Chapter XVII.

Of Chance And Its Elimination.

§ 1. Considering, then, as empirical laws only those observed uniformities respecting which the question whether they are laws of causation must remain undecided until they can be explained deductively, or until some means are found of applying the Method of Difference to the case, it has been shown in the preceding chapter that until a uniformity can, in one or the other of these modes, be taken out of the class of empirical laws, and brought either into that of laws of causation or of the demonstrated results of laws of causation, it can not with any assurance be pronounced true beyond the local and other limits within which it has been found so by actual observation. It remains to consider how we are to assure ourselves of its truth even within those limits; after what quantity of experience a generalization which rests solely on the Method of Agreement can be considered sufficiently established, even as an empirical law. In a former chapter, when treating of the Methods of Direct Induction, we expressly reserved this question,(174) and the time is now come for endeavoring to solve it.

We found that the Method of Agreement has the defect of not proving causation, and can, therefore, only be employed for the ascertainment of empirical laws. But we also found that besides this deficiency, it labors under a characteristic imperfection, tending to render uncertain even such conclusions as it is in itself adapted to prove. This imperfection arises from Plurality of Causes. Although two or more cases in which the phenomenon *a* has been met with may have no common antecedent except A, this does not prove that there is any connection between *a* and A, since *a* may have many causes, and may have been produced, in these different instances, not by any thing which the instances had in common, but by some of those elements in them which were different. We nevertheless observed, that in proportion to the multiplication of instances pointing to A as the antecedent, the characteristic uncertainty of the method diminishes, and the existence of a law of connection between A and *a* more nearly approaches to certainty. It is now to be determined after what amount of experience this certainty may be deemed to be practically attained, and the connection between A and *a* may be received as an empirical law.

This question may be otherwise stated in more familiar terms: After how many and what sort of instances may it be concluded that an observed coincidence between two phenomena is not the effect of chance?

It is of the utmost importance for understanding the logic of induction, that we should form a distinct conception of what is meant by chance, and how the phenomena which common language ascribes to that abstraction are really produced.

§ 2. Chance is usually spoken of in direct antithesis to law; whatever, it is supposed, can not be ascribed to any law is attributed to chance. It is, however, certain that whatever happens is the result of some law; is an effect of causes, and could have been predicted from a knowledge of the existence of those causes, and from their laws. If I turn up a particular card, that is a consequence of its place in the pack. Its place in the pack was a consequence of the manner in which the cards were shuffled, or of the order in which they were played in the last game; which, again, were effects of prior causes. At every stage, if we had possessed an accurate knowledge of the causes in existence, it would have been abstractedly possible to foretell the effect.

An event occurring by chance may be better described as a coincidence from which we have no ground to infer a uniformity--the occurrence of a phenomenon in certain circumstances, without our having reason on that account to infer that it will happen again in those circumstances. This, however, when looked closely into, implies that the enumeration of the circumstances is not complete. Whatever the fact be, since it has occurred once, we may be sure that if *all* the same circumstances were repeated it would occur again; and not only if all, but there is some particular portion of those circumstances on which the phenomenon is invariably consequent. With most of them, however, it is not connected in any permanent manner; its conjunction with those is said to be the effect of chance, to be merely casual. Facts casually conjoined are separately the effects

of causes, and therefore of laws; but of different causes, and causes not connected by any law.

It is incorrect, then, to say that any phenomenon is produced by chance; but we may say that two or more phenomena are conjoined by chance, that they co-exist or succeed one another only by chance; meaning that they are in no way related through causation; that they are neither cause and effect, nor effects of the same cause, nor effects of causes between which there subsists any law of co-existence, nor even effects of the same collocation of primeval causes.

If the same casual coincidence never occurred a second time, we should have an easy test for distinguishing such from the coincidences which are the results of a law. As long as the phenomena had been found together only once, so long, unless we knew some more general laws from which the coincidence might have resulted, we could not distinguish it from a casual one; but if it occurred twice, we should know that the phenomena so conjoined must be in some way connected through their causes.

There is, however, no such test. A coincidence may occur again and again, and yet be only casual. Nay, it would be inconsistent with what we know of the order of nature to doubt that every casual coincidence will sooner or later be repeated, as long as the phenomena between which it occurred do not cease to exist, or to be reproduced. The recurrence, therefore, of the same coincidence more than once, or even its frequent recurrence, does not prove that it is an instance of any law; does not prove that it is not casual, or, in common language, the effect of chance.

And yet, when a coincidence can not be deduced from known laws, nor proved by experiment to be itself a case of causation, the frequency of its occurrence is the only evidence from which we can infer that it is the result of a law. Not, however, its absolute frequency. The question is not whether the coincidence occurs often or seldom, in the ordinary sense of those terms; but whether it occurs more often than chance will account for; more often than might rationally be expected if the coincidence were casual. We have to decide, therefore, what degree of frequency in a coincidence chance will account for; and to this there can be no general answer. We can only state the principle by which the answer must be determined; the answer itself will be different in every different case.

Suppose that one of the phenomena, A, exists always, and the other phenomenon, B, only occasionally; it follows that every instance of B will be an instance of its coincidence with A, and yet the coincidence will be merely casual, not the result of any connection between them. The fixed stars have been constantly in existence since the beginning of human experience, and all phenomena that have come under human observation have, in every single instance, co-existed with them; yet this coincidence, though equally invariable with that which exists between any of those phenomena and its own cause, does not prove that the stars are its cause, nor that they are in anywise connected with it. As strong a case of coincidence, therefore, as can possibly exist, and a much stronger one in point of mere frequency than most of those which prove laws, does not here prove a law; why? because, since the stars exist always, they *must* co-exist with every other phenomenon, whether connected with them by causation or not. The uniformity, great though it be, is no greater than would occur on the supposition that no such connection exists.

On the other hand, suppose that we were inquiring whether there be any connection between rain and any particular wind. Rain, we know, occasionally occurs with every wind; therefore, the connection, if it exists, can not be an actual law; but still rain may be connected with some particular wind through causation; that is, though they can not be always effects of the same cause (for if so they would regularly co-exist), there may be some causes common to the two, so that in so far as either is produced by those common causes, they will, from the laws of the causes, be found to co-exist. How, then, shall we ascertain this? The obvious answer is, by observing whether rain occurs with one wind more frequently than with any other. That, however, is not enough; for perhaps that one wind blows more frequently than any other; so that its blowing more frequently in rainy weather is no more than would happen, although it had no connection with the causes of rain, provided it were not connected with causes adverse to rain. In England, westerly winds blow during about

twice as great a portion of the year as easterly. If, therefore, it rains only twice as often with a westerly as with an easterly wind, we have no reason to infer that any law of nature is concerned in the coincidence. If it rains more than twice as often, we may be sure that some law is concerned; either there is some cause in nature which, in this climate, tends to produce both rain and a westerly wind, or a westerly wind has itself some tendency to produce rain. But if it rains less than twice as often, we may draw a directly opposite inference: the one, instead of being a cause, or connected with causes of the other, must be connected with causes adverse to it, or with the absence of some cause which produces it; and though it may still rain much oftener with a westerly wind than with an easterly, so far would this be from proving any connection between the phenomena, that the connection proved would be between rain and an easterly wind, to which, in mere frequency of coincidence, it is less allied.

Here, then, are two examples: in one, the greatest possible frequency of coincidence, with no instance whatever to the contrary, does not prove that there is any law; in the other, a much less frequency of coincidence, even when non-coincidence is still more frequent, does prove that there is a law. In both cases the principle is the same. In both we consider the positive frequency of the phenomena themselves, and how great frequency of coincidence that must of itself bring about, without supposing any connection between them, provided there be no repugnance; provided neither be connected with any cause tending to frustrate the other. If we find a greater frequency of coincidence than this, we conclude that there is some connection; if a less frequency, that there is some repugnance. In the former case, we conclude that one of the phenomena can under some circumstances cause the other, or that there exists something capable of causing them both; in the latter, that one of them, or some cause which produces one of them, is capable of counteracting the production of the other. We have thus to deduct from the observed frequency of coincidence as much as may be the effect of chance, that is, of the mere frequency of the phenomena themselves; and if any thing remains, what does remain is the residual fact which proves the existence of a law.

The frequency of the phenomena can only be ascertained within definite limits of space and time; depending as it does on the quantity and distribution of the primeval natural agents, of which we can know nothing beyond the boundaries of human observation, since no law, no regularity, can be traced in it, enabling us to infer the unknown from the known. But for the present purpose this is no disadvantage, the question being confined within the same limits as the data. The coincidences occurred in certain places and times, and within those we can estimate the frequency with which such coincidences would be produced by chance. If, then, we find from observation that A exists in one case out of every two, and B in one case out of every three; then, if there be neither connection nor repugnance between them, or between any of their causes, the instances in which A and B will both exist, that is to say will co-exist, will be one case in every six. For A exists in three cases out of six; and B, existing in one case out of every three without regard to the presence or absence of A, will exist in one case out of those three. There will therefore be, of the whole number of cases, two in which A exists without B; one case of B without A; two in which neither B nor A exists, and one case out of six in which they both exist. If, then, in point of fact, they are found to co-exist oftener than in one case out of six; and, consequently, A does not exist without B so often as twice in three times, nor B without A so often as once in every twice, there is some cause in existence which tends to produce a conjunction between A and B.

Generalizing the result, we may say that if A occurs in a larger proportion of the cases where B is than of the cases where B is not, then will B also occur in a larger proportion of the cases where A is than of the cases where A is not; and there is some connection, through causation, between A and B. If we could ascend to the causes of the two phenomena, we should find, at some stage, either proximate or remote, some cause or causes common to both; and if we could ascertain what these are, we could frame a generalization which would be true without restriction of place or time; but until we can do so, the fact of a connection between the two phenomena remains an empirical law.

§ 3. Having considered in what manner it may be determined whether any given conjunction of phenomena is casual, or the result of some law, to complete the theory of chance it is necessary that we should now consider those effects which are partly the result of chance and partly of law, or, in other words, in which the effects of

casual conjunctions of causes are habitually blended in one result with the effects of a constant cause.

This is a case of Composition of Causes; and the peculiarity of it is, that instead of two or more causes intermixing their effects in a regular manner with those of one another, we have now one constant cause, producing an effect which is successively modified by a series of variable causes. Thus, as summer advances, the approach of the sun to a vertical position tends to produce a constant increase of temperature; but with this effect of a constant cause, there are blended the effects of many variable causes, winds, clouds, evaporation, electric agencies and the like, so that the temperature of any given day depends in part on these fleeting causes, and only in part on the constant cause. If the effect of the constant cause is always accompanied and disguised by effects of variable causes, it is impossible to ascertain the law of the constant cause in the ordinary manner by separating it from all other causes and observing it apart. Hence arises the necessity of an additional rule of experimental inquiry.

When the action of a cause A is liable to be interfered with, not steadily by the same cause or causes, but by different causes at different times, and when these are so frequent, or so indeterminate, that we can not possibly exclude all of them from any experiment, though we may vary them; our resource is, to endeavor to ascertain what is the effect of all the variable causes taken together. In order to do this, we make as many trials as possible, preserving A invariable. The results of these different trials will naturally be different, since the indeterminate modifying causes are different in each; if, then, we do not find these results to be progressive, but, on the contrary, to oscillate about a certain point, one experiment giving a result a little greater, another a little less, one a result tending a little more in one direction, another a little more in the contrary direction; while the average or middle point does not vary, but different sets of experiments (taken in as great a variety of circumstances as possible) yield the same mean, provided only they be sufficiently numerous; then that mean, or average result, is the part, in each experiment, which is due to the cause A, and is the effect which would have been obtained if A could have acted alone; the variable remainder is the effect of chance, that is, of causes the co-existence of which with the cause A was merely casual. The test of the sufficiency of the induction in this case is, when any increase of the number of trials from which the average is struck does not materially alter the average.

This kind of elimination, in which we do not eliminate any one assignable cause, but the multitude of floating unassignable ones, may be termed the Elimination of Chance. We afford an example of it when we repeat an experiment, in order, by taking the mean of different results, to get rid of the effects of the unavoidable errors of each individual experiment. When there is no permanent cause, such as would produce a tendency to error peculiarly in one direction, we are warranted by experience in assuming that the errors on one side will, in a certain number of experiments, about balance the errors on the contrary side. We therefore repeat the experiment, until any change which is produced in the average of the whole by further repetition, falls within limits of error consistent with the degree of accuracy required by the purpose we have in view.(175)

§ 4. In the supposition hitherto made, the effect of the constant cause A has been assumed to form so great and conspicuous a part of the general result, that its existence never could be a matter of uncertainty, and the object of the eliminating process was only to ascertain *how much* is attributable to that cause; what is its exact law. Cases, however, occur in which the effect of a constant cause is so small, compared with that of some of the changeable causes with which it is liable to be casually conjoined, that of itself it escapes notice, and the very existence of any effect arising from a constant cause is first learned by the process which in general serves only for ascertaining the quantity of that effect. This case of induction may be characterized as follows: A given effect is known to be chiefly, and not known not to be wholly, determined by changeable causes. If it be wholly so produced, then if the aggregate be taken of a sufficient number of instances, the effects of these different causes will cancel one another. If, therefore, we do not find this to be the case, but, on the contrary, after such a number of trials has been made that no further increase alters the average result, we find that average to be, not zero, but some other quantity, about which, though small in comparison with the total effect, the effect nevertheless oscillates, and which is the middle point in its oscillation; we may conclude this to be the effect of some constant cause; which cause, by some of the methods already treated of, we may hope

to detect. This may be called *the discovery of a residual phenomenon by eliminating the effects of chance*.

It is in this manner, for example, that loaded dice may be discovered. Of course no dice are so clumsily loaded that they must always throw certain numbers; otherwise the fraud would be instantly detected. The loading, a constant cause, mingles with the changeable causes which determine what cast will be thrown in each individual instance. If the dice were not loaded, and the throw were left to depend entirely on the changeable causes, these in a sufficient number of instances would balance one another, and there would be no preponderant number of throws of any one kind. If, therefore, after such a number of trials that no further increase of their number has any material effect upon the average, we find a preponderance in favor of a particular throw; we may conclude with assurance that there is some constant cause acting in favor of that throw, or, in other words, that the dice are not fair; and the exact amount of the unfairness. In a similar manner, what is called the diurnal variation of the barometer, which is very small compared with the variations arising from the irregular changes in the state of the atmosphere, was discovered by comparing the average height of the barometer at different hours of the day. When this comparison was made, it was found that there was a small difference, which on the average was constant, however the absolute quantities might vary, and which difference, therefore, must be the effect of a constant cause. This cause was afterward ascertained, deductively, to be the rarefaction of the air, occasioned by the increase of temperature as the day advances.

§ 5. After these general remarks on the nature of chance, we are prepared to consider in what manner assurance may be obtained that a conjunction between two phenomena, which has been observed a certain number of times, is not casual, but a result of causation, and to be received, therefore, as one of the uniformities of nature, though (until accounted for *a priori*) only as an empirical law.

We will suppose the strongest case, namely, that the phenomenon B has never been observed except in conjunction with A. Even then, the probability that they are connected is not measured by the total number of instances in which they have been found together, but by the excess of that number above the number due to the absolutely frequency of A. If, for example, A exists always, and therefore co-exists with every thing, no number of instances of its co-existence with B would prove a connection; as in our example of the fixed stars. If A be a fact of such common occurrence that it may be presumed to be present in half of all the cases that occur, and therefore in half the cases in which B occurs, it is only the proportional excess above half that is to be reckoned as evidence toward proving a connection between A and B.

In addition to the question, What is the number of coincidences which, on an average of a great multitude of trials, may be expected to arise from chance alone? there is also another question, namely, Of what extent of deviation from that average is the occurrence credible, from chance alone, in some number of instances smaller than that required for striking a fair average? It is not only to be considered what is the general result of the chances in the long run, but also what are the extreme limits of variation from the general result, which may occasionally be expected as the result of some smaller number of instances.

The consideration of the latter question, and any consideration of the former beyond that already given to it, belong to what mathematicians term the doctrine of chances, or, in a phrase of greater pretension, the Theory of Probabilities.