

Chapter IV.

Of Laws Of Nature.

§ 1. In the contemplation of that uniformity in the course of nature, which is assumed in every inference from experience, one of the first observations that present themselves is, that the uniformity in question is not properly uniformity, but uniformities. The general regularity results from the co-existence of partial regularities. The course of nature in general is constant, because the course of each of the various phenomena that compose it is so. A certain fact invariably occurs whenever certain circumstances are present, and does not occur when they are absent; the like is true of another fact; and so on. From these separate threads of connection between parts of the great whole which we term nature, a general tissue of connection unavoidably weaves itself, by which the whole is held together. If A is always accompanied by D, B by E, and C by F, it follows that A B is accompanied by D E, A C by D F, B C by E F, and finally A B C by D E F; and thus the general character of regularity is produced, which, along with and in the midst of infinite diversity, pervades all nature.

The first point, therefore, to be noted in regard to what is called the uniformity of the course of nature, is, that it is itself a complex fact, compounded of all the separate uniformities which exist in respect to single phenomena. These various uniformities, when ascertained by what is regarded as a sufficient induction, we call, in common parlance, Laws of Nature. Scientifically speaking, that title is employed in a more restricted sense, to designate the uniformities when reduced to their most simple expression. Thus in the illustration already employed, there were seven uniformities; all of which, if considered sufficiently certain, would, in the more lax application of the term, be called laws of nature. But of the seven, three alone are properly distinct and independent: these being presupposed, the others follow of course. The first three, therefore, according to the stricter acceptance, are called laws of nature; the remainder not; because they are in truth mere *cases* of the first three; virtually included in them; said, therefore, to *result* from them: whoever affirms those three has already affirmed all the rest.

To substitute real examples for symbolical ones, the following are three uniformities, or call them laws of nature: the law that air has weight, the law that pressure on a fluid is propagated equally in all directions, and the law that pressure in one direction, not opposed by equal pressure in the contrary direction, produces motion, which does not cease until equilibrium is restored. From these three uniformities we should be able to predict another uniformity, namely, the rise of the mercury in the Torricellian tube. This, in the stricter use of the phrase, is not a law of nature. It is the result of laws of nature. It is a *case* of each and every one of the three laws: and is the only occurrence by which they could all be fulfilled. If the mercury were not sustained in the barometer, and sustained at such a height that the column of mercury were equal in weight to a column of the atmosphere of the same diameter; here would be a case, either of the air not pressing upon the surface of the mercury with the force which is called its weight, or of the downward pressure on the mercury not being propagated equally in an upward direction, or of a body pressed in one direction and not in the direction opposite, either not moving in the direction in which it is pressed, or stopping before it had attained equilibrium. If we knew, therefore, the three simple laws, but had never tried the Torricellian experiment, we might *deduce* its result from those laws. The known weight of the air, combined with the position of the apparatus, would bring the mercury within the first of the three inductions; the first induction would bring it within the second, and the second within the third, in the manner which we characterized in treating of Ratiocination. We should thus come to know the more complex uniformity, independently of specific experience, through our knowledge of the simpler ones from which it results; though, for reasons which will appear hereafter, *verification* by specific experience would still be desirable, and might possibly be indispensable.

Complex uniformities which, like this, are mere cases of simpler ones, and have, therefore, been virtually affirmed in affirming those, may with propriety be called *laws*, but can scarcely, in the strictness of scientific speech, be termed Laws of Nature. It is the custom in science, wherever regularity of any kind can be traced,

to call the general proposition which expresses the nature of that regularity, a law; as when, in mathematics, we speak of the law of decrease of the successive terms of a converging series. But the expression *law of nature* has generally been employed with a sort of tacit reference to the original sense of the word law, namely, the expression of the will of a superior. When, therefore, it appeared that any of the uniformities which were observed in nature, would result spontaneously from certain other uniformities, no separate act of creative will being supposed necessary for the production of the derivative uniformities, these have not usually been spoken of as laws of nature. According to one mode of expression, the question, What are the laws of nature? may be stated thus: What are the fewest and simplest assumptions, which being granted, the whole existing order of nature would result? Another mode of stating it would be thus: What are the fewest general propositions from which all the uniformities which exist in the universe might be deductively inferred?

Every great advance which marks an epoch in the progress of science, has consisted in a step made toward the solution of this problem. Even a simple colligation of inductions already made, without any fresh extension of the inductive inference, is already an advance in that direction. When Kepler expressed the regularity which exists in the observed motions of the heavenly bodies, by the three general propositions called his laws, he, in so doing, pointed out three simple suppositions which, instead of a much greater number, would suffice to construct the whole scheme of the heavenly motions, so far as it was known up to that time. A similar and still greater step was made when these laws, which at first did not seem to be included in any more general truths, were discovered to be cases of the three laws of motion, as obtaining among bodies which mutually tend toward one another with a certain force, and have had a certain instantaneous impulse originally impressed upon them. After this great discovery, Kepler's three propositions, though still called laws, would hardly, by any person accustomed to use language with precision, be termed laws of nature: that phrase would be reserved for the simpler and more general laws into which Newton is said to have resolved them.

According to this language, every well-grounded inductive generalization is either a law of nature, or a result of laws of nature, capable, if those laws are known, of being predicted from them. And the problem of Inductive Logic may be summed up in two questions: how to ascertain the laws of nature; and how, after having ascertained them, to follow them into their results. On the other hand, we must not suffer ourselves to imagine that this mode of statement amounts to a real analysis, or to any thing but a mere verbal transformation of the problem; for the expression, Laws of Nature, *means* nothing but the uniformities which exist among natural phenomena (or, in other words, the results of induction), when reduced to their simplest expression. It is, however, something to have advanced so far, as to see that the study of nature is the study of laws, not *a* law; of uniformities, in the plural number: that the different natural phenomena have their separate rules or modes of taking place, which, though much intermixed and entangled with one another, may, to a certain extent, be studied apart: that (to resume our former metaphor) the regularity which exists in nature is a web composed of distinct threads, and only to be understood by tracing each of the threads separately; for which purpose it is often necessary to unravel some portion of the web, and exhibit the fibres apart. The rules of experimental inquiry are the contrivances for unraveling the web.

§ 2. In thus attempting to ascertain the general order of nature by ascertaining the particular order of the occurrence of each one of the phenomena of nature, the most scientific proceeding can be no more than an improved form of that which was primitively pursued by the human understanding, while undirected by science. When mankind first formed the idea of studying phenomena according to a stricter and surer method than that which they had in the first instance spontaneously adopted, they did not, conformably to the well-meant but impracticable precept of Descartes, set out from the supposition that nothing had been already ascertained. Many of the uniformities existing among phenomena are so constant, and so open to observation, as to force themselves upon involuntary recognition. Some facts are so perpetually and familiarly accompanied by certain others, that mankind learned, as children learn, to expect the one where they found the other, long before they knew how to put their expectation into words by asserting, in a proposition, the existence of a connection between those phenomena. No science was needed to teach that food nourishes, that water drowns, or quenches thirst, that the sun gives light and heat, that bodies fall to the ground. The first

scientific inquirers assumed these and the like as known truths, and set out from them to discover others which were unknown: nor were they wrong in so doing, subject, however, as they afterward began to see, to an ulterior revision of these spontaneous generalizations themselves, when the progress of knowledge pointed out limits to them, or showed their truth to be contingent on some circumstance not originally attended to. It will appear, I think, from the subsequent part of our inquiry, that there is no logical fallacy in this mode of proceeding; but we may see already that any other mode is rigorously impracticable: since it is impossible to frame any scientific method of induction, or test of the correctness of inductions, unless on the hypothesis that some inductions deserving of reliance have been already made.

Let us revert, for instance, to one of our former illustrations, and consider why it is that, with exactly the same amount of evidence, both negative and positive, we did not reject the assertion that there are black swans, while we should refuse credence to any testimony which asserted that there were men wearing their heads underneath their shoulders. The first assertion was more credible than the latter. But why more credible? So long as neither phenomenon had been actually witnessed, what reason was there for finding the one harder to be believed than the other? Apparently because there is less constancy in the colors of animals, than in the general structure of their anatomy. But how do we know this? Doubtless, from experience. It appears, then, that we need experience to inform us, in what degree, and in what cases, or sorts of cases, experience is to be relied on. Experience must be consulted in order to learn from it under what circumstances arguments from it will be valid. We have no ulterior test to which we subject experience in general; but we make experience its own test. Experience testifies, that among the uniformities which it exhibits or seems to exhibit, some are more to be relied on than others; and uniformity, therefore, may be presumed, from any given number of instances, with a greater degree of assurance, in proportion as the case belongs to a class in which the uniformities have hitherto been found more uniform.

This mode of correcting one generalization by means of another, a narrower generalization by a wider, which common sense suggests and adopts in practice, is the real type of scientific Induction. All that art can do is but to give accuracy and precision to this process, and adapt it to all varieties of cases, without any essential alteration in its principle.

There are of course no means of applying such a test as that above described, unless we already possess a general knowledge of the prevalent character of the uniformities existing throughout nature. The indispensable foundation, therefore, of a scientific formula of induction, must be a survey of the inductions to which mankind have been conducted in unscientific practice; with the special purpose of ascertaining what kinds of uniformities have been found perfectly invariable, pervading all nature, and what are those which have been found to vary with difference of time, place, or other changeable circumstances.

§ 3. The necessity of such a survey is confirmed by the consideration, that the stronger inductions are the touch-stone to which we always endeavor to bring the weaker. If we find any means of deducing one of the less strong inductions from stronger ones, it acquires, at once, all the strength of those from which it is deduced; and even adds to that strength; since the independent experience on which the weaker induction previously rested, becomes additional evidence of the truth of the better established law in which it is now found to be included. We may have inferred, from historical evidence, that the uncontrolled power of a monarch, of an aristocracy, or of the majority, will often be abused: but we are entitled to rely on this generalization with much greater assurance when it is shown to be a corollary from still better established facts; the very low degree of elevation of character ever yet attained by the average of mankind, and the little efficacy, for the most part, of the modes of education hitherto practiced, in maintaining the predominance of reason and conscience over the selfish propensities. It is at the same time obvious that even these more general facts derive an accession of evidence from the testimony which history bears to the effects of despotism. The strong induction becomes still stronger when a weaker one has been bound up with it.

On the other hand, if an induction conflicts with stronger inductions, or with conclusions capable of being correctly deduced from them, then, unless on reconsideration it should appear that some of the stronger

inductions have been expressed with greater universality than their evidence warrants, the weaker one must give way. The opinion so long prevalent that a comet, or any other unusual appearance in the heavenly regions, was the precursor of calamities to mankind, or to those at least who witnessed it; the belief in the veracity of the oracles of Delphi or Dodona; the reliance on astrology, or on the weather-prophecies in almanacs, were doubtless inductions supposed to be grounded on experience:(113) and faith in such delusions seems quite capable of holding out against a great multitude of failures, provided it be nourished by a reasonable number of casual coincidences between the prediction and the event. What has really put an end to these insufficient inductions, is their inconsistency with the stronger inductions subsequently obtained by scientific inquiry, respecting the causes on which terrestrial events really depend; and where those scientific truths have not yet penetrated, the same or similar delusions still prevail.

It may be affirmed as a general principle, that all inductions, whether strong or weak, which can be connected by ratiocination, are confirmatory of one another; while any which lead deductively to consequences that are incompatible, become mutually each other's test, showing that one or other must be given up, or at least more guardedly expressed. In the case of inductions which confirm each other, the one which becomes a conclusion from ratiocination rises to at least the level of certainty of the weakest of those from which it is deduced; while in general all are more or less increased in certainty. Thus the Torricellian experiment, though a mere case of three more general laws, not only strengthened greatly the evidence on which those laws rested, but converted one of them (the weight of the atmosphere) from a still doubtful generalization into a completely established doctrine.

If, then, a survey of the uniformities which have been ascertained to exist in nature, should point out some which, as far as any human purpose requires certainty, may be considered quite certain and quite universal; then by means of these uniformities we may be able to raise multitudes of other inductions to the same point in the scale. For if we can show, with respect to any inductive inference, that either it must be true, or one of these certain and universal inductions must admit of an exception; the former generalization will attain the same certainty, and indefeasibleness within the bounds assigned to it, which are the attributes of the latter. It will be proved to be a law; and if not a result of other and simpler laws, it will be a law of nature.

There are such certain and universal inductions; and it is because there are such, that a Logic of Induction is possible.