On causation : with a chapter on belief / by Charles A. Mercier.

CAUSATION AND BELIEF

CHARLES A. MERCIER

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Had I not continually exercised my judgement, the greater part of the books on these subjects would have turned my brain. This effect they have certainly had upon many who have not used the same precaution. I know the advantage which I might derive from perplexing the understanding by recurring to abstruse reasoning and logical quibbles. But I wave it all. I shall speak nothing but common sense, and what may be understood by anyone, however slender his acquirements.

—*Horne Tooke.*

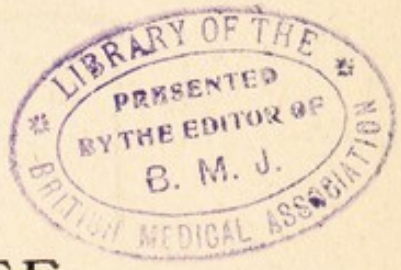
I myself frequently meditate by myself long and intently; but in vain; unless I find an antagonist, I have no hope of success.—*Scaliger.*

ON

CAUSATION

WITH

A CHAPTER ON BELIEF



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TO THE MEMORY

OF

THE BEST WIFE EVER MAN HAD

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PREFACE.

EXASPERATED by the fatuity of an expert in heraldry whom he was cross-examining, Sir William Harcourt at length exclaimed: 'Why, the silly man does not understand even his own silly business!' The reader of a book on orthodox Logic is constantly tempted to make the same comment. Every book on Inductive Logic contains a chapter in which an attempt is made to investigate the nature of Causation, to define it, and to explain how causes are ascertained and assigned; but why Causation should be considered subject-matter of Logic, any more than rotation or imitation, is hard to understand. The proper task of Logic is to describe and explain the principles and methods of reasoning, and causation is not a principle or method of reasoning, nor is the definition of causation or the ascertainment of causation a principle or method of reasoning. These are applications of reasoning. They are examples of reasoning. The results are arrived at by methods of reasoning, but they are not themselves methods or principles of reasoning, and are, therefore, no part of Logic. Mill says, and all subsequent writers have followed him, that causation lies at the very root of Induction. It does nothing of the sort. It is one of very many relations that may be discovered by Induction, but it is no more the basis of Induction than rotation or imitation is the basis of Induction.

However, logicians have appropriated to themselves the examination of causation, and it is not surprising, therefore, that its true nature has never been discovered, and that the

subject is entangled in confusion and contradiction; for it is thus that logicians leave the subjects they investigate. Mill is the model and great exemplar, as well as the leader of latter-day logicians, and though it may almost be said that men of all sorts take a pride to gird at him, yet it may also be said that he is not only confused and muddled in himself, but the cause of confusion and muddle that are in other men. He enumerates five Methods of Experimental Inquiry, and he calls them four, and in seventy years not one of his commentators has discovered the inaccuracy; some of his most important terms, such as effect and condition, he never defines at all; others, such as cause, causation, and conditionality, he defines over and over again in senses that are different, incongruous, or inconsistent; his Canons for discovering causes are cumbrous, uncouth, and clumsy in expression, and in meaning are absurd. They never have been used, and never could be used. It is time, therefore, to take the matter out of the hands of logicians, and investigate it by the light of common sense.

Everyone has an approximate notion, good enough for most working purposes, of what is meant by causation, and by cause and effect, but no one has been able to put that notion into a verbal expression that will stand criticism, and some of the attempts to do so have resulted in expressions that are preposterous beyond belief, as will appear when they are examined. It may seem that if we know what we mean with sufficient accuracy for working purposes, this is enough, and we need not strive to attain pedantic precision; but apart from the general desirability of defining our terms, the approximate accuracy which is enough for rough working purposes is not enough when subtle, intricate, and important problems have to be determined. Issues involving the determination of causes are frequently brought before Courts of Law, and of late years such issues have become much more frequent in connection with causes of disease, of death, of accident, and of injury. In trying such cases, judges have expressed the embarrassment they have suffered from the want of a trustworthy definition of cause. Many nice points of causation have lately come before the Courts, and have been decided in the absence of

any clear or precise notion of what causation consists in, without that guidance from philosophers which judges have a right to expect. They have looked to philosophers for light and order, and they have found Cimmerian darkness and primæval chaos.

Nor is it only in the determination of individual issues that a knowledge of the nature of causation is important in law. A definition of causation, or at least a clear knowledge of what causation means and is, is the root and the basis of one very important department of law, reference to which is made in every case that is tried in the Courts. It is the basis of the Law of Evidence. According to that very high authority, Mr. Justice Stephen, the facts that may be proved in Courts of Law are the facts in issue, and those facts that are relevant to the issue, and he defines relevancy thus: 'A fact is relevant to another fact when the existence of the one can be shown to be the cause, or one of the causes, or the effect, or one of the effects, of the existence of the other,' etc. Clearly, then, to determine what facts are relevant, and this has to be determined many times in the course of every trial, a knowledge of what is meant by causation, and of the nature of cause and of effect, is necessary. Mr. Justice Stephen, in fact, says that his work on Evidence is founded on Mill's Logic, and that a previous work on the Law of Evidence is founded upon Locke's 'Essay.' As to this previous work, I can give no opinion, but I am sure that Mr. Justice Stephen was mistaken when he said his work was founded upon Mill's Logic, for his treatise on the Law of Evidence is as clear and consistent as Mill's Logic is the opposite. Mr. Justice Stephen's admission is important, however, as showing that in his opinion the Law of Evidence does need a foundation in a proper apprehension of Causation.

In other important matters also the need for a clear notion of the meaning of cause and effect is imperative, and the want of it leads to grave disadvantages. The instructions issued by the General Register Office for assigning the causes of death are such that no doctor can understand them, and their unintelligibility is owing to the want of a definite notion of cause. The causes of insanity published in the annual tables of the Board

of Control are mostly guesses ; some of them are manifestly not causes at all ; others may or may not be causes, but no reason is given why they should be so considered ; and in the absence of any definition of a cause, and of any trustworthy method of assigning causes, no reason could be given.

It is always assumed by writers on the subject that the only investigations that are worth making into the methods of assigning causes are investigations into the methods pursued by scientific workers, and that result in scientific discoveries. These writers, following Mill, formulate five methods, which, as I have said, they count as four, which they say are used by scientific workers. Scientific workers, however, never use these methods, and could not use them, for they are utterly futile, as will hereinafter appear. Moreover, the assumption that the methods employed by scientific workers to discover causes are in any respect different from the methods employed in every-day life by the cook, the gardener, the plumber, and the rest of us, is quite groundless and mistaken. Men who work at science have no monopoly of methods of discovering causes. Their methods are not novel or peculiar, but are the same as those that we all constantly use in the course of our daily lives. For this reason I have not followed the course usual in books on Causation, of restricting my illustrative instances to examples of discoveries in science.

The chapter on Belief has been added at the request of a friend who, like most of us, has found himself often puzzled what to believe and what to disbelieve. It makes no pretensions to philosophical profundity, and to those who are accustomed to the ponderous tomes that have been written on the foundations of belief, and upon epistemology generally, it will appear, I am afraid, a trifling performance. These books, however, are scarcely accessible to the general reader, and if they were, it is doubtful whether he would take advantage of them. Some work accessible to him and intelligible by him is sorely needed. It is curious that in an age that prides itself before all things upon being scientific, there are as many prevalent beliefs that are irrational, baseless, absurd, and self-contradictory, as at any former time of which we have any record.

CHAPTER I.

SOME THEORIES OF CAUSATION.

IN the whole of philosophy, confused as it is, there is scarcely any subject in such utter confusion as causation. There are references to it in the writings of his predecessors, but Hume was the first writer of note who discussed it at length, and he got it into a tangle which has been worse and worse entangled by subsequent writers, until the latest contributors to the discussion have essayed to cut the knots by denying altogether that there is such a thing as causation at all. Few writers treat the subject without contradicting themselves, and none without outraging common sense, a result which does not trouble them, for the first qualification for a philosopher is to set common sense at defiance. The consequence is that no one who retains any remnant of common sense can rise from the perusal of a discussion of causation without a feeling of dazed perplexity. He finds long discussions in which the cardinal terms are used in several different senses, and are either defined in several different ways or never defined at all. He finds things that are quite distinct, such as cause, condition, and agent, confounded together; he finds problems that are quite distinct, such as the nature of causation and the universality of causation, confounded together; and through all the discussions runs the difficulty inherent in examining and defining a notion that is almost primitive.

Primitive notions are by their very nature impossible to

define or explain satisfactorily. They can only be described, and even description is not always easy or always satisfactory. Matter cannot be described except in terms of force, nor force except in terms of matter. It is manifest that defining and explaining more complicated notions in terms of simpler notions cannot be continued indefinitely. The process reaches its natural limit when at last we come to notions of primitive simplicity, just as the chemical analysis of substances reaches its natural limit when we have at last reduced them to elements. The notion of causation is almost elementary. Cause and effect, like matter and force, are terms that everyone understands more or less vaguely, more or less precisely, but that it is difficult to express more simply for want of simpler terms. At any rate it has been found impracticable hitherto to express them, for every effort that has been made to do so has resulted in an expression that is either more obscure than cause and effect themselves, or that does not truly express what they mean.

Dr. Fowler says 'That a cause is . . . ; that every event has a cause; that the same cause is always attended by the same effect; are obviously three different propositions, and still there are few writers who in their treatment of the question of causation have not more or less confounded them.' This is quite true, and he might have added a fourth—we derive our notion of causation from . . . or the origin of our notion of causation is . . .

It is this fourth proposition that is the main theme of Hume's discussion, and he arrived at the conclusion, which is no doubt correct, that we get our notion of causation from witnessing repeated instances of it—that, in fact, as we should say now, it is a generalisation from many individual experiences. So far no doubt he was right; but he went on to assume, and his whole argument rests upon the assumption, that because the notion of causation is a generalisation from repeated experiences, therefore causation itself does not exist in isolated or single instances, and, in fact, does not exist at all, but is a mental fiction, without any corresponding relation in fact.

The common sense doctrine that Hume undertook to de-

molish is 'that the idea of causation necessarily implies the idea of *power* or *necessary connection*, that is to say, between the cause and the effect, or *power* in the cause to produce the effect.' He set himself to show that power and necessary connection had been illegitimately imported into the idea of causation, and that what we call cause and effect is nothing but casual antecedence and consequence. Antecedence and consequence are all that we ever observe, or can observe; but when we have witnessed many instances of the same antecedent being followed by the same consequent, we jump to the conclusion, without any justification for doing so, that there is between them a tie other and more than bare sequence—that there is a power in the antecedent to bring about the consequent, and a necessary connection between them. Thus Hume teaches.

Briefly put, his argument is that all our ideas are in the last resort analysable into simple ideas, which are themselves copies of impressions or original sentiments, by which he seems to mean what we now call percepts. 'These impressions are strong and sensible. They admit not of ambiguity.' Such are solidity, extension, and motion, each of which we can perceive, so Hume teaches, in a single experience; 'but the power of force . . . is entirely concealed from us, and never discovers itself in any of the sensible qualities of body.' He means, apparently, that we cannot see it: 'It is impossible, therefore, that the idea of power can be derived from the contemplation of bodies in single instances of their operation; because no bodies ever discover any power which can be the original of this idea.' Since, then, we obtain the notions of force or power and necessary connection, not from single experiences, but by generalisation from many experiences, these notions are fictitious, imaginary, and have no basis in fact, neither have they any existence except in our own misguided imaginations. This is Hume's doctrine.

It is very curious that this doctrine should have been practically accepted by every writer since Hume's time, and that no present-day philosopher should have detected any of the fallacies in it. Modern psychologists are pretty familiar, I

should have thougnt, with the doctrine that every one of our concepts of the simplest properties of bodies—solidity, extension, motion, and the rest—is a generalisation from many experiences, and is in no case derived from a single instance, but is slowly built up in our early years under the guidance of experience. As far and in the same way as solidity, extension, and motion are revealed to us by experience, so far and in that way is force or power; and if force or power is not revealed in a single instance, neither is existence, extension, or motion. The only force that exists wholly in the imagination, and is without any counterpart outside it, is the force of Hume's argument.

'The generality of mankind never find any difficulty in accounting for the more common and familiar operations of Nature, such as the descent of heavy bodies . . . but suppose that, in all these cases, they perceive the very force or energy of the cause by which it is connected with its effect and is for ever infallible in its operation. They acquire, by long habit, such a turn of mind that upon the appearance of the cause they immediately expect, with assurance, its usual attendant, and hardly conceive it possible that any other event could result from it.' They do, undoubtedly; but are they not justified in so accounting for these operations of Nature? What is the test? What is the inexpugnable, infallible test? It is that, acting on this supposition, they should never meet with experience that contradicts it; and is not this test satisfied? Hume says that force or power is never revealed in a single instance; but, when the mind has been prepared by previous experiences to entertain the notion, is not the single instance of carrying a bucket of water sufficient to reveal the force or power of the weight of the bucket? If a breaking wave, thundering upon the beach, and carrying away cartloads of shingle in the undertow, does not convey the idea of force or power; if a hurricane, uprooting great trees, unroofing houses, and whirling haystacks into the air, does not convey the idea of force or power; if an avalanche, carrying away woods and villages, and diverting the course of torrents, does not convey the idea of force or power; then no 'contemplation of any body in

single instances of its operation' can afford any idea of any description.

Hume denies that we derive the idea of power from subjective experience, from finding 'that by the simple command of the will we can move the organs of our body or direct the functions of our mind.' He denies it on the ground that 'we learn the influence of our will from experience alone, and experience only teaches us how one event constantly follows another; without instructing us in the secret connection which binds them together and renders them inseparable.' But why should it? We might as well deny that we derive from experience the idea that glue sticks to wood, because we know it from experience alone, and experience does not instruct us in the secret connection which binds the glue and the wood together and renders them inseparable.

Thus he summarises his conclusions: 'It appears that, in single instances of the operation of bodies, we never can, by our utmost scrutiny, discover anything but one event following another, without being able to comprehend any force or power by which the cause operates, or any connection between it and its supposed effect. . . . All events seem entirely loose and separate. One event follows another; but we never can observe any tie between them. They seem *conjoined*, but never *connected*.' Thus he virtually denies causation altogether, and, as we shall see later, recent writers accept this conclusion, and bring it forward as original with themselves; but it is clear that this is Hume's position, though he never actually puts it into these words. Having arrived at this conclusion, which is a virtual denial that there is any such thing as causation, he admits that when a man has observed several similar instances of such conjoined events he 'can readily foretell the one from the appearance of the other'; and then Hume astounds us by defining a cause as '*where, if the first object had not been, the second had never existed*.' It would be difficult to put the necessary connection between them in stronger terms, and Hume seems frightened at having made the admission, for he begins at once to hedge, and offers another, his third, definition of a cause: *an object followed by another, and whose appearance*

always conveys the thought to that other. Thus he removes the reference from the world of things to the world of thoughts, and places the matter on an entirely different basis. At length he concludes: 'I know not whether the reader will readily apprehend this reasoning. I am afraid that, should I multiply words about it, or throw it into a greater variety of lights, it would only become more obscure and intricate.' In this he is no doubt right. His argument is based on a premiss that is thoroughly unsound, and leads to a conclusion that is repugnant to universal experience, and that he is himself compelled to repudiate. However, the mischief was done. He opened the floodgates of confusion, and his successors have ever since been floundering in the swamp.

Mill's whole treatment of the problem of causation is a most deplorable muddle, and that he should have been regarded as an oracle for two generations is a startling proof of the poverty of critical acumen and philosophic insight that has prevailed since his Logic appeared. It is evident on the most superficial perusal of his chapters on the subject that he has never thought it out; he wanders on from conjecture to surmise, and from surmise to conjecture, stating his surmises and conjectures as inexpugnable facts; he defines cause and causation over and over again in eighteen different ways, most of them inconsistent with each other, and some of them contradictory of others, and neither he nor his commentators and followers recognise the inconsistencies or the contradictions. The only explanation of his astonishing and overwhelming reputation is that amongst the blind the one-eyed is king; but even Mill's one eye was purblind.

Mill first states Hume's doctrine in its bare nakedness: 'The Law of Causation . . . is but the familiar truth, that invariability of succession is found by observation to obtain between every fact in Nature and some fact that has preceded it.' It may be noted in passing that however familiar and however true this may be, it is certainly not found by observation, and Mill's study of Hume should have warned him not to make so absolute an assertion; for Hume says very truly 'on the discovery of extraordinary phenomena, such as

earthquakes, pestilence, and prodigies of every kind, they find themselves at a loss to assign a proper cause,' and there are still innumerable facts in nature which baffle all our attempts to discover their causes. However, Mill goes on: 'To certain facts, certain facts do, and, as we believe, will continue to succeed. The invariable antecedent is called the cause; the invariable consequent, the effect.' He does not recognise that this statement differs very materially from the former. First he says that every fact has an invariable antecedent, and then he says that every fact has an invariable consequent, and he regards the two assertions as equivalent. In his next statement he goes back to his first position, and says: 'The universality of the law of causation consists in this, that every consequent is connected in this manner [invariably] with some particular antecedent, or set of antecedents.' In this he airily gives away Hume's whole position, and introduces a new and vitally important element, without in the least recognising that he is doing more than restating his previous doctrine. The antecedent now not only invariably precedes the consequent, but also is connected with it, a doctrine which Hume positively denies, and which, when introduced into what is virtually a restatement of Hume's doctrine, requires at least some justification or explanation; but none is given.

As is well known, Reid demolished Hume's definition of causation as invariable succession by adducing the case of night and day. Night invariably follows day, and day invariably follows night, and yet neither is the cause of the other. Clearly, some qualification and addition is necessary, and Mill, though he gives the expressions quoted above as complete and sufficient statements of the nature and relation of cause and effect, evidently recognises that some qualification and addition is required, and supplies one, in fact, he supplies a good many, not as successive approximations to a complete definition, not as tentative proposals to be discarded when found inappropriate, but all of them as final and complete definitions, which are immediately superseded by others, which are superseded in their turn.

It is very common, he says, when there are many antecedents

(as if there were ever an effect that had not many antecedents, and he does not say invariable antecedents connected with the consequent, though presumably he means such antecedents) to single out only one of them under the denomination of cause, calling the others merely conditions. 'But though we may think proper to give the name of cause to that one condition, the fulfilment of which completes the tale, and brings about the effect without further delay; this condition has really no closer relation to the effect than any other of the conditions has.' This leads him to his fourth definition, different from all the rest. 'The cause, then, philosophically speaking, is the sum total of the conditions, positive and negative taken together; the whole of the contingences of every description, which being realised, the consequent invariably follows.'

Having given this final definition of what the cause is, philosophically speaking, he discusses it further, and finds that it won't do. He now finds it necessary 'to advert to a distinction which is of first-rate importance,' which, in spite of its first-rate importance, has been omitted from his previous definitions. Invariable sequence is not synonymous, he now finds, with causation, unless the sequence, besides being invariable, is also unconditional; and this he says immediately after he has defined the cause as 'philosophically speaking,' the sum total of the conditions. It is, therefore, philosophically speaking conditional, and speaking otherwise unconditional. This leads him to his fifth definition, according to which a cause is 'the antecedent, or the concurrence of antecedents, of a phenomenon, on which it is invariably and *unconditionally* consequent.' Still dissatisfied, as well he may be, he tries again, and gives a sixth definition, 'which confines the meaning of the word cause, to the assemblage of positive conditions without the negative, and then, instead of unconditionally, we must say "subject to no other than negative conditions"'; and if this does not satisfy, he has 'no objection to define a cause, the assemblage of phenomena, which occurring, some other phenomenon invariably commences or has its origin.' So that after asserting in the most positive terms that invariable sequence is not causation unless the sequence, besides being invariable is also uncondi-

tional, he now drops unconditionalness, and goes back without a word of apology to invariable sequence.

It would be tedious and unprofitable to examine any further the mass of confusion and contradiction contained in Mill's exposition of causation, but lest it should be thought that I have at all exaggerated, I will set down here a series of extracts from his Logic.

He prefaces his discussion of causation with the following warning: 'The notion of cause being the root of the whole theory of Induction [it is not], it is indispensable that this idea should, at the very outset of our inquiry be, with the utmost practicable degree of precision, fixed and determined.' This he says, and more than two hundred pages later he is still altering his definition of cause; more than three hundred pages later he alters his definition of causation. This is how he fixes and determines his notion of cause with the utmost practicable degree of precision:—

'The Law of Causation . . . is but the familiar truth that invariability of succession is found by observation to obtain between every fact in Nature and some other fact which has preceded it.' I, 376.

'The invariable antecedent is termed the cause, the invariable consequent, the effect.' I, 377.

'If it [the fact] has begun to exist, it was preceded by some fact or facts with which it is invariably connected.' I, 377.

'The real Cause is the whole of those antecedents.' I, 378.

'All the conditions were equally indispensable to the production of the consequent; and the statement of the cause is incomplete unless in some shape or other we introduce them all.' I, 379. Condition is not defined.

'The cause, then, philosophically speaking, is the sum total of the conditions, positive and negative, taken together; the whole of the contingencies of every description, which being realised, the consequent invariably follows.' I, 383. Contingency is not defined.

'It is necessary to our using the word cause, that we should believe not only that the antecedent always *has* been followed

by the consequent; but that, as long as the present constitution of things endures, it always *will* be so.' I, 391.

'That which will be followed by a given consequent when, and only when, some third circumstance also exists, is not the cause, even though no case should have occurred in which the phenomenon took place without it.' I, 392.

'Invariable sequence, therefore, is not synonymous with causation, unless the sequence, besides being invariable, is unconditional.' I, 392.

'We may define, therefore, the cause of a phenomenon, to be the antecedent, or concurrence of antecedents, on which it is invariably and *unconditionally* consequent'; or

'The antecedent, or the concurrence of antecedents, on which it is invariably and subject to no other than negative conditions consequent'; or

'The antecedent, or the concurrence of antecedents, in which it is invariably and whatever supposition we may make about other things, consequent.' I, 392.

'The series of the earth's motions, therefore, though a case of sequence invariable within the limits of human experience, is not a case of causation.' I, 394.

'I have no objection to define a cause, the assemblage of phenomena, which occurring, some other phenomenon invariably commences, or has its origin.' I, 397.

'There is no Thing produced, no event happening in the known universe, which is not connected by an uniformity, or invariable sequence, with some one or more of the phenomena which preceded it.' I, 400.

'The state of the whole universe at any instant, we believe to be the consequence of its state at the preceding instant.' I, 400.

'The law of causation is, that change can only be produced by change.' I, 407.

'In this example we may go further, and say, it is not only the invariable antecedent but the cause.' I, 450.

The *cause* of it, that is, the peculiar conjunction of agents from which it results.' I, 511.

That which would not be followed by the effect unless

something else had preceded, and which if that something else had preceded, would not have been required, is not the cause, however invariable the sequence may in fact be.' II, 37.

'Fresh causes or agencies.' II, 38.

'The uniformity in the succession of events, otherwise called the law of causation.' II, 108.

From these dicta we may extract the following definitions or descriptions of cause, and in repeating them I will put in italics the words which are discordant or incongruous with previous utterances.

A cause is :

(1) The invariable antecedent.

(2) The preceding fact with which the effect is invariably *connected.*

(3) *The whole* of the antecedents.

(4) All the *conditions.*

(5) The *sum total* of the conditions.

(6) The whole of the *contingencies.*

(7) The antecedent which not only always *has* been followed, but that always *will* be followed by the consequent ; although

(8) That which always has been and always will be followed by the consequent is *not* necessarily the cause.

(9) The invariable *unconditional* antecedent.

(10) The antecedent on which the effect is invariably *and subject to no other than negative conditions* consequent.

(11) The antecedent on which the effect is invariably consequent *whatever suppositions we may make about other things.*

(12) *The assemblage of phenomena,* which occurring, some other phenomenon commences or has its origin.

(13) *The peculiar conjunction of agents* from which the consequence results.

(14) *An agency.*

Causation, or the Law of Causation, is :

(1) Invariability in succession.

(2) Invariable *and unconditional* sequence.

(3) *Uniformity* in the succession of events.

(4) *That change can only be produced by change.*

In addition to the discordances in these definitions, account must be taken of the following pairs of assertions :

'Causation is invariability of succession.'

'The series of the earth's motions, though a case of sequence invariable within the limits of human experience, is not a case of causation.'

'The cause is the invariable antecedent.' 'The invariable antecedent is the cause.'

'That which would not be followed by the effect unless something else had preceded, and which if that something else had preceded, would not have been required, is not the cause, however invariable the sequence may in fact be.'

'Causation is invariability of succession.'

'Invariability of sequence is not synonymous with causation unless the sequence, besides being invariable, is unconditional.'

The majority of writers since Mill have followed rather slavishly in his footsteps, but a few recent writers have struck out more independent courses, and some of these must be examined. I confine the examination to the writings of Mr. Welton, Prof. Karl Pearson, Mr. Bertrand Russell, and Dr. McTaggart.

Mr. Welton accepts Mill's doctrine that the cause is the sum of the conditions, though he prefers to call it the totality of the conditions, but he rejects altogether the time factor, or antecedence and consequence, which every previous writer on the subject considers a necessary ingredient in our concept of causation. 'The cause,' he says, 'is not dependent on time sequence. For if we analyse any case of causation we find that time sequence is not an essential aspect of it.' I am not so sure. *Gutta cavat lapidem.* The continual dropping of water wears away a stone, and surely this takes time. The ploughing, harrowing, and sowing of the ground are causes of the subsequent harvest, but the harvest is not simultaneous with these operations. It gradually matures for months, and not until months have elapsed is the effect produced. The administration of an excess of food causes a pig to grow fat, but the pig does not instantaneously explode into a state of obesity. Perhaps, however, in giving these examples I should be tripped

up by the expression, 'essential aspect.' What an essential aspect may be I do not know, but whatever it is, I find it hard to reconcile Mr. Welton's assertion with his subsequent assertion that the fact to be accounted for is change. Change, he says, implies something which changes. So it does, but it implies something else also. It implies duration. We speak of instantaneous changes, but in fact and in Nature there is no such thing. Change implies duration. It implies an antecedent state from which, and a subsequent state to which, the change takes place. If the fact to be accounted for is change, which Mr. Welton says it is, and which it is sometimes, then causation does imply sequence in time, and time sequence is an 'essential aspect' of it, if by an essential aspect of it Mr. Welton means a necessary factor in it.

But he has another reason for rejecting time sequence as a factor in causation. We cannot, he says, find the explanation of change in preceding change; for that would simply land us in infinite regress; by which he means that for each cause we must find a preceding cause, and so *ad infinitum*. I do not see the necessity. In following the chain of causes backward we can stop where we please, and we usually have a good reason for stopping at a certain point; but supposing that time sequence in causation does land us in infinite regress, why not? There is nothing inconsistent with our knowledge of the universe in supposing that the causes of any change go back to an infinity of past time. Infinite regress is no argument against the time element in causation. Mr. Welton might as well say that the explanation of night and day cannot be found in the rotation of the earth, for that would simply land us in a movable earth. No doubt it would, and what then?

Instead of sequence in time, Mr. Welton presents us with contiguity in space as the necessary element, or, as he calls it, the essential aspect, of causation; for, he says, it is only under the form of space that we can rationalise our experience of the influence of bodies on each other. I must confess I cannot fathom this cryptic reason. I do not know what the form of space is, nor do I know how to rationalise an experience; but

if by essential Mr. Welton means necessary, and if by contiguity he means contact, or even nearness in space, of an acting body to a body acted on and consequently changing, then I deny altogether that contiguity is essential to causation. The instance that must at once occur to everyone is the action of an astronomical primary in causing the motion of its satellite to pursue a certain path. Mr. Welton sees this, and his way out of the difficulty is a very extraordinary one. 'How,' he says, 'can we conceive a causal influence exerted on an object distant in space from the agent; as *e.g.* that of the sun on the planets? In reply to this it must be said that in one very true and important sense of its reality a body must be thought to be where its influence is felt: the power of exerting influence is one of its properties, and where, therefore, that power is felt there the agent truly *is* in this, the only applicable sense. Of course in another sense of its reality—the sense in which reality is identified with visible and tangible form and tangible resistance—the body may be absent, but then that aspect of its reality is, in this case, beside the mark.' If Mr. Welton succeeds in deceiving himself by thus juggling with the word reality, the abracadabra of the philosophy that is made in Germany, I know not whether he is more to be envied or pitied, but I am very sure that he will not deceive anyone else who has any appreciation of the meaning of words. He might as well say that the German Emperor is omnipresent throughout Central Europe, for that is where his influence is felt. He might as well say, when a drunken man gives his companion a black eye, that in a very true and important sense of its reality the drink is in the black eye, for that is where its influence is felt. Of course, in another sense of its reality the drink is absent from the eye, but then that aspect of its reality is beside the mark on the eye. Mr. Welton is, I am afraid, not so thoroughly Germanised as he tries to make out. No truly Germanised philosopher would spell the word Reality without the capital, which makes it so much more imposing. It will not, however, impose upon anyone who looks to the meaning of words.

Cause and effect, says Mr. Welton, are not successive, but

simultaneous; and to prove this he instances the formation of water. 'The cause of the formation of water is the combination in definite proportions of hydrogen and oxygen, but this combination does not precede the formation of water, it *is* that formation.' Of course it is. He is juggling with words again. The formation of water is the same thing as the combination of the two gases. That is a truism. It is an identical expression. It is expressing the same thing in two different sets of words. But the combination of oxygen and hydrogen, which is the formation of water, is not the cause of that formation. The cause of the formation, or of the combination, is first the mixture of the gases, and then the passage of a spark through them. And though the formation of water and the combination of the gases is simultaneous, if, that is to say, a process can be said to be simultaneous with itself, it is not simultaneous with the mixing of the gases, nor is it simultaneous with the passage of the spark. The mixing of the gases may precede the combination by days, months, or years; and though the combination follows very rapidly on the passage of the spark, they are not simultaneous. The combination begins in the neighbourhood of the spark, and spreads throughout the mixture, and this spreading takes time—a very short time, it is true—but it takes time. The passage of the spark is antecedent, the formation of water is consequent.

'So it is,' says Mr. Welton, 'in every other case.' I agree to this extent, that in every other case of change in which he makes out that the cause is simultaneous with the effect, either what he calls the cause is not the cause, or what he calls the effect is not the effect.

Mr. Welton continues thus: 'We, then, arrive at this. Cause and effect are not two but one.' So we advance from contiguity in space to simultaneity in time, and from simultaneity in time to identity! How a body, supposing, as Mr. Welton supposes, that a cause can be a body, can be contiguous in space to itself, I do not know. I suppose that is another aspect of its Reality. The question that arises in my mind is whether the body is beside itself, or whether the person who makes the assertion is beside himself.

A dropping of ink, says Mr. Welton, upon paper causes a blot, but the blot is there as soon as the contact of ink and paper is made : it *is* that contact. But on his own showing it ought not to be. What he says is that cause and effect are one, but the one he takes is neither cause nor effect. The cause is the dropping of the ink : the effect is the blot. If cause and effect are one, the blot ought to be the dropping of the ink ; but Mr. Welton says it is not. It is the contact of the ink with the paper. Such confusion and self-contradiction could scarcely be found outside a book on logic. By a parity of reasoning, when a man gets into bed, the getting into bed *is* the man, or, if we take Mr. Welton's second alternative, which he does not recognise as an alternative, but asserts as the same thing, then the contact of the man with his bed *is* the man. It ought not to be necessary to clear up such a very simple matter, but seemingly it is necessary to point out that the blot is not the contact of the ink with the paper : the blot is the layer of ink in contact with the paper. And this layer of ink on the paper does not appear simultaneously with the dropping of the ink, it follows the dropping of the ink. The blot is not on the paper until the dropping is arrested by the paper, is over and done.

The fact to be accounted for, says Mr. Welton, is change ; and the first example of causation that he adduces is that the weight of the atmosphere is the cause of the height of the mercury in the barometer. But the height of the mercury in the barometer is not a change. Quite the contrary. The fact to be accounted for in this case is not change, but the absence of change. The fact to be accounted for is that the mercury in the barometer does not sink. Perhaps the explanation is to be found in another aspect of Reality, and it may be that in a very true and important sense of its reality the absence of change is the same as change. It is perhaps a Reality of Identity, or an Identity in Reality, such as Mr. Bradley and Dr. Bosanquet delight in.

'We, then, arrive at this,' says Mr. Welton, 'cause and effect are not two, but one. That they are inseparable is indeed recognised by the relativity of the terms themselves. A cause

without an effect, or an effect without a cause, is a contradiction in terms and unthinkable.' So it is, but it is not more unthinkable than a cause which is identical with its effect, or an effect which is identical with its cause. 'But we must go further,' says Mr. Welton, 'and say that in *content* they are absolutely identical. It is only in *form* that they can be distinguished.' Here is the hoof—it is not a cloven hoof, but a soliped—of Germanism again. Content is another of its shibboleths or abracadabras. Content and form, reality and identity, are its stock-in-trade, they are the four hoofs on which it goes. Lug them in by head and shoulders, use them in any sense or nonsense that you please, mix them up anyhow, and you will pass for an up-to-date philosopher. Mr. Welton confines his illustrations to cause and effect, but it seems a pity so to limit the application of such a fertile philosophical principle, and I rejoice in being able to extend it to other pairs of relatives. Parent and offspring are not two but one. That they are inseparable is indeed recognised by the very relativity of the terms themselves. A parent without offspring, or an offspring without a parent, is a contradiction in terms, and unthinkable. But we must go further, and say that in *content* they are absolutely identical. It is only in *form* that they can be distinguished. And the same is true of higher and lower, outside and inside, murderer and victim, robber and robbed. In *content* they are absolutely identical. It is only in *form* that they can be distinguished. How charming is divine philosophy!

If cause and effect are not two, but one; if they are absolutely identical (I leave out content, for I do not know what the content of a cause is, or how it can have any content. A cause is not a box or a bag); if, I say, they are absolutely identical, how idle it is to seek for causes or for effects! The main occupation of the whole human race, ever since it attained the status of humanity, is founded on a chimæra. What is the cause of the alternation of day and night? That silly man, Copernicus, thought he had discovered it. What is the cause of the spout of blood from a severed artery? The stupid Harvey thought he had discovered it. What is the cause of the suppuration of wounds, of pyæmia, of septicæmia? The

foolish Lister pretended that he had discovered it. What is the cause of malarial fever? of earthquakes? of Brown's success in growing roses? of Jones' failure to secure the hand of Miss Robinson? What is the cause of mimicry in animals? What makes the days warmer in summer than in winter?

> What makes the price of corn and Luddites rise?
> What fills the butchers' shops with large blue flies?

And finally, what is the cause of philosophers writing nonsense? Nothing could be clearer. Nothing could be plainer or more manifest. The chief, the most important, the most absorbing occupation of mankind has always been the search for causes. What folly! The causes were under their noses all the time. They saw the effects, and the effects are absolutely identical with the causes.

Another recent writer on the subject is Prof. Karl Pearson, whose *Grammar of Science* has achieved a popularity remarkable for a work of the kind. It is disfigured by much uncouth phraseology, and by the Papal infallibility that the author claims for his own doctrines, which he attributes to a personified science. On nearly every page he speaks of 'a routine of experience,' a 'routine of sense impressions,' a 'routine of perceptions.' These are his fundamental terms, but he never defines them, and we are left to conjecture what he means by them. Far on in the book he speaks of the routine of perceptions as equivalent to 'the uniform order of phenomena,' and 'the uniformity with which sequences of perception are repeated'; but whether this is another name for causation, or whether it is merely our old familiar friend the Uniformity of Nature, we are left in doubt. Even if he does mean the Uniformity of Nature, we are no better off, for no two philosophers agree on what is to be meant by the Uniformity of Nature. The only thing on which they agree, and when they do agree their unanimity is wonderful, is that Nature is not uniform.

Much of the authority that Prof. Pearson's *Grammar of Science* has unquestionably achieved is due to his habit of attributing his own opinions to a personified science, a trick that enables him to pose as infallible, while adroitly avoid-

ing the appearance of arrogance that such a pose carries with it. When he says that for science cause is meaningless, he means that Prof. Pearson does not understand the meaning of it; when he says that science can in no case demonstrate this or that, he means that Prof. Pearson cannot demonstrate it; when he says that science can find no element of enforcement in causation, he means that Prof. Pearson is too blind to see the element of enforcement; and so on. This is an adroit method of imposing on the gullibility of his readers, for who, in these 'scientific' days, would have the temerity to question the pronouncements of science? But I must confess to some surprise that it has been so successful. I should have thought that it might have occurred to some one that science in this sense is only a name for a body of opinion; a body of fluctuating opinion, varying from time to time and from person to person, so that what is science to-day was heresy yesterday, and will be superstition to-morrow; what is science to one is stupidity to another, and falsehood to a third. What is science to Prof. Pearson, for instance, is nonsense to me.

Professor Pearson belongs to the school of Hume and Mill, and with them denies that there is any 'enforcement' of an effect by its cause, or any necessary connection between them. The cause is merely the antecedent, the effect merely the subsequent. The one happens to follow the other, but there is no reason or necessity why it should do so: they are in no way connected; but when we see repeated instances of the same succession of events, we deludedly jump to the conclusion that the predecessor is the cause of the successor. Almost as soon as it was stated, Reid blew this doctrine sky high by adducing the instance of night and day. Day always precedes night, and night always follows day, but no one supposes that day is the cause of night or that night is the effect of day. And why not? Manifestly because they are merely antecedent and subsequent; because there is no power in day to produce night; because there is no enforcement of night by day. Prof. Pearson bases his repudiation of enforcement on practically the same ground as Hume does, *viz.*, that our notion of force is purely imaginary, and has no counterpart in the world outside

our imagination. In this he confuses, as Hume does, imaginary with conceptual. Our concept of force, like all our concepts of primitive things, such as motion, resistance, extension, duration, and so forth, is a generalisation from many experiences of individual instances ; and if we are to discard the one because it is conceptual, that is to say, a generalisation, then we must discard the rest for the same reason. In that case our minds are left blank, and reasoning is impossible for want of pabulum. In contradiction to this doctrine it is enough to appeal to universal experience. By cause we do not mean mere antecedence, nor by effect do we mean mere succession. If we did, we should accept day as the cause of night, and night as the effect of day. If we did, the old and notorious fallacy, *post hoc, ergo propter hoc*, would be no fallacy : it would be an unassailable truth ; yet the same logicians who declare in their Chapters on Causation and Induction that causation is nothing but sequence, declare in their Chapter on Fallacies that it is fallacious to argue from *post hoc* to *propter hoc*. But no inconsistency or self-contradiction in a doctrine ever yet deterred logicians from teaching it ; and no doubt they will continue to teach this self-contradiction along with the rest, until the whole silly pseudo-science is swept away, and goes to join Judicial Astrology, Phrenology, and Humoral Pathology upon the rubbish heap. In forming our idea of cause and of causation, the enforcement of the effect by the cause enters as an inseparable and necessary element into the notion, and if that element is extruded, that which appeared to be a cause is a cause no longer. 'The necessity,' says Prof. Pearson, 'thus lies in the nature of the thinking being, and not in the perceptions themselves ; thus it is conceivably a product of the perceptive faculty.' How it can be a product of the perceptive faculty and not be a percept or perceived ; how that can be perceived which is purely imaginary, and has no sensory impression as a basis or provocation to perception, Prof. Pearson does not inform us. His psychology is as hazy as his notion of causation.

However, Prof. Pearson goes with the crowd, and quotes as from Mill the definition that causation is uniform antecedence ; and this definition, says Prof. Pearson, is perfectly in accord

with scientific concept—that is, with Prof. Pearson's concept. It may be a good definition, but when Prof. Pearson says it is John Stuart Mill's definition, he is mistaken. Among all of Mill's many definitions of cause and causation this one is not to be found. In this instance 'science' is at fault.

'For science,' that is, for Prof. Pearson, 'cause, as originating or enforcing a particular sequence of perceptions, is meaningless—we have no experience of anything which originates or enforces something else.' The most obvious answer to this is that it is not true. It contradicts the whole experience of the whole human race. Every time we move a thing from one place to another we demonstrate the falsity of the assertion. The word 'originating' is used equivocally. A change in anything is originated when the change begins; that is, when the thing begins to change. But it seems from the context that Prof. Pearson denies that change—the sequence of perceptions, as he calls it—is then originated, because it can always be traced to previous change, and therefore in this sense it is not 'originated.' This is an obvious confusion. The particular change in the thing changing is none the less originated, although it may be the effect of some previous change in something else. What Prof. Pearson means is that the total sequence of changes never originates, or, as I should say, begins. It is the same difficulty that Mr. Welton calls infinite regress, and which he takes as a conclusive argument against the time element in causation, while Prof. Pearson takes it, with equal inconsequence, as an argument against causation itself. In so far as it is an argument at all, it is as much an argument against the existence of change as against the existence of causation, or of a time element in causation; but it is no argument against either. Grant that change generally, apart from individual changes, never begins, but can be traced back until it is lost in the infinity of past time, still that is no argument against causation. It merely shows that every cause has itself a cause; and so far from abolishing causation, it renders causation more than ever certain, and necessary, and universal. But I need not labour the argument, for Prof. Pearson has himself refuted it. On

p. 9 he says, 'the man who has accustomed himself to marshal facts, to examine their complex mutual relations, and predict upon the result of this examination inevitable sequences.' Here he is evidently referring to himself, and if a sequence is inevitable, it is enforced; it is necessary; it is not the mere casual sequence that he says causation is. To say that a sequence is inevitable, and to say that it is enforced, is to say the same thing in different words.

However, Prof. Pearson sees what Hume did not appear to see, and what Mill certainly did not see, that if we take away from causation the element of enforcement, or of power in the cause to produce the effect, causation vanishes with it, and the only logical attitude is to deny altogether that there is any such thing as causation. To this necessary result of their teaching, Hume and Mill were blind; but Prof. Pearson sees it, and Mr. Bertrand Russell sees it, though they both see it only in transitory and occasional glimpses, and for the most part lose sight of it. They both deny that causation exists, and they both define what it is—not what it means, but what it is. Prof. Pearson asserts that the 'category of cause and effect' is a fetish; that the law of causation is a figment; that no experience demonstrates causation; that for science, that is to say, for him, cause is meaningless; and he asks whether causation is anything but a conceptual limit to experience, a cryptic question that, for my own part, I am unable to answer until I know what it means. Having said this, he says he will show how antecedents are true scientific causes; he states the law (which, by the way, is nonsense, as he himself in another place shows, though he endorses the law) that the same set of causes is always accompanied by the same effects; he says that no phenomenon has only one cause; and he even goes so far as to say we fail to comprehend a world to which the conception of cause and effect would not apply. How he reconciles these contradictions in his own mind I shall not speculate, but I am very sure that he will not succeed in reconciling them in the mind of anyone else, except, perhaps, in the minds of Mr. Bradley and his followers or in the mind of a German of the school of Hegel.

The most popular doctrine of Prof. Pearson's is his distinction between *how* and *why*, a distinction which is either the cause, or the chief effect, of his theory of causation. He denies that we can ever discover *why* a thing happens, or explain it; and limits our powers to saying *how* it happens, or describing it. In this he is demonstrably wrong. It is often as impossible to describe how things happen as to explain why they happen: it is often as easy to explain why they happen as to describe how they happen. The fact is that both *how* and *why* are equivocal words, having more than one meaning; but whichever meaning we take, what I have said is true. *How* may mean in what manner, or it may mean by what means. *Why* may mean for what purpose, or it may mean in obedience to what law, in conformity with what rule. In any of the four cases the answer may be easy, or difficult, or impossible; and as to either *how* or *why*, we may be able to answer one meaning and not the other. If, for instance, we ask how, in the sense of by what means, gravity acts, we cannot answer. It is impossible to imagine by what means a body can attract another through an immeasurably great distance. It is only when we ask how, in the sense of in what manner, gravity acts that we are able to answer that it acts inversely as the square of the distance. If we ask why, in the sense of with what purpose, the sap circulates in the tree, we have no difficulty in explaining that it is that the sap may be aerated, the tree nourished, its life maintained, and its growth increased. It is only when we ask why, in the sense of in conformity with what law, the sap circulates, that we are unable to answer. We do not know whether it is capillary attraction or what it is.

A good example of the manner in which Prof. Pearson poses as a superior being is the advice he gives to his readers, to analyse what is meant by such statements as that the law of gravitation *causes* bodies to fall to the earth. The law, he says, really describes how bodies do fall. Of course it does; but before Prof. Pearson gave this advice to his readers, he should have shown some evidence that some one besides himself had ever said such a silly thing. As far as I know, no one has ever pretended that the *law* of gravitation

causes bodies to fall to the earth ; but if anyone should say that the *fact* of gravitation—the fact that they attract each other—causes bodies to fall to the earth, he would say what is exactly and punctually true. The law of gravitation describes *how* bodies fall : the fact of gravitation explains *why* they fall ; and the explanation is as good and as valid as the description. As far as I know, Prof. Pearson never answers the actual arguments of real antagonists ; and if he prefers the easier task of answering silly arguments that he puts into the mouth of an imaginary antagonist, then, whatever we may think of his courage and sincerity, we cannot question his wisdom.

Mr. Bertrand Russell follows Professor Pearson in denying the existence of causes. He says there are no such things. He wants the word abolished, and regards the law of causation, or, as he calls it, of causality, as a relic of a bygone age. To prove this contention he selects from Baldwin's *Dictionary* the definitions given therein of Causality, of the notion of Cause and Effect, and so forth ; he takes one of Mill's definitions of Causation, and an expression of Bergson's, and analyses them all destructively.

All these expressions assume, and Mr. Russell repeatedly in his own expressions assumes, that repetition of instances is necessary before we can identify causation, and I think it is not too much to say that he regards recurrence or repetition as a necessary element, either in causation itself, or in our idea of causation. The definitions that he quotes all countenance this supposition. They run : *Whenever* the cause ceases to exist ; *whenever* the effect comes into existence ; the Law of Causation is *invariability* of succession ; the *same causes* produce the *same effects* ; a certain phenomenon will not fail to *recur* ; and so on ; and he himself says that an ' event ' in the statement of the law is obviously intended to be something that is likely to recur ; and he makes this the basis of his criticism. Criticism directed against such notions of causation, however destructive of them it may be, is not relevant against a definition of cause or of causation into which the element of repetition or recurrence does not enter. To me, repetition or recurrence is not a necessary ingredient, either of causation

itself, or of my idea of causation, and therefore against my definition Mr. Russell's attack is not directed ; but even against the definitions that he does attack, erroneous as I believe them to be, his criticisms do not appear to me to be destructive, or even damaging.

Thus he confutes the succession in time of cause and effect, or that antecedence and consequence on which Mill and his school lay so much stress : 'No two instants are contiguous, since the time series is compact.' I cannot see that the conclusion follows from the premiss. It seems to me that the more compact the time series, the more closely contiguous must be its instants. If Mr. Russell means that time is continuous, and not made up of instants separated from one another by intervals that are not time, or in which there is no time, I should agree with him ; but it is only in such an interrupted time series that the instants would not be contiguous. An instant, like an hour or a day, is a portion of time arbitrarily divided by an imaginary limit from that which precedes and that which follows, with both of which it is continuous or contiguous. But if Mr. Russell is right, and no two instants are contiguous, and if serial contiguity in time between cause and effect is necessary to causation, then this settles the question : then causation is impossible, and Mr. Russell's further argument is redundant, supererogatory, and unnecessary. But he does not think so, for he goes on : 'Hence either the cause or the effect or both must, if the definition [Baldwin's definition of Cause and Effect] is correct, endure for a finite time . . .' I agree that both the cause and the effect must endure for a finite time, though I do not see how this follows from the supposition that no two instants of time are contiguous. 'But then we are faced with a dilemma : if the cause is a process involving change within itself, we shall require (if causality is universal) causal relations between its earlier and later parts ; moreover, it would seem that only the later parts can be relevant to the effect, since the earlier parts are not contiguous to the effect. Thus we shall be led to diminish the duration of the cause without limit, and however much we may diminish it, there will still remain an earlier part

which might be altered without altering the effect, so that the true cause, as defined, will not have been reached.' This may or may not be an effective criticism of a definition of cause and effect that defines them as contiguous in time, but to me it is too much like the old problem of Achilles and the tortoise to be convincing. Zeno proved quite satisfactorily that Achilles could never overtake the tortoise—only he did ; and Mr. Russell proves less satisfactorily that there is no such thing as causation, but yet he, in common with the rest of us, always acts on the supposition that there is such a thing, and, so acting, he never meets with experience that contradicts the supposition ; and this is for us the conclusive and inescapable proof, first that the supposition is true, and second that Mr. Russell is convinced that it is true.

He goes on to show that if cause and effect are not contiguous in time, then there must be an interval between them ; and 'since there are no infinitesimal time intervals' this lapse of time must be finite. But if there is a finite interval of time between cause and effect, something may happen in that interval to prevent the effect following the cause. It is all very pretty word spinning, and for all I know it may apply to the kind of 'causality' that occurs in the moon, or in a universe of one dimension, but it has no relation whatever to causation as it is known on this earth. Mr. Russell assumes that effect follows cause in the sense of what carpenters call a butt joint, in the sense that the effect does not begin until the cause has ceased to act. That may be what happens in some other universe, but it is not what happens here. What happens here is quite different, as Mr. Russell might have known if he had considered an actual case of causation instead of speculating with e_1, e_2, . . . e_n, and t_1, t_2, . . . t_n, and τ. When, for instance, a man pushes a trolley, he causes it to move. The pushing is the cause, the movement is the effect. But the effect is not postponed until the cause has ceased to act. The effect does not come suddenly into existence at an instant contiguous to the cessation of the cause. The effect begins as soon, or almost as soon, as the cause begins ; thereafter, cause and effect, the pushing and the movement, accom-

pany one another, and proceed contemporaneously for a certain time; and at length, when the cause ceases, the effect ceases. Cause is contiguous to effect in this case, not end to end, but side by side for the greater portion of their duration. The joint is not a butt joint but a fish joint; and all Mr. Bertrand Russell's pretty word spinning goes for nothing.

His own statement of 'causality,' cannot, he says, be put accurately in non-mathematical language; the nearest approach would be as follows: 'There is a constant relation between the state of the universe at any instant, and the rate of change at which any part of the universe is changing at that instant, such that the rate of change in the rate of change is determinate when the state of the universe is given.' It is with diffidence that I comment on this mysterious formula, but it seems to me clear that if anything can be discovered by its means, it is not the cause of a change, but the rate at which a change takes place, or rather the rate of change in a rate of change; which may be a desirable thing to know, but by no perversity of ingenuity can be twisted or tortured into a cause. But suppose the impossible to be true, and suppose that no cause of anything can be discovered or assigned unless and until the state of the whole universe is known; then it is clear that no cause of anything ever has been discovered or ever can be discovered, for we can never know the state of the whole universe. But in fact many causes of many things are known, and more are being discovered every day. I know, for instance, that pushing a trolley is a cause of the movement of that trolley. I know that reading such disquisitions as Mr. Welton's, Professor Pearson's, and Mr. Bertrand Russell's, are among the causes of the estimate I have formed of philosophers. Mr. Bertrand Russell may be a great mathematician, Professor Pearson a great statistician, and Mr. Welton a great authority on education; but there is a certain proverb about the cobbler and his last that I would commend to the notice of all three. It may be that I must determine the state of this earth, and of everything upon it, in it, and around it; of all its continents, seas, rivers, lakes, and islands; of all its minerals, from the coal

to the diamond ; of all its vegetables, from the bacillus to the oak and the orchid ; of all its animals, from the spirochæte to the whale ; of all its human inhabitants, from the Bushman to Mr. Russell himself ; and beyond this, of all the solar system, with its planets, planetary streams, satellites, and comets ; of all the stars which we call fixed, with their temperatures, positions, sizes, movements, and chemical composition—it may be that I must know all these things with accuracy before I can discover what it is that is tickling my nose ; but for my own part I don't believe it. In fact, I do not know all these things, I know only some of them, and I have already discovered the cause. No doubt Mr. Bertrand Russell knows best, but my own private belief is that though mathematics cannot err, mathematicians can.

The last view of causation that I shall examine is Dr. McTaggart's, which I select because it is the latest to be published, having appeared only last July. Like Mr. Russell, he calls it causality, which, to be sure, is a more imposing term ; but sometimes he fails to maintain the philosophical nomenclature, and drops back into common causation. For thorough mystification, and for the most extreme departure from plain meaning and common sense, Dr. McTaggart runs Mr. Bertrand Russell very hard. According to Dr. McTaggart, 'causation is a relation of implication between existent realities—or to put it more precisely, between existent substances.' This does not on the face of it afford us much help in understanding what causation is, but unlike most philosophers, Dr. McTaggart defines his terms, and for this one cannot be sufficiently grateful to him, not only on general grounds, but also for the surprising meanings that he shows lurk unsuspected in the most ordinary terms. A substance, for instance, according to Dr. McTaggart, is anything that can have qualities and relations ; so that, for instance, the battle of Waterloo and a flash of lightning are substances in the McTaggartian sense. This is a bit startling, but definitions are so rare in philosophy that we must be thankful for any we can get, even if they leave us more mystified than before. The battle of Waterloo is presumably not only a substance but also an existing substance in the McTaggartian world, though to the

rest of us it ceased to exist a hundred years ago. Causation, then, is a relation of implication between such existing substances as the battle of Waterloo and a flash of lightning; but what is a relation of implication? Here again Dr. McTaggart comes to the rescue with a definition. A relation of implication is a relation between two propositions, P and Q, such that P implies Q, when, if I know P to be true, I am justified by that alone in asserting that Q is true, and, if I know Q to be false, I am justified by that alone in asserting P to be false.

So far, so good, but still we are a long way from attaining a clear idea of causation; but Dr. McTaggart is not done yet. 'Strictly speaking,' he says, 'implication is a relation between propositions or truths [is a proposition, then, necessarily true?] and not between events. But it is convenient to extend our use of it, so as to say that if one proposition implies another, then the event asserted in the first implies the event asserted in the second [but how if neither of them asserts an event?]. It is in this sense that the cause implies the effect'—causes it, in fact. The jump from propositions to events is a bit startling to those who are not accustomed to the proper meaning of realities and substances, but interpreting these expressions to the best of my ability, I gather that when we say the cause implies the effect, we mean that if the cause is true the effect is true, and if the effect is false the cause is false. But what on earth is the meaning of a cause or an effect being true or false? It does not appear that by a true cause Dr. McTaggart means the *causa vera* of the Schools, but what he does mean I cannot conjecture; and supposing this difficulty to be cleared up, what is the meaning of a false effect? Is it an effect that never happens? or is it an effect that is wrongly attributed to a certain cause? or is it something else? It is to be regretted that Dr. McTaggart has not supplemented his definitions with others, explaining the meaning of these terms. In this difficulty the only practicable expedient is to clothe the expression in circumstances—to apply the general rule to an individual case.

I take, therefore, two propositions, *Brutus killed Cæsar*, and *Brutus and Cæsar were contemporaries*, which stand in a rela-

tion of implication ; for if P, or Brutus killed Cæsar, is true, then we are justified by that alone in asserting the truth of Q, that they were contemporaries ; and if Q, or Brutus and Cæsar were contemporaries, is false, then we are justified by that alone in asserting the falsity of P, that Brutus killed Cæsar. This specimen fulfils all Dr. McTaggart's conditions. The relation is undoubtedly a relation of implication ; and the killing of Cæsar by Brutus is a substance, for it can have qualities, such as treachery, unexpectedness, rapidity, and so forth. It does not seem to me to be an existing substance, it is true, but it is as much an existing substance as the battle of Waterloo. The contemporaneousness of Brutus and Cæsar is a relation, and therefore this also is a substance, and to the same extent as the other is an existing substance. All the conditions being satisfied, we may therefore predicate a relation of causation between these two existing substances; but now our difficulties begin, for I cannot understand whether the fact that Brutus killed Cæsar caused them to live at the same time, or whether the fact that they were contemporaries caused Brutus to kill Cæsar. If the latter, why did not all his other contemporaries kill Cæsar? and why did not Cæsar kill Brutus? If the former, what caused Brutus and Cæsar to have so many other contemporaries? I have puzzled over these problems till my brain is almost turned, and I am no nearer a solution, and am obliged to give them up. I doubt whether anyone but Dr. McTaggart could solve them ; and a method which is useless in the hands of everyone but its inventor is never likely to become popular.

Dr. McTaggart arrives at certain other conclusions that are interesting. He decides that there is no reason to believe 'that a cause exerts an activity or an effect.' What is meant by a cause exerting an effect I do not know, and another definition would be useful here ; but if Dr. McTaggart means that a cause does not produce an effect, then I respectfully submit that it is not a cause. Moreover, if a cause does not exert an activity, it is only because it *is* an activity, or more properly an action. Cause and activity can no more be divorced than heat and motion, or solidity and resistance. Dr. McTaggart decides

that cause and effect are not identical, a discovery that will not, I think, astonish anyone but Mr. Welton ; that the effect is not necessarily subsequent to the cause, and, indeed, he is not quite sure that the effect may not sometimes come first, and the cause follow after it ; and at last he declares, in despair it seems to me, that though cause and effect are not identical, yet there is no means of knowing which is which, or at any rate, there is no clear distinction between them ; and therefore, though we may speak of causal relations as existing between two terms, yet we ought not to speak of one of those terms as cause, and of the other as effect. I think we may legitimately complain that Dr. McTaggart does not tell us what we ought to call them. Ought we to call them both X, or the one X and the other Y ? Ought we to call the one beef, and the other Yorkshire pudding? Or ought we to call the one petticoat and the other trousers ? Dr. McTaggart gives us no guidance, and the reader must choose for himself.

The lecture in which Dr. McTaggart expounded these views was delivered at Newnham College, presumably to an audience of young women, and I trust he developed to them his views of the impropriety of naming the related terms when describing relations. He convinced them, I trust, that it is convenient to speak of the relation of marriage, but inconvenient (and perhaps improper), to speak of bride and bridegroom, or of husband and wife ; that it is convenient to speak of parentage, but not of parents or of children ; that it is convenient to speak of the relation of cousinhood, but that they should never allow themselves to use such expressions as Harry or Mary.

In concluding this survey of certain theories of causation, I beg to assure the reader that they are stated with accuracy, in the *ipsissima verba* of their authors. They are not garbled, altered, or modified in any way. Everything material has been stated, and nothing has been mis-stated. They are not the theories of Laputa, nor are they the ravings of Bedlam. They are not jokes, nor are they intended for caricatures. They are the serious attempts of philosophers of position and repute to solve a simple problem that every ploughman and artizan, though he may not be able to put his solution into words, has

solved in practice for ages. Carlyle, in his genial way, characterised a certain philosophy as pig-philosophy. I should qualify the philosophers' treatment of causation with the name of another domestic animal, unlike a pig in that its hoofs are not cloven, nor its long ears drooping.

My view is that when we common people who are not philosophers speak of causation, and, as we do in spite of Dr. McTaggart's warning, of cause and of effect, we attach to these words very positive and downright meanings. We feel and know that in seeking for causes, in noting effects, in trying to identify causation, endeavours that occupy the greater part of our lives, we are not pursuing an *ignis fatuus*, but we are doing that without which it is impossible for men to live profitably, nay, it is impossible for them to live at all. If we have no very clear notion of what we mean by cause, effect, and causation, this want of precision, which is largely due to the fog in which they have been enveloped by philosophers, does not interfere with our practical pursuit of them. If the plain man, immersed in practical affairs, cannot precisely define what he means by these terms, neither can he define precisely the meaning of capital, of labour, of rent, of interest, of life, of death, of disease, or of hundreds of other terms that he uses in his daily work, and that represent things of the utmost moment to his welfare, his happiness, and his life. But because he cannot define them, is he therefore to say that the things they stand for have no existence? that they are empty words, that represent nothing outside his own misguided imagination? This is the conclusion to which philosophers are driven by their inability to define cause and effect. On the same ground, and for the same reason, they should deny the existence of life and death. This is the result of living in the moon, and ignoring all the efforts of the toiling millions of mankind. The only way to discover the meaning of cause and effect is—to find out what men mean by them; and we shall not do this by word-spinning; by pretending a difference between connection and conjunction; by denying the existence of force; by contradicting ourselves twenty times over; by calling sequence simultaneity, and simultaneity identity; by posing oracularly

as embodied science; by ingenious puzzles about the divisibility or indivisibility of time; or by defining that which is easy to understand by that which is impossible to understand. No. To find the meaning of cause and effect, and of cognate terms, we must come out of the moon, and go, not merely into the laboratory and the observatory, but into the home, the kitchen, the workshop, the factory, the garden, the field, and all the busy haunts in which men and women are all day long seeking causes, studying effects, and watching the course of causation.

Summary.

Hume's denial that force or power exists, and that there is any connection between cause and effect, is based on faulty reasoning, and in the light of modern psychology cannot be sustained. He himself so defines causation as to assert a necessary connection between cause and effect.

Mill's treatment of the subject is confused, wavering and contradictory. He defines cause and causation many times over, and never adheres to one definition. Generally, he follows Hume in identifying causation with invariable antecedence and sequence, but he does not adhere to this, nor to any, opinion.

Mr. Welton denies that antecedence or sequence, or any time element, enters into causation. In place of the time element he asserts that contiguity in space is necessary to causation. From this he argues that cause and effect are not in sequence, but are simultaneous; and at length decides that they are identical. His reasoning is inconsequent, and his conclusions are opposed to universal experience and to common sense.

Professor Pearson follows Hume and Mill in denying any enforcement of the effect by the cause, and in regarding causation as invariable sequence. He also denies the occurrence of causation, and says it is meaningless; nevertheless, he quotes with approval the law of causation, and asserts that some sequences are inevitable. His treatment of the subject is as self-contradictory as that of Hume and Mill.

Mr. Bertrand Russell, like Prof. Pearson, denies the existence of causation, and like him formulates a law of causation, which

is not a law of causation. It is so expressed as to require, before we can determine what Mr. Russell calls the cause, which is in fact not the cause, of anything, a knowledge of the whole universe.

Dr. McTaggart defines causation as a relation of implication between existing substances. Application of the definition to a test case shows that the definition is absurd, and affords no guidance in practice.*

In conclusion, it is suggested that the inability of philosophers to define causation in consistent and intelligible terms argues, not that causation is imaginary, but that philosophers are incompetent.

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* Nevertheless, a leader of the Germanised school of philosophers refers to Dr. McTaggart's essay in the following terms: "The greater part of what he says possesses, as one would expect from him, an almost convincing lucidity and vraisemblance." Lucidity and vraisemblance! Well, well! And convincing! Heavens!

CHAPTER II.

EFFECT, REASON, RESULT, CAUSE.

THE subject we are about to examine is the relation of causation, and a relation comprises three things—the two terms, and the link that relates them, and unites them in a relation. The link is usually called a relation, which thus becomes an ambiguous term, standing both for the link, and for the triple whole of term—link—term. I have therefore, in my New Logic, called the link the ratio. Mr. Bertrand Russell, in a recent publication, calls it the relating relation, which is possibly a better term, but is at any rate longer. The terms of the relation we are about to examine are Cause and Effect, and the ratio or link which binds them together and unites them in a relation is Causation or Effectuation, according to the point of view from which we regard it. It will be convenient to begin our examination with the terms, and we may select for this purpose either term we please. I shall begin with effect.

The first thing, then, to settle is What is an effect? What do we mean, what do we think of, what have we in our minds, when we use the term effect? I think it is indisputable that the idea of effect is inseparably connected with the idea of change. Changes may be contemplated in and by themselves, as changes and no more; and this is how we contemplate changes to which we are well accustomed, such as the change from day to night, and from night to day, the change from rain

to sunshine, and from sunshine to rain, the changes in the face of the sky, the growth of herbage, the change from heat to cold and from cold to heat, and all the customary changes of Nature. These changes we may, and usually do, contemplate merely as changes, without feeling any compulsion or need to regard them as effects also, or to look behind them for their causes. But then these changes are, in a sense, not changes *to us*. They are parts of a routine, a changing routine, but a routine whose changes are customary, and part of the routine; a routine that, as a routine, does not change, or changes but little. In such changes the change to us is minimised, and the greater change would be if the regular routine should cease to change. The changes that are changes to us, that impress us as changes, are not the regular customary changes of the routine, but the breaks in the routine. But any change that impresses us as change, any break in our customary routine of changes, especially if it is rapid, and more especially if it is sudden, carries the mind irresistibly to the notion of cause, and impresses us as an effect. In such cases change is identified with effect, or, if not identified, is inevitably associated with effect. It is true that in contemplation we can separate them. We can contemplate a change either as change pure and simple, or as effect; but though separable in contemplation, in occurrence they are inseparable. Just so we may contemplate gold without taking into account its specific gravity, or we may contemplate it with reference to its specific gravity; but whether we choose so to contemplate it or not, we know that its specific gravity is inseparable from it. Whether we regard a change as simply a change, or whether we regard it also as an effect, or whether we regard it primarily as an effect, depends on the way we choose to contemplate it. How close is the association between change and effect is conspicuously displayed in the case of an unaccustomed noise. When we hear a noise, especially a sudden and loud noise, to which we are unaccustomed, the natural and inevitable reaction is What's that! And in putting to ourselves this question, we do not mean, as the form of the question seems to imply, What is the nature of that noise? That we already know. Our meaning is What is the cause of

that noise? Instantly and inevitably the mind passes from change to cause, and regards the change as an effect; and so it is with every change to which we are unaccustomed, that is, with every change that impresses us as change.

On the other hand, we do not, except in special cases that will be examined directly—we do not seek for a cause for things remaining unchanged, or regard want of change as an effect. If, upon waking in the morning, or on entering a room, we find the position of the furniture and all the other objects the same as when we last saw them, we do not look upon their unchanged position as the effect of anything, or seek for a cause for it. When we come home after an absence, and find the house, the trees, the bushes, the lake, and the distant hills, all as we left them, we do not associate this want of change with causation, nor do we regard it as an effect. It needs no accounting for, no attribution of cause.

This is the general rule. Every change may be contemplated as an effect, and will be so contemplated in proportion as it is unusual, for unusualness is what logicians would call the essence of change; that is to say, it is the element in change that attracts our attention, and impresses us. It is what to us constitutes change. A change that happens continually soon ceases to be contemplated as change. It becomes to us a continuity, and the change to us is when it stops—when the clack of the mill ceases, when the roar of the streets subsides, when the train arrives at the terminus. But if we choose so to regard it, every change is an effect.

It does not follow, however, that every effect is a change. As a rule, no cause is assumed for the want of change, or for things remaining the same; but this rule has very important exceptions, constituted by the circumstances we have just considered. There are cases in which we do assume a cause for the retention by a thing of its state unchanging, cases in which we regard the absence of change as an effect. There are two such cases.

When a change is customary, and yet does not take place, we assume that the absence of change is the effect of some cause. The weather, for instance, in this country changes so

frequently, and change in the weather is become so much a part of our customary routine, that when a change in the weather takes place, we forget to regard it as an effect; but should the weather remain uninterruptedly stormy, or dry, or wet, for six months together, we should at length be driven to assume a cause for this want of change, for the want of change would be itself a change in the routine to which we are accustomed.

The second case is when we know of forces in operation tending to produce a change which yet does not take place. In such a case, if our attention is called to the operation of such forces, we inevitably assume a counter-cause for things remaining unchanged, and regard this want of change, or unchange, as an effect. If we pull the handle of a drawer, and the drawer yields and opens, we regard the change in the position of the drawer as the effect of the pull; but if we make no attempt to open the drawer, we do not regard its remaining closed as the effect of anything. As there is no change, and nothing tending to produce change, there is no effect. But if we pull the drawer and it does not move, then the want of change, in circumstances tending to produce change, at once becomes an effect, and carries the mind irresistibly to the necessity of a cause. When the mercury in a cup remains level, we do not regard the maintenance of the level as an effect, for it is no change from the customary behaviour of mercury; but when the mercury in a Torricellian tube remains high above the level of that in the cup, we do at once assume that this is the effect of some cause; for the unchanging state, or briefly the unchange, is maintained in spite of a cause—the weight of the mercury—that we know is tending to change it.

We are driven by these considerations to regard change as a necessary element in our concept of effect, and if we first formulate the definition that

An Unchange is the maintenance of an unchanging state in spite of forces in operation tending to change it,

Then we may formulate our provisional definition of effect in the following terms:

An Effect is a change or an unchange.

Reason.

Between these two kinds of effect there is a clear difference, which is easily distinguished, which is generally felt, and which is, in fact, embodied in language; for while we always call that which produces a change the Cause of the change, we usually do not give this title to that which opposes a change. This latter we usually call a *Reason*. The variations in the height of the barometer are *caused* by variations in the pressure of the air; but the constant pressure of the air is the *reason* why the mercury does not sink to the level of the cup. The pull we exert on the handle of the drawer is the *cause* of the drawer opening: the drawer being locked is the *reason* it does not yield to the pull. It would be quite inappropriate to say that the changes in the weather are due to some reasons: but it would be quite appropriate to say there must be some reason why the weather does not change. It would seem that the full force of effectuation is felt only when the effect is change, and that when it is unchange the effectuation is felt to be attenuated and diminished; so that we may add to our definitions the following:

The cause of an unchange is called a Reason.

The definition of an effect as a change or unchange is avowedly provisional, and needs to be completed. As already explained, the nature of a thing, as it appears to us, depends on the way in which we contemplate it. We may, if we please, contemplate a change or an unchange in and by itself, merely as change or unchange, without contemplating it as an effect. In order to constitute it an effect, a change or unchange must be contemplated from a special point of view, that is to say, with reference to its causation. To become an effect it must be associated in our minds with causation and a cause; but as we have not yet arrived at any definition of these terms, it would not be legitimate to use them in defining effect. Still, we may legitimately go as far as this: we need not, and do not, always contemplate a change as an effect, but when we do regard it as an effect we always contemplate it in relation with some antecedent action on the thing changed. We need not regard an unchange as an effect, but if we do so contemplate it, we con-

template it in relation with some action that maintains the thing unchanged. We may therefore develop our definition into this:

An Effect is a change or an unchange connected with an action on the thing changed or unchanged.

Still the definition is not complete. A cup may fall and break. The fall of the cup is a change produced on the cup, and is an effect. The impact of the cup on the floor is an action on the cup, and is connected with the fall; but the impact of the floor on the cup is not the cause of the fall; and why not? Evidently because it succeeds the fall. The cause of a change must be sought in some action that precedes the change; it is no use looking among the consequents for the cause. Most writers on causation have been able to appreciate this, and since the cause of a change must always precede the change, they have muddled up causation with antecedence, and declare that they are the same thing. They are not. Antecedence often goes with causation, but there are many cases of causation in which the cause does not precede the effect; and there are many antecedents of a change that are not its causes; and to identify causation with antecedence is a gross blunder, whether the antecedence is invariable or not.

When the mercury in a Torricellian tube remains high above the level of that in the cup, the pressure of the air, which is the action that maintains the unchange, does not precede the maintenance of the unchange, which is the effect: it is continuous with the unchange. They are contemporaneous. When the action of the engine on the axles maintains the motion of the motor car or the locomotive engine in spite of the forces in action tending to arrest the motion, this action does not precede the motion of the car or of the engine, but accompanies it. The tension of a string that sustains a weight, and that is the cause that prevents the weight from falling, does not precede the suspension of the weight: it accompanies it. It begins at the instant of suspension, it lasts while the suspension continues, and it ceases the instant the string is cut and the weight falls. It is true that the drawer may be locked long before and long after it is pulled upon to open it; but it is not the drawer being locked that is the cause of the unchange: it is the resist-

ance of the tongue of the lock; and this resistance begins and ends with the pull upon the drawer.

A time element, or time relation, of one kind or the other is therefore a necessary and indispensable element in the definition of effect, but the time relation is manifestly not the same in the two kinds of effect, and therefore effect cannot be defined in a single expression. The complete definition of effect must run something as follows:—

An Effect is a change connected with a preceding action, or an unchange connected with an accompanying action, on the thing changed or unchanged.

Result.

'Some phenomena,' says Mill, 'are in their own nature permanent; having begun to exist, they would exist for ever unless some cause intervened having a tendency to alter or destroy them . . . no object at rest alters its position without the intervention of some conditions extraneous to itself: and when once in motion, no object returns to a state of rest . . . unless some new external conditions are superinduced. It, therefore, perpetually happens that a temporary cause gives rise to a permanent effect. The contact of iron with moist air for a few hours, produces a rust which may endure for centuries; or a projectile force which launches a cannon ball into space, produces a motion which would continue for ever unless some force counteracted it.'

As usual, Mill founds a general statement upon the *enumeratio simplex*, without taking into consideration the *instantia contradictoria*. It is not true of living animals that they never alter their position without the intervention of some condition extraneous to themselves: the mere internal accumulation of energy is enough. But passing that, and making the necessary qualification, Mill's limitation of the assertion to some phenomena, as if it were not true of all, is utterly unjustifiable. If the first Law of Motion is true, if Mill's own Law of Universal Causation is true, that no event happens without a cause, it is difficult to see how any change can take place in any 'phenomenon' whatever without a cause; and it seems clear that

not some phenomena only, but all phenomena whatever, are in their nature permanent, and having begun to exist will exist for ever, unless some cause intervenes to alter them. Mill adduces these instances as instances of permanent effects; but here he is evidently using the word effect, which he never defines except as an invariable consequent, in a popular sense, and in a sense which even popular usage does not always sanction. According to my definition, a permanent state is not an effect unless it is an unchange; and none of these is an unchange. Once at rest, a body needs no cause to keep it at rest, unless there is some action on it tending to move it; and without such action, its remaining at rest is neither a change nor an unchange, and is therefore not an effect. A body at rest needs a cause to set it in motion, and the setting in motion, the change from rest to motion, is an effect: but once in motion, its continuing in motion is not an effect. When iron rusts, the rusting is an effect, for it is a change from metallic iron to oxide; but once it is rusty, there is no cause in action tending to change it back again, and therefore its remaining rusty is not an effect. In none of these cases does the effect continue. None of them is a permanent effect. What Mill means by a permanent effect is that iron once rusted does not change back again, and that a man once killed does not come back to life again. It is a manifest misnomer to say that if an effect is not reversed, the non-reversal is an effect. It is true that in common speech it is a frequent practice, but by no means an invariable practice, to say that an effect continues, even when the effect is a change, and to speak of the state of death and the state of rust as effects; but these are not accurate expressions, are eschewed by accurate speakers, and are utterly unpardonable in philosophical writing. What persists when a body is brought to rest or set in motion, when iron is rusted, or a man is killed, is not the effect, not the change, but the changed state—the new state that has resulted from the change. A change implies a state from which and a state to which the change is made, and the state brought about by the change is a very different thing from the change itself, which alone is the effect. The changed state is not the effect, it is the *result*, and thus we arrive at a sixth definition:—

A Result is the changed state of a thing on which an effect has been produced.

Cause.

The definition of effect, as a change or unchange connected with an action, points straight to the nature of cause. I do not think it is possible to imagine any change or unchange that is not produced by the action of some agent. Of course, it may be said that things may exist or occur, although we cannot imagine them; but we are not here dealing with transcendental possibilities. We are dealing with events in this world as we know them in experience, and our experience is such that we can no more imagine change to be produced or prevented without action upon the thing changed, than we can imagine resistance without extension, force without matter, or solid without surface. In each case the one presupposes the other. The only consideration that can be plausibly advanced against this view is, I think, that we regard some changes as spontaneous. But by a spontaneous change we do not mean a change produced without action on the changing thing, we mean a change due to the action of the changing thing itself, as contrasted with change due to the action upon it of something outside the changing thing.

The only formal repudiation of this doctrine is that of Hume, which has already been examined. Hume taught that there is no such thing as force or power, which I here call action; that it exists only in our imagination; that the notion we have of it rests upon no evidence, and corresponds with nothing in the external world. His reason for this opinion was that we gain our notion of force or power not from any single individual experience, but as a generalisation from many experiences; and he thought that in this it contrasted with our notions of resistance, extension, and motion. We now know that in this he was mistaken. All such notions are generalisations from many experiences, and the notion of force or power is not singular in this respect, does not differ in this respect from other primitives, nor is it invalidated, as a true representation of externals, by being a generalisation.

But all Hume's discussion of its origin is beside the question. Whatever its origin, it is indisputable that we have this notion of force, or power, or action, and that we regard it as having a real existence in the world outside of us; and the crucial test is this: that we act upon the assumption that it does exist, and that the consistent action, on that assumption, of the whole human race has never brought anyone up against experience that contradicts the assumption. This is the ultimate and unimpugnable test of empirical truth. This test being satisfied, it is quite out of our power to doubt that the assumption is true. We may in words express a doubt, or even a denial, for language was acquired by man in order that he might deny his beliefs; but in fact we do not and cannot doubt it. It is quite possible to deny in words that matter exists, that there is an external world to be appreciated, and that we have minds to appreciate it with; it is quite possible to deny that things that are equal to the same thing are equal to one another; but the test of belief is action; and when we come to act, we act in conformity with the beliefs which we deny, and prove by so doing that our denial is a sham and an imposture—an imposture that does not impose even upon ourselves.

We may take, therefore, as our first provisional definition of a cause:

A Cause is an action.

Though we may speak of change and of unchange in isolation and abstraction from other things, yet in thinking of change or unchange it is impossible to expel from our minds the notion of a thing that changes, or that is prevented from changing. Change and prevention of change alike imply a changeable thing. That which produces change in a thing cannot be thought of otherwise than as an action on that thing either from without or from within. That which keeps a thing unchanged in spite of something that is trying or tending to change it, cannot be thought of otherwise than as an action on or by the unchanging thing. Hence, by a cause not only do we always mean an action, but we always mean an action on a thing. It is quite possible to entertain the notion of action

without taking into account anything acted on, as when we contemplate the rotary action of the arms of a windmill; but when we so contemplate an action we exclude from our minds the notion of cause. Cause always carries with it the notion, not merely of action, but of the transference of action from the acting agent to the thing acted on, or the initiation of action by the changing or unchanging thing; and the notion of cause is not complete unless this transference or initiation of action is taken into account. Hence we arrive at a further stage in our provisional definition of cause:

A Cause is an action upon a thing.

But not yet is our notion of cause complete. We may contemplate an action upon a thing in and by itself, without letting our contemplation run forward to the consequent change or prevention of change in the thing acted on; and unless we do thus extend our contemplation, our notion of cause is incomplete and unformed. When we contemplate the action of a breeze blowing upon a rock, we do not, or need not, regard this action as the cause of any change or unchange in the thing acted on. To complete our concept of cause, we must add to the provisional definition a reference to the change or unchange that is connected with the action on the thing, and develop our definition of cause as follows:

A Cause is an action connected with a change or unchange in the thing acted on.

The pressure of steam in a boiler is an action on the boiler: the rise in temperature of the boiler is a change in the boiler—the thing acted on—and is connected with the steam pressure; but the pressure of steam is not the cause of the rise in the temperature of the boiler: it is the other way about. The pressure of the air is an action on the locomotive engine, and it is connected with the unchange—the running of the locomotive—for it increases with the speed; but it is not the cause of the unchange. Evidently some further qualification is required in the definition. Why cannot the pressure of steam in the boiler be the cause of the rise in temperature of the boiler? Manifestly because the steam-pressure does not precede, but follows the rise of temperature. Why is not the

pressure of the air the cause of the running of the locomotive? Manifestly because, the running being an unchange, the pressure of the air is not contemporanous with it. The pressure exerted its action before the running began, and continued after the running had ceased. In order, therefore, to accommodate our definition to these considerations we must modify it as follows:

A Cause is an action connected with a following change or a contemporaneous unchange in the thing acted on.

There are some usages that conflict with this doctrine. One of these is that we often give the name of cause to that which is not an action. We say the cause of the stoppage of a motor car is a broken sparking plug, a leak in the water circulation, grease in the commutator, dirt in the carburettor, and so forth. Similarly, we say the cause of a man's death is failure of his heart to act; the cause of the stoppage of the machinery is the the stoppage of the engine; the cause of the stoppage of the engine is the fire going out; and so forth. In each the cause is not an action, but is the cessation of action, or the agent which produces cessation of action; and in every such case, the change, which is the effect, is the cessation of an unchange. Now an unchange is the maintenance of a continuous state in spite of the operation of forces tending to change it: and that which we call the cause of the cessation of the unchange, or the destruction of this continuous state, is not an actual cause, not an action, but the removal or cessation of the cause of the unchange. In each of the foregoing cases, what we call a cause is really the removal or cessation of a cause. The unchanged motion of the car is caused by the action of the sparking plug, of the water circulation, of the commutator, of the carburettor; arrest any of these actions, and the running of the car ceases, and ceases by the operation of causes—friction, etc.—that were all along tending to stop it, and are now permitted, by the cessation of the causes of the unchange, to become effectual. Similarly, the life of man is an unchange maintained by the action of the heart in spite of causes in action tending constantly to bring life to an end. Cessation of the heart's action does not kill the man, but allows him to die. The movement of the machinery is an unchange, maintained,

in spite of causes tending to end it, by the action of the engine. The stoppage of the engine does not stop the machinery, but allows it to be brought to rest by friction and other resistances.

It is scarcely consonant with our notion of cause to call the cessation of action a cause, but, undoubtedly, in individual cases that occur in experience, such as those that have been instanced above, we do in fact regard the cessation of action as a cause, although a stricter logic would compel us to look upon it as the removal of a cause. If the latter view is to prevail, the last definition will stand as the final definition of Cause, but if we are to fall in with current usage, our definition will run:

A Cause is an action, or cessation of action, connected with a sequent change or accompanying unchange in the thing acted on.

Another usage that conflicts with both of these definitions is that of Mill and the logicians, as well as of other writers who should know better, in speaking of things which are not actions nor cessations of action as causes. It is fruitless to try to fix responsibility for the practice, but I am afraid that ultimately it might be traced to writers on Causation. A flagrant example is afforded by writers on medicine, who still divide the causes of disease into predisposing causes and exciting causes. Among the predisposing causes it is usual to enumerate the age and sex of the patient, the climate and locality of his residence, his occupation, and so forth; and none of these is an action, nor is any of them a cessation of an action. Occupation is indeed action, but it is not action upon the thing changed—upon the patient. It is action by the patient, a very different thing. It is evident that in calling these passive states causes of disease, we are using the word cause in a very strained and unnatural sense, and this is often acknowledged even by medical writers themselves. Yet it is beyond doubt that these states have an influence upon the effect. Certain diseases are limited to a certain age; others are limited to one sex; others are found to attack those only who live in certain localities or pursue certain occupations; and yet there is a felt and acknowledged incongruity in calling them causes. No one has ever specified what it is that arouses this feeling of incongruity, but

I think there can be no doubt that it arises from the recognition that they are neither actions nor cessations of action, and that it is only to actions, and perhaps to cessations of action, that the term cause can be properly applied. The connection that these passive circumstances have with the effect, a connection which is undoubted, and cannot be questioned for a moment, is that they are *Conditions* of the effect; and this leads us to inquire into the meaning of Condition, and to ascertain in what it differs from Cause.

Summary.

This Chapter is an examination of the relation of which the two terms are Cause and Effect, and the ratio, or relating relation, is Causation.

Effect is inseparably connected with the idea of change, and every effect is that which impresses us as change or as the prevention of change. The latter is called an unchange. By successive approximations we reach the definition that an Effect is a change connected with a preceding action, or an unchange connected with an accompanying action, on a thing.

The cause of an unchange is often called a Reason.

The changed state that is left when an effect has been produced is called a Result.

By successive approximations we reach the definition that a Cause is an action (or cessation of action) connected with a sequent change or accompanying unchange of the thing acted on.

CHAPTER III.

CONDITION.

WHILE it is generally understood that a cause and a condition are different things, and stand in different relations to the effect, yet even in common speech and in practice they are often confused, and writers on causation admit no distinction whatever between them. Mill was the worst offender in this respect, and his evil example has corrupted all subsequent writers. I do not know of any writer on the subject who formally distinguishes between cause and condition, though all writers use both terms; but they jumble them up together, sometimes using them interchangeably, and sometimes assuming a difference without ever distinguishing them.

Whenever a distinction is made in common speech, we may be pretty sure that it represents and indicates a distinction in thought which the common user feels and appreciates, though he is usually unable to formulate and define it. Not one person in a thousand makes a mistake in the use of the phrases 'I did it' and 'I have done it,' and not one person in a thousand could formulate and explain the precise difference in the meanings of the two. Whenever two different words or phrases are used to express nearly the same thought, it will always be found that they never express quite the same thought. It is, in fact, impossible to keep two commonly used words in the same language synonymous. They soon begin to take on different meanings and to be used on different occasions, and gradually

the meanings diverge more and more. A familiar instance is in the different meanings that now attach to large, big, great, and gross. In the face of such common usage, the proper attitude of a careful student of language and thought is not to assume a haughty superiority to the commonalty who have made the distinction; not to assume, as Mill does, that it is the mere confusion of ignorance and illiteracy, pretending a difference where no distinction exists; but to examine, probe, penetrate, and realise the thought that underlies the practice, to discover the difference, and to clothe it in an appropriate definition. Cotton stuffs are often confounded with woollen stuffs, to the disadvantage of the purchaser; but not on that account ought the expert to persuade the purchaser that there is no difference between cotton and woollen, and that he has been all his life calling one thing by two names. A sure, though not a clear, discernment has convinced him that there is a difference, though he cannot say in what the difference consists. A helpful guide would teach him how they are to be distinguished. Mill, however, and every subsequent logician, finding that the populace makes a distinction between cause and condition, but is not very clear as to the nature of the distinction, seek, not to find and formulate the difference between them, but to persuade us that no difference exists.

That Mill did dimly, and in his fumbling manner, feel, rather than recognise, that there is a difference between cause and condition appears from his treatment of them. He says 'It is very common to single out one only of the antecedents under the denomination of Cause, calling the others merely Conditions. . . . The real Cause is the whole of these antecedents: and we have, philosophically speaking, no right to give the name of cause to one of them, exclusively of the others.' This, it may be observed, is his sixth definition of cause, different from all the previous five. 'What, in the case we have supposed [that of eating a particular dish and dying in consequence], disguises the correctness of the expression, is this: that the various conditions, except the single one of eating the food, were not *events*, but *states*, possessing more or less of permanency.' Supposing this were the correct dis-

tinction between causes and conditions, surely it is a distinction worth making, and entitles them to separate treatment. Again, he says 'There is, no doubt a tendency to associate the idea of causation with the proximate antecedent *event* rather than with any of the antecedent *states*.' If this is so, the obvious duty of an investigator is to discover the reason and meaning of this tendency, and this Mill seems to feel, for he gives a reason, a very inconclusive reason, which explains nothing, but still he gives one, 'the reason being that the event not only exists, but begins to exist immediately previous; while the other conditions may have pre-existed for a considerable time.' 'But though we may think proper to give the name of cause to that one condition, the fulfilment of which completes the tale, and brings about the effect without further delay; this condition has really no closer relation to the effect than any of the other conditions has. All the conditions were equally indispensable to the production of the consequent; and the statement of the cause is incomplete unless in some shape or other we introduce them all.' 'The cause, then, philosophically speaking, is the sum total of the conditions.' Thus, after fluttering on the edge of finding a distinction between cause and condition, he makes up his mind that they are identical, and comes down with a flop on the wrong side. It would be difficult to find an argument more perverse, and the statements by which it is supported are nearly all of them erroneous.

If, as Mill says, we think proper to give the name of cause to one antecedent rather than to the rest, is it not manifest that we do so because we recognise a difference between this antecedent and the rest? Why else should we single it out for different treatment? The bestowal of a separate and different name is *primâ facie* evidence that a difference is felt to exist; and Mill, though he does not discover the true difference, yet does discover a difference, and then treats it as if it were non-existent. If a glass bottle is broken by the blow of a stick, is it true to say that the blow of the stick has no closer relation to the breaking of the bottle than the existence of the stick, or the muscles of the arm of the man who struck

the blow? And is the 'statement of the cause' of the fracture of the bottle incomplete unless in some shape or other we introduce the growth of the tree from which the stick was cut, and the birth of the man who struck the blow? for they were 'equally indispensable to the production of the consequent.' 'Nothing,' says Mill, 'can better show the absence of any ground for the distinction between the cause of a phenomenon and its condition, than the capricious manner in which we select from among the conditions that which we choose to denominate the cause.' Never was assertion more unwarrantable. As well might a man who is colour blind assert that nothing can better show the absence of any ground for the distinction between red and yellow than the capricious manner in which we select from the yellows that which we choose to denominate red. The distinction is there right enough. Between cause and condition there is a distinction that is perfectly clear and very useful, and that is none the less a clear and useful distinction because it is not always observed; because we do not always need to observe it; or because Mill and his successors are too blind to observe it.

Mill says we have a 'tendency' to associate the idea of causation with antecedent *events* rather than with antecedent *states*. If this were so, it would be a distinction of sufficient importance to warrant us in separating the events (causes) from the states (conditions) and discussing them apart; and though this is not the truth, yet it is an adumbration of the truth. Mill would have been much nearer the mark, though he would not have been within it, if he had said that we associate the idea of effect with events. An event is that which comes out of something else, and an effect is that which comes out of the cause. An event, whatever else it may be, is a change, and as we have seen, an effect is often a change, and is always associated with change. We do not necessarily associate the idea of causation with either events or antecedence, but we may associate it with an event if we contemplate the event as an effect. Nor is it true that we associate the idea of condition with 'states possessing more or less of permanency' merely

because they are states and more or less permanent. The state of activity of an engine is a state possessing more or less of permanency, but we do not regard it as a condition of the movement of the train. We regard it as the cause, and rightly so regard it, because it is an action. A cause is an action, and so to regard cause points to the difference between cause and condition, for

A Condition is a passive state.

That is the true distinction between cause and condition. Cause is active: Condition is passive. A cause is an action: a condition is a passive state; not necessarily a permanent state, though as a state it must have some endurance, even if the endurance is but brief. One of the conditions of the discharge of a gun is that the hammer must be at cock. This is a passive state, but it is not a permanent state. It must, however, have some endurance, even though the endurance may be but momentary.

Clearly, however, the definition of a condition as a passive state is not a complete definition with reference to any given case of causation. There are many passive states of many things quite unconnected with the causation of any given effect. The position of the hammer of a gun at half-cock is a passive state, but it is not a condition of the occultation of Jupiter. To complete the definition of a condition it is necessary to state the connection of the passive state with the causation of the effect. A cause is an action upon a thing, connected with a change or unchange in the thing acted on. A condition is a passive state: of what? Of the thing acted on? It would seem so, for that is the only thing admitting of a condition mentioned in the definition; and many instances can be adduced of conditions which are passive states of the thing acted on. The pulling of the trigger is the cause of the discharge of a gun: the position of the hammer at full cock, and the presence of a cartridge in the barrel, are passive states of the gun, the thing acted on, and satisfy the definition of conditions. The striking of a key on the piano is the cause of the sound of the note. The tension of the wires and the integrity of the mechanism are conditions of the occurrence of the sound: they

are passive states of the thing acted on. The application of moisture to the flap of an envelope is a cause of the flap sticking. The presence of a film of gum on the flap is a condition of the flap sticking: it is a passive state necessary to the occurrence of the effect. In this case, we may regard the presence of the film of gum as a state of the envelope itself, or we may regard it, more accurately perhaps, as adjoining and in contact with the envelope, but not a part of the envelope—a passive state, not of the thing acted on, but of something about the thing acted on. In other cases the distinction becomes clear. The cause of a plant's growth is the action of heat on the plant; but the effect on the plant would not be produced but for a condition—the existence of food within reach of the roots of the plant. This condition is a passive state, not of the plant—the thing acted on—but of the soil in which the plant grows, that is, of something about the plant. The cause of the sound of a bell is the action of the tongue on the bell: but this effect would not be produced were it not that the bell is bathed in air, and the existence of the air is a passive state, not of the bell, but of something about the bell. The cause of a plant twining up a support is the action of the plant in rotating about an axis; but the effect would not be produced but for the presence of a support up which the plant could twine. The presence of the support is a condition of the effect, and is a passive state, not of the plant, the thing which, acting on itself, produces the effect, but of something about the plant. And so we find with many other conditions, that they are passive states, not necessarily of the thing acted on, but of something about that thing. Thus we must modify our first tentative definition of a condition and say

A Condition is a passive state of or about the thing acted on by a cause.

The definition is not yet complete, however. It requires further limitation, for there are many passive states in and about a thing acted on which yet are not conditions of any effect produced by the action. The sun shines upon a wall, and by its action warms the wall; and against the north side of the wall rests a ladder. The presence of the ladder is a

passive state about the thing acted on, but it is not a condition of the warming of the wall. A red-haired man takes medicine in a room with a parquet floor and a painted ceiling. The medicine produces its effect, but the red hair, the parquet floor, and the painted ceiling, though they are passive states of and about the thing acted on, are not conditions of the production of this effect. A fall of rain causes a road to be muddy: the dust on the road is a condition of the road becoming muddy, but the presence of a house by the side of the road, though it is a passive state about the thing acted on, is not a condition of the formation of mud. It is clear that a passive state of or about the thing acted on need not be a condition of the effect of that action, and is not a condition unless the existence of the state is necessary to the effect, or material to the effect. If a ship is careened by a gale, we may cause her to right herself by taking in sail. The action of taking in sail is the cause of the ship's righting. But no taking in of sail would cause this movement of the ship unless she were already careened. The careening of the ship is a passive state of the thing upon which the cause acts, and it is necessary to the result. Being a passive state, it is not a cause; and it will be admitted that it would be an absurd misnomer to speak of the careening of the ship as a cause of her righting herself. But the careening is necessary to the righting. It is a condition, an indispensable condition, of her righting herself. Hence we arrive at the following complete definition.

A Condition is a passive state of or about the thing acted on by a cause, and material to the effect.

As an example of the confusion which he attributes to people in general, but which really exists in his own mind, and scarcely anywhere else but in the minds of his followers, Mill gives the following example, which it will pay us to examine in some detail:

'A stone thrown into water falls to the bottom. What are the conditions of this event? In the first place there must be a stone, and water, and the stone must be thrown into the water, but these suppositions forming part of the enunciation of the phenomenon itself, to include them among the conditions

would be a vicious tautology.' To include them all among the conditions would certainly be erroneous, for the throwing of the stone is not a passive state, but an action; and an action directly concerned, as an action, with the effect. It is therefore not a condition, but a cause. The existence of the stone and of the water are certainly conditions, and are so according to Mill's own definition, for they are included in the sum total, 'the whole of the contingencies of every description, which being realised, the consequent invariably follows.' 'The next condition is, there must be an earth: and accordingly it is often said that the fall of the stone is caused by the earth; or by a power or property of the earth, or a force exerted by the earth, all of which are roundabout ways of saying that it is caused by the earth; or, lastly, the earth's attraction; which also is only a technical mode of saying that the earth causes the motion, with the additional peculiarity that the motion is towards the earth, which is not a character of the cause but of the effect.' It would not be easy to find a better example of Mill's thorough muddleheadedness. No one with any sense of propriety in the use of words, or with any attention to the meaning of words, could possibly say that the earth was the cause of a stone thrown into water falling to the bottom; but anyone who should say that the fall of the stone was caused by a power of the earth, or by a force exerted by the earth, or by the earth's attraction, would assert precisely and accurately what the cause is. These are not roundabout ways of saying that the fall is caused by the earth: on the contrary, if anyone were inaccurate enough, and slipshod enough, to speak of the fall being caused by the earth, he would be using an elliptical expression, taking it for granted that his hearers would understand that he was using 'the earth' for the sake of brevity, instead of the power, or force, or attraction exerted by the earth, or briefly, the action of the earth: in short, that he was speaking of the agent as a cause when he meant the action of the agent, a mistake not infrequent with uneducated people, but one that makes us stare when we find it formally adopted by the authoritative writer on causation.

'Let us now pass to another condition. It is not enough

that the earth should exist ; the body must be within that distance from it, in which the earth's attraction preponderates over that of any other body.' Well, yes, so it must, for if not, there would be no water for it to sink in. At this rate a book the size of Mill's Logic would be needed to contain a list of all the conditions necessary to the sinking of the stone. We should have to go back to the geological conditions under which the stone was formed: and so back to the primordial nebula that gave rise to the solar system. 'Accordingly we say, and the expression would be confessedly correct, that the cause of the stone's falling is its being *within the sphere* of the earth's attraction.' It is cool of Mill to say that this expression would be confessedly correct. I know not who has made the confession, but I know that not the rack nor the thumbscrews would wring such a confession out of me. Being within the sphere of the earth's attraction is not an action, and therefore cannot be a cause of anything. It is a state, and for the purpose in hand a passive state, and therefore is not a cause, but a condition.

'We proceed to a further condition. The stone is immersed in water: it is therefore a condition of its reaching the ground, that its specific gravity exceeds that of the surrounding fluid, or in other words that it surpass in weight an equal volume of water. Accordingly anyone would be acknowledged to speak correctly who said, that the cause of the stone's going to the bottom is its exceeding in specific gravity the fluid in which it is immersed.' Mill might make this acknowledgment, but I doubt if anyone else would, and for my part I certainly should not. According to the rule I have laid down, the specific gravity, being a passive state and not an action, is a condition, not a cause.

Mill sinned against the light. He was not ignorant of the view here adopted: it was brought to his notice by a reviewer, and after examination he deliberately rejects it. The reviewer says 'we always apply the word cause rather to that element in the antecedents which exercises *force*.' Thus he had the temerity to defy Hume, and he came nearer than any other writer to the view taken in this book. One of Mill's instances is 'The army was surprised because the sentinel was off his

post.' He considers this as a justifiable and proper expression, which no doubt it is, and that it means 'The cause of the army being surprised was the sentinel's being off his post,' which it does not, or does not necessarily. Mill, though he always expresses himself clearly, rarely expresses himself accurately, and here he is inaccurate. 'Because' may indicate a cause, a condition, or a reason. What Mill is contending for is that it is correct to use the second expression about the surprise of the army. The reviewer says, and I agree with him, that it is incorrect, and I add that it is incorrect because the sentinel's being off his post is not an action, but a passive state, and therefore a condition. The reviewer says, and again I agree with him, that the allurement or force which drew the sentinel off his post may rightly be called the cause of the surprise of the army, and to this Mill objects that it can scarcely be wrong to say the surprise took place *because* the sentinel was absent; and right to say it took place *because* he was bribed to be absent. This is *ignoratio elenchi*. We are dealing with causes only, and 'because' may refer to causes, conditions, or reasons, and Mill, like other logicians, never uses a univocal word if he can find an ambiguous word to serve his purpose. Let us put it into accurate language. It is wrong to say the cause of the surprise was the sentinel's *being* off his post, for that implies a passive state and a condition. It is right to say the cause of the surprise was the sentinel's *going* off his post, or *deserting* his post, for these imply action; and for the same reason the bribing of the sentry may properly be called a cause of the surprise.

In every book on medicine we find age, sex, race, time of year, climate, and so forth enumerated among the causes of diseases. It is clear that none of these is an action. None of them therefore can be a cause of disease. Occupation also is called a predisposing cause of disease; but though the occupation of the patient is an action, it is an action not on the patient, the thing changed, but by the patient, which is a very different thing. When occupation is a factor in producing disease it is therefore usually a condition, not a cause; but there are some cases in which it may properly be called

a cause. Dry grinding produces a quantity of irritating dust, which is inhaled by the dry grinder, and irritates the delicate walls of the air-cells of the lungs, in such a way as to produce inflammation in them, which is called grinder's phthisis. In this case the occupation of the patient is an indirect cause of the disease. It causes a result—the presence of dust in the air—which is a condition of the disease.

A condition has been defined as a passive state . . . material to the effect, or such that without it the effect would not have been produced; and according to this definition, every condition must be necessary to the effect; yet we often speak of favouring conditions, with the implication that they favour or assist the production of the effect, which yet might be produced without them. The expression 'favouring condition' is a convenient expression, and is not inaccurate if it is properly understood and defined. Under given conditions a seed will germinate, and the plant will grow to maturity, flower, and seed. All the conditions necessary to its life and growth to maturity must therefore have been present; but under other conditions of aspect, moisture, soil, and so forth, it might have reached maturity sooner, might have attained a larger growth, might have produced more flowers and more seed, and might have lived longer. These other conditions were not necessary to the life, growth, and maturity of the plant; but they favoured its life, growth and maturity; and though not necessary to the production of some effect, they were necessary to the full or extra effect over and above that produced in the first set of conditions. A favouring condition is, therefore, a condition without which some effect will be produced on a given thing by a given cause, but with which more of that effect will be produced, or the effect will be produced more speedily by the operation of the same cause, or both. With respect to the production of some effect, the second condition is a favouring condition: with respect to the production of the extra effect, or the earlier effect, it is a necessary condition.

There is another sense in which the terms necessary condition and favouring condition are contrasted. If in certain con-

ditions a certain amount of an action is necessary to produce a certain effect, and if, when a new condition is introduced, less of that action will produce that effect, then this new condition is called a favouring condition. It is not necessary to the production of the effect by a given intensity of action, but it is necessary to the production of the effect by a less intensity of action. Thus, though a condition is always necessary for the production of an effect by a given action, yet it is convenient and justifiable to distinguish between necessary and favouring conditions if we bear in mind the conventional meanings of 'favouring.'

Frost, if sufficiently intense, will infallibly kill the blossom of pepin fruits. A less degree of frost will not kill the blossom if it is dry, but will infallibly kill it if it is wet. Wetness of the blossom is a necessary condition to the destruction of the blossom by this less degree of frost, but it is not a necessary condition to the destruction of the blossom by frost in general. It is called, and may justifiably be called, a favouring condition of the killing of the blossom by frost.

Summary.

A condition has never hitherto been satisfactorily distinguished from a cause. The true distinction is that a cause is an action, a condition a passive state.

By successive approximations we reach the definition that a condition is a passive state of or about the thing acted on by the cause, and material to the effect.

The difference between a necessary and a favouring condition is verbal. A condition is always necessary to the production of a given effect by a given action; but, if, under an additional condition, the effect would be produced sooner, or more of the effect would be produced, or the same effect would be produced by less of the action, then that additional condition may be termed a favouring condition with respect to the general causation of that effect, though it is a necessary condition with respect to particular cases.

CHAPTER IV.

CAUSATION.

We may now turn to the consideration of the third constituent in the relation. We have considered the terms—Cause and Effect—and we now turn to the consideration of the link, or 'relating relation' which binds them together, and which I call the ratio. The question we now have to discuss is What is the nexus between cause and effect? or, Given an action on a thing, and a following change or contemporaneous unchange in that thing, what is it that converts this time relation into a relation of causation? in short, What is the mark or character of Causation?

Hume, after arguing at length that there is no connection at all between cause and effect, astounds us by defining their relation as 'if the first object (the cause) had not been, the second (the effect) had never existed,' and thus declares not merely connection, but necessary connection, between them. Mill, as we have seen, proposes one definition after another, not as successive approximations to a final clarified expression, not even as alternatives of equal value, but he wanders on, giving one definition after another, not noticing that they are incompatible, and seemingly forgetting, when he formulates a new one, that he had ever formulated one before. The two qualities on which he most insists are invariableness and unconditionalness, but he soon abandons invariableness, and he insists throughout that conditions are necessary to causation. Dr. Fowler pins his

faith to invariableness of succession, but Mr. Welton denies sequence as being necessary to causation, and in this no doubt he is right; but he goes farther, and denies that sequence or any time relation enters into causation, and in this he is unquestionably wrong. According to him, 'the relation of causation is found in the securing of those conditions, which are, consequently, at once both cause and effect,' not a very illuminating statement, and not quite consistent with his definition of cause as 'a totality of conditions whose existence secures the effect'—causes it, in fact. Professor Carveth Read, whose pronouncements always deserve consideration, enumerates five marks of causation, which it will be well to examine, since one or more of them are adopted by most other writers. 'The Cause of any event, then, when exactly ascertainable, has five marks: it is (quantitatively) *equal* to the effect, and is (qualitatively) its *immediate, unconditional, invariable antecedent.*'

The quantitative equality of cause and effect is frequently assumed and asserted, but it seems to me to rest upon a very insecure foundation, and to be based upon very misty notions of what a cause is, and of what an effect is. The instances given are almost always chemical combinations, and it is said 'When oxygen combines with hydrogen to form water, or with mercury to form red precipitate, the weight of the compound is exactly equal to the weight of the elements combined in it.' No doubt it is, but what are equated here are two weights, and I do not see how it can be maintained that the weight of the elements is the cause, or the weight of the compound the effect, of the combination. The causes of the combination of oxygen and hydrogen are first, the mixing of them, and second, the passage of an electric spark through them; and I cannot see that the mixing is equal to the effect, or that the spark is equal to the effect, which is not the weight of the water, but the formation of water. The effect in this case is a change—the change from a mixture of gases to a liquid; and there is nothing in this change that is equal to the spark. The cause of the maintenance of the mercury in a Torricellian tube is the weight of the air, and the weight of the air is certainly equal to the weight of the mercury; but the effect is not the weight of the

mercury, but the maintenance of the height of the mercury, and this cannot equal the weight of the air. As another instance of equality of cause and effect, Professor Carveth Read says the numbers of any species of plant or animal depend on the food supply, and no doubt they do in part, but the numbers are not equal to the food supply. The number of lions in a district is not necessarily equal to the number of antelopes in that district; and if they were, the antelopes are not the cause of the lions. Another instance of causation adduced by Professor Carveth Read is still more to seek. 'How learn to play the fiddle? Go to a good teacher (then, beginning young enough, with natural ability and great diligence, all may be well).' I am at a loss to discover how the cause in this case can be quantitatively equal to the effect. No. I think the quantitative equality of cause and effect is as idle a dream as the identity of cause and effect : it is founded upon misapprehensions, and is not true, nor even is it sense.

The next mark or character of causation is *immediacy*. The relation of causation is said to be immediate, by which is meant immediate sequence. Mr. Welton, as we have seen, confuses immediate sequence with simultaneity. He takes it that an effect which immediately follows a cause is simultaneous with the cause, and from this he jumps to the further conclusion that simultaneity means identity, so that an effect that immediately follows the cause must needs be identical with the cause. I do not think that either of these views needs serious refutation; but the assumption that an effect must necessarily follow immediately on a cause does require careful examination. Certainly in common speech, and in the light of that common sense which philosophers so much and so universally despise, there is no such necessity; nor is there any necessity in law. If a man wounds another, and if that other dies of the wound at any time within a year and a day of the assault, the assault is in law the cause of the death, and the assailant may be guilty of murder. Of course, philosophers are not bound to make their definitions conform to the definitions of law; but it is very desirable that philosophers should not live wholly in a balloon of speculation, out of all touch with mundane and

practical affairs. The use of opinion is to be a guide to conduct, a truth that philosophers rarely recognise; and lawyers have this advantage over philosophers, that their definitions are perpetually being put to the test of practical use; and if they are found to be faulty from this point of view, the definition must be discarded or amended. Philosophers are under no such obligation. They can, if they please, define 'the Knave of Hearts as the Jackovarts,' or that which depends on conditions as unconditional, or sequence as simultaneity, or simultaneity as identity, or causation as implication, or that which cannot be perceived as a product of perception, or a battle as a substance, and no one can prevent them; nor are they under any obligation to make their definitions square with their practice; but when one is immersed in practical affairs, and is writing for the guidance of those whose business it is to discover and record the causes of actual occurrences, it is prudent to take into account the notions that are prevalent among men of affairs, and not lightly to reject them.

The General Register Office is a department of the State maintained at considerable expense, and engaged in collecting and presenting to Parliament immense statistics of the causes of death; and the Registrar General has no hesitation in admitting into his Tables, and presenting to Parliament, causes of death that may have preceded the effect by weeks, months, and years. Neither he, nor his staff of officials, nor the tens of thousands of medical men who furnish him with items, nor the High Court of Parliament, nor any of the multitude of scientific men who have used these tables, have ever made any objection to them on the score that the alleged causes of death are not causes of death because the result does not immediately follow on the cause. The Tables are not immaculate: they are open to objection, as I shall presently show; but they are of very great value to Officers of Health and others in the prevention of disease, even though it is from time to time found that some of the alleged causes of death are, after all, not causes; but if immediacy is a necessary element of causation, the alleged cause of death would be the true cause in scarcely one of the millions of instances which the General

Register Office has recorded; and if the alleged cause were in every case false, then the usefulness of the Tables would be destroyed, and they would be of no value at all, either to Officers of Health or to any other human being. The *primâ facie* presumption against immediacy as a quality or mark of causation is therefore very strong.

As I have shown in the previous discussion, immediacy in the strict sense of the term cannot obtain in any case of effectuation, for an effect is a change or an unchange, and an unchange by its very nature implies duration, and cannot be immediate; while in experience every change takes time, however short that time may be. Perhaps the nearest approach to immediacy that we know is the effect of lightning upon our mind the instant the flash passes; but this we know takes time—time for the light to travel to our eyes, time for it to traverse the media, time for a change to take place in the retina, time for an impulse to travel to the brain, time for it to produce its effect there. Strict immediacy between cause and effect is unknown to us; but is not this pushing matters too far? May there not be a practical immediacy that is required for causation, although immediacy in the pedantically accurate sense there cannot be? In other words, ought we not to limit our notion of causation to that change which appears to our senses to follow immediately upon an action, even though in strict accuracy some infinitesimal fraction of a second may separate them? Well, as has already been shown, even in such a restricted sense immediacy is not required in the current and accepted meaning of causation; and if it is to be imported into the philosophical meaning, then philosophy cuts herself off, in this respect, finally and for all from utility and common sense; and this is inadvisable if it can be avoided. But there is no earthly reason why philosophy should thus make a fool of herself. One of the favourite maxims of logic is *Nota notæ, nota rei ipsius.* As a logical maxim it is of little or no value, but in the present connection it has this value, that it effectually estops logicians from objecting to the maxim that I here present to them:—*Causa causæ, causa rei ipsius.* The cause of a cause is the cause of the effect.

Πάντα ῥεῖ: all things flow. The universe is a series of continuous change. In this continuous series we may take, anywhere we please, a longitudinal section of any length we please, and call the first change the cause of all or any that follow, and the last the effect of all or any that have gone before: or we can call the first the cause of the last, and the last the effect of the first. The process is familiar to us from childhood, and was solved for us long before our infantine minds were sophisticated by reading books on logic. If the cat began to catch the rat, the rat began to gnaw the rope, the rope began to hang the butcher, and so on until the pig began to get over the stile, and the old woman reached her destination, then the action of the cat was the cause of the rope being gnawed, of the butcher being in peril of death, and of all the other events in succession down to the old woman getting home in time. The cat's action was the cause, immediately or mediately, of each effect, and it was not less efficacious when it acted mediately than when it acted immediately. It is just as scientific, and just as philosophical, to attribute one man's death to the bite of a mosquito twenty years before, as to attribute the death of another to the explosion of a shell which blew him to bits in a moment.

The third distinguishing mark of causation is *unconditionalness*. Mill invented the term, and gives, as is his custom, several definitions of it, each different from the rest. It is synonymous with necessity; it means whatever supposition we make about all other things; it means subject to no other than negative conditions; it means as long as the causes do not vary; it means, in short, pretty much what you please. Mill's discussion of unconditionalness is a striking example of his utter muddle-headedness. Invariable sequence, he says, is not synonymous with causation, unless the sequence, besides being invariable, is also unconditional, and this he says immediately after he has defined the cause as 'philosophically speaking' the sum total of the conditions! It is therefore philosophically speaking conditional, and speaking otherwise unconditional. This, however, is only a beginning. His fifth or sixth definition of a cause 'confines the meaning of the word cause to the assemblage of positive conditions without the negative, and

then, instead of "unconditionally" we must say "subject to no other than negative conditions."' So that in the first place the cause is the sum of the conditions, both positive and negative; in the second place, it is the positive conditions without the negative; and in the third place it is the negative conditions without the positive. There is only one other possible alternative, that the cause is neither the positive nor the negative conditions, and this, which is the correct view, is the only one that Mill does not give. Hume is inconsistent enough, goodness knows, but Hume is a miracle of consistency in comparison with Mill.

Professor Carveth Read adopts unconditionality as a mark of causation, and his meaning of the term is quite different from any of Mill's, though he says it is what Mill means. When Mill defines the cause of any effect as its unconditional antecedent, he means, according to Professor Carveth Read, that it is that group of conditions which, without any further condition, is followed by the event in question. According to this, when Mill said unconditional he meant un-further-conditional; and it is possible that Mill may have had sometimes in his mind some such meaning as this; but the only thing we can be sure of is that what he meant at one time was not what he meant at another time, and there is no evidence or indication that he had any definite meaning at all. However, there are few writers on causation who do not adopt Mill's assertion that it is unconditional, and all of those who assert that it is unconditional assert, as Mill does, that it is conditional, and never recognise the contradiction. They all identify or confuse causes with conditions; they most of them speak of the cause as the sum-total of the conditions; and even a writer who owes so little allegiance to Mill as Mr. Welton calls it the totality of the conditions; and how that which depends upon conditions can be unconditional, I confess I do not understand. I suppose Mill must have had something in his mind when he said that to constitute a cause the conditions must be unconditional, but what it was we do not know, and whether Professor Carveth Read is correct in his surmise that it was un-further-conditiona cannot now be known. Anyhow, to speak of that as uncondi-

tional which is on all hands admitted and proclaimed to be subject to conditions seems to me an inadmissible abuse of language.

The fourth of Professor Carveth Read's stigmata of causation is *invariability*. Mill adopted the notion from Hume, and every writer of that school pins his faith to invariability; but when we seek the meaning that they attach to the term, we find ourselves in wandering mazes lost. Does it mean that the cause is invariable? or that the effect is invariable? or that the cause invariably precedes the effect? or that the effect invariably follows the cause? As far as I can make out, sometimes one and sometimes another, but most often none of these meanings is intended. Mill varies in his statements about invariability as in those about everything else. The most definite opinion he gives is this: 'That we should believe not only that the antecedent always *has* been followed by the consequent, but that so long as the present constitution of things endures, it always *will* be so.' It seems from this passage that 'invariably' means, with Mill, 'always,' and I believe that this is the meaning that his followers attach to it when they mean anything at all; but like their leader, they never keep long to the same meaning of any important word or doctrine, and Mill himself, on the very next page, says, 'Invariable sequence . . . is not synonymous with causation unless the sequence, besides being invariable, is unconditional.'

When it is said that the cause is the invariable antecedent, what ought to be meant, though I doubt very much if it is meant, is that the cause is that antecedent which does not vary. If this is the meaning, it is doubly wrong, for in the first place, a cause need not be an antecedent, and in the second, if it is an antecedent it may vary, and usually does vary. If the antecedent must not vary, then the pressure of the gas of an exploding cartridge is not the cause of the propulsion of the projectile, for the pressure of gas varies from moment to moment as the projectile travels along the bore of the gun.

When it is said that the effect is the invariable consequent, what ought to be meant, though I believe it never is meant, is that the effect is that consequent which does not vary. If this

is the meaning, it is undoubtedly wrong, for an effect need not be a consequent, and when it is a consequent, it may vary. If the consequent must not vary, then the movement of a motor car is not due to the action of the engine, for the speed varies with the gradient, and with the surface of the road.

When it is said that causation is invariable sequence, what ought to be meant is that the time and manner in which the cause precedes the effect, or in which the effect follows the cause, do not vary. But in the first place, causation need not be sequence, and in the second, when it is sequence, it may be variable. The time at which the report of a gun reaches us does vary with our distance from the gun; and the remittent manner in which the light from the fixed stars reaches us varies from the steady manner in which the light from the planets reaches us.

But suppose, what I believe is the case, that writers on causation express their meaning in this matter, as in other matters, inaccurately, and when they say invariably they mean always; is it true that there is no causation unless the cause is always followed by the effect, and the effect is always preceded by the cause? Then how if cause and effect are contemporaneous, as they are in the causation of an unchange? If sequence is always necessary to causation, then such unchanges as the maintenance of the motion of a locomotive, or the maintenance of animal life, or the suspension of a weight by a cord, or the prolonged boiling of water, are not caused. They are not effects, nor instances of causation. But even supposing there is no causation except the sequence of change on action, is it true that there is no causation unless this sequence always happens? Then how if it happens once only? Once, as the boy said to the man who declared that he was once as active as the boy, 'Once ain't often.' Still less is it always. If I see a bottle of wine fall on a stone floor and smash, am I to deny that the fall of that bottle on to the floor was the cause of the smash? It has happened only once and can never happen again. 'Oh, but,' says the logician, 'when similar bottles have fallen on stone floors they have always broken.' Indeed? I have it in mind that this very bottle

had previously slipped out of my hand and fallen a sixteenth of an inch on to the very same stone floor, and yet was not broken. 'But then the cause was not the same, for the bottle did not fall so far.' Granted, but your definition says nothing about the same cause, it says the cause is always followed by the effect; and you now say that the cause of the bottle breaking was its fall for a certain distance; but I had previously let that bottle fall the very same distance on to a truss of straw, and the bottle did not break. 'Ah yes, but when I say the same cause I mean the same cause acting in the same conditions.' But if the same cause had acted in the same conditions the bottle would have smashed before, and you cannot be always smashing the same bottle, you know. It seems to me that *cadit ampulla, cadit quæstio.* But may we never predicate causation until an event has occurred repeatedly? Then how often must it be repeated before we can say it always *has* happened? how often before we can say it always *will* happen? Suppose a man hits me in the eye, how many times must I get him to repeat the blow before I can be sure that it is the cause of my eye turning black? 'But,' says the logician, 'a blow on the eye always *has* been followed by the blackening of the eye, and always *will* be followed by the same phenomenon.' Has it? What do you know about black eyes amongst Mousterian or Neanderthal men? And will it? Why? 'Because the same cause is invariably followed by the same effect.'

'My friend,' I reply, 'you are a logician; did you never hear of the *circulus in probando?*'

I can imagine the tormented logician answering these objections something in this way:—

'When I say invariably, of course I don't mean invariably; I mean always. At least I don't exactly mean always. You are so confoundedly particular. You expect me always to mean precisely what I say, and to say precisely what I mean; and you expect me always to have a precise meaning to express. You forget that I am a logician. When I say the effect invariably follows the cause, I mean of course that it follows unconditionally, that is to say, in certain conditions.'

'That,' I should answer, 'is a curious meaning for unconditionally; but waiving that, what are these conditions?'

'Why, of course, the same conditions in which it happened before.'

'But, *ex hypothesi*, it never has happened before.'

'Well then, the same conditions in which it would have happened before if it had happened before.'

'Thank you very much, but on your own showing, the same conditions never are, and never can be repeated.'

'Really, sir, I cannot bandy words further with a person who knows nothing of logic. Allow me to bring to your notice the well-known philosophical principle, of which you have never heard, that all reasoning is through a universal. I wish you a very good morning, and take my leave of you.'

It would be difficult for me to suppress Hamlet's answer—You cannot, sir, take from me anything that I more willingly will part withal.

No, I am afraid invariability must go after equality and immediacy and the rest of the marks that are supposed to characterise causation, and with them must go the last of Professor Carveth Read's distinguishing marks of cause, that of *antecedence*. It is manifest to everyone who is not wilfully blind, that the cause of a change must be antecedent to the effect, even when cause and effect are apparently simultaneous. The fracture of a glass bottle by the blow of a stick seems to be instantaneous, and no doubt the time consumed is very short. But if the operation were photographed by a rapidly moving kinematograph, and the film was to be put through the lantern very slowly, we should see the glass yield and bend before the pressure of the stick, and give way first on the surface remote from the stick, and gradually spread until it involved the whole thickness. We should see the splinters separate, not simultaneously, but successively, and that the whole operation took time. This, I think, is one answer to Mr. Bertrand Russell's contention that we can divide up the cause, or the duration of the cause, into many successive instants, of which the last only is entitled to the name of cause; and that it is this last division only upon which the

effect follows instantly, and with which the effect is virtually continuous. These are not his words, but this is the meaning of his doctrine as I understand it. It is not so. The cause has a certain duration; and during every instant of that duration it is a cause, and is in action, and is causing more and more of the effect. The effect also has a certain duration. As the cause begins to act, the change begins to occur; as the cause continues, the change increases; when the cause ceases, the effect reaches its maximum. As soon as the cause ceases to act, the effect, as an effect, that is as a progressing change, also ceases, and becomes a result. The total effect is not reached until the cause ceases to act, and it is in this sense, and in this sense only, that the effect succeeds the cause, and that cause and effect are antecedent and consequent.

But when the effect is an unchange, the cause does not and cannot precede, nor can the effect follow. In this case cause and effect are contemporaneous; the only exception, which is but an apparent exception, being the delay due to inertia in the starting and cessation of that unchange which is the motion of a body, such as a cart, a motor car, or a railway train, that owes its motion to continuous action.

What, then, is the quality which characterises and marks causation? It is not at all difficult to discover, and indeed it was discovered and assigned long before the day of Hume, but he took a violent prejudice against it, and all his successors have been afraid of it. They have avoided it as if it were an asp or a viper, and few of them even dare to mention it; and yet there is nothing frightful about it, and if the nettle is firmly grasped, it not only fails to sting, but even furnishes a grateful and sufficient support.

Daily the tide rises on our coasts, and daily thereafter men and women in this country marry; and in some respects the consequents are invariable. They invariably marry two at a time and with some sort of ceremony. Moreover, this consequence always follows the antecedent: not a rise of the tide occurs but some marriage follows it. As far as history goes back, this consequent *has* always followed this antecedent; as far as we can foresee, the consequent *will* follow the antecedent

'as long as the present constitution of things endures'; and these are the conditions that are said to convert mere time-sequence into causation. But they don't. No one but a lunatic or a logician would regard the rise of the tide as the cause of men and women marrying; and why not? Ask the first man, woman, or child (not being a lunatic or a logician) you may come across why they do not regard the rise of the tide as the cause of marriage, and he, she, or it will answer 'Because there is no connection between them.' This is the obvious answer, and it is a very good answer as far as it goes, though it is not quite a sufficient answer.

There are two reasons why it is not quite a sufficient answer: first, because things may be connected together in sequence without being cause and effect, and second, because it does not explain the nature of the connection.

Night always follows day, and the two are connected, but yet night is not the effect of day. The flight of the projectile always follows the recoil of the gun, and is connected with it, but the recoil of the gun is not the cause of the flight of the projectile. The sinking of the stone always follows the splash, and is connected with it, but the splash is not the cause of the sinking of the stone. Although, however, these instances prove that mere connection in sequence does not constitute causation, even when the sequence is constant (which is what logicians mean by invariable) yet it is clear in each case that the connection in sequence does depend upon causation. The connection between day and night is that they have a common cause, the rotation of the earth. The connection between the recoil of the gun and the flight of the projectile is that they have a common cause, the explosion of the charge. The connection of the sinking of the stone with the splash is that they have a common cause, the fall of the stone into the water. It is evident that we are getting 'warm.' If the connection between antecedent and consequent does not itself constitute causation, yet it is evident that it is indispensable to causation, and that we may say provisionally

Causation is the connection between cause and effect.

Although, however, this is true, it does not carry us much

forwarder. It does not display the nature of the connection. In order to get a complete definition of causation, and to clarify the concept, we must substitute for the terms cause and effect the definitions of them at which we have previously arrived. We shall then get the following definition :—

Causation is the connection between an action and the following change or accompanying unchange in the thing acted on.

If we apply this definition to the foregoing test cases we find that it fits, and satisfactorily explains why they are not cases of causation although they are causally connected. Night always follows day, and is connected with it ; but night is not the effect of day, and why not ? Because, although there is a connection between them, the connection is not between an action and a change in the thing acted on. Day does not act upon anything to cause night. The recoil of the gun always precedes the flight of the projectile, and is connected with it ; but the recoil of the gun is not the cause of the flight of the projectile, and the reason is manifest—the recoil of the gun does not act on the projectile, the thing in which the effect is produced. Similarly, the reason the splash is not the cause of the sinking of the stone is that the splash does not act upon the stone, the thing in which the change occurs.

The same formula satisfies all Mr. Welton's difficult cases. 'The dryness of a boy's clothes before his immersion in water is not the cause of their subsequent wetness.' It certainly is not, and I doubt if even a logician has ever suggested that it is; 'that cause can only be found in that spatial relation between the clothes and the water which we call contact.' It is true that we may speak of the contact of the water with the clothes as the cause of the wetness of the clothes, but what we mean, or ought to mean, by contact, in this case, is not being in touch, but bringing into touch. The cause of the wetness of the clothes is the action of bringing water into contact with them, and then the action of water upon them. Once the clothes are wet, the continued contact of the water with them is not the cause of their wetness, it is their wetness. The bringing of the water into contact with the clothes is the cause, the effect is not wetness, it is becoming wet. Wetness is not an effect, it is a

result. Mr. Welton's statement is vitiated by two confusions. He says wetness when he means becoming wet, and he says contact when he means bringing into contact.

'A dropping of ink upon paper causes a blot, but the blot is there as soon as the contact of ink and paper is made; it *is* that contact.' Here again there is confusion. The dropping of the ink upon the paper is rightly called the cause of the blot, for the dropping of the ink is an action on the paper, and the blot is the change in the thing acted on, and is connected with the action. It is true that the blot is there as soon as the contact is made, as every effect is there as soon as the causing action is complete; but I see no ground for asserting that the blot *is* the contact. As well might we say when a man lies in bed, that the contact of the man with the bed *is* the man. The blot is not the contact. The blot is the layer of ink adhering to the paper.

There is yet one thing wanting to the definition of causation. It is, we find, the connection between an action upon a thing and the sequent change or accompanying unchange in that thing; but we have yet to ascertain the nature of the connection. This cannot be put much better than in the words in which Hume stultifies his whole previous argument,—'where, if the first object had not been, the second had never existed.' In other words, the connection is a necessary connection. Much superfluous verbiage has been wasted in discussing the nature of necessity, which is perfectly clear to everyone but philosophers. By necessary connection I mean that the action is so connected with the change or unchange that if the action had not taken place, the change or unchange would not have occurred; and the action taking place in the conditions in which it did, the change or unchange connected with it was unavoidable and unpreventable. That, I believe, asserts the true nature of causation, which may be finally defined thus:—

Causation is the necessary connection between an action and the sequent change or accompanying unchange in the thing acted on.

Mill boggles at the term necessary, and suggests that its meaning is not clear. 'If,' he says, 'there be any meaning which confessedly belongs to necessity, it is *unconditionalness*,' and thus he substitutes for a plain clear word which everyone

understands, a word which no one else understands, and which he does not understand himself. What he means by 'confessedly' it is difficult to surmise, for no one but himself has ever defined necessity as unconditionalness, and not even his followers confess that they mean the same thing. It is another of his wandering and unwarrantable assertions, adopted, apparently, on the spur of the moment, without consideration or justification. No one has ever confessed that necessity means unconditionalness; and it doesn't. Whichever of Mill's various definitions of unconditionalness we adopt, it bears no resemblance to necessity.

But is causation the necessary connection that I have asserted it is? It may be said that if the severing of an artery which causes a man's death had not taken place, the death would still have occurred sooner or later, and therefore the connection between the cause and the effect was not necessary. The obvious answer is that though the connection between the severing of the artery and the death of the man was not necessary, the connection of the severing of the artery with his death by hæmorrhage at that time and place was necessary. It was necessary to that particular effect. And it may be said that the death did not necessarily follow, for if a surgeon had been present, and had tied the artery, the man would not then and there have died, so that the change was neither unavoidable nor unpreventable; and this is true, but then the conditions would not have been the same. The conditions being what they were, the change followed necessarily, in the sense in which I have defined necessarily, on the action; and it is this necessary connection between the cause and the effect that constitutes causation.*

* A doubt, I find, is felt by a reader, whether the maintenance of the motion of a locomotive can properly be called an unchange; for it may be said—Are not all parts of the machinery continuously changing in position? Animal life also is a perpetual series of changes; how then can it be called an unchange? The answer is that the nature of things as it appears to us, and as for our purposes it is, varies according to the way in which we choose to contemplate them. An unchange, as I have defined it, is a way of contemplating things, just as a class is a way of contemplating things. No such thing as a class exists except in our minds. When several individual things have some quality in common, such as hardness, or whiteness, or motion, we may mentally group them together, and contemplate them together as all possessing that quality; and by the possession of that quality they are grouped together in our minds, and consolidated into a

Summary.

This chapter examines the five characters or marks that are said to be characteristic of causation, viz., equality of cause and effect, immediacy, unconditionality, invariability, and antecedence; and shows that not one of them properly or necessarily pertains to causation.

By successive approximations the definition is reached that Causation is the necessary connection between an action and the sequent change or accompanying unchange in the thing acted on.

The meaning of 'necessary' in connection with causation is defined.

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single object of contemplation—a class of hard, or white, or moving things. They are not grouped together in fact, or outside of our minds. Both the North Pole and the South Pole are white, and may be contemplated together as adjoining white things in the class of white things; but in fact they do not adjoin, but are wide asunder. To call things a class is to contemplate them together; and to separate them, not actually, but in contemplation, from other things that have not the class-quality. Just in the same way, we may take all the successive changes of a locomotive, both the internal changes of its parts, and the changes of position of the whole with respect to its surroundings, and contemplate them all together, as grouped and consolidated into a single object of contemplation, which we call, not a class, but an unchange. We call it an unchange, or the maintenance of an unchanging state, because, as movement, it does not change to rest, although there are forces in action—friction, gravity, and so forth—tending to bring it to rest. Each movement of the parts is a change, and may be so contemplated if we choose; but we need not so contemplate it. The movement of the whole is change of place with respect to surroundings, and may be so contemplated; but it need not be so contemplated. We may, if we please, regard the movement, not in contrast with surrounding things which remain at rest, but in contrast with its own possible state of rest, or in contrast with its being brought to rest, which would be a change of another kind, but still a change. So contemplated, the state of motion is not a change, but the maintenance of the unchanging state of motion. In short, it is an unchange.

CHAPTER V.

SUBSIDIARY PROBLEMS.

I. Plurality of Causes.

Mill is the inventor of the phrase Plurality of Causes, and he gets into his usual muddle over it, a muddle which even his followers have discovered to be a muddle, but which they have only partially cleared up. It will be remembered that one of his statements of the Law of Causation is 'that every consequent is connected in this manner [invariably] with some particular antecedent, or set of antecedents. Let the fact be what it may, if it has begun to exist, it was preceded by some fact or facts with which it is invariably connected.' It would be difficult to put the statement more positively or more strongly, and as he himself would say more unconditionally. It is an unqualified assertion; and yet in a subsequent Chapter he says 'There are often several independent modes in which the same phenomenon could have originated. . . . Many causes may produce mechanical motion: many causes may produce some kinds of sensation: many causes may produce death.' Inconsistency is, as I have said elsewhere, with other people a vice to be avoided. With logicians it is an end to be pursued for its own sake. A writer on any other subject who should thus stultify himself by self-contradiction would be discredited, but with logicians self-contradiction is rather a virtue than otherwise.

It is clear that in this use the term Plurality of Causes is

wrong, and doubly wrong. In the first place it does not mean that any single instance of effect is due to more than one cause, and in the second it does not mean that more than one cause may be necessary to produce a certain effect. What is meant is that an effect of a certain kind may be due on one occasion to one cause and on another occasion to another cause. This is not Plurality of Causes: it is Alternity of Causes, or, as Professor Carveth Read calls it, Vicariousness of Causes. When an effect is said to be due to a plurality of causes, what is meant is that if several effects resemble one another in some particular, one may be due to one cause and another to another. The death of A by drowning is due to one cause—drowning—and no more. It is not due to a plurality of causes. The death of B by shooting is due to a different cause, it is true, but then it is a different effect. It is a different effect, occurring on a different occasion, under different circumstances, to a different person. Both effects include the element or ingredient of death, but the effects are not death, but deaths; and when it is said that many causes may produce death, what is meant is that many different causes may produce many different deaths; which is not so very paradoxical.

When Mill said many causes may produce some kinds of sensation, we may suppose that what he had in his mind was sound, which is a kind of sensation. But sound in general is not an effect: it is a generalisation from many individual instances of sound, each of which was an effect, and an effect of one single cause. Mill's blunder consists in generalising the effects without generalising the causes. If we generalise many instances of sounds into the one concept of sound, and call the generalisation a single effect, we should also generalise the causes of all these sounds, and call the common ingredient in them the cause of sound. Each separate sound will then have its separate cause; and the common ingredient in them all will have its common cause in aerial vibration. Similarly, if we generalise the common ingredient in many deaths, and call it death, we must generalise the common ingredient in all the causes of these deaths and call it cessation of the heart's action. There is no such thing as Plurality of Causes in

Mill's sense, unless we generalise the effects while leaving the causes particular, which is not a very legitimate logical operation.

It is of course perfectly legitimate, and may be very useful, to investigate all the cases in which effects have a common ingredient, such as deaths, or sounds, and to determine as many as we can of the combined causes and conditions by which the effects are produced that have this common ingredient: this is very proper, and may be very useful; but in such cases we are seeking the causes, not of an effect, but of a common ingredient in many effects; and the plurality of causes applies to the plurality of effects, and not to the common ingredient in them, although for the sake of brevity and convenience we may allow ourselves to speak as if it did. In any case, Plurality of Causes is clearly a misnomer here; what is meant is not Plurality of Causes but Alternity of Causes.

There is a sense in which plurality of causes is a perfectly justifiable expression. There is a sense in which every event has many causes, innumerable causes, and there are certain effects that admit, and others that require, the cooperation of more than one cause to bring them about. These we will examine in their turn.

II. The Regression of Causes and the Progression of Effects.

A cause is an action in certain conditions upon a thing: an effect is a change or unchange in the thing acted on, and leads to a result. In the physical world, action means the transfer or liberation of energy. It is now a commonplace that energy neither appears out of nothing nor disappears into nothing, but that every manifestation of energy is the release of energy from store or its transfer from one thing to another. If it is expended from store, then at some past time it must have been put into store by some action or other. If it is transferred from place to place, such transfer is action, and action was as necessary to put it into the place from which it comes as to put it into the place to which it goes. In short,

action, which is cause, is also always either effect or result. It is always produced by previous action.

The action of the pig in getting over the stile was caused by the action of the dog in biting him. The action of the dog was caused by the action upon it of the stick. The action of the stick was caused by the action of the fire, which was caused by the action of the water, which was caused by the action of the ox, which was caused by the action of the butcher, and so back to the action of the cat. There was a continuous regression of causes from the last effect to the first action; and a continuous progression of effects from the first action to the last effect.

What is true of this dramatic and perhaps fictitious series is true of every other case of cause and effect. The actions stretch backwards in series as far as we like to trace them, or can trace them; and the effects proceed forwards down to the present moment in which, as actions, they are carrying on the chain of effects into a futurity of indefinite duration.

The motion of a train is the effect of the action of the wheels upon the rails, which is the effect of the action of the piston-rods on the cranks, which is the effect of the expansion of steam in the cylinders, which is the effect of heat upon the water in the boiler, which is the effect of the combustion of the coal, which is the effect of the action of the fireman in lighting and stoking, which is the effect of the action of his immediate superior in giving the order, which is the effect of the action of his superior, and so back to the directors, whose action is determined by the action of the travelling public in demanding means of travelling, which is determined in the long-run by the action of their predecessors in building up the complicated structure of the nation with its needs for travel; and so we might, if we had the knowledge and patience, pursue the series of actions back to the time when men first wandered into this country, to the time when men first were, to the beginnings of life, to the beginnings of the solar system, and further back *ad infinitum*. In this long precession every action was caused by some previous action, and produced, as its effect, a subsequent action; and the same is true of every other cause of

change and of every other change. Action once taken goes on producing its effects in succession for ever.

It is a commonplace that the institutions of a nation are the results of the past history of that nation. The Napoleonic wars, the Revolution, the revocation of the Edict of Nantes, the Great Rebellion, the discovery of America, the Hundred Years' War, the Norman, Saxon, Danish, and Roman invasions, have each and all contributed to making our institutions what they are, and to making us what we are. If Julius Cæsar had not invaded Britain, I should not now be writing on the Regression of Causes, and should probably never have been born.

It is evident, therefore, that although the phrase 'Plurality of Causes,' in the sense in which Mill used it, was a misnomer, and rests upon a confusion of thought, yet there is a sense in which every effect has a plurality of causes—has an indefinitely great multitude of causes, stretching back in continuous series to infinity of past time.

III. The Radification of Causes.—Indirect Causes.

There is more than this, however. The series is not the simple series that has just been sketched. It is a complicated web of infinite intricacy. To take a very simple case, the birth of every child is the effect, and the child is the result, of the actions of its two parents. Two actions were necessary to the production of the effect. The birth of each of these parents was the effect of similar actions on the part of the grandparents, and the parents are the results of these actions, so that in the second generation upwards there were four causes. In the third there were eight, in the fourth sixteen, and at every step backwards, with every preceding generation, the number of causes increases in geometrical progression until it is controlled by the intermarriage of descendants of the same pair. But for this, the number of causes, even in historical times, would be unimaginably great.

It is the same with all other effects. An effect is produced by action upon a certain thing in certain conditions; and for the production of the effect, the thing and the conditions are

just as necessary as the action that is the immediate cause. This thing and these conditions are themselves the results of causes, which are therefore also necessary to the effect. In order to produce the discharge of a gun, it is necessary to pull the trigger. This action is the cause of the discharge. It is the direct and approximately immediate cause; but every action that went to build up the conditions necessary for the discharge was a cause, more or less remote, more or less indirect, of the discharge. A necessary condition of the discharge is that the hammer should be at full cock. The action of cocking the gun was the direct and immediate cause of this result, and as the result is a condition of the discharge, the cause of this condition is a cause of the discharge; an indirect cause, but still a cause, and a cause not very remote. Anyone who is accustomed to scrutinize carefully the meaning of words must feel a certain incongruity in speaking of the cocking of a gun as the cause of its discharge; but I think that the incongruity is much diminished, if indeed it is not altogether removed, but calling it an indirect cause. We may, I think, formulate the following definition :—

An Indirect Cause is a cause of a condition.

Though the trigger is pulled with the hammer at cock, the gun will not be discharged unless it is loaded. The presence of the cartridge in the barrel is a condition of the discharge, and the action of loading the gun is the cause of the gun being loaded, a result which becomes, with respect to the discharge, a condition of the effect. The cause of this result, the loading of the gun, is therefore another indirect cause of its discharge.

It is a condition of the discharge of the gun on the pulling of the trigger that the mechanism of the lock should exist in good order: and the actions of making the lock, nay, on the same principle, all the actions involved in making the gun, are indirect causes of the discharge of the gun. There is more than this, however. The gun is made of certain materials; and the existence at hand of these materials is a necessary condition of making the gun. The actions by which these conditions were brought about, by which the

materials were made, prepared, and collected, are all indirect causes of the discharge of the gun, and causes that are not only indirect, but remote also. And so we may go back to the growth of the tree of which the stock was made, to the deposit of the ore from which the metal was extracted, to the covering by alluvium of the forests which became the coal wherewith the ore was smelted, to the growth of these forests, and as much further back as we please. All these are causes, more and more remote, more and more indirect, of the discharge of the gun.

The action of pulling the trigger is a direct cause of the discharge of the gun, but it is not the only direct cause. The pulling of the trigger caused the fall of the hammer, which caused the explosion of the detonator, and each of these actions was a direct cause of the discharge of the gun. The soldier had orders to fire as soon as the enemy should come within a certain distance. The action of the officer in giving the order was a cause of his pulling the trigger, and so a direct, but a mediate cause of the discharge of the gun. The action of the enemy in coming within the stated distance was another direct cause, but a mediate cause, of the discharge; and all the actions that led up to these causes were causes of the discharge itself, direct causes, but causes more and more remote as the number of actions between the cause and the ultimate effect increases. Thus we may carry the line of direct causes back, through the orders of intermediate officers on both sides to those of the generalissimos; to the causes of the war; to the multitudinous actions of the members of the nations at war that produced their antagonism; and so on. We have already seen that at a very early stage the line of direct causes divides into two, the actions of the soldier's superiors on the one side, and the actions of the enemy on the other; and it would be easy to show that at each step backwards the causes multiply like the ancestry of every individual man, until at length they become unimaginably multitudinous. They still remain direct causes, however remote they may become, as long as action produces action, and the line is not interrupted by the interposition of a condition.

It is manifest from these examples that both the direct and the indirect causes ramify, or rather radify (for causes are evidently rather the roots than the branches of effects), as we go backwards from the effect; and that the further back we go, the more numerous they become. The conditions may be many, and each may have many causes, depending on other conditions, which again may be many, and so on. The direct causes go back in series to an indefinitely remote past; and not in single series, but in series that spread like the spokes of a fan, and that divide and redivide and radify indefinitely.

Yet out of all these different series of innumerable causes, both direct and indirect, it is usual to select one, and to call it *the* cause. On what principle is this selection made? What, for instance, is the cause of the kettle boiling over? The action of the fire, says the master. Leaving the kettle too long on the fire, says the mistress. The neglect of the kitchenmaid, says the cook. The cook sending me upstairs, says the kitchenmaid. The cook's forgetfulness in leaving her apron upstairs, says the housekeeper. Every one of them is right. Each of these is *a* cause; but which is *the* cause?

It may seem that, strictly speaking, we should limit *the* cause to the direct immediate cause, to the action that is nearest to the effect and immediately precedes it; as for instance, in the case of the discharge of the gun to the pulling of the trigger. But we find upon trial that this will not do. In fact we very often assume, as the cause, an action that by no means immediately precedes the effect; and in fact we often do not know the immediate cause, and when we do know it, we often do not take it into consideration. It seems at first blush that the pulling of the trigger is the immediate cause of the discharge of the gun, but a moment's thought shows that it is not. Between the immediate cause and the effect nothing can intervene, nothing can interpose; but the trigger acts through the medium of the mechanism of the lock, and if this mechanism is impaired the discharge may not follow. After passing through the mechanism of the lock the action must reach the hammer, and cause it to fall; and

the action of the hammer is more nearly immediate than that of the trigger. The fall of the hammer strikes the detonator, but even this is not quite immediate, for the detonator may not explode. The truly immediate cause of the discharge is the explosion of the detonator, but this is never spoken of as the cause of the discharge, and is rarely thought of as the cause. We may put immediacy on one side, therefore: it does not determine us in fixing on the cause. Even apparent immediacy does not determine us, for we may as legitimately look upon the order to fire as the cause of the firing as the pulling of the trigger. What then should, and what does determine us in fixing upon one among the innumerable causes of an effect, and calling it *the* cause?

It depends entirely upon the purpose in view, that is, upon the aspect of the matter in which we are interested. The master, the mistress, the cook, the kitchenmaid, and the housekeeper are each of them right about the cause of the kettle boiling over, but they all look at it from different points of view, and for different purposes. The master looks at the matter from the point of view of the physicist, and to him the cause is the physical cause, which happens also to be the immediate cause. The women all look at the matter from the point of view of responsibility, and for the purpose of fixing the responsibility. According to the mistress, the cause was such that someone was responsible. The cook seizes upon the cause that makes the kitchenmaid responsible. The kitchenmaid selects the cause that throws responsibility upon the cook; and the housekeeper chooses the cause that not only supports the kitchenmaid but throws a double measure of responsibility on the cook.

During shooting at the butts, a trespasser gets into the line of fire, and is killed by a bullet. What is the cause of his death? That depends entirely on the point of view and the purpose of the person who makes the enquiry. To the physiologist it is arrest of the heart's action; to the pathologist it is the effusion of blood round the heart which stopped the heart's action; to the student of ballistics it is the low trajectory of the bullet; to the marksman it was the force of the wind, which deflected

the bullet from the line of aim; to the ammunition expert it is the issue of the new light bullet, which yields more to the force of the wind than the men are accustomed to; to the squad instructor it was the failure of the marksman to respond promptly enough to the order 'Cease fire'; to one leader-writer it is the deplorable carelessness of the soldier; to another it is the stupidity of the civilian in crossing the line of fire; and so we could go on multiplying causes *ad infinitum*. The fact is that everyone of these may quite legitimately be considered a cause, but if we ask which is *the* cau se it is evidently quite impossible to reply until we know for what purpose the question is asked. Is it to fix responsibility? Is it to prevent similar effects in future? Is it to determine the mode of flight of the new bullet? Is it to clear up a nice point in pathology? It may be any of these, and according to the purpose of the argument will be the answer to the question What was the cause?

IV. The Cooperation of Causes.

Every effect is, as we have seen, the product of a long and complicated web of causes stretching back into infinity, all of which are necessary to produce the effect; and therefore every effect is in a sense due to a cooperation of causes. There are, however, cases in which an effect is due in a special sense to a cooperation of causes. We have found that it is a frequent and a legitimate practice to single out one of the multitude of causes to which a given effect is due, and to call that *the* cause, which it is from a certain point of view, and for a particular purpose. We call it *the* cause, because it is that one of the causes in which we are for some purpose interested, and because on that account we allow our contemplation to rest upon it to the ignoring of the rest. Just in the same way, and for a purpose, we may select from a series of causes a certain length of the series, comprising a certain number of successive causes, and limiting or extending our contemplation to them, we may regard them as in a special sense the causes of the effect; and in such a case we regard them as cooperating more closely and more specially with one another

to produce the effect than the other causes, which, for the purpose in hand, we leave out of our consideration. Or two actions may simultaneously take place on one body, so that the changes they severally produce are merged and blended in a single change; and then we naturally contemplate them in association with each other, and regard them as cooperating to produce that change. Every effect is in fact due to the cooperation of many causes, direct and indirect, immediate and remote; but according to the purpose in hand we limit our contemplation to one, two, or a limited number.

Thus regarding them, we may make several classes of cooperating causes, according, first, as the causes we consider are like or unlike, and second, as they operate successively or simultaneously.

Cooperation of Like Causes in Succession.

An instance of like causes cooperating in succession to produce a certain effect is seen when a nail is driven home by repeated blows of a hammer. Each blow produces a certain effect on the nail, and drives it further in. In a sense, and from one point of view, it is the final blow only that drives the nail home; but if it is more convenient for any purpose to contemplate the operation as a whole, then we may regard, not each blow as driving the nail for a certain distance, but the whole series of blows as causes cooperating in producing the complete effect of driving the nail home.

Actions may be like in kind though they are unlike in sign. The action of paying money into the bank is like in kind to the action of drawing money out of the bank, since they are both transfers of money with reference to the bank; but they are unlike in sign, the one kind adding to the balance and the other diminishing it; but the two causes cooperate in succession to bring about the result, the amount of the bank balance.

Simultaneous Cooperation of Like Causes.

The flow of a large body of water from the upper reaches of a tidal river may coincide with an unusually high tide to pro-

duce in the lower reaches a flood, that would not have occurred but for the simultaneous cooperation of the two causes. The simultaneous rush of all the passengers to the side of the boat may cooperate to make the boat capsize. If a bullet or a bird flies across in front of a photographic camera at the moment the shutter acts, an image of the flying object will be formed upon the plate. If the actions are not simultaneous, no such effect will be produced.

Indifferent Cooperation of Like Causes.

Like causes may cooperate to produce an effect or a result independently of whether they act successively or simultaneously. If one force acts upon a body so as to move it to the north, and another equal force acts upon it for an equal time so as to move it to the east, the effect will be that the body will reach a certain point to the north-east, which will be the same whether the forces act simultaneously or in succession. If we add the two components of a Seidlitz powder to a glass of water, the effect is the same whether we add them simultaneously or successively.

Successive Cooperation of Unlike Causes.

When unlike causes cooperate in succession to produce an effect, it is almost always necessary that they should operate in a certain order; and unless this order of succession is strictly observed, the effect will not be produced. The great majority of effects and results that are produced by human agency are of this class. When a thing is to be made, the materials must first be provided, and then one operation after another is followed in a certain order, and the effect and the result are looked upon as due to the cooperation of all these processes. When bread is to be made, the flour and water are first provided, then the dough is mixed, then it is leavened, then kneaded, then allowed to rise, divided, and baked; and these operations must follow one another strictly in this order if the effect is to be produced. The final effect, the production of bread, is due to the cooperation of the various causes in orderly succession.

If any one is omitted, or done out of its turn, or bungled, the effect is spoilt, the result is a failure. And so whenever anything is made by art of man, it is made by certain actions in orderly succession, and the whole series of actions cooperate to produce the thing made. There is actually no break in the long chain of causes, direct and indirect, stretching back indefinitely into the past; nor in the long chain of effects and results stretching forward from the moment the thing was made; but the beginning and ending of the making form convenient artificial or conventional boundaries to the section of the chain to which we limit our contemplation. We must limit the scope of our contemplation, because of the limitation of our powers, which cannot grasp an indefinite length of chain; and boundaries must be placed somewhere; and the boundaries fixed by the beginning and ending of the making of a thing are apt for our purpose. In contemplating causes, no less than in every other operation of mind and body, we have a purpose in view, and it is their indifference to purpose, and their ignoring of it, that render the speculations of the philosophers described in the first Chapter so curiously detached, irrelevant, and pointless. Our purpose in investigating how a thing is made, or comes to be, is to make it or prevent its being made, to cause it or help it to be, or to prevent or hinder it being; or in any case to get some advantage out of our knowledge, even if it is only the advantage of satisfaction in knowing more than we did before. The only causes we need take into consideration are those that answer our purpose, whatever that may be: to consider more would only lead to confusion and embarrassment. That is why, in grouping together as cooperating causes the actions whereby a thing is made, or comes to be, we fix an arbitrary limit beyond which we do not at the moment go. We stop short at that stage, not because we imagine that the causes began at that stage, but because it is among the causes subsequent to that stage that we expect to find those that we can initiate, facilitate, hinder, or destroy. For the purpose in view, the group is a natural group, and the limits are convenient limits, and none the less so because for some other purpose we may find it desirable to extend or to contract them.

Simultaneous Cooperation of Unlike Causes.

Unlike as well as like causes may cooperate simultaneously or contemporaneously to produce an effect which, but for their simultaneous or contemporaneous cooperation, would not have been produced. Plants will not thrive except under the combined action of light, warmth, and moisture. Without light they will grow, but they will not thrive. Without some degree of warmth, varying with the nature and habits of the plant, it will not thrive, or even live; neither will it thrive if desiccation is carried beyond a certain point, or live if it is carried beyond a certain further point. Iron rusts under the simultaneous cooperation of moisture and of oxygen. In dry air it will not rust, though constantly in contact with oxygen. Immersed in water free from dissolved oxygen it will not rust, although it is kept constantly wet. It requires the simultaneous operation of the two causes to produce the effect. A man who refuses to do a thing under threat of punishment for non-performance, and refuses to do it for reward, may yet be induced to do it by combining the threat of punishment with the promise of reward. When a glass tube is held horizontally in a flame until it softens, it will bend; and the bending is the effect of the cooperating action of heat and gravity acting simultaneously. The running of a motor car, the action of an engine, are the effects of numbers of causes acting contemporaneously.

Indifferent Cooperation of Unlike Causes.

Finally, unlike causes may cooperate to produce an effect when it is immaterial in what order the causes act, or whether they act simultaneously or in succession. A business firm may be ruined by the cooperation of the defalcation of a clerk and the failure, either at the same time, or before, or after, of a debtor for a large amount. A man's death may be due to the cooperation of several diseases, which would have effectually killed him in whatever order they attacked him, together or

successively. Rain and frost combine to produce the fall of a mass of earth from a cliff, and in what order they act upon the cliff is immaterial.

V. The Law of Universal Causation.

We are now done with the first of Dr. Fowler's propositions, and may consider the second, that every event has a cause. This is what is known as the Law of Universal Causation, and not only do logicians commonly confuse it, as Dr. Fowler points out, with the definition of cause, and with the Uniformity of Nature, but also it comprehends within itself four distinct problems which are usually confused together. They are as follows:—Does everyone believe that every event has a cause? If so, what is the warrant for the belief? Is it true? and How do we come by it?

In the first place, what is meant by an event? I think we may say without fear of objection that an event is that which happens, and inevitably implies a change; and as we have seen, the idea of change is necessarily bound up in the idea of effect. But changes are not the only effects. The prevention of change equally demands a cause for its existence; and, with some straining of the sense of words, unchanges may be included in events. Taking this to be the meaning of event, then it is evident that events are synonymous with effects; or, if unchanges be excluded from the denotation of events, then event is synonymous with one of the two classes of effect. The first question then becomes Does everyone believe that every effect has a cause? or Does everyone believe that a particular kind of effect has a cause? It seems to me that these questions must necessarily be answered in the affirmative. Effect implies cause, as husband implies wife, or any other relative implies its correlative. They are of course separable in thought, as, indeed, they are separable in fact, but, being correlative, their constant association in fact cannot be denied.

Moreover, I think there is abundant evidence that not only human beings, but many of the lower animals also, assume causation for every change which is a change to them—which

is appreciated by them as change. Horses shy, dogs bark, birds and animals of various kinds rush away, when events occur to which they are unaccustomed, that is to say, which are out of their ordinary routine, and to them imply change. And I think we may safely assume that when horses shy and dogs bark at such things they do so because they apprehend danger, which is as much as to say that they have causation in their minds. They apprehend the causation of harm to themselves. In the same circumstances all timid animals either bolt, or conceal themselves, or behave otherwise in a way that indicates that they apprehend danger. In all such cases the change is viewed as the effect of some cause, and the cause of that effect may produce other effects, and effects detrimental to the witness. Of all the changes in surroundings that excite in both animals and man the danger reaction, none is more potent than an unexpected noise; and no one apprehends danger from noise. The apprehension is that, as there is a noise, there must be an agent to cause the noise, and that what has caused this effect may cause other effects. I think therefore that the evidence is that every man does believe that every event has a cause.

This opinion is corroborated by considering the way by which we come into possession of it. I do not say that it is the only way, but I do not think it can be disputed that the chief source of this belief is as follows:—Man, and all his ancestors throughout an immeasurable past, have lived by action; and every act of theirs has been an instance of causation. It has been an action on something, and has produced or prevented a change in the thing acted on. It has been a cause, and has produced an effect. Hence the notion of causation is in every individual of very early origin, and with respect to his own action is inescapable and perpetual. Contemporaneous with this enormous body of positive experience, is the negative experience, equally inescapable, and equally perpetual, that we cannot produce or prevent change in anything without acting on that thing, either directly or indirectly. Hence experience, from the dawn of consciousness to its last oblivion, perpetually enforces upon

us the conviction that change or prevention of change cannot occur without action of or on the thing changed; in other words, that every event has a cause. I think, therefore, that the evidence warrants us in saying that everyone who is capable of forming the notion of causation does believe that every event has a cause, and that he derives this belief from experience. It may be well to point out that though I hold the empirical origin of this belief, I do not found it upon the supposition that the will is the cause of bodily movements. Whether this is or is not a case of causation, it does not enter into the demonstration.

The next question is, Granted that we do entertain this belief, what is our warrant for it? The warrant has already been indicated. It is in experience. It is experience repeated with incalculable frequency without a single contrary instance. When I say without a single contrary instance, I do not mean that in every case of change or prevention of change we are able to assign a particular cause, or identify the cause; that of course would be directly contrary to experience. I mean that in no case of change or prevention of change that has ever occurred in experience are we able to exclude a cause, or to be certain that no cause has acted. As I have said elsewhere, this is the conclusive test of truth for us—that conduct founded upon a supposition never brings us up against experience that contradicts the supposition. This is the highest warrant we can have. Granted that the experience is obtainable, granted that actions on the supposition are incalculably numerous and diverse, then the fact that experience has never shown the supposition to be false, not merely warrants us in believing that it is true, but compels us to believe it is true. The belief is inescapable; and however strongly we may in words deny it, the first time we act we shall prove our belief in it by acting upon it.

The third of the four questions put at the beginning of this section was Is it true? Apart from our belief in it, is it true that every event has a cause? After the foregoing discussion, this question ceases to have any meaning. If we have in support of a supposition, and based upon it, incalculably

numerous experiences, not one of which has ever contradicted the supposition, then *for us* that supposition is true. It is certain. We are precluded from doubting it. We may put together the words expressing a doubt, but those words have no answering relation in our minds. That every event has a cause is true in the sense that we cannot doubt it. Whether it is noumenally true we cannot know, and it would not matter if we did. It is true for us. It is true as far as we are concerned. To ask whether it is really true is to ask whether there is a higher degree of certainty than certainty itself—whether that which is true for us may not be false in some sense which we cannot clearly conceive, and with which we are not concerned. The importance of knowledge is its influence upon conduct; and in the influence they respectively exert upon conduct there is no appreciable difference between that which is universally true to all men, at all times, in all places, and that which is noumenally true.

VI. The Uniformity of Nature.

The Axiom of Causation.

We are now arrived at the third of those propositions which Dr. Fowler justly says few writers have not more or less confounded, that the same cause is always attended with the same effect. Dr. Fowler calls this the Law of the Uniformity of Nature, and the title may as well be retained, though other writers use it in other senses. In this case again there are four different problems comprised in the one proposition; that is to say, Do men universally believe that the same cause is always attended by the same effect? If so, How do they come to believe it? Is it true? and What is their warrant for believing it?

Does every man believe 'that the same cause is always attended by the same effect'? This is the way in which the problem is stated by Dr. Fowler, but Mill puts it differently, and few writers seem to appreciate the difference. Mill puts it that every consequent has an invariable ante-

cedent; by which he probably means that the same effect is always due to the same cause; which is the converse of Dr. Fowler's problem; and as we have seen, Mill says this although he has a whole Chapter on the Plurality of Causes, by which he means that the same effect may be due to very different causes.

It is clear that the answers to both of these questions must depend upon the definitions that we adopt of cause and effect, and will be very different if we adopt one definition from what they will be if we adopt another; but most of all they will be influenced by our definition of the word 'same,' which most writers on this subject, I think I may say all, interpret so that it includes 'different.' It is perhaps this uncertainty about the meaning of the chief terms employed that is responsible for the differences of different writers on the subject. Some assert that Nature is uniform; some deny that Nature is uniform; some neither assert nor deny it; some, like Mill, both assert and deny it; and few of them mean by it the same thing. In this chaos I shall follow Dr. Fowler, who does at any rate say clearly what he means in this, as in most things.

His reading of the Law of the Uniformity of Nature is that the same cause is always attended by the same effect. Is this true? As I have already said, it depends on what we mean by the chief terms employed. If a cause means the invariable antecedent of an effect, and if an invariable antecedent means an antecedent that is the same in every case, then whether or no the same effect always is attended by the same cause, it does not follow that the same cause is always attended by the same effect, and Mill's Plurality of Causes forbids us to suppose that it does. In Mill's sense of cause, therefore, Nature is certainly not uniform in Dr. Fowler's sense. Whether it is uniform in Mill's sense we cannot tell, for Mill muddles up the Uniformity of Nature with the Law of Universal Causation. To Mr. Welton, cause and effect are the same thing, and in this meaning of the word 'cause' of course Nature is Uniform, for the same cause must always be attended by itself, which is the same effect;

and the same effect must always be attended by the same cause—by itself. Professor Karl Pearson denies the existence of both cause and effect, but yet his expressions 'a routine of perceptions,' 'a routine of experience,' 'a routine of sense impressions' appear, when taken with their context, to mean what other writers mean by the Uniformity of Nature. If, however, there is no cause and no effect, of course there can be no Uniformity of Nature in Dr. Fowler's sense. Mr. Bertrand Russell's statement of 'causality' includes the assertion that there is a constant relation between the state of the universe at one instant, and a certain rate of change at that instant. The constancy of the relation would seem to imply that the nature of the universe is uniform; but as Mr. Russell denies that the law of causality (whether his own or only that of others I do not know) is anything but a relic of a bygone age, it would seem that he does not admit that Nature is uniform in Dr. Fowler's sense. All that Dr. McTaggart can conclude after an exhaustive discussion is that it is impossible to prove empirically that the law does not hold universally. Here I will leave the authorities, and discuss the matter on the basis of my own definitions.

Does the same cause always produce the same effect? That is the problem we have to solve. According to my first provisional definition, a cause is an action. Does the same action always produce the same effect? Take the blow of a hammer for instance: does the blow of a hammer produce the same effect whether it falls on the head of a nail, or the side of a bell, or a man's fingers, or a bale of wool, or a sheet of water? Clearly, in this sense of the word 'cause' the same cause does not always produce the same effect, and Nature is not uniform. But this definition of cause was provisional only. It was subsequently elaborated into this: that a cause is an action upon a thing; and the question now becomes Does the same action on the same thing always produce the same effect? Again let us take our hammer and strike with it our sheet of water. The effect is a splash. Now let the same water be frozen, and let us strike it again. The same effect is not produced. It may be objected that the thing on which the cause acts is no longer the same

thing, but it is quite arguable that it is the same thing. It is certain, however, that it is not for the purpose of the argument the same thing. Then in what respect does it differ? Liquidity and solidity are, for the purpose of the argument, passive states of the thing acted on by the cause, and according to the definition already given, a passive state of the thing acted on by the cause is a condition. It is evident, therefore, that the question we are discussing, Does the same cause always produce the same effect? must be answered in the negative unless we amend it by inserting a reference to the conditions; and the question ought to be put in the form Does the same cause in the same conditions always produce the same effect? But this is instance of the fallacy erroneously called the fallacy of many questions, which should be called, as it is called in my *New Logic*, the fallacy of the previous question. It implies that a previous question, which has not been answered, has been answered. It implies that the same action can take place for a second time upon the same thing in the same conditions; and this is not only impossible, but is acknowledged to be impossible by many of those who insist that the same cause always, or as they say invariably, produces the same effect.

Πάντα ῥεῖ. 'All existence,' says Mr. Welton for instance, 'is continuous and uninterrupted transition,' and 'uniformity itself is not to be taken to mean resemblance. It is in identity alone, not in mere resemblance, that we can find a firm basis of inference.' But if all existence is continuous and uninterrupted transition, or change, it is clear that a state of things once passed can never in all respects be reproduced, unless time should flow backwards, and of this we have no experience; and it is a commonplace that the same state of things never is reproduced. To get the same effect, the same cause must act on the same thing in the same conditions; and the cause is never the same, the thing is never the same, and the conditions are never the same. Therefore *cadit quæstio*. In this sense, there is certainly no such thing as Uniformity in Nature.

Yet the aphorism that the same cause invariably produces the same effect, clumsily though it is asserted, and untrue though it is, is the adumbration of a truth, and of a most valuable

truth. It is not true in any sense that the same cause invariably produces the same effect; but if we recognise what logicians are groping after, and put it into precise and accurate language, we can assert a very important truth, upon which all our methods but three of ascertaining causes are founded, a truth without which but few causes would ever be discovered. It is this, that *Like actions on like things in like conditions produce like effects;* and *The more nearly alike the actions, the things acted on, and the conditions, the more closely alike will the effects be.* We may put the same thought more concisely in the following aphorism :—*Like causes in like conditions produce like effects.*

I do not think this aphorism needs proof. I doubt whether it is susceptible of proof. It seems to me to be an axiom. As soon as its meaning is grasped, it claims and secures our assent. Its contradictory, if not actually inconceivable, is certainly incredible. Whether its truth is manifest *a priori* or is based upon experience I do not care to speculate. The universal experience of mankind goes to show that, whether of empirical origin or not, it is empirically true ; and if we like to call it an instance, or an example, or a proof, of the Uniformity of Nature, I don't know that any harm will be done—or any good. We may, if we please, call it a proof of the Uniformity of Nature, just as we may call the axiom that things that are equal to the same thing are equal to one another, and the axiom that two straight lines cannot enclose a space, proofs or examples of the Uniformity of Nature.

Whether it is or is not the principle of the Uniformity of Nature, or an example or a proof of this principle, the aphorism is the fundamental Axiom of Causation, and upon it all our reasonings about causation are founded, and all but three of our means of ascertaining causes are based. In practice it is one of the most important guides of life, and is employed continually throughout life by everyone, either in the fundamental form in which it has been stated, or in one or other of its very numerous variants and derivatives. Of these, that which is perhaps most frequently employed is the axiom *Like effects in like conditions are due to like causes;* but as I have said, the derivatives are numerous, and every one of them is of frequent application. It

would be tedious to cite them all, but the following are samples, and we may, if we please, call each of them an instance or a statement of the Uniformity of Nature.

Like causes in like conditions produce like effects.
Like causes in unlike conditions produce unlike effects.
Unlike causes in like conditions produce unlike effects.
Like effects in like conditions are due to like causes.
Unlike effects in like conditions are due to unlike causes.
If like causes produce like effects the conditions are alike.
If like causes produce unlike effects the conditions are unlike.
And so on.

Summary.

There is no such thing as Plurality of Causes in Mill's sense. What he meant was that in different cases different causes produce different effects that have some element in common, and this common element he called the effect, and said that it might have many causes. His error was in generalising the effects without generalising the causes.

But every effect is due to a series of causes stretching back into infinity.

And this series is not single, but every effect requires both a cause and conditions, and the conditions are themselves the results of causes; every effect is therefore due to an indefinitely large number of series of causes converging on the effect.

The cause of a condition is an indirect cause.

The cause of a cause is a direct, but more or less remote cause.

The cause of an effect is that cause in which for a certain purpose we are most interested.

To produce an effect, causes may cooperate in any of the following ways.

Like causes may cooperate in succession, simultaneously, or indifferently.

Unlike causes may cooperate in succession, and then must preserve a certain order; or simultaneously; or indifferently.

The Law of Universal Causation has, in the books, several incompatible meanings. It appears to be indisputable that we believe that every event has a cause, and that this belief is shared with us by many of the lower animals. This belief is founded upon the constancy of our experience, and is true, or at any rate is inescapable.

The Law of the Uniformity of Nature, as stated in the books, is nonsense. Neither the same cause nor the same effect is ever repeated. The true Axiom of Causation is that Like causes in like conditions produce like effects, and the more closely alike the causes and the conditions, the more closely alike will be the effects. On this axiom almost all our reasonings with respect to causation are founded.

CHAPTER VI.

METHODS OF ASCERTAINING CAUSES.

WHEN we have discovered an action upon the thing changed or maintained unchanged, and have determined that the action precedes the change or accompanies the unchange, we have still not ascertained the cause; we have only cleared the ground in preparation for doing so. The cause is not ascertained until we have established a necessary connection between the action and the effect. This is what Mill's Methods of Experimental Enquiry are designed to secure. Mill assumed, and the assumption is adopted from him by subsequent writers on the subject, that the only way to discover causes is by experiment, and that the only aim of experiment is to discover causes. Both assumptions are manifestly and transparently false, and are contradicted by everyday experience. Some of the methods described by Mill himself as experimental are not experimental, indeed he admits that one of them is not; and some of the instances he gives of the determination of causes are instances of the determination not of the causes of things, but of their existence, or their nature.

Logicians as a rule know nothing of natural science except what they mug up for the purpose of finding instances wherewith to illustrate Mill's five methods, which he and they all call four. They have therefore no means of knowing whether these methods are used or not; but they accept Mill's confident assertion that in scientific investigations these methods and no others are used. But though logicians know nothing of natural science or of its methods except what they learn from

Mill, they cannot help, in common with the rest of the world, assigning causes for the various events they meet with in their daily lives; nor can they help seeing that in thus ascertaining causes, none of Mill's methods is ever used. They naturally conclude that the methods of science and the methods of daily life are utterly and totally different; that when a man enters his observatory or his laboratory he strips himself at the door of all the methods he is accustomed to use, and employs an entirely new set, a set of methods that are mysterious, recondite, and complicated, that logicians regard with awe, and do not venture to criticise. To these methods they give the name of the Logic of Science, and they suppose that non-scientific people have to be satisfied with a different and very inferior Logic. This is all moonshine.

I assert, and the present chapter is designed to prove, that the methods by which scientific men ascertain the causes of those phenomena that are called scientific are precisely and exactly the same as those by which the cook ascertains the cause of the dinner being spoilt, and the child ascertains the cause of its toy being broken. I assert, and will presently prove, that the methods so clumsily and uncouthly described by Mill are in fact never employed; that they never could be employed, for they are absurd, and when applied to actual cases result in futility; and I assert that when we seek to ascertain the causes of things, and when we do ascertain them, we look for an action upon the thing on which the effect is produced, that is, on the thing changed or maintained unchanged; and we are guided in our search, as well as determined in our choice, by one or more of the following considerations:—

I. Instant sequence of the effect on the action.

II. Subsumption of the case in hand under a general law.

III. Assimilation of the case in hand to a known case of causation.

IV. Association of the action with the effect.

V. Concurrent and proportional variation of the action and the effect.

VI. Common rarity of the action and the effect.

VII. Correspondence of a quality in the effect with a quality in the agent.

VIII. Coincidence in space of an action or a condition with the effect.

IX. Coincidence in time of the action with the effect.

The fifth of these methods, that of establishing an association between the action and the effect, is further divisible into four subordinate methods; so that altogether there are at least twelve methods of ascertaining causation; and these we may now proceed to examine.

I. Instant Sequence.

When an action upon a thing is instantly followed by a change in that thing, we are irresistibly driven to conclude that the action is the cause of the change.

When a china cup falls to the ground and breaks at the instant of its impact on the ground, we do not need to witness 'two or more instances in which the phenomenon occurs' or 'two or more instances in which the phenomenon does not occur' before we can make up our minds that the action of the impact was the cause of the breakage. We are driven to the conclusion that this action was the cause of this effect; and the main, if not the only reason for our conclusion is the instant sequence of the effect on the action. As already said, the writers upon causation seem to think that causes never are attributed, and that there is no need for the discovery of causes, except in the laboratory or the observatory, or in matters that are called, with more or less justice, scientific. There was never a greater mistake. We are all of us engaged daily, hourly, and almost momentarily, in the ascertainment and attribution of causes; and it is much more important to each of us in our lives to attribute causation correctly in matters that pertain to our immediate welfare, than that we should ascertain the causes of the perturbation of a planet, or of the mimicry of butterflies. Among the means by which we ascertain causes in our daily work, the instant

sequence of an effect upon an action is perhaps the most frequent, and is by no means the least important. Nor is the employment of this means confined to trivial matters of daily occurrence. It is just as important and just as trustworthy in the laboratory. When the chemist adds one clear liquid to another, and a precipitate is instantly formed, he concludes at once that the addition of the reagent was the cause of the formation of the precipitate; and he forms this conclusion because of the instant sequence of the turbidity of the liquid on his action in adding the reagent.

If we see a match applied to a thing, or a blow struck upon it, and that thing instantly explodes, we attribute the explosion to the application of the match or the striking of the blow; and this we do without any need of two or more instances in which the phenomenon occurs, and two or more instances in which it does not occur. The instant sequence of the change on the action assures us that they are effect and cause. Anyone quite ignorant of military evolutions who should see the troops alter their formation immediately on hearing a bugle call, would instantly regard the call as the cause of the movement. If we pour oil into the bearings of an engine, and the engine instantly increases its speed, or if we do the same to a foot-lathe, and the lathe instantly runs easier, we have no hesitation in attributing the change of speed, or the easier working, to the action of lubrication. If a horse's head is turned towards home, and he instantly improves his pace, we inevitably connect the improvement causally with the change of direction. If a bell rings or a whistle sounds in a factory, and the workmen all instantly drop their tools, we cannot help regarding the cessation from work as the effect of the sound; and similarly, when the air is thick with the chirruping of birds, if a gun is fired, instantly a dead silence ensues. We cannot help attributing the sudden occurrence of the silence to the report of the gun.

In some of these cases there may be other reasons which corroborate our judgment, and in fact our judgment of causation is seldom formed upon one method alone. Usually two or more methods corroborate one another, and the third method, the Method of Similarity, is seldom quite absent; but

in others of the cases that have been instanced it is clear that the conclusion was based upon the instant sequence of the effect on the action, and upon no other method. One who had never before seen a galvanometer, and knew nothing of electric action, who should see the needle move the instant the key was depressed, could scarcely avoid attributing the change to the action.

Of course, the method is not infallible. In this imperfect world few methods are infallible. In some cases it needs corroboration or testing by some one or more of the other methods. But for all that, it is a method; it is a method that is constantly in use; it is a method that by itself may lead to a perfectly reliable conclusion; and it is a method that is not mentioned by any previous writer on the subject. Its fallibility is shown by the familiar instance by which a child is made to believe that he can cause the cover of a watch to fly open by blowing on it; but what is more important, the same instance shows how very early in life the conclusion is thrust upon us, that a change that follows instantly upon an action is the effect of that action.

Mill and his commentators must each of them have used this method thousands of times, but they none of them record it, whether because it is difficult to put it into cumbrous and obscure language, or because they do not consider it sufficiently 'scientific,' I do not know.

II. Subsumption.

The second method of establishing a causal connection between an action and an effect is by subsuming the instance in hand under a general law. If this can be done, causal connection is assured, and neither Mill's Canons nor any other device is required to assure us of the necessary connection between the action and the effect.

Whether the tides were associated with the moon before the discovery of gravitation I do not know; but as soon as gravitation was discovered, and was applied to the action of the moon upon the seas, it must have become apparent at once

that the moon's attraction must be the cause of tidal changes in the level of the seas; and if tides had never before been observed they would now be looked for. The action of the moon on the sea, and the sequent change in the level of the sea, are subsumed under the general causal law of gravitation, and this subsumption gives us the assurance that the action is the cause of the change.

When our waterpipes burst in winter, we find the cause at once by subsuming the case under the general law that water in freezing expands with immeasurable force; and by this subsumption the action of the frost and the bursting of the pipes are connected. When the cook goes to the cupboard for a pot of jam, and finds it is not there, she says at once 'Someone must have taken it.' She subsumes this instance under the general law that inanimate things do not move from their places without external agency. When the price of fish rises, and we hear of gales in the North Sea, we assume a causal connection between the action and the change, and we do so on the strength of the general law that, other things remaining the same, restriction of supply raises prices; and we know that gales in the North Sea do restrict the supply of fish to this country. If the river overflows its banks, we assume, unless it is a tidal river, that there has been much rain in its catchment basin, and we make this assumption on the strength of the general law that *caeteris non mutandis*, the level of a river depends on the rainfall in the catchment area. If we find an object of gold or silver that shows signs of having been melted, we assume at once that it has been subjected to great heat, for it is a general law that great heat is necessary to the melting of gold and silver. If we find iron rusty, we assume that it must have been damp, for it is a general law that dry iron does not rust. When we are seeking the cause of a rare disease, and we find that it affects the members of several families in conformity with the laws of Mendel, we have no hesitation in concluding that the cause is hereditary transmission.

Neither in these cases do we look for two or more instances of the phenomenon, and ask if they have only one circumstance

in common, nor do we look for two or more instances in which the phenomenon does not occur, and ask if they have nothing in common but the absence of the phenomenon. What we do is to subsume the case in hand as an instance under a general law applicable to such instances; and if the subsumption is good, then the causal connection is made out to our satisfaction. This method, which is distinct enough in cases like the tides and the Mendelian inheritance of disease, is in other cases less pronounced, and graduates and merges into the next.

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 move-

ments 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 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.*

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 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 o ut; 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 of 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 confirmed. 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, 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 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 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 unconscious 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 consideration 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 *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 consisted 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.

(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.

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 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 contributory) 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 resistance 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 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 suspect 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 important, 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.

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

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).

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

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 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 associa-

tion 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.

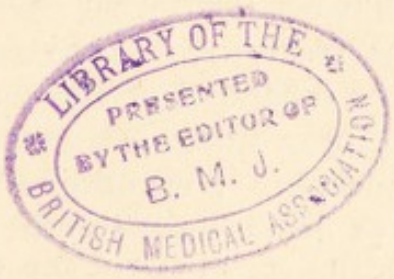
The first three pairs may all be included under Primary and Secondary. The last pair cannot be so included.

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 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 assertion 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. 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 ventrilo-

quist 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 Ercildoune, 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 axio-

matic 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 constancy 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, 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 aborious 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 of 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 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.

Disproof is evidence inconsistent with the 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. Unfor-

tunately 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 innumerable 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.

Disproof is evidence inconsistent with 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.

ADLARD AND SON AND WEST NEWMAN, IMPR., LONDON AND DORKING.