more than one estimate of the relative importance must be made. It is clear that no single set of Tables could be compiled from both points of view; and if more than one purpose is to be served by compiling these Tables, the purposes should be clearly before the mind of the compiler, and each purpose should have a separate set of Tables to itself. The suggestion may be a counsel of perfection, and very likely the Registrar General would say that it is impracticable; and with the funds and the staff at his disposal it may be so; but what I have said is true for all that. Relative importance cannot be gauged until purpose is settled; and causes, rightly selected for their importance for one purpose, will be wrongly selected if used for another purpose; and whatever the purpose of the Registrar General in selecting this or that cause of death, he should have it clearly before his mind, and he should stick to it.

https://doi.org/10.1192/bjp.62.257.241 Published online by Cambridge University Press

II. Causes of Insanity.

Among the Tables of Statistics issued by the Board of Control is a Table of the Ætiological Factors and Associated Conditions of Insanity. The former Table, now superseded, spoke frankly of Causes of insanity, but this term is now replaced by Ætiological Factors, which is more vague and more cautious. The table is as follows:

Heredity.
- Insane
- Epileptic
- Neurotic
- Eccentricity
- Alcoholism

Mental Instability, as revealed by
- Moral Deficiency
- Congenital Mental Deficiency not amounting to insanity
- Eccentricity

Deprivation of Special Sense
- Smell and Taste
- Hearing
- Sight

Critical Periods
- Puberty and Adolescence
- Climacteric
- Senility

Child-bearing
- Pregnancy
- Puerperal State (non septic)
- Lactation

Mental Stress
- Sudden
- Prolonged

Physiological Defects and Errors
- Malnutrition in early life
- Privation and Starvation
- Over-exertion, physical
- Masturbation
- Sexual Excess

Toxic
- Alcohol
- Drug habit
- Lead and other such poisons
- Tuberculosis
- Influenza
- Puerperal sepsis
- Other Specific Fevers
- Syphilis, acquired
- Syphilis, congenital
- Other toxins

Traumatic
- Injuries
- Operations
- Sunstroke

Diseases of the Nervous System
- Lesions of Brain
- „ „ Spinal Cord
- Epilepsy
- Other Definite Neuroses (limited to Hysteria, Neurasthenia, Spasmodic Asthma, Chorea).
- Other Neuroses which occurred in infancy (limited to convulsions and night terrors).

Other Bodily Affections
- Hæmopoietic System
- Cardio-vascular Degeneration
- Valvular Heart Disease
- Respiratory System and Tuberculosis
- Gastro-intestinal System
- Renal and Vesical System
- Generative System, excluding Syphilis
- Other general affections not above included

The Committee that drew up this Table was cautious, but it was not clear. The Table previously in force was headed and called a Table of the Causes of Insanity; and a queer hotch-potch it was, in which overwork appeared in one place, and over-exertion in another, and a previous attack was entered as a cause of the existing attack of insanity. I had pointed out that several of the 'causes' enumerated in that Table were not causes, and could not be causes of anything, and it may have been my protest which induced the Committee to substitute for the term Causes the term Ætiological Factors. The old legal maxim says that fraud lurks in generalities, and to the uncritical it often seems that safety lies in generalities. Certain it is that refuge in generalities is a great saving of thought, and appears a great safeguard against criticism. Any criticism of any item in the Table, based on the ground that it is not a cause, may be met by the defence that it is an Ætiological Factor, or at any rate an Associated Condition; and of course it would be difficult to show, if it existed at all, that it was not one or the other. The manœuvre, adroit as it is, has the defect, frequent in such manœuvres, of being too clever by half. It is true that it eludes criticism of the items in the Table, but at the cost of transferring the criticism to the Table as a whole. What is the use of a Table which includes both Ætiological Factors and Associated Conditions, and, it may be added, other things as well, and does not distinguish the one class from the other?

Some of the items in the Table are neither Ætiological Factors nor Associated Conditions. Mental Instability, for instance, may be sufficiently great to amount to insanity, but then it *is* the insanity, at least it is so in the eyes of the compilers of the Table, to whom insanity means disorder of mind. Mental Instability can no more be an Ætiological Factor of insanity, or an Associated Condition of insanity, than a movement of the air can be an Ætiological Factor of wind, or an Associated Condition of wind; or than sunshine can be an Ætiological Factor of light, or an Associated Condition of light. The movement of the air is the same thing as wind: the sunshine is the same thing as light: and the Mental Instability is, at any rate in the eyes of the compilers, the same thing as insanity. It is in truth a part of insanity.

Again, there are many items in the Table that are not of the

https://doi.org/10.1192/bjp.62.257.241 Published online by Cambridge University Press

slightest value there, and that make one wonder what on earth they were included for. I conjecture that the Committee was nervous lest anything should be omitted, and therefore put in everything its members could think of. Defect of smell and taste are, no doubt, conditions that may be associated with insanity, and so are baldness and tight boots, a Roman nose and a fondness for pickles; and it is about as useful to know that any one of these is associated with insanity as any of the others. The last two clauses include, or may include, every disease to which humanity is subject, and I can conceive that it may in certain connections be useful to know whether any particular disease is particularly frequent or infrequent among mad people; but I cannot conceive that a disease that may affect a person years before or years after he becomes mad, can have any rightful place in a Table of Ætiological Factors of insanity. To mix up with Ætiological Factors of insanity conditions that are manifestly only accidentally associated with it seems to me to go out of the way and undertake a laborious task in order to introduce confusion, and destroy what usefulness the Table might otherwise have had.

The influence of Heredity in the causation of Insanity seems to me misconceived, or rather perhaps unconceived, in spite of the explanation that I gave a quarter of a century ago, an explanation which has never been even examined or criticised by any subsequent writer, although it carries a fundamental revolution in the concept of the causation of insanity. Insanity is the breakdown of the human machinery; and when a machine becomes unable to do its work, the reasons cannot be anything but the original construction of the machine and the strains or stresses that it has had to bear. The strains or stresses that it has to bear are actions upon the thing, the human machine or organism, in which the change or effect of insanity is produced; and are therefore rightly called causes; but the constitution of the machine, the way in which it is put together, the stability of its construction, is not an action. It is a passive state; and at the utmost cannot be more than a condition. Indeed it almost requires a stretch of language to call it a condition. The man is the thing on which the action takes place and on which the effect is produced; and the man is the result of his heredity, that is to say of the mixture of the qualities of his ancestors. This mixture is, therefore, at the

utmost the cause of a condition, which means an indirect cause. By the study of the patient's heredity, that is to say of the qualities of his parents and ancestors, we can make a very rough guess at the nature of the thing, the man, upon which a cause acts so as to produce the result insanity, and that is the utmost that a study of heredity can give us.

The causes of insanity, properly so called, are the actions brought to bear upon the man which produce in him the change from sanity to insanity, and the result of insanity. For the purpose of the argument, the man is summarised in his brain; and actions that produce insanity are actions on the brain, which may most conveniently be divided into the direct actions of physical agents, the indirect actions of physiological processes, such as child-bearing, and the still more indirect action of emotion-producing situations of the man in the world around him. This is the natural grouping and classification of the strains or stresses that produce insanity; but for some reasons known only to themselves, writers on insanity refuse to adopt it. I do not know what their reasons are, but I surmise that one reason is that the classification is a clear, useful, and scientific classification, and the other is that it is proposed by me, who am not a German. Had it been proposed by a German, it would have been adopted with acclamation long ago, but no German would be capable of discovering a classification so clear and logical.

However, taking the list—it cannot be called a classification—proposed by the compilers of this table, it will be interesting to inquire into the grounds for the supposition that the alleged causes, or ætiological factors, are in fact causes.

Heredity has already been examined. The next group, Mental Instability, includes no cause of insanity, and nothing that by the utmost stretch of the meaning of words can be called a cause of insanity, or of anything else; for nothing in the group is an action. The same may be said of the third group. Deprivation, by which is evidently meant not deprivation, but absence, of a special sense, is not an action: it is a passive state; and I know of no evidence that, as a passive state, the absence of a special sense is material to the result of insanity; and if not, then it cannot be even a condition.

The next group is composed of critical periods of life; and these come in the class of physiological strains or stresses that

may be causes of insanity, because they undoubtedly are, or may produce, actions on the brain. But what evidence is there that these do in fact exert such action on the brain as may disorder its mode of working, and so produce the change from sanity to insanity? Many people, the great majority of people, who go through these physiological crises do not become insane. Many people become insane at other times than at the times of these crises. On what ground, then, are they regarded as causes of insanity, and what is the justification for so regarding them? These are questions which no writer on insanity has ever answered, or ever asked, or ever considered; but they are questions that demand an answer, for until they are satisfactorily answered, the writers have no business to assume that these crises are causes at all; and the same may be said of all the other alleged causes of insanity. Does the belief that these alleged causes are causes of insanity rest upon the application of Mill's Canons, or of any of them? It certainly does not. No one has ever yet discovered, or ever will discover, two or more cases of insanity that have nothing in common but the circumstance that the patient was going through one of these crises. No one has ever discovered, or ever will discover, an instance in which insanity occurs, and an instance in which it does not occur, which have every circumstance in common except adolescence or senility. No one has ever discovered, or ever will discover, two or more instances of insanity having only adolescence and senility in common, and two or more instances of sanity that have nothing in common but the absence of adolescence or senility; and no one, as far as I know, has ever wasted time in an unprofitable search after such impossible instances. Yet there is a general consensus that these and other physiological crises are causes, or at least occasions, of insanity, a consensus not merely of opinion, but of deep-rooted conviction. What is the justification for the belief? It is to be found in those methods of assigning causes that I have grouped together under the heading of Association. It is found in experience that these physiological crises are associated as antecedents with insanity, not in isolation, not constantly, but either more frequently than casual concurrence will account for, or, when associated, the insanity has some peculiar feature which does not occur in other cases of insanity, not so associated. In other words, the causal connection is

ascertained by the Method IV. C., or IV. D.; and the same methods are employed in almost every case in which causes are assigned for the occurrence of insanity; but not in every case.

In the insanity that is due to drunkenness, and in that which follows immediately or rapidly upon the absorption of other drugs, the first Method, that of Instant Sequence, which in these cases becomes Rapid Sequence, is employed, together with Method IV. A, Association in Isolation. The effect follows rapidly after the action, and so raises a presumption that it is due to the action. The action is isolated: it takes place in circumstances which enable us to say with considerable confidence that no other material action has occurred; and this confirms the presumption. Further, in many cases the association is, in the same person, constant; whenever he takes the alcohol or other drug, the insanity of intoxication constantly follows: when he does not take it, the insanity does not occur. But suppose the association is not constant, or that no opportunity of observing constancy has occurred? Suppose that an excess of alcohol has been taken only once, and that insanity has occurred only once, and then following the drink? Then the Method of Common Rarity is applicable, and is applied. In other cases it is found that a little drink is followed by but slight indications of insanity, and that the more drink is taken the more complete and profound the insanity becomes. In such cases the Method of Concurrent and Proportional Variation confirms our conviction. Commonly, too, the insanity that follows drinking has peculiar qualities that are present in other cases of such insanity, and are not present when insanity is not preceded by drinking; and the Method of Association D becomes applicable. In short, whenever causation is rightly assigned, it is assigned by the application of one or more of the twelve Methods here described; and never by any of the Methods prescribed in Mill's Canons.

SUMMARY.

Causes of Death.

The instructions of the Registrar General require us to distinguish primary from secondary causes of death, but give

us no clear guidance what is to be considered primary and what secondary.

Death is inevitable, and its causes are inherent in human nature. That which we call the cause of death in any individual case is the cause of death happening at the particular time and in the particular way it does. Life is an unchange, and death the cessation of the unchange.

When more than one cause co-operate to produce death, the causes may be combined in one of four ways.

I. The first cause may be a cause of the second.

II. The first cause may be a necessary condition of the second.

III. The first cause may be a favouring condition of the second.

IV. The several causes may be independent.

In the first case the causes may be called Principal and Subordinate, or Principal and Precipitating; in the second case, Preparatory and Consummating; in the third, Preponderant and Adjuvant; and in the fourth, Earlier and Later.

The first three pairs may all be included under Primary and Secondary. The last pair cannot be so included.

The relative importance of different causes of the same death must depend on the purpose the observer has in view.

Causes of Insanity.

The Table issued by the Board of Control rightly does not pretend to be a Table of Causes exclusively; but to mix up causes, conditions, and associated states in the same Table deprives the Table of any value whatever for any purpose; and some of the headings in the Table are neither causes, conditions, nor associates of insanity.

By following the rules laid down in Chapter VI., it might be possible to identify many causes of insanity, and to avoid the useless confusion of the Table.

CHAPTER IX.

ON BELIEF.

EVERY philosophical discussion, and most of other discussions, are discussions about the meaning of words, either of single words, or of phrases, or of propositions; and most philosophical discussions, and many others, are barren and inconclusive because the different disputants, and often the same disputant, attach different meanings to the same word, phrase, or proposition, and often attach to it no clear meaning at all. In order to use a word, or a phrase, or a proposition, correctly and with propriety, it is by no means necessary that the user should be able to formulate in other words what his meaning is. The ability to feel and appreciate nice shades of meaning, and to express them in appropriate words, long precedes the ability either to define the distinctions or to formulate the meaning. The difference between 'I shall' and 'I will' is felt by every Englishman, though by no Irishman or Scot; but not one in ten thousand of those who use these expressions correctly, and never confuse them, could formulate in words the difference of meaning. It is the same with the great majority of words and expressions in common use. We feel their meanings: we always use them correctly; but if we are asked to define them in other words, not one of us in ten thousand could do so satisfactorily.

In common use, and on common occasions, the want of formal definitions of the words we employ does not matter much, for we understand each other, and ourselves, sufficiently well for common purposes; but discussions, and especially discussions upon matters that have puzzled mankind for ages, are quite futile unless we fix beforehand, as accurately as we can, the meaning of the words and phrases upon which the discussion hinges. In common use, the words Belief and Believe have many different meanings. As used in the Catechism—'All this I steadfastly believe'—and in the

https://doi.org/10.1192/bjp.62.257.241 Published online by Cambridge University Press

Creeds of the Christian Church, the phrase 'I believe' means 'I am convinced,' 'I accept that statement as an assertion of fact.' In current use, as when we say 'I believe he is gone out,' it means uncertainty. It means not 'I am convinced he is gone out,' or 'It is a fact that he is gone out,' but 'I think he is gone out, but I am not sure.' Again, Belief may mean, not only at one time, as in the first example, assured conviction, and at another time, as in the second example, doubt inclining to affirmation, but it may be used, as I have used it at the head of this chapter, as a generic term, to mean at one and the same time every degree and shade of belief, from axiomatic certainty, through approximate certainty, and every degree of increasing doubt, to utter disbelief and inconceivability. In this sense the name Belief has many meanings, all, however, referring to states of mind or attitudes of mind. Attitudes of mind towards what? Towards fact, most people would say, and the answer would be approximately true, but fact is not the only thing to which we attune our beliefs, and if it were, and as far as it is, we must know precisely what we mean by fact.

Belief, Truth, Doubt, Certainty, Opinion, Possibility, Credibility, Probability, and many more, are all words germane to this discussion, and if we scrutinise them with care, we shall see that they fall naturally into three classes. Some of them we can predicate of ourselves, but not of impersonal things. We can say I doubt, I believe, I think, I am of opinion; but we cannot say It doubts, it believes, it thinks, or it is of opinion. Others we can predicate of impersonal things, but not of ourselves. We can say It is true, it is probable, it is credible, or possible, or likely; but we cannot say I am true, I am probable, I am credible, or possible, or likely. A third set of words, which are but few, we use indifferently either way. We can say I am certain, and It is certain; I am doubtful, and It is doubtful. In these cases, however, we are conscious of a certain impropriety in one of the uses. 'I am doubtful' means no more and no less than 'I doubt,' and the latter, as the shorter and more direct expression, is the one that ought to be preferred. 'I am certain' means no more and no less than 'I know'; and might be discarded in favour of I know. Discarding the words of this mixed and intermediate class, there remain those which we predicate of ourselves, and which indicate states of our minds, and those which we predicate not of ourselves, but of

impersonal things. The question arises To what kind of things do words of the second class refer? What is in apposition to the 'It' which is the subject of the proposition?

About this there is no room for doubt: 'It' refers to a statement. It is true that——, or probable that——, or credible that——. In every case the predication refers to a statement; but in every case an attitude of mind is implied, and in every case the statement is a statement of fact: so that in every case of the kind there are three things to consider and investigate: the fact, the statement about the fact, and the attitude of mind towards this statement. These three factors may at once be reduced to two. When we express the attitude of our minds towards a statement of fact, we are adopting an indirect method of expressing an attitude towards the fact itself. This is clearly shown by those cases in which we use the same word towards both. 'I am certain that hens lay eggs' indicates our attitude of mind towards a fact. 'It is certain that hens lay eggs' is an assertion directly about the statement that hens lay eggs, indirectly about the fact that hens lay eggs. It seems that it does not matter much which form we use, and in this particular case it does not matter; but in many cases it is more convenient to assert indirectly our mental attitude towards a fact through a statement than to assert directly our mental attitude towards a fact, and this for two reasons. In the first place, a statement is a form of words that may embody fact, or pseudo-fact or quasi-fact, or what is not fact; and we can express our attitude of mind towards such a statement without inconsistency; but we cannot without inconsistency, or at least incongruity, express our attitude of mind towards what is not a fact. We can say with propriety 'I believe hens lay eggs,' but we cannot say without a sense of irksomeness and impropriety 'I disbelieve hens lay chickens,' or 'I disbelieve hens do not lay eggs,' for in these expressions we are virtually asserting and denying the same fact in the same breath. The incongruity is at once removed by inserting the relative 'that,' for by so doing we transfer our opinion from the fact or quasi-fact to a statement of it. There is no sense of impropriety or incongruity in saying 'I disbelieve (the statement) that hens lay chickens' or 'I disbelieve (the statement) that hens do not lay eggs.'

The second reason that induces us often to prefer an asser-

tion about a statement to an assertion about a fact is that by using the former method of expression we have at our command a larger choice of shades of meaning than is available by the other mode: and with both at our command, the number of shades of meaning that we can express is largely increased, as we may see from the following examples.

'It is certain' means 'I affirm that the statement is true'; and corresponds nearly with 'I know that the fact is so', but is rather more emphatic.

'It is true' means 'I admit that the statement is true'; and corresponds nearly with one of the senses of 'I believe that the fact is so', but is perhaps more emphatic.

'It is probable' means 'I incline to believe that the statement is true'; and corresponds in some cases with 'I think', in others with 'I suspect that the fact is so.'

'It is possible' means 'I do not deny that the statement may be true'; and corresponds with 'I dare say the fact is so' or 'may be so.'

'It is doubtful' means 'I neither affirm nor deny that the statement is true'; and corresponds pretty accurately with 'I do not know whether the fact is so or not.'

In all these cases the last assertion expresses the attitude of mind towards a fact; the second expresses the attitude of mind directly towards a statement, indirectly towards a fact; and the first expresses explicitly an assertion about a statement, and implicitly the attitude of the mind towards, first, the statement, and second, the fact, or quasi-fact, expressed in the statement.

In the foregoing discussion the term 'fact' has been freely used. It is time to define it, and to ascertain how it is expressed. Of course, originally and strictly, a fact means a thing done, but few words have been more abused, battered and transmogrified; and by many writers and speakers it is used pretty much in any sense they please at the moment. I discard all these meanings, and define it for the present purpose as anything existing or happening, in the past, present, or future. To us, however, a fact is always a relation, and we have no means of expressing, or indeed of apprehending, a fact except as a relation. Our expression of a fact is always in the form 'A is related to B,' and this empty form is filled out and vitalised by substituting appropriate terms for A and B, and by interposing between them a verb as a connecting link, as for

instance, Hens lay eggs. This is an expression of a fact, and the fact is expressed by asserting a relation of laying, which means in this case origination or parentage, between the eggs and the hens. It is manifest that there are as many relations known to us as there are verbs to express them; and moreover, we are constantly inventing new verbs to express relations that we newly appreciate. I mention this because the teaching of every book on logic is that there is only one relation between things, and that there is only one verb in any language, namely, the verb 'to be'; or if there is any other verb, it cannot be used to express a fact, or to argue or reason about it. This is what logicians teach, although they use all the verbs in the dictionary as freely as anyone else, and cannot, any more than other people, conduct their arguments without these verbs. The doctrine is a curious superstition, and well worthy the attention of students of irrational beliefs, but it need not detain us now.

Things exist or do not exist, happen or do not happen. Our business, if we think about them at all, is to bring our attitude of mind into conformity with fact, so that if a thing has, does, or will exist or happen, we should so believe; and if it has not, does not, or will not exist or happen, we should attune our minds accordingly, and disbelieve. Now, it is a common-place of philosophy that we have no experience of things themselves, but only of their appearance; and with respect to many things that we rightly believe, such as the landing of Cæsar in Britain, and the great earthquake at Lisbon, we have no experience even of appearance to go upon. How, then, are we to bring our beliefs into accordance with facts, our disbeliefs into accordance with the absence of facts? In this way: Between facts, or the existence and happening of things, and our minds, which should be moulded into conformity with the facts, there is an intermediary, which we term evidence. The facts give rise to evidence, and it is the evidence and not the fact that impresses our minds. We can never have any direct knowledge of things or facts external to our minds: all that we can ever know is the evidence for or against them, and it is notorious that evidence may mislead. Still, though it may mislead, it is the only means we have of attaining a knowledge of fact, and therefore it is of the utmost importance that we should discover what is evidence and what is not; what

evidence is trustworthy and what is not; what are the sources of error in interpreting evidence, and how they may be avoided; what kinds of evidence there are; and, generally, ascertain how to bring our beliefs into accordance with the best evidence we can get.

For, as belief should rest upon evidence, so it should be in accordance with the evidence. Of some things, as of the size and position of a possible crater on the other side of the moon, we have no evidence at all, and therefore ought not to have any opinion at all. Of many other things, such as the existence of an enormous sea-serpent, the evidence is imperfect and inconclusive, and towards these the attitude of our minds should be one of doubt or scepticism. We have no right either to believe or disbelieve. Of yet other things, such as the existence of the moon, and the recurrence of the tides, the evidence is conclusive and unassailable, and towards these our attitude of mind should be one of belief.

It is customary to speak of a 'knowledge of the fact,' as if such knowledge were practicable, and indeed frequent; and no doubt when the evidence is quite conclusive it would be pedantic and ridiculous to object to the expression. In such cases we may, for the common purposes of life, leap over the evidence, and conclude that the knowledge and belief conform to the fact; but the habit of leaping over the evidence has its dangers. It leads very often to accepting a knowledge of evidence as a knowledge of fact; and to a disregard of flaws in evidence which should make us hesitate. The attitude of hesitation is, however, irksome, inconvenient, and painful; and few will maintain it until they have trained their minds to submit to it.

EVIDENCE.

Evidence of fact is of three kinds, and is derived from three sources: evidence of sense, evidence of reason, and evidence of hearsay; and any one of these may be conclusive or inconclusive, convincing or worthless.

Evidence of Sense.—The evidence that facts themselves afford directly to the senses of hearing, sight, touch, and so forth, is commonly regarded as conclusive and irrefragable. 'Seeing is believing' is an aphorism that everyone accepts.

https://doi.org/10.1192/bjp.62.257.241 Published online by Cambridge University Press

That which is palpable cannot be gainsaid. These statements are in one sense the truest of truths, but in another they may be very misleading. When we have an impression on a sense, when we see a light, hear a sound, or feel a touch, these are facts of ultimate certainty; and it is not open to us to doubt that we do experience the sensation; but a sensation no more remains a bare sensation when it is received by the mind than a fly remains a bare fly when it is received into a spider's web. In the one case as in the other, the intruder is instantly enveloped in a web of new material furnished by the owner of its new surroundings, which distorts and transforms it, and makes of it a very different thing. The mind is rarely content to receive a sensation and let it remain a bare sensation. It instantly begins to work upon it, to interpret it, and to infer from it to some external fact which corresponds with it and gives rise to it. This is seen by the character of the response that is instantly made by the mind to any sudden and unexpected sensation. When we receive a sudden and unexpected flash of light, or sound, or touch, the instant and unfailing response is 'What's that?' The question does not refer to the sensation. We know perfectly well what the sensation is. It is a flash of light, it is a loud crack or boom, it is a touch, light or heavy; and no investigation can give us any further knowledge of the sensation itself. What the question refers to is not the sensation, but the source or origin of the sensation: not the feeling, but the fact that gives rise to the feeling. We say or think 'What's that?', but if we were to express our meaning with pedantic accuracy we should say 'What has happened?' 'What fact has occurred to give rise to this sensation?' The sensation is evidence; the knowledge of the external fact that gives rise to the sensation is arrived at by interpreting the evidence; and the knowledge will be true or false according as the interpretation is correct or incorrect; and so will be the belief. I hear a booming rumbling noise, and this noise is evidence to me that something has happened in the world outside of me; but what it is that has happened, the noise does not tell me. What conclusion I come to about the origin of the noise must be arrived at by interpretation; that is to say, by the activity of the mind working upon the materials it possesses. I interpret the sound as thunder. I may be right: I may be wrong. It may be thunder, or it

may be heavy guns. The sensation itself does not tell me. It is from the interpretation of the sensation that I derive my belief; and although sensation cannot err, the interpretation of sensation may be very erroneous; and the moment interpretation steps upon the scene, the chances of error begin. At how early a stage interpretation begins, and how irresistibly it may lead us to false conclusions, are shown by the many examples of what is called sensory illusion. If we touch a marble with two adjoining fingers, we have two sensations of touch which we interpret as due to one object; but if we cross the fingers and again touch the marble simultaneously with both, we cannot help interpreting the sensation as due to two objects. The familiar experiences afforded by the conjurer and the ventriloquist give us examples of illusion of the senses of sight and hearing, illusions which are in every case due to misinterpretation of what we see and hear; but it would be quite a mistake to suppose that misinterpretation is limited to the cases in which others lay elaborate schemes to deceive us. When sight or hearing is impaired, misinterpretation of these sensations becomes frequent, and it is occasional with all of us, as the many cases of mistaken identity testify. For a long time it was in doubt, and for aught I know it may still be in doubt, whether there are or are not rectilinear markings on the surface of the planet Mars; and the interpretation of the markings, if they exist, is still a matter of dispute.

Interpretation of a sensation consists in likening it to some previous sensation that we have had, the source of which we have ascertained. Thus, when I hear that deep booming sound, I mark its resemblance to such sounds that I have heard in the past, and say 'That must be thunder,' or 'That must be guns.' Which source I choose must depend upon my recollection of the sounds of thunder and of guns; and upon which of these the sound that I now hear most resembles. When I identify a man as one that I have seen before, my interpretation of the visual sensation depends on the faithfulness of my memory of what I have seen before, and on the degree of likeness that I can trace between the present sensation and the memory of the past sensation. Accuracy of interpretation depends partly on faithfulness of memory, and partly on the ability to discern likeness and difference.

A powerful aid to interpretation, in cases in which it can be

employed, is the checking of the evidence of one sense by the evidence of another. If a thing looks as if it were hard or soft, we can test that interpretation by the sense of resistance. If it looks as if it were at a certain distance, we can traverse that distance, and note whether we reach it. The corroboration of one sense by another usually removes the possibility of doubt; but we find that seeing is not always believing, or if it is, the belief may be erroneous; and although the evidence of sense may usually be trusted, and in almost every case must be trusted, yet possibilities of error lurk in the interpretation of this evidence, and there are cases in which these possibilities ought to be borne in mind, and judgement, even of the evidence of sense, suspended.

Evidence of Reason.—As we have just seen, the whole cogency of the evidence of the senses lies in the way we interpret it; and we interpret it by the activity of the mind working on the material with which the senses furnish it. Interpretations of sensations, or perception, is, in short, an example and a method of reasoning; very elementary reasoning it is true, but still reasoning of a kind, and of a kind that is the model of a very large part of our reasoning. The only difference is that in the rest of this kind of reasoning the material is not the direct evidence of the senses, but other evidence—evidence that has been gradually accumulated in our minds by experience and hearsay, and which the mind can work upon and interpret in the same way as it works upon and interprets the evidence of sense; that is to say, by remembering, and by tracing likeness and difference between the things remembered. The general rule is that the more completely the evidence harmonises and accords with what we know to be true, the more readily we may accept that evidence as evidence of truth; and *vice versâ*, the more incongruous and discrepant the evidence with what we know to be true, the more cautious we should be in admitting it.

This raises the crucial question, What do we know to be true? and this question has, curiously enough, two answers, one derived from reason and one from experience.

As we have already found, a statement is not bound to conform to truth. We can form the statements 'Paris is in London,' 'The Thames is run dry'; but we cannot assert either of these statements, for assertion means that we intend

what is asserted to be received as true. Now there are certain statements that are not merely false, like the instances just given, but that the mind refuses to entertain. A statement consists, as we have already found, of two terms predicated to hold towards each other a certain relation. It is possible to take any two terms we please, and to couple them in a statement by any verbs we please, and the resulting statement then comes before the mind for acceptance, or rejection, or any other operation the mind can perform upon it. With this wide liberty of concocting statements it is evident that we can, if we please, form some that are nonsensical, and that convey no idea to the mind, as for instance 'Two o'clock is solid,' 'Limestone reasons downward,' 'Hens shine pocket-books.' Such statements the mind has nothing to do with. It neither accepts nor rejects, but disregards them. It is impossible even to consider whether they are true or not. There is a second kind of statement which is not nonsensical, which can be entertained by the mind, but which the mind instantly rejects, because it cannot conceive the terms to stand in the relation which the statement purports to assert. Such are the statements 'The hen laid an egg larger than itself,' 'The space was enclosed by two straight lines,' 'The solid body is liquid,' 'The pain was unconsciously felt.' In these cases the relation expressed in the proposition is inconceivable. The mind cannot put the terms together in the relation that is predicated. It is intuitively perceived that the statement is false, and that its contradictory is true. Thus, by the light of reason alone, by the very nature of the terms, it is seen that they cannot exist in the relation predicated, and that the contradictory of that relation must be true. The realisation of this truth does not rest upon experience. It is independent of experience, and apart from it; and it is the highest and most assuredly certain truth that the mind can entertain. We need no experience to assure us that the hen did not lay an egg larger than itself, that the space was not enclosed by two straight lines, that the solid body is not liquid, or that the pain was consciously felt. Such truths, which are the contradictory of what is inconceivable, are called Axioms; and as already said, axiomatic truth, or axiomatic certainty, is the uttermost certainty of belief that the human mind can entertain. The terms are bound up indissolubly in the relation, and no effort of mind can tear them asunder.

Axiomatic truth is the contradictory of what is inconceivable. Herbert Spencer arrived at the conclusion that the test of truth is the inconceivability of the opposite, and this doctrine was strenuously opposed by Mill; who declared that it is no test, since many things, such as the antipodes, the rotation of the earth, and gravitation, were inconceivable to our forefathers, but are become commonplaces to us. The contradictory of these beliefs was accepted by our forefathers as true, and is known by us to be false. The contradictory of what is inconceivable is therefore, in Mill's opinion, not necessarily true. It may be as mistaken and false as any other belief. Spencer felt that he was right, and he was right; but he had great difficulty in meeting Mill's objection, and never met it satisfactorily. He maintained that in the cases adduced by Mill, the relations that had been thought to be inconceivable were not really inconceivable, but had been thought to be so because they were not clearly represented or pictured in the mind. When, however, we do clearly represent a relation in the mind and find it indissoluble, it must, so Spencer said, be true, and we cannot help admitting that it is true. Spencer rested his defence upon a wrong ground, and it is easy to demolish. There is no difficulty in clearly representing or picturing in the mind the antipodes and the rotation of the earth; and both their existence and its contradictory are easily conceivable, and have in fact been conceived. The true defence is that Spencer, when he said that the contradictory of the inconceivable must be true, was referring to axiomatic truth; Mill, when he denied it, was referring to empirical truth; and thus both were right and both were wrong. That the earth rotates, or does not rotate, is a relation whose terms do not refuse to exist in either relation. The mind can put them together in either relation, and does not intuitively perceive that either is true or false. Which is true and which is false is for evidence drawn from experience to decide. But to perceive the truth of an axiom we need no evidence. We need no evidence to enable us to decide whether a hen can lay an egg larger than itself, or whether two straight lines can enclose a space, or whether a pain can exist without being felt, or whether a solid thing is liquid. As soon as we have experience enough to comprehend the relation that is asserted, we see that it must be false. The mind refuses to entertain it, and asserts at once that the contradictory must be

true. Mill's instances are not of this nature. Whether they are true or false is matter for discussion: it is for experience to decide: their truth or falsity is not intuitively perceived the moment they are stated and the mind grasps their meaning. In short, they are not axiomatic truths or certainties, they are empirical beliefs.

Rightly apprehended, an axiomatic truth cannot be doubted. Of course we may frame a statement which purports to deny an axiom, but it is beyond human capacity to doubt an axiom, and anyone who pretends to do so is either deliberately lying, or is so muddle-headed as not to know the meaning of what he says.

Empirical certainty is a degree less assured than axiomatic certainty. Empirical truth, once established, must be believed; but it is always open to us to conceive the contradictory, though we may not be able to believe it. Empirical truth is, as its name implies, founded upon experience, and our warrant for it is experience alone. Conceivably the fact might be otherwise. In experience it never is and never has been otherwise. Consequently, as long and as far as our knowledge that it never has been otherwise extends, we are precluded from believing that it ever will be otherwise. It is to us an empirical certainty. The basis of empirical certainty is constancy in experience, by which is meant, in the first place, the accumulation of instances without exception. The greater the number of experiences of a given fact that we can accumulate without finding any exception, the firmer becomes our belief that the fact is universally true, and that no exception will be experienced; until at last conviction becomes unshakeably assured.

No one nowadays doubts that mankind are necessarily mortal—that every man, woman, and child that now lives will die, and that there is no one now living who was alive two centuries ago. This is not an axiomatic truth. The contradictory of it is not only conceivable, but has by many people been believed. There have been few primitive peoples who have not believed in the immortality of some chief or prominent character who impressed himself powerfully on their minds during his lifetime, and became the centre of legend after his death. We have our King Arthur, our Merlin, our Thomas of Ercildonne, the Germans their Frederick Barbarossa, Denmark its Holger Danske, and other nations their analogous

characters; but such beliefs have prevailed only among primitive people, belonging to small communities without authentic memorials of past times, and without any critical faculty of interpreting evidence. As far as we know, there has never been an instance, there is no evidence worth the name, that of all the millions of millions of mankind who have lived in past ages anyone has escaped the fate of dying.

This complete constancy in experience of the sequence of death upon life in men is of itself sufficient to produce in us an empirical certainty that the sequence never will be broken, and that all children who are born into the world will die sooner or later; but this constancy in experience is reinforced and corroborated by a constancy of far greater extent. Men are living beings, and with respect to what they have in common with other living beings we can argue from other living beings to men; and our constant experience of all living beings, animal and vegetable alike, is that after a period of life they die. More even than this, the slowly accumulating experience of mankind through the centuries, and the insight that we have gained in the last few generations into the processes of nature, all go to show that destruction, dissolution, decay, or at least change, is the universal law of all material things; and man's body is a material thing. This vast concourse of experiences, to none of which can any permanent exception be shown, breeds in us a corresponding fixity of belief in the inherent mortality of man, a belief that is not axiomatically certain, for it is not difficult to conceive that a man should go on living for an indefinite time, and indeed, many have conceived, and even in a sense believed it; but the belief is empirically certain, for, with the evidence now at our command, it is impossible to admit that any man has lived much beyond a century, and this complete constancy in our experience of an indefinitely great multitude of cases of men and other living things, justifies and compels an empirical certainty of belief.

A very similar empirical certainty is that heavy bodies, if unsupported, fall to the ground. This, again, is not an axiomatic certainty. It is easy to imagine heavy bodies without support remaining suspended above the ground; and the case of Laputa shows how easily it can be imagined, while the case of Mahomet's coffin shows that it can be not only imagined but believed. We have, in fact, many experiences of heavy bodies

without visible support which yet do not fall to the ground. Every flying bird is such an instance, and we frequently see leaves, straws, and other things tossed about by the wind without falling. In such cases we soon learn that the air, though invisible, is a support, and that the rule is not really broken; and so at length, by the accumulation of innumerable experiences without any real exception, experiences constantly recurring throughout every moment of our lives, we are driven and compelled to adopt as quite certain the belief that heavy bodies, if unsupported, will inevitably fall to the ground; and although we can imagine exceptions, we cannot believe that there ever has been or ever will be a real exception, and the belief is inescapable. It is an empirical certainty.

These, it will be seen, are cases of that *enumeratio simplex, ubi non reperitur instantia contradictoria* which Bacon and subsequent logicians have scouted as utterly untrustworthy as a ground of belief. It is unquestionable that it is, on the contrary, the ground of the most certain and inescapable of all our empirical beliefs.

It is true that it is not always a satisfactory ground of belief, or at least that the evidence may be so interpreted as to give rise to beliefs that are unwarranted. The ancients believed, on somewhat similar grounds, that every swan is and will be white, and that no such thing as a black swan is credible. Since their day, black swans have been discovered, and they have been shown to have been in a sense wrong; but they were not wholly wrong. Let us see what were the grounds of their belief. They had had many experiences of swans, and in every case without any exception the swans had been white. According to rule, therefore, it seems that they were justified in entertaining the certain conviction that all swans thereafter discovered would be white, and no swan of any other colour would ever be found. It will be seen at once, however, that the number of cases, in which swans had been seen and found without exception to be white, were as nothing in comparison with the number of cases in which unsupported things had fallen to the ground, or with the number of cases in which men and other living beings had proved their mortality by dying. A very important element in confirming the certainty of an empirical belief is the number of cases in which the conjunction or relation has been witnessed and found to be constant. Constancy,

however complete, that extends over but few cases ought never to be accepted as ground for a certain belief; and the acceptance of a few cases as proof of a general law is one of the most fertile sources of erroneous belief. If, upon visiting a new country, the first man we met was six foot four, or even the first two or three men we met were more than six feet high, it would be manifestly very unsafe to form the belief that all the inhabitants of that country were exceptionally tall. Although the relation would be constant in experience as far as experience went, the experience would be far too limited to justify a belief in the general prevalence of the relation. A similar error, not so gross, but similar in kind, though less in degree, vitiated the belief of the ancients in the universal whiteness of swans. The instances were too few.

But there was another and more serious error. We have seen how enormous a corroboration and justification for the belief in the mortality of men is afforded by the constancy in experience of the mortality of other living things, that is to say, of things that, for the purpose of the argument, are like men. It is manifest that if all birds, and still more if all animals also, had been white, and no instance of a bird or an animal of any other colour had ever been known, the certainty of the belief that all swans are and will be white would have received a tremendous corroboration. But this is not so. Not only animals, but birds also, exhibit a great diversity of colour, and even some birds that are, for the purpose of the argument, not unlike swans, such as geese, exhibit some diversity of colour. Therefore the belief that all swans are and will be white was risky, and should have been held lightly, and subject to further experience.

Nevertheless, as far as it went, and as they understood it, the belief of the ancients that all swans are white was justified, and was true. By 'swans' they meant the species and breed of swans that they knew, and with respect to these 'swans' they were right; for no swan of that species has ever yet been of any other colour, as far as we know, in the two thousand years that have elapsed since their day; and with every generation of these swans the appearance of an individual of any other colour becomes less likely. The black breed of birds resembling swans, that has since been discovered, we call by the name of swans, but they are not the same kind of swans as were

known to the ancients, and might very well have been called by some other name. They may be swans, but they are swans with a difference; and as far as the swans which the ancients believed to be always white are concerned, their assertion was true.

It is clear, I think, that empirical beliefs in the general truth of relations always depend upon the constancy in experience of those relations, and are the more justifiable, the more confirmed, and the more inescapable, the greater the number of instances in which the experience has been constant.

Supposing, however, that the relation is not constant in experience, but is liable to exceptions, in which its terms are experienced dissevered from one another, what effect will this inconstancy in experience have upon the attitude of mind? For instance, cancer is generally a fatal disease, but every now and then there occurs a case in which a cancer, after having advanced to a certain stage, shrinks up, dwindles away, and disappears, or leaves a mere remnant, and the patient recovers his former health. If we have had, directly or indirectly, that is to say by ourselves or by others, experience of a very large number of cases of cancer, every one of which has been fatal, our belief in the fatality of cancer will be strong in proportion to the number of cases in which a fatal issue has without exception occurred. Now if a case occurs in our experience in which recovery ensues, we have two alternatives of interpretation. We may believe that we have been mistaken in supposing that the disease is cancer, and may adhere to our original belief that cancer is always fatal; or we may modify our belief about the fatality of cancer, and admit that though it is very generally fatal, yet it is not always so. There is no doubt that in every case in which the experiences of constancy have been very numerous, the safest course is the first. We should assume that we have been mistaken in supposing that the constancy has been broken, and should require the most stringent and unimpugnable evidence, first that the tumour really was cancer, and second that it really did shrink up, dwindle away, and allow the patient to recover. Unless and until evidence on both these points is established beyond reasonable doubt, we ought not to admit that cancer can ever recover. But if these two matters are satisfactorily established, then we can no longer doubt, but must modify our original belief, and

admit that, although cancer is generally fatal, yet it is not universally or necessarily so.

The number of cases in which cancer has been watched and has been found to be fatal is many thousands, many tens of thousands, perhaps many hundreds of thousands; and the number in which the result has not been fatal has been few, perhaps a few dozen, perhaps a few score; but in any case, constancy in experience, even if complete, and even in hundreds of thousands of instances, does not warrant the assured certainty that is derived from the constancy in experience of the fall of unsupported bodies. Of this we have experiences by myriads, experiences daily and hourly all our lives long, experiences that are common to ourselves, our companions, our predecessors, and as far as we know to the whole human race. To such constancy in experience no exception ought to be admitted on any ordinary evidence. Any apparent instance to the contrary should be *primâ facie* disbelieved, and no approach to belief should be admitted until the instance has been examined, and tested, and re-examined, and retested, in every possible aspect and by every possible means. Mere eyewitness of such an instance is worthless, and should not be admitted for an instant. If a person thinks he sees a heavy object, such as a table or a man, rise from the ground and remain suspended in the air without visible means of support, he should assume as a matter of course that there are means of support invisible to him; and in the improbable event of his investigating the matter closely and still discovering no means of support, his proper attitude of mind is to assume that the means of support are so cleverly hidden that he is not able to discover them. In face of the universal experience of the human race that the relation is constant in experience, he would be guilty of unjustifiable credulity if he believed, on the evidence of a single instance, that an exception could occur.

In many things experience exhibits little or no constancy. In this country there is very little constancy in the sequences of the weather. A fine day may be followed by a fine day, or it may be followed by a wet day; and as there is no constancy in experience, so there can be no assured belief, and in any individual case no assured expectation. We may, indeed, be able on other grounds to forecast with some success what the weather will be to-morrow, but we cannot do so on any con-

stancy in experience of the succession of a wet day on a fine one, or *vice versâ;* but though we cannot rightly form any belief of the kind of weather that will occur on the day following a wet day or a fine day, we are not altogether debarred from belief. On the contrary, our experience has been in some respects constant, and consequently in some respects we have very definite and positive beliefs about the weather generally. As far back as our records go, and as far as the memory of the oldest inhabitant serves, the weather in these islands has been generally inconstant, with occasional spells of uninterrupted rain, and occasional spells of uninterrupted fine weather. We are therefore justified in believing, and indeed compelled to believe, that in future the weather here will continue to exhibit these characters, and that we shall go on indefinitely having spells of fine weather, spells of wet weather, and spells of changeable weather. In short, in whatever respect experience has been constant, even in inconstancy, in that respect we are justified in believing, and compelled to believe, that it will continue to be constant.

Empirical belief rests, therefore, upon two elements in experience: first on the absolute number of the experiences of the particular relation. If these experiences are sufficiently numerous, and are all one way, we must believe that the experience is necessary and will continue. The smaller the number of experiences, even if they are all one way, the less are we justified in arguing to other similar cases, and the more cautious should we be to keep an open mind. When experiences are not constant, but are sometimes one way and sometimes another, we are not warranted in believing that any new experience of the kind will be either way; but when experiences of one way preponderate numerically over experiences of the other way, and the total of experiences of both kinds is very large, we are justified in believing, and compelled to believe, that a similar proportion will hold of such experiences in the future, and that the chances of a new experience being one way rather than the other will be in the proportion that the ways have borne to one another in the past.

Evidence of Hearsay.—Immense numbers of our beliefs are based on this kind of evidence; and as it is manifestly open to more sources of error than either of the other kinds,

https://doi.org/10.1192/bjp.62.257.241 Published online by Cambridge University Press

it is incumbent on us to examine it with some care. It is more open to sources of error than the other kinds because all evidence, including that of hearsay, is ultimately derived from experience or from reasoning, and hearsay evidence has additional sources of error in the untrustworthiness of the witness, either from bias, or from deliberate intention to deceive, or from defect of memory, or from other causes.

With respect to every assertion, the first necessity is that it shall be understood in the same sense by both the assertor and the recipient, and this is often not the case. The ancients asserted that all swans are white. A modern zoologist will assert that all swans are not white—that in fact some swans are black. Either assertion may be true or false, according as it is understood. If by 'swans' we mean the familiar European species, the ancients were right; but if we include in the term 'swans' birds that are sufficiently like the European species to be included in the same genus, and extend the name so as to cover this genus, then the moderns are right and the ancients are wrong. Again, there is another sense in which both are wrong. No swans are wholly white or wholly black. The legs and beak of the white swan are not white, and the beak of the black swan is not black. Still, it would be pedantic and unnecessary to deny, on account of these exceptions, that the one is white or the other black. Neither statement is strictly accurate; but this does not matter, because both assertor and recipient are quite aware of the exception, and both understand the assertion in the same sense. If I assert that all gnats bite, the assertion is true in one sense and false in another. It is true that gnats of every species bite, but the males of some species do not bite; and while it is true that the females of every species bite if they get the chance, many individual female gnats never do get the chance, and therefore in this sense all female gnats do not bite. Still, though exception may be taken to the mode of expression, the mode of expression is of no importance as long as both parties understand it in the same sense.

Having ascertained that we understand the assertion in the sense in which it is meant, the next question we are to ask ourselves is Is it true? It may be true or false, and if false, it may be false with or without the knowledge of the assertor; in other words, it may be a lie or a mistake; and if a mistake, it

may be a sane or an insane mistake—it may be a sane mistake or a delusion.

The first question to determine is whether the witness is a witness of truth as far as he knows it—whether he is asserting what he believes to be true, or what he knows to be false, or recklessly, what he does not know to be either true or false. As to this we must be guided mainly by two considerations :—by the previous record of the witness, and by his responsibility. The previous record of the witness for truthfulness and carefulness must go far to determine our judgement whether he is truthful and careful on this occasion. That is unavoidable, and in accordance with the general principle of induction, by which we infer that that which has been constant in experience will continue, and infer it with a confidence proportioned to the number of uncontradicted experiences. In the absence of any such record, we ask, first, if he is responsible, and our opinion of the *bona fides* of his assertion rests largely upon the degree of his responsibility ; that is to say, upon how far he would suffer in reputation by telling a lie. Hence we are always ready to accept as truthful in intention the assertions of prominent persons on important and public occasions, and accept them the more readily the more prominent the position of the assertor, and the more public and important the occasion on which the assertion is made. It is true that our faith is sometimes unwarranted, but the rule is a wholesome one, and is usually justified.

A third consideration, which must influence us, rightly or wrongly, is whether the assertor has a personal interest in getting the assertion accepted.

Having determined that the witness is in intention a witness of truth as far as he knows it, the next stage is to estimate how far he does know the truth, and this is the matter that is most often neglected. In order to estimate it we must consider, first, what his opportunities of knowing are, and second, what his bias is likely to be.

It is surprising how implicitly most people receive as true the evidence of those who have no better means of knowledge than the recipients themselves. 'They say' is an authority that is accepted with unquestioning submission, without even a query as to who are the 'They' who say it. The whole fabric of popular superstition about what is lucky and what is unlucky

rests entirely upon what 'They say.' Who 'They' are, or what opportunities 'They' have of knowing, are questions that are never asked, and that the superstitious people who entertain these beliefs never think of asking. They would, I fancy, regard it as presumptuous, and almost irreligious, to ask. But it is not only with respect to beliefs like these, that are *primâ facie* irrational and absurd, that the omission is made. Many prevalent beliefs on other subjects are equally without rational foundation. There is a prevalent belief, for instance, that cigarette smoking is more injurious to the smoker than the smoking of pipes; and this belief is widely and firmly held on no better ground than the belief that it is unlucky to look at the new moon through glass. Occasionally we may obtain the assurance that 'doctors have said it,' but it is usually found that 'doctors' is but another expression equivalent to 'They.' Supposing, however, that the dictum can be traced to a doctor, I have never found, and I have often tried to run to earth the origin of this strange belief,—I have never found that the doctor has any better ground for his belief than the fact that 'They say.' In discussing the matter with an intelligent person who is not a doctor, I have been told that he felt bound to accept the dictum of a doctor, because the doctor was in a position to know. This is an instance of simple faith comparable with the confident assurance that was reposed in the middle ages on the assertions of an ecclesiastic. It is clear to anyone who gives a moment's thought to the matter, that to determine whether cigarette-smoking is or is not more deleterious to health than pipe-smoking would require a very long and laborious course of experimentation, such as no one has ever yet undertaken, or an accumulation of non-experimental evidence, such as has certainly never been attained.

The belief that canker and other diseases of fruit trees are due to sourness of the subsoil rests also upon what 'They say.' Most people who are not gardeners accept it upon the evidence of gardeners, and assume that gardeners 'must know.' But why must they know? I am pretty sure that no gardener except myself has ever tested the subsoil to discover whether it is sour, nor is there any evidence to show that if the subsoil were sour it would be any more favourable to the growth of canker than an alkaline subsoil.

Many people believe in the occurrence of what has been

called telepathy, and many believe in the genuineness of the 'manifestations' of 'spiritualism.' In some cases the belief is founded upon the experiences of the believer, but there is now besides these a large number of people who hold these beliefs upon hearsay evidence. Certain persons profess their faith in the existence of telepathy, or in the 'manifestations,' and a ruck of other persons hold the belief on the evidence of those witnesses, without any critical enquiry into the worth of that evidence. 'So and so,' they say, 'that is, Sir Roland Illogic and Sir William Hookes, say so, and they are scientific men; and what a scientific man says on a scientific subject is good enough for me. I myself have no personal experience, but as a sensible man I must accept the opinion of an expert. No, I shall not suspend my judgement about it. You might as well ask me to suspend my judgement about the revolution of the earth. To me it seems that the sun goes round the earth, but scientific men who are in a position to know tell me that it is not so, and that the earth goes round the sun, and I accept their evidence. How can I consistently accept the evidence of scientific men in the one case, and reject it in the other?'

The reasoning seems plausible on the face of it, and is representative of such a large body of opinion on so many subjects that it is worth examination. The assumption that underlies it is that the evidence of a witness who is a witness of truth, and is in a position to know the fact to which he testifies, ought to be accepted. There is no question about these witnesses being witnesses for truth in intention, that is, of what they believe to be truth; but the assumption that they are in a position to know the facts to which they testify is altogether unwarranted. That we must trust the expert is a sound general maxim; but before we trust him we must make sure that he is an expert. The greatest possible eminence of an expert in one branch of science adds not a grain of weight to his opinion in another branch of science. However profound may be a man's knowledge of chemistry, his opinion is not on that account more to be trusted than that of a farmer or a fishmonger upon a question of astronomy. But, it may be said, he is accustomed to weigh evidence? He may or may not be. Many scientific men are very poor hands at weighing evidence; and in any case, no scientific man has any experience at all in weighing the kind of evidence that is necessary to distinguish between

genuineness and imposture in spiritualistic 'manifestations.' The 'manifestations' are the kind of occurrences that, if not genuine, can only be produced by conjuring tricks, and the only expert whose opinion of them is of any value is a conjurer. The opinion of a professor of electricity or of spectrum analysis is of no more value in such cases than the opinion of a ship's-captain or a carpenter. The evidence for the revolution of the earth rests upon quite a different basis. The experts who testify to this are experts in this very subject. The whole of their science is founded upon this supposition; and upon this supposition is founded the compilation of the *Nautical Almanack*, by means of which innumerable ships find their way across the pathless ocean with unerring certainty to their destinations. In other words, conduct founded upon the supposition never leads to experience inconsistent with the supposition; and this is the conclusive test of truth.

'They say' was the foundation, and the only foundation, for the belief in judicial astrology—the belief that the position of the planets, and especially of the moon, influences and regulates the course of human lives, and the fortunes and misfortunes to which human beings are subject. In the long history of judicial astrology, extending over six thousand years, it scarcely ever occurred to any one to ask the crucial question, 'What opportunity have the assertors of knowing whether their assertions are true? What is the evidence on which their belief is founded?' Moreover, never did anyone test whether conduct founded on the belief led to experiences inconsistent with the belief; or if they did, these experiences were powerless against the overwhelming efficacy of 'They say.'

Galen thought that the arteries carry the vital spirit from the heart to all parts of the body; and if this is so, there must be a hole in the septum of the heart to allow the spirit to pass from the arteries of the lungs into the arteries of the rest of the body. He taught, therefore, that there is such a hole, and for fourteen hundred years anatomists believed him, and in spite of the plain evidence of their senses, followed his teaching, and believed that a hole is there, although they could not find it; so strong is the power of 'They say.' He taught also that the veins carry the blood from the heart, and so sure were anatomists that he must be right, that when a valve was found in the azygos vein, a valve which effectually prevents the blood

in that vein from flowing away from the heart, they again refused to believe the evidence of their senses, and declared that the valve operates in the direction the reverse of that in which they saw it operate.

It would be a great mistake to suppose that the efficacy of what 'They say' is abolished in these latter days, or that it influences the minds of the uncultured and the vulgar only. Logicians were told by Aristotle that a universal is necessary in every act of reasoning, and they believed him, and still believe him as faithfully as ever an anatomist of the School of Salerno believed Galen about the hole in the heart. In many arguments, as for instance in the argument *a fortiori*, there is no universal. Logicians have been trying for two thousand years to find a universal in the argument *a fortiori*, and they have failed, just as the anatomists failed to find Galen's hole in the heart; but does this failure modify their belief? Not a bit of it. 'They say' there must be a universal in that argument, and a universal there must be. To doubt it would be to doubt the omniscience and infallibility of Aristotle, and no logician would dare to be guilty of such blasphemy. What are two thousand years of failure? Did not belief in judicial astrology, founded on precisely the same kind of evidence, last three times as long? and may not the belief in the universal in reasoning hope for similar longevity? To doubt it would be to doubt the efficacy of 'They say.'

For nearly as long 'They' have said that insanity is disorder of mind, and disorder of mind is insanity. In vain it is pointed out that that there are many disorders of mind that are not insane, and that there is much in insanity besides disorder of mind. Reason, observation, experience, the plain evidence of the senses, are powerless against the authority of 'They say.' What they have said, that they continue to say, and that they will continue to say to the end of time. In vain it is asserted, in vain it is proved, that what a man says and does is alone enough to prove his insanity, which also cannot be proved without this evidence. 'They say' it is not, and what 'They say' must prevail, and does prevail.

The influence of bias upon opinion has been so thoroughly considered by Herbert Spencer in his Chapters on the subject in the Study of Sociology, that little need be said of it here. There is one kind of bias, however, that Spencer does not

mention, and as it is perhaps as frequent as any other, a word may be said of it. We are strongly biassed against any assertion made by a person we dislike, and against opinions we dislike. The former is too frequent to need illustration; of the latter the following instances will suffice. A certain professor of philosophy in Padua asked Galileo to explain to him the meaning of the word parallax, so that he might refute the doctrine it expressed, which was opposed, so he had heard, to the teaching of Aristotle. Another admirer of the Stagyrite refused to look through a telescope, lest he should be convinced of the existence of Jupiter's moons. It would be a great error to suppose that this attitude of mind did not survive the sixteenth century. The greater part of the opposition to the *New Logic*, and to the doctrine that madness is disorder of conduct, rests on precisely the same prejudice.

From the foregoing considerations it would appear that hearsay evidence is open to so many sources of error that it can never have any great value, and that it would be most dangerous to base any firm belief on any important subject upon hearsay alone, or even chiefly. Such an attitude would be very erroneous, even if we could adopt it; and we cannot adopt it. It is quite true that hearsay evidence should be received with care and discrimination; and it is true also that all our most grossly and flagrantly erroneous beliefs are founded upon hearsay; but on the same evidence are founded some beliefs that are but little inferior in justification to the empirical certainties, such as that noise always proceeds from movement, that yield only to axiomatic certainties in justification and inescapability. Besides the intrinsic credibility of hearsay evidence that arises from our trust in the truthfulness of the witness, and our estimate of his opportunity of knowing the fact, there are extrinsic circumstances which may add such weight to hearsay evidence as compels us to accept it as true, or may demolish its cogency altogether, and leave us no alternative but to reject it. These are, first, the congruity of the hearsay evidence with already existing beliefs, and, second, the concurrence of witnesses; or we may put it corroboration by experience, and corroboration by other witnesses.

In days when knowledge was less diffused than it is now, a sailor on his return to his native village reported that he had seen in his travels mountains of sugar, rivers of rum, and fishes

that flew like birds. The village gossips received the first two items of information with acquiescence, 'for,' they said, 'we have seen sugar and rum, and they must come from somewhere; but flying fishes are a traveller's tale; you cannot deceive us with such a cock and bull story as that.' The judgement was erroneous, but the principle on which it was founded was correct. It was the comparison of the hearsay evidence with knowledge already in possession, and the reception or rejection of the evidence according to its congruity or incongruity with what is already known. They were wrong in believing in the alleged origin of sugar and rum, because the corroboration was insufficient. The known existence of these commodities proved that they must have some origin, but did not point to one origin rather than another. But they were right in disbelieving in the existence of flying fish, for such animals are so incongruous with all the experience that the audience had had of fish, that they ought not to have believed it upon mere hearsay from a single witness; and they were none the less right in spite of its happening to be true. Such a startling incongruity ought not to be accepted without strong corroboration. Similarly, when the reported discovery of the *X* rays reached this country, some scientific men disbelieved it, and many suspended their judgement, and refused to believe it until it was corroborated. The latter were undoubtedly right, and the former were not very far wrong. That any rays but those of light could affect a photographic plate was so incongruous with all our experience up to that time, that scepticism was not only justifiable but proper. That radiant forces could penetrate solid and opaque substances was, indeed, familiar in the cases of gravitation and magnetism, but neither of these has the power of precipitating silver from its combination in a colloid, and the cases were not in point.

It is customary for the newspapers in the summer, when Parliament is not sitting and news is scanty, to make jocose remarks about the sea serpent; and it is generally assumed that no such animal exists. There is nothing, however, in the evidence we have of the existence of a gigantic sea serpent that is incongruous with zoological knowledge. Many fabulous animals, such as the griffin, the cocatrice, the phœnix, the centaur, the dragon, are zoologically impossible. They are inconsistent with what we know of the necessary structure or

animals. The griffin, for instance, is represented with the body of a quadruped, the claws and head of a bird, and the wings of a bat, and with the ability to fly. Now it is quite beyond question that the ability to fly with wings implies the existence of very powerful muscles, and therefore of very large muscles, such as constitute the breast of a flying bird; and without such muscles a pair of wings would be of no more use for flying than if they were cut out of paper and stuck on with glue; but in the fabulous griffin there is no sign of any more muscles than are needed for quadrupedal progression, and we may therefore be sure that such an animal could have no wings. There is no such incongruity in the structure of the sea serpent. The only thing unusual in the reported appearance of the animal is its size, and we know that very large animals do inhabit the sea. There is therefore no reason on the ground of incongruity why we should positively disbelieve in the existence of such an animal as has been described as the sea serpent. It may be wise to suspend our judgement, but that is a very different attitude of mind, and is inconsistent with disbelief.

As long as I can remember, and I am now growing old, 'They' have said that this or that prominent personage has been addicted to drink; and as long as I can remember the question has been put to me, or to others in my presence, 'Do you believe it?' Rightly conceived, the question is an insult to the intelligence of the person to whom it is put. It assumes that he will form a belief, without any adequate grounds for doing so, on the mere authority of what 'They say.' It is on a par with asking if we believe that there is a crater fifty-one and a half miles in diameter on the other side of the moon. There may be, or there may not be; but as we have no evidence either one way or the other, it would be a sign of weak intellect to believe either way. It is true that the interrogator does not really want an answer to his question. What he wants is to obtain a momentary factitious importance as the retailer of a spicy bit of gossip. The question is merely an excuse for the gossip; but it does not make the gossip excusable. None the less is it an insult to the intelligence of the person to whom the question is put; and to meet such an assertion of what 'They say' with an indignant denial, as a worthy but ill-advised bishop did on one occasion in a sermon,

is injudicious and disproportionate. The proper course for the interrogatee is to resent the insult to his intelligence.

Suspension of judgement is an extremely important attitude of mind, and one that it is frequently most important to adopt; but it is an attitude of mind that is not always easy to adopt, even for cultivated persons, and one that many persons are quite incapable of. They must either believe or disbelieve, and no middle course is possible for them. There are, however, so many cases in which suspension of judgement is the right attitude to adopt, that it is the plain duty of everyone to cultivate this attitude, and not to allow himself to be enticed out of it by anything but evidence.

In this respect nothing is more important to remember, and nothing is more often forgotten than this:—*Whoso makes an assertion, upon him lies the burden of proof.* The time, labour, paper, ink, and temper that are wasted every year by neglect of this maxim are altogether incalculable; and the waste is not less, indeed I think it is more, in matters that are called scientific, and by men that are called scientific, than in any other field of human endeavour. When we are confronted with an assertion that appears to be false, or pernicious, or extravagant, or baseless, our first and natural impulse is to deny and controvert it; and hence arise most of the endless controversies of scientific men on scientific subjects. The impulse is a natural one, but it is injudicious, and the course adopted is injudicious and unnecessary. When such an assertion is made, the proper course is not to deny it, nor to attempt to controvert it, but to call upon the asserter for proof. If, as sometimes happens, he can bring forward no evidence in support of his assertion, *cadit quæstio.* Except for fanatics and other irrational persons, the matter is at an end. If he responds to the invitation, and brings forward evidence, or what he thinks is evidence, of his assertion, then our duty is to examine that evidence, and ascertain whether it does in fact bear out the assertion or not. In many cases it will be found that what is adduced as evidence has no bearing at all on the assertion; and when it has, it will usually be found that what is merely evidence is put forward as proof.

For there is a vast difference between evidence and proof, a difference that is not often recognised. I have found the assertion of this difference has aroused astonishment and

https://doi.org/10.1192/bjp.62.257.241 Published online by Cambridge University Press

incredulity when I have made the assertion even to very intelligent and highly educated men, accustomed to form independent opinions. The difference is this :—

Anything germane to the issue and consistent with the assertion is Evidence of the assertion.

Proof is evidence that is inconsistent with any alternative assertion.

Thus, to take an illustration of Lord Bowen's, if a man is seen coming out of a public house and wiping his mouth, that is evidence that he has been having a drink. It is germane to the issue, and is consistent with the assertion. But it is not proof that he has had a drink. It is consistent with several alternatives. For instance, he may have gone in to the public house to fetch a friend out, and that friend may have hit him in the mouth for his pains. But if he has been seen to raise a full pint pot to his mouth, and if when he lowered it the pot was found empty, that is proof that he has had a drink, for it is evidence that is inconsistent with any alternative.

If these three principles are faithfully observed :—to lay the burden of proof upon the assertor, to examine the evidence, and to accept nothing as proof but that which is inconsistent with any alternative, we shall effectually safeguard ourselves from believing any assertion that we ought not to believe. Unfortunately for the cause of truth, this is not the common practice. Not only are assertions commonly received, accepted, and believed without proof, but they are commonly believed without the evidence for them being examined and tested, and even without any evidence, worthy the name, at all. Many instances have already been given in previous chapters in this book, and many more must be known by experience to every thoughtful person. The belief in witchcraft was supported by abundant evidence, much of it of a very cogent character ; but in no case was there proof, and it is now generally abandoned. I say the evidence was cogent, and in fact it was a great deal more cogent and satisfying than the evidence for many beliefs that are still very generally held. Many persons confessed that they were witches, that they used charms and spells and the other armamentaria of witchcraft, that they had personal colloquies with the devil, that they rode on broomsticks, and so forth ; and they confessed these things well knowing that their confessions would bring upon them a cruel and agonising

death. Yet they confessed. As to part of these confessions, there is little doubt that they were true. The witch believed in the efficacy of spells and charms, and no doubt she did use them. The effects for which she employed them did no doubt in some cases follow. The objects of her malevolence did fall ill; their cows did slip their calves; their milk did turn sour; their children did have fits; and so forth. The evidence was abundant; and it was cogent; but it was not proof. It was not proof, but in an uncritical age it passed for proof, and the wonder is, not that the belief prevailed so extensively, but that it ever died out; for we find other beliefs now held with equal tenacity, beliefs that have not behind them any of the ancient prescription that attached to witchcraft, and that have not in their favour a twentieth part the tithe of the evidence that witchcraft could show. We should no longer believe in the efficacy of the spell that has been quoted on a previous page, but we still believe in the efficacy of two tablespoonsful three times a day; and a sick man would consider himself defrauded if he did not get them.

Such a belief, too, is that in the efficacy of what is called psycho-analysis. The fundamental doctrine of this strange faith is that every disorder of mind is caused by repressed sexual passion. Of this doctrine there is not only no proof, but there is positively no evidence that is worth the name of evidence. In the first place, the universal repression of sexual passion is a mere assertion, and no proof and no evidence is adduced of any such general state of affairs. Secondly, granting the universal repression of sexual passion, there is no evidence that this repression can produce mental disorder. Not one of the nine or twelve methods, that are set forth in Chapter VI for ascertaining causes, has ever been applied to show that repressed sexual passion has or can have any causal influence in producing mental disorder. The assertion is exactly on a par with the assertion that sour subsoil produces canker in fruit trees. There is no evidence that the subsoil is sour, or if it were that it could cause canker. It is much less rational than the assertion that the positions of the planets govern the fortunes of human beings, for there is plenty of evidence that the planets do exist, but there is no evidence at all that repressed sexual passion exists in most cases of mental disorder.

Another assertion of the psycho-analyst is that if you have difficulty in recalling a word, the difficulty is caused by an involuntary exertion of will (which is of course a contradiction in terms) or an unconscious exertion of will (which also is a contradiction in terms) by which the word is thrust out of the memory. There is no evidence of any such exertion of the will, and a contradiction in terms is an axiomatic impossibility. It is inconceivable, and its contradictory is the strongest and most assured certainty that the mind can entertain. This unconscious volition is exerted because of the association of the forgotten word with some painful experience or painful idea: that is the assertion of the psycho-analyst. Of course, in the multitude of words that are forgotten there must be some that have some unpleasant association; but there are many that have no such association. How do the psycho-analysts surmount this difficulty? With the utmost ease. They say 'You cannot remember any such painful association, but it is there nevertheless. The fact that it is painful causes you to drive it out of your mind, and so to forget the association. The word is painful to you, but you do not know that it is painful. The pain is unconscious pain.' Well, if it pleases them to juggle with words in this manner, there is no reason why we should interfere with such a childish occupation, until they proceed to apply their doctrine with disastrous effects to the treatment of cases of mental disease. Then I think it is time to protest. Then I think every honest man should call upon them for evidence. Not, indeed, for evidence of unconscious pain, for we might as well ask for evidence of a solid liquid, or a round square, or a protuberant hollow; but for evidence, first that every forgotten word has a painful association attached to it, and second, that if it has, this painful association is the cause of the forgetting. Of course there is and can be no such evidence, let alone proof.

But although there is not and cannot be any such evidence, the resources of the psycho-analyst are not exhausted. He makes assertions that may be evidence, but that he pretends are proof. Look, he says, at the cures that I effect by proceeding on the hypothesis that my doctrine is true! And he relates case after case that can only be paralleled by So and So's Institute for the Treatment of the Deaf, or Thingamy's Cure for Consumption. It is no doubt quite true that some cases of

mental disorder will recover even if treated by psycho-analysis, though how much sooner they would have recovered without it we do not know; but it is also certain that many cases that might, according to our experience of similar cases, be expected to recover rapidly, remain ill for an indefinite time under treatment by psycho-analysis. I am reminded of a case that was related to me at the height of the craze for treatment by sour milk, which preceded the craze for psycho-analysis. A physician, who had had no experience of cases of mental disease, told me that he had treated by the administration of sour milk a gentleman who, from the physician's account, was suffering from a mild attack of melancholy, 'and' said the physician triumphantly, 'in six months he was quite well!' I did not tell my friend that six months is the usual maximum duration of that malady, and he departed rejoicing in his adoption of such an efficacious mode of treatment. The recovery of the patient was evidence of the efficacy of his treatment, but it was not proof. It was not inconsistent with every other explanation. It was a good case of the fallacy *post hoc, ergo propter hoc.* The effect did follow the alleged cause, but no connection between them was traceable.

It is a little surprising that in these days, when the merits and wonders of Science are so loudly acclaimed, that so few people, even in a learned profession like that of medicine, should have even a rudimentary notion of what constitutes proof; of what constitutes evidence; of the difference between evidence and proof; and of the grounds upon which causation may properly be assumed. It has been the part of Logic to teach these things, but unfortunately logicians have even less knowledge of them than physicians, and it is a safe assumption that anything taught by logicians is false.

Assertion may be accepted, then, when it is borne out by experience; but there is another mode in which assertion may be corroborated, and when this mode is fully and freely employed, hearsay evidence may properly become the ground of belief as assured and as certain as even the concurrence of innumerable experiences. This method is the concurrent testimony of a plurality of witnesses. Hearsay evidence becomes more trustworthy the more numerous, the more unanimous, and the more independent of one another the witnesses; and when innumer-

https://doi.org/10.1192/bjp.62.257.241 Published online by Cambridge University Press

able independent witnesses concur unanimously in an assertion, that assertion must be accepted, unless it violates our own experience. If, however, the assertion violates our own experience, experience which has been tested, considered, and proved, which is plain and inescapable, then no concurrence of testimony, however numerous, independent, and unanimous the witnesses, ought to shake our belief.

Whately argued, ironically, the non-existence of Napoleon Buonaparte, by showing that each witness, or set of witnesses for his existence, taken separately, might have had good reason for lying. His argument was directed against the independence of the witnesses, and is based upon the assumption, which is sound as far as it goes, that the unanimity of different witnesses goes for nothing if it can be shown that they had a common and paramount interest in lying. The difficulty of establishing the thesis increases, of course, with the number and variety of the witnesses; and if the number is small, and all are bound together in a common interest and a common character, it may well be established; and thus do counsel often try to discredit the corroborative evidence of witnesses in courts of law. But when, as in the case of Napoleon Buonaparte, the witnesses are innumerable, and are of the most divergent interests—friends and foes, admirers and contemners, rich and poor, natives and foreigners, beneficiaries and sufferers,—the attempt to discredit them all must be hopeless. No one familiar with the history of the time can really doubt that Napoleon Buonaparte existed; and the belief is as assured and certain as any empirical belief can be. We can no more doubt it than we can doubt that trees grow upward, or that unsupported bodies fall downward.

Our belief, that is to say the belief of stay-at-homes, in the existence of India, rests upon similar grounds, and is similarly assured and unassailable. We have never been there: we have never seen it: we have no experience of it; but we cannot doubt it. We can no more doubt it than we can doubt the existence of our own parish or our own home. The belief rests upon no experience of our own: it rests entirely upon hearsay; but upon the hearsay of witnesses innumerable, independent, and unanimous. It is the accumulated evidence of at least five generations of men. The witnesses belong to many countries, many classes, many occupations, and have many, and often conflicting interests. They are thus completely

independent of one another. And they are unanimous. No one has set out to find India and come back to deny its existence. We believe it implicitly, and we ought to believe it. The evidence is sufficient.

But however numerous, unanimous, and independent the witnesses to an assertion, we ought not to believe it if it plainly contradicts our own plain experience. If ten thousand men of integrity and character should unanimously assure me that the sun gives no light, or that it rises in the West and sets in the East, or even that on but one portentous occasion it did so, I should not believe them; and I ought not to believe them. It might be said that an occasion so bizarre could never occur, and that it is futile to make such a supposition; but it is not futile. No such number of persons have ever made this particular assertion, it is true; but a very large number have made, and continue to make, assertions that contradict quite as flatly experiences quite as constant. For instance, every writer of a book on Logic, and their name is Legion, for they are very many, asserts that the only form of proposition is the proposition which has 'is' or 'are' for its principal verb; and virtually that this is the only verb in use in any language. I, being familiar with many verbs, and finding many verbs used by every one of the writers who assert that there is only one, refuse to believe this, and rightly refuse. So, too, every writer on Logic declares that every act of reasoning consists in bringing a particular instance under a general rule, or proceeds through a universal, as he calls it. As I know of multitudes of modes of reasoning which are not thus constituted, and in which there is no universal; and as logicians admit that there are arguments in which they cannot find a universal, though they have been searching for it for two thousand years, I refuse to entertain this belief. In fact, I could not if I tried. The unanimous testimony of innumerable logicians does not weigh a featherweight with me against incontrovertible experience. Again, innumerable alienists testify unanimously that madness and unsoundness of mind are the same thing; but when I find many forms of unsoundness of mind that are quite compatible with sanity, and frequently occur in the sane without disturbing their sanity in the least, I do not believe, and cannot believe, the testimony of the alienists, even though they are very many, and they are unanimous.

In the last two cases, those of the logicians and the alienists, it will be seen that although they are numerous and unanimous, yet the third element is wanting—they are not independent, and this it is that vitiates their testimony. The logicians are not independent of one another, for they have all drunk of the same fount; they have all been indoctrinated with the same belief from the same ultimate source; they have all learnt the same silly system; and none of them has had sufficient independence of mind to trust to his own experience rather than to authority. It is much the same with the alienists. They have all been taught the same false doctrine with the same air of assurance as if it were an axiomatic certainty, and none of them has taken the trouble to compare the teaching with his own experience. No doubt the retention of these beliefs in the teeth of plain and frequent experience to the contrary is partly due to intellectual inertia, or, to use a plainer term, laziness; partly to timidity of authority, or, to use a plainer term, cowardice; but it is also largely due to that influence of all upon each which is one of the penalties we pay for the benefits of social life. It is difficult to maintain a belief, or to reject a belief, against the unanimous opinion of our fellows—of those of our fellows with whom we are associated. It is the tyranny of what 'They say' that quells our opposition. These beliefs of the logician and the alienist rest upon the same basis as the belief that it is unlucky to spill the salt, or to cross the knives, or to view the new moon through glass, and a hundred other such absurdities. You can no more persuade a logician that he is constantly constructing, and asserting, and denying propositions with active verbs, or an alienist that he is constantly witnessing disorders of mind that are not insane, than you can persuade a seafaring man that it is not unlucky to go to sea on a Friday, or a rustic that it is not unlucky for a hare to cross his path. Superstitions are not assailable by reason, nor do they depend upon evidence; and counter-evidence has no effect upon them.

NOTE ON THE MEANING OF 'FACT.'—Strictly speaking, a fact is a thing done, and means 'that which has happened'; and in this sense I have defined and used it in previous writings. In this book I have somewhat extended the meaning of the word, and the extension needs justification. The

extension to that which exists, or has existed, and also to that which happens or is happening, needs but little justification, and will, I think, be generally allowed. That which exists has come to exist by way of some happening; and though it is not itself, strictly speaking, that which has happened, it is the result of that which has happened; and the same is true of what has existed. There would be little or no impropriety in speaking of the existence of the earth or of Julius Cæsar as a fact. The real need of justification is for the extension to the future. Can we justifiably speak of that which will certainly happen as a fact? Manifestly, in the strict meaning of the term we cannot. But there is no other word that will cover both what has happened and what is about to happen, and a word to cover them both is wanted. I have therefore taken this liberty with the word 'fact' in this essay, and for the present purpose; but in other connections I should still use it in its strict sense.

Sir Clifford Allbutt takes me to task for speaking of the 'fact' of gravitation. This, he says, is an illegitimate use of the word, and an instance of the detestable misuse, which I deprecate as much as he does, of the term 'fact' for the term 'theory.' Gravitation, he would say, is not a fact, but a theory to account for facts. The facts are that ponderable bodies move towards each other, and we account for this movement, this fact, this actual happening, by the theory that they attract each other. Manifestly he is right, and at first I was inclined to confess *aliquando dormito;* but on retracing the course of my thought, I find the use defensible. As explained in the text, we have no direct knowledge of fact. All that we have direct knowledge of is evidence; but when the evidence is conclusive, it is legitimate shorthand to speak of our knowledge as if it were knowledge of fact. Now, if ponderable bodies do attract each other, that is fact: that is what happens; and in any individual case of attraction, such as a heavy body falling to the ground, the appearance of falling is evidence of the fact of falling; and the fact of falling is evidence of the attraction that produced the fall. And in the latter case the evidence we now have is as conclusive as in the former. The fact-in-itself we do not know: we know only the evidence for it; but the evidence that the body falls is conclusive, and therefore we may speak of the fall as a fact; and I submit

that the evidence of gravitation is quite as conclusive, and that we may, without undue straining of the meaning of the word, speak of gravitation also as a fact. At any rate, we may so speak of it in any individual case.

Summary.

The different meanings of 'believe' are defined, and the meanings of various cognate expressions explained. An assertion of any degree of belief or disbelief expresses an attitude of mind either directly towards a fact, or, while directly towards a statement, indirectly towards the fact stated.

A fact means anything existing or happening, in the past, present, or future.

Belief ought to conform to fact, but cannot be directly related to fact, for we have no direct knowledge of fact. Between belief and fact there is always the intermediary of evidence. It is evidence and not fact that impresses our minds, and when we have brought our belief, or the want of it, into accordance with the evidence, we have done all we can, and can do no more.

Evidence is of three kinds:—Evidence of sense, evidence of reason, evidence of hearsay.

Evidence of sense is certain as to the sensation only; but sensation is of little value until it is interpreted, that is, until its source or cause is arrived at by the elementary process of reasoning called perception. This process may be faulty, and the percept false, or erroneous.

Evidence of reason gives us two criteria of certainty. That which cannot be conceived is certainly false, and its contradictory is certainly true, and constitutes an axiomatic truth or certainty. It is necessary, in using this test, to be careful not to confuse, as Mill and Spencer did, inconceivability with incredibility.

Empirical certainty rests upon constancy in experience. That relation which has been found constant (*i.e.* never contradicted) in experiences diverse and incalculably numerous, is true for us, and cannot be believed to be false, although its contradictory may be conceivable.

If the relation is not constant in experience, then the degree of belief ought to correspond with the proportion that the

positive instances in experience of the relation bear to the negative instances, in which the terms of the relation occur apart. The more nearly constant in experience the relation, the more carefully should apparent exceptions be scrutinised.

Evidence of hearsay may be maximally trustworthy or may be worthless. The following are the criteria to be depended on :—

(1) The statement must be understood in the same sense by the receiver as by the assertor.

(2) The witness must be a witness of truth so far as he knows the truth.

(3) The witness must have means of knowing the truth.

(4) The hearsay evidence must not be inconsistent, or even incongruous, with experience.

Whoso makes an assertion, on him lies the burden of proof. No attention should be paid to bare assertion unsupported by evidence.

Evidence is anything germane to the issue, and consistent with the assertion.

Proof is evidence inconsistent with any alternative to the assertion.

The evidence of a single witness may be received in proportion to his previous record for truthfulness, and in proportion to his responsibility, that is to say to the ill-consequences that would accrue to him if he were found to have given false testimony; also to his freedom from interest and bias in making his assertion.

The evidence of a plurality of witnesses is valuable in proportion to their independence of one another. Evidence of many independent witnesses goes to prove an assertion if they have means of knowing the truth, and if the assertion is consistent with experience. Otherwise, the evidence of witnesses, however many and however unanimous, has no value.