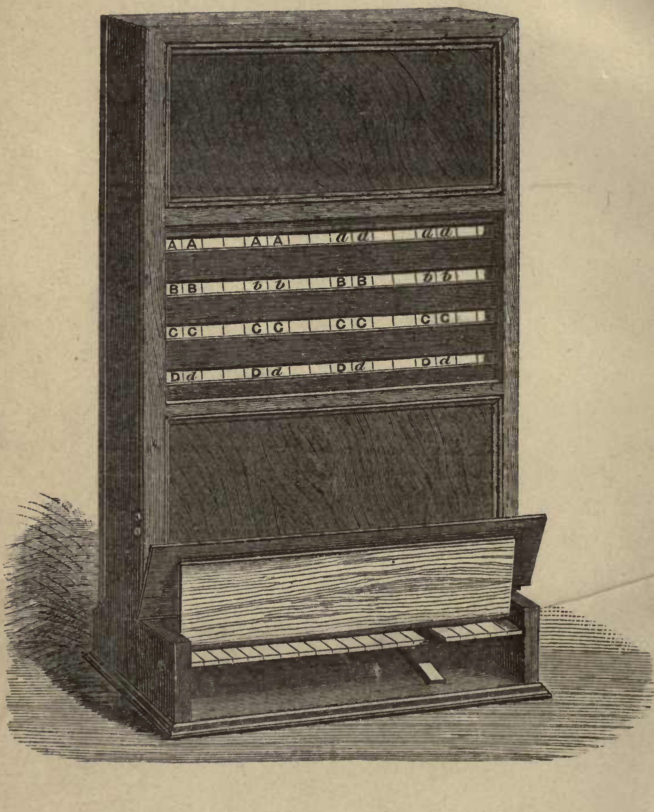

THE LOGICAL MACHINE.

THE PREDICATE-KEY C IS DEPRESSED.

THE PRINCIPLES OF SCIENCE:

A TREATISE ON LOGIC

AND

SCIENTIFIC METHOD.

BY

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PREFACE.

It may be truly asserted that the rapid progress of the physical sciences during the last three centuries has not been accompanied by a corresponding advance in the theory of reasoning. Physicists speak familiarly of Scientific Method, but they could not readily describe what they mean by that expression. Profoundly engaged in the study of particular classes of natural phenomena, they are usually too much engrossed in the immense and ever-accumulating details of their special sciences, to generalize upon the methods of reasoning which they unconsciously employ. Yet few will deny that these methods of reasoning ought to be studied, especially by those who endeavour to introduce scientific order into less successful and methodical branches of knowledge.

The application of Scientific Method cannot be restricted to the sphere of lifeless objects. We must sooner or later have strict sciences of those mental and social phenomena, which, if comparison be possible, are of more interest to us than purely material phenomena. But it is the proper course of reasoning to proceed from the known to the unknown—from the evident to the obscure—from the material and palpable to the subtle and refined. The physical sciences may therefore be properly made the practice-ground of the reasoning powers, because they furnish us with a great body of precise and successful investigations. In these sciences we meet with happy instances of unquestionable deductive reasoning, of extensive generalization, of happy prediction, of satisfactory verification, of nice calculation of probabilities. We can note how the slightest analogical clue has been followed up to a glorious discovery, how a rash generalization has at length been exposed, or a conclusive *experimentum crucis* has decided the long-continued strife between two rival theories.

In following out my design of detecting the general methods of inductive investigation, I have found that the more elaborate and interesting processes of quantitative induction have their necessary foundation in the simpler science of Formal Logic. The earlier, and probably by far the least attractive part of this work, consists, therefore, in a statement of the so-called Fundamental Laws of Thought, and of the all-important Principle of Substitution, of which, as I think, all reasoning is a development. The whole procedure of inductive inquiry, in its most complex cases, is foreshadowed in the combinational view of Logic, which arises directly from these fundamental principles. Incidentally I have described the mechanical arrangements by which the use of the important form called the Logical Abecedarium, and the whole working of the combinational system of Formal Logic, may be rendered evident to the eye, and easy to the mind and hand.

The study both of Formal Logic and of the Theory of Probabilities, has led me to adopt the opinion that there is no such thing as a distinct method of induction as contrasted with deduction, but that induction is simply an inverse employment of deduction. Within the last century a reaction has been setting in against the purely empirical procedure of Francis Bacon, and physicists have learnt to advocate the use of hypotheses. I take the extreme view of holding that Francis Bacon, although he correctly insisted upon constant reference to experience, had no correct notions as to the logical method by which, from particular facts, we educe laws of nature. I endeavour to show that hypothetical anticipation of nature is an essential part of inductive inquiry, and that it is the Newtonian method of deductive reasoning combined with elaborate experimental verification, which has led to all the great triumphs of scientific research.

In attempting to give an explanation of this view of Scientific Method, I have first to show that the sciences of number and quantity repose upon and spring from the simpler and more general science of Logic. The Theory of Probability, which enables us to estimate and calculate quantities of knowledge, is then described, and especial attention is drawn to the Inverse Method of Probabilities, which involves, as I conceive, the true principle of inductive procedure. No inductive conclusions are more than probable, and I adopt the opinion that the theory of probability is an essential part of logical method, so that the logical value of every inductive result must be determined consciously or unconsciously, according to the principles of the inverse method of probability.

The phenomena of nature are commonly manifested in quantities of time, space, force, energy, &c., and the observation, measurement, and analysis of the various quantitative conditions or results involved, even in a simple experiment, demand much employment of systematic procedure. I devote a book, therefore, to a simple and general description of the devices by which exact measurement is effected, errors eliminated, a probable mean result attained, and the probable error of that mean ascertained. I then proceed to the principal, and probably the most interesting, subject of the book, illustrating successively the conditions and precautions requisite for accurate observation, for successful experiment, and for the sure detection of the quantitative laws of nature. As it is impossible to comprehend aright the value of quantitative laws without constantly bearing in mind the degree of quantitative approximation to the truth probably attained, I have devoted a special chapter to the Theory of Approximation, and however imperfectly I may have treated this subject, I must look upon it as a very essential part of a work on Scientific Method.

It then remains to illustrate the sound use of hypothesis, to distinguish between the portions of knowledge which we owe to empirical observation, to accidental discovery, or to scientific prediction. Interesting questions arise concerning the accordance of quantitative theories and experiments, and I point out how the successive verification of an hypothesis by distinct methods of experiment yields conclusions approximating to but never attaining certainty. Additional illustrations of the general procedure of inductive investigations are given in a chapter on the Character of the Experimentalist, in which I endeavour to show, moreover, that the inverse use of deduction was really the logical method of such great masters of experimental inquiry as Newton, Huyghens, and Faraday.

In treating Generalization and Analogy, I consider the precautions requisite in inferring from one case to another, or from one part of the universe to another part, the validity of all such inferences resting ultimately upon the inverse method of probabilities. The treatment of Exceptional Phenomena appeared to afford an interesting subject for a further chapter illustrating the various modes in which an outstanding fact may eventually be explained. The formal part of the book closes with the subject of Classification, which is, however, very inadequately treated.

I have, in fact, almost restricted myself to showing that all classification is fundamentally carried out upon the principles of Formal Logic and the Logical Abecedarium described at the outset.

In certain concluding remarks I have expressed the conviction which the study of Logic has by degrees forced upon my mind, that serious misconceptions are entertained by some scientific men as to the logical value of our knowledge of nature. We have heard much of what has been aptly called the Reign of Law, and the necessity and uniformity of natural forces has been not uncommonly interpreted as involving the non-existence of an intelligent and benevolent Power, capable of interfering with the course of natural events. Fears have been expressed that the progress of Scientific Method must therefore result in dissipating the fondest beliefs of the human heart. Even the 'Utility of Religion' is seriously proposed as a subject of discussion. It seemed to be not out of place in a work on Scientific Method to allude to the ultimate results and limits of that method. I fear that I have very imperfectly succeeded in expressing my strong conviction that before a rigorous logical scrutiny the Reign of Law will prove to be an unverified hypothesis, the Uniformity of Nature an ambiguous expression, the certainty of our scientific inferences to a great extent a delusion. The value of science is of course very high, while the conclusions are kept well within the limits of the data on which they are founded, but it is pointed out that our experience is of the most limited character compared with what there is to learn, while our mental powers seem to fall infinitely short of the task of comprehending and explaining fully the nature of any one object. I draw the conclusion that we must interpret the results of Scientific Method in an affirmative sense only. Ours must be a truly positive philosophy, not that false negative philosophy which, building on a few material facts, presumes to assert that it has compassed the bounds of existence, while it nevertheless ignores the most unquestionable phenomena of the human mind and feelings.

I have to thank my colleague, Professor Barker, for carefully revising several of the sheets most abounding in mathematical considerations. It is approximately certain that in freely employing illustrations drawn from many different sciences, I have frequently fallen into errors of detail. In this respect I must throw myself upon the indulgence of the reader, who will bear in mind, as I hope, that the scientific facts are generally mentioned purely for the purpose of illustration, so that inaccuracies of detail will not in the majority of cases affect the truth of the general principles illustrated.

December 15th, 1873.

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THE INDUCTIVE OR INVERSE APPLICATION OF THE THEORY OF PROBABILITIES.

WE have hitherto considered the theory of probability only in its simple deductive employment, by which it enables us to determine from given conditions the probable character of events happening under those conditions. But as deductive reasoning when inversely applied constitutes the process of induction, so the calculation of probabilities may be inversely applied; from the known character of certain events we may argue backwards to the probability of a certain law or condition governing those events. Having satisfactorily accomplished this work, we may indeed calculate forwards to the probable character of future events happening under the same conditions; but this part of the process is a direct use of deductive reasoning (p. 260).

Now it is highly instructive to find that whether the theory of probabilities be deductively or inductively applied, the calculation is always performed according to the principles and rules of deduction. The probability that an event has a particular condition entirely depends upon the probability that if the condition existed the event would follow. If we take up a pack of common playing cards, and observe that they are arranged in perfect numerical order, we conclude beyond all reasonable doubt that they have been thus intentionally arranged by some person acquainted with the usual order of sequence. This conclusion is quite irresistible, and rightly so; for there are but two suppositions which we can make as to the reason of the cards being in that particular order:—

1. They have been intentionally arranged by some one who would probably prefer the numerical order.

2. They have fallen into that order by chance, that is, by some series of conditions which, being wholly unknown in nature, cannot be known to lead by preference to the particular order in question.

The latter supposition is by no means absurd, for any one order is as likely as any other when there is no preponderating tendency. But we can readily calculate by the doctrines of permutation the probability that fifty-two objects would fall by chance into any one particular order. Fifty-two objects can be arranged in—

$$52 \times 51 \times 50 \times \ldots\ldots \times 4 \times 3 \times 2 \times 1 \text{ or } 8066 \times (10)^{64}$$

possible orders, the number obtained requiring 68 places of figures for its full expression. Hence it is excessively unlikely, and, in fact, practically impossible, that any one should ever meet with a pack of cards arranged in perfect order by pure accident. If we do meet with a pack so arranged, we inevitably adopt the other supposition, that some person having reasons for preferring that special order, has thus put them together.

We know that of the almost infinite number of possible orders the numerical order is the most remarkable; it is useful as proving the perfect constitution of the pack, and it is the intentional result of certain games. At any rate, the probability that intention should produce that order is incomparably greater than the probability that chance should produce it; and as a certain pack exists in that order, we rightly prefer the supposition which most probably leads to the observed result.

By a similar mode of reasoning we every day arrive, and validly arrive, at conclusions approximating to certainty. Whenever we observe a perfect resemblance between two objects, as, for instance, two printed pages, two engravings, two coins, two foot-prints, we are warranted in asserting that they proceed from the same type, the same plate, the same pair of dies, or the same boot. And why? Because it is almost impossible that with different types, plates, dies, or boots some minute distinction of form should not be discovered. It is barely possible for the hand of the most skilful artist to make two objects alike, so that mechanical repetition is the only probable explanation of exact similarity. We can often establish with extreme probability that one document is copied from another. Suppose that each document contains 10,000 words, and that the same word is incorrectly spelt in each. There is then a probability of less than 1 in 10,000 that the same mistake should be made in each.

If we meet with a second error occurring in each document, the probability is less than 1 in $10,000 \times 9999$, that such two coincidences should occur by chance, and the numbers grow with extreme rapidity for more numerous coincidences. We cannot indeed make any precise calculations without taking into account the character of the errors committed, concerning the conditions of which we have no accurate means of estimating probabilities. Nevertheless, abundant evidence may thus be obtained as to the derivation of documents from each other. In the examination of many sets of logarithmic tables, six remarkable errors were found to be present in all but two, and it was proved that tables printed at Paris, Berlin, Florence, Avignon, and even in China, besides thirteen sets printed in England, between the years 1633 and 1822, were derived directly or indirectly from some common source[a]. With a certain amount of labour, it is possible to establish beyond reasonable doubt the relationship or genealogy of any number of copies of one document, proceeding possibly from parent copies now lost. Tischendorf has thus investigated the relations between the manuscripts of the New Testament now existing, and the same work has been performed by German scholars for several classical writings.

Principle of the Inverse Method.

The inverse application of the rules of probability entirely depends upon a proposition which may be thus stated, nearly in the words of Laplace[b]. *If an event can be produced by any one of a certain number of different causes, the probabilities of the existence of these causes as inferred from the event, are proportional to the probabilities of the event as derived from these causes.* In other words, the most probable cause of an event which has happened is that which would most probably lead to the event supposing the cause to exist; but all other possible causes are also to be taken into account with probabilities proportional to the probability that the event would have happened if the cause existed. Suppose, to fix our ideas clearly, that E is the event, and C_1 C_2 C_3 are the three only conceivable causes. If C_1 exist, the probability is p_1 that E would follow; if C_2 and C_3 exist, the like probabilities are respectively p_2 and p_3. Then as p_1 is to p_2, so is the probability of C_1 being the actual cause to the probability of C_2 being it; and, similarly, as p_2 is to p_3, so is the probability of C_2 being the actual cause to the probability of C_3 being it. By a very simple mathematical

[a] Lardner, 'Edinburgh Review,' July 1834, p. 277.

[b] 'Mémoires par divers Savans,' tom. vi.; quoted by Todhunter in his 'History of Theory of Probability,' p. 458.

process we arrive at the conclusion that the actual probability of C_1 being the cause is

$$\frac{p_1}{p_1 + p_2 + p_3};$$

and the similar probabilities of the existence of C_2 and C_3 are,

$$\frac{p_2}{p_1 + p_2 + p_3} \quad \text{and} \quad \frac{p_3}{p_1 + p_2 + p_3}.$$

The sum of these three fractions amounts to unity, which correctly expresses the certainty that one cause or other must be in operation.

We may thus state the result in general language. If it is certain that one or other of the supposed causes exists, the probability that any one does exist is the probability that if it exists the event happens, divided by the sum of all the similar probabilities. There may seem to be an intricacy in this subject which may prove distasteful to some readers; but this intricacy is essential to the subject in hand. No one can possibly understand the principles of inductive reasoning, unless he will take the trouble to master the meaning of this rule, by which we recede from an event to the probability of each of its possible causes.

This rule or principle of the indirect method is that which common sense leads us to adopt almost instinctively, before we have any comprehension of the principle in its general form. It is easy to see, too, that it is the rule which will, out of a great multitude of cases, lead us most often to the truth, since the most probable cause of an event really means that cause which in the greatest number of cases produces the event. But I have only met with one attempt at a general demonstration of the principle. Poisson imagines each possible cause of an event to be represented by a distinct ballot-box, containing black and white balls, in such ratio that the probability of a white ball being drawn is equal to that of the event happening. He further supposes that each box, as is possible, contains the same total number of balls, black and white; and then, mixing all the contents of the boxes together, he shows that if a white ball be drawn from the aggregate ballot-box thus formed, the probability that it proceeded from any particular ballot-box is represented by the number of white balls in that particular box, divided by that total number of white balls in all the boxes. This result corresponds to that given by the principle in question [c].

Thus, if there be three boxes, each containing ten balls in all, and respectively containing seven, four, and three white balls, then on mixing all the balls together we have fourteen white ones; and if we draw a white ball, that is if the event happens, the probability that it came out of the first box is $\frac{7}{14}$; which is exactly equal to $\frac{\frac{7}{10}}{\frac{7}{10}+\frac{4}{10}+\frac{3}{10}}$, the fraction given by the rule of the Inverse Method.

Simple Applications of the Inverse Method.

In many cases of scientific induction we may apply the principle of the inverse method in a simple manner. If only two, or at the most a few hypotheses, may be made as to the origin of certain phenomena, or the connection of one phenomenon with another, we may sometimes easily calculate the respective probabilities of these hypotheses. It was thus that Professors Bunsen and Kirchhoff established, with a probability little short of certainty, that iron exists in the sun. On comparing the spectra of sunlight and of the light proceeding from the incandescent vapour of iron, it became apparent that at least sixty bright lines in the spectrum of iron coincided with dark

[c] Poisson, 'Recherches sur la Probabilité des Jugements,' Paris, 1837, pp. 82, 83.

lines in the sun's spectrum. Such coincidences could never be observed with certainty, because, even if the lines only closely approached, the instrumental imperfections of the spectroscope would make them apparently coincident, and if one line came within half a millemetre of another, on the map of the spectra, they could not be pronounced distinct. Now the average distance of the solar lines on Kirchhoff's map is 2 millemetres, and if we throw down a line, as it were, by pure chance on such a map, the probability is about one-half that the new line will fall within $\frac{1}{2}$ millemetre on one side or the other of some one of the solar lines. To put it in another way, we may suppose that each solar line, either on account of its real breadth or the defects of the instrument, possesses a breadth of $\frac{1}{2}$ millemetre, and that each line in the iron spectrum has a like breadth. The probability then is just one-half that the centre of each iron line will come by chance within 1 millemetre of the centre of a solar line, so as to appear to coincide with it. The probability of casual coincidence of each iron line with a solar line is in like manner $\frac{1}{2}$. Coincidence in the case of each of the sixty iron lines is a very unlikely event if it arises casually, for it would have a probability of only $(\frac{1}{2})^{60}$ or less than 1 in a trillion. The odds, in short, are more than a million million millions to unity against such casual coincidence[d]. But on the other hypothesis, that iron exists in the sun, it is highly probable that such coincidences would be observed; it is immensely more probable that sixty coincidences would be observed if iron existed in the sun, than that they should arise from chance. Hence by our principle it is immensely probable that iron does exist in the sun.

All the other interesting results given by the comparison of spectra, rest upon the same principle of proba-

[d] Kirchhoff's 'Researches on the Solar Spectrum.' First part, translated by Professor Roscoe, pp. 18, 19.

bility. The almost complete coincidence between the spectra of solar, lunar, and planetary light renders it practically certain that the light is all of solar origin, and is reflected from the surfaces of the moon and planets, suffering only slight alteration from the atmospheres of some of the planets. A fresh confirmation of the truth of the Copernican theory is thus furnished.

A vast probability may be shown to exist that the heat, light, and chemical effects of the sun are due to the same rays, and are so many different manifestations of the same undulations. For a photograph of the spectrum corresponds exactly with what the eye observes, allowance being made for the great differences of chemical activity in different parts of the spectrum; and delicate experiments with the thermopile also show that, where there is a dark line, there also the heat of the rays is absent.

Sir J. Herschel proved the connexion between the direction of the oblique faces of symmetrical quartz crystals, and the direction in which the same crystals rotate the plane of the polarisation of light. For if it is found in a second crystal that the relation is the same as in the first, the probability of this happening by chance is $\frac{1}{2}$; the probability that in another crystal also the direction would be the same is $\frac{1}{4}$, and so on. The probability that in $n+1$ crystals there would be casual agreement of direction is the n^{th} power of $\frac{1}{2}$. Thus, if in examining fourteen crystals the same relation of the two phenomena is discovered in each, the probability that it proceeds from uniform conditions is more than 8000 to 1 [e]. Now, since the first observations on this subject were made in 1820, no exceptions have been observed, so that the probability of invariable connexion is incalculably great.

[e] 'Edinburgh Review,' No. 185, vol. xcii. July 1850, p. 32; Herschel's 'Essays,' p. 421; 'Transactions of the Cambridge Philosophical Society,' vol. i. p. 43.

A good instance of this method is furnished by the agreement of numerical statements with the truth. Thus, in a manuscript of Diodorus Siculus, as Dr. Young states [g], the ceremony of an ancient Egyptian funeral is described as requiring the presence of forty-two persons sitting in judgment on the merits of the deceased, and in many ancient papyrus rolls the same number of persons are found delineated. The probability is but slight that Diodorus, if inventing his statements or writing without proper information, would have chosen such a number as forty-two, and though there are not the data for an exact calculation, Dr. Young considers that the probability in favour of the correctness of the manuscript and the veracity of the writer on this ground alone, is at least 100 to 1.

It is exceedingly probable that the ancient Egyptians had exactly recorded the eclipses occurring during long periods of time, for Diogenes Laertius mentions that 373 solar and 832 lunar eclipses had been observed, and the ratio between these numbers exactly expresses that which would hold true of the eclipses of any long period, of say 1200 or 1300 years, as estimated on astronomical grounds [h].

It is evident that an agreement between small numbers, or customary numbers, such as seven, one hundred, a myriad, &c., is much more likely to happen from chance, and therefore gives much less presumption of dependence. If two ancient writers spoke of the sacrifice of oxen, they would in all probability describe it as a hecatomb, and there would be nothing remarkable in the coincidence.

On similar grounds, we must inevitably believe in the human origin of the flint flakes so copiously discovered of late years. For though the accidental stroke of one stone

[g] Young's 'Works,' vol. ii. pp. 18, 19.

[h] 'History of Astronomy,' Library of Useful Knowledge, p. 14.

against another may often produce flakes, such as are occasionally found on the sea-shore, yet when several flakes are found in close company, and each one bears evidence, not of a single blow only, but of several successive blows, all conducing to form a symmetrical knife-like form, the probability of a natural and accidental origin becomes incredibly small, and the contrary supposition, that they are the work of intelligent beings, approximately certain [i].

An interesting calculation concerning the probable connexion of languages, in which several or many words are similar in sound and meaning, was made by Dr. Young [k].

Application of the Theory of Probabilities in Astronomy.

The science of astronomy, occupied with the simple relations of distance, magnitude, and motion of the heavenly bodies, admits more easily than almost any other science of interesting conclusions founded on the theory of probability. More than a century ago, in 1767, Michell showed the extreme probability of bonds connecting together systems of stars. He was struck by the unexpected number of fixed stars which have companions close to them. Such a conjunction might happen casually by one star, although possibly at a great distance from the other, happening to lie on the same straight line passing near the earth. But the probabilities are so greatly against such an optical union happening often in the expanse of the heavens, that Michell asserted the existence of a bond between most of

[i] Evans' 'Ancient Stone Implements of Great Britain.' London, 1872 (Longmans).

[k] 'Philosophical Transactions,' 1819; Young's 'Works,' vol. ii. pp. 15–18.

the double stars. It has since been estimated by Struve, that the odds are 9570 to 1 against any two stars of not less than the seventh magnitude falling within the apparent distance of four seconds of each other by chance, and yet ninety-one such cases were known when the estimation was made, and many more cases have since been discovered. There were also four known triple stars, and yet the odds against the appearance of any one such conjunction are 173,524 to 1[l]. The conclusions of Michell have been entirely verified by the discovery that many double stars are in connexion under the law of gravitation.

Michell also investigated the probability that the six brightest stars in the Pleiades should have come by accident into such striking proximity. Estimating the number of stars of equal or greater brightness at 1500, he found the odds to be nearly 500,000 to 1 against casual conjunction. Extending the same kind of argument to other clusters, such as that of Præsepe, the nebula in the hilt of Perseus' sword, he says[m]: 'We may with the highest probability conclude, the odds against the contrary opinion being many million millions to one, that the stars are really collected together in clusters in some places, where they form a kind of system, while in others there are either few or none of them, to whatever cause this may be owing, whether to their mutual gravitation, or to some other law or appointment of the Creator.'

The calculations of Michell have been called in question by the late James D. Forbes[n], and Mr. Todhunter vaguely

[l] Herschel, 'Outlines of Astronomy,' 1849, p. 565; but Todhunter, in his 'History of the Theory of Probability,' p. 335, states that the calculations do not agree with those published by Struve.

[m] 'Philosophical Transactions,' 1767, vol. lvii. p. 431.

[n] 'Philosophical Magazine,' 3rd Series, vol. xxxvii. p. 401, December, 1850; also August, 1849.

countenances his objections [o], otherwise I should not have thought them of much weight. Certainly Laplace accepts Michell's views [p], and if Michell be in error, it is in the methods of calculation, not in the general validity of his conclusions.

Similar calculations might no doubt be applied to the peculiar drifting motions which have been detected by Mr. R. A. Proctor in some of the constellations [q]. Against a general tendency of stars to move in one direction by chance, the odds are very great. It is on a similar ground that a considerable proper motion of the sun is found to exist with immense probability, because on the average the fixed stars show a tendency to move apparently from one point of the heavens towards that diametrically opposite. The sun's motion in the contrary direction would explain this tendency, otherwise we must believe that myriads of stars accidentally agree in their direction of motion, or are urged by some common force from which the sun is exempt. It may be said that the rotation of the earth is proved in like manner, because it is immensely more probable that one body would revolve than that the sun, moon, planets, comets, and the whole of the stars of the heavens should be whirled round the earth daily, with a uniform motion superadded to their own peculiar motions. This appears to be nearly the reason which led Gilbert, one of the earliest English Copernicans, and in every way an admirable physicist, to admit the rotation of the earth, while Francis Bacon denied it [r].

In contemplating the planetary system, we are struck with the similarity in direction of nearly all its move-

[o] 'History,' &c., p. 334.

[p] 'Essai Philosophique,' p. 57.

[q] 'Proceedings of the Royal Society,' 20 January, 1870. 'Philosophical Magazine,' 4th Series, vol. xxxix. p. 381.

[r] Hallam's 'Literature of Europe,' 1st ed. vol. ii. p. 464.

ments. Newton remarked upon the regularity and uniformity of these motions, and contrasted them with the eccentricity and irregularity of the cometary orbits [s]. Could we, in fact, look down upon the system from the northern side, we should see all the planets moving round from west to east, the satellites moving round their primaries and the sun, planets, and all the satellites rotating in the same direction, with some exceptions on the verge of the system. Now in the time of Laplace eleven planets were known, and the directions of rotation were known for the sun, six planets, the satellites of Jupiter, Saturn's ring, and one of his satellites. Thus there were altogether 43 motions all concurring, namely :—

Orbital motions of eleven planets . .	11
Orbital motions of eighteen satellites . .	18
Axial rotations	14
	43

The probability that 43 motions independent of each other would coincide by chance is the 42nd power of $\frac{1}{2}$, so that the odds are about 4,400,000,000,000 to 1 in favour of some common cause for the uniformity of direction. This probability, as Laplace observes [t], is higher than that of many historical events which we undoubtingly believe. In the present day, the probability is much increased by the discovery of additional planets, and the rotation of other satellites, and it is only slightly weakened by the fact that some of the outlying satellites are exceptional in direction, there being considerable evidence of an accidental disturbance in the more distant parts of the system.

Hardly less remarkable than the uniformity of motion

[s] 'Principia,' bk. ii. General scholium.

[t] 'Essai Philosophique,' p. 55. Laplace appears to count the rings of Saturn as giving two independent movements.

is the near approximation of all the orbits of the planets to a common plane. Daniel Bernouilli roughly estimated the probability of such an agreement arising from accident at $\frac{1}{(12)^6}$, the greatest inclination of any orbit to the sun's equator being 1-12th part of a quadrant. Laplace devoted to this subject some of his most ingenious investigations. He found the probability that the sum of the inclinations of the planetary orbits would not exceed by accident the actual amount (·914187 of a right angle for the ten planets known in 1801) to be $\frac{1}{10}(\cdot 914187)^{10}$, or about ·00000011235. This probability may be combined with that derived from the direction of motion, and it then becomes immensely probable that the constitution of the planetary system arose out of uniform conditions, or, as we say, from some common cause[u].

If the same kind of calculation be applied to the orbits of comets the result is very different[y]. Of the orbits which have been determined 48·9 per cent. only are direct or in the same direction as the planetary motions[z]. Hence it becomes apparent that comets do not properly belong to the solar system, and it is probable that they are stray portions of nebulous matter which have become accidently attached to the system by the attractive powers of the sun or Jupiter.

Statement of the General Inverse Problem.

In the instances described in the preceding sections, we have been occupied in receding from the occurrence

[u] Lubbock, 'Essay on Probability,' p. 14. De Morgan, 'Encyc. Metrop.' art. *Probability*, p. 412. Todhunter's 'History of the Theory of Probability,' p. 543. Concerning the objections raised to these conclusions by the late Dr. Boole, see the 'Philosophical Magazine,' 4th Series, vol. ii. p. 98. Boole's 'Laws of Thought,' pp. 364–375.

[y] Laplace, 'Essai Philosophique,' pp. 55, 56.

[z] Chambers's 'Astronomy,' 2nd ed. pp. 346–49.

of certain similar events to the probability that there must have been a condition or cause for such events. We have found that the theory of probability, although never yielding a certain result, often enables us to establish an hypothesis beyond the reach of reasonable doubt. There is, however, another method of applying the theory, which possesses for us even greater interest, because it illustrates, in the most complete manner, the theory of inference adopted in this work, which theory indeed it suggested. The problem to be solved is as follows :—

An event having happened a certain number of times, and failed a certain number of times, required the probability that it will happen any given number of times in the future under the same circumstances.

All the larger planets hitherto discovered move in one direction round the sun ; what is the probability that, if a new planet exterior to Neptune be discovered, it will move in the same direction ? All known permanent gases, except chlorine, are colourless ; what is the probability that, if some new permanent gas should be discovered, it will be colourless ? In the general solution of this problem, we wish to infer the future happening of any event from the number of times that it has already been observed to happen. Now, it is very instructive to find that there is no known process by which we can pass directly from the data to the conclusion. It is always requisite to recede from the data to the probability of some hypothesis, and to make that hypothesis the ground of our inference concerning future happenings. Mathematicians, in fact, make every hypothesis which is applicable to the question in hand ; they then calculate, by the inverse method, the probability of every such hypothesis according to the data, and the probability that if each hypothesis be true, the required future event will happen. The total probability that the event will happen, is the sum of the separate probabilities contributed by each distinct hypothesis.

To illustrate more precisely the method of solving the problem, it is desirable to adopt some concrete mode of representation, and the ballot-box, so often employed by mathematicians, will best serve our purpose. Let the happening of any event be represented by the drawing of a white ball from a ballot-box, while the failure of an event is represented by the drawing of a black ball. Now, in the inductive problem we are supposed to be ignorant of the contents of the ballot-box, and are required to ground all our inferences on our experience of those contents as shown in successive drawings. Rude common sense would guide us nearly to a true conclusion. Thus if we had drawn twenty balls, one after another, replacing the ball after each drawing, and the ball had in each case proved to be white, we should believe that there was a considerable preponderance of white balls in the urn, and a probability in favour of drawing a white ball on the next occasion. Though we had drawn white balls for thousands of times without fail, it would still be possible that some black balls lurked in the urn and would at last appear, so that our inferences could never be certain. On the other hand, if black balls came at intervals, I should expect that after a certain number of trials the future results would agree more or less closely with the past ones.

The mathematical solution of the question consists in nothing more than a close analysis of the mode in which our common sense proceeds. If twenty white balls have been drawn and no black ball, my common sense tells me that any hypothesis which makes the black balls in the urn considerable compared with the white ones is improbable; a preponderance of white balls is a more probable hypothesis, and as a deduction from this more probable hypothesis, I expect a recurrence of white balls. The mathematician merely reduces this process of thought to exact numbers. Taking, for instance, the hypothesis that there are 99 white and one black ball in the urn, he can calculate the probability that 20 white balls should be drawn in succession in those circumstances; he thus forms a definite estimate of the probability of this hypothesis, and knowing at the same time the probability of a white ball reappearing if such be the contents of the urn, he combines these probabilities, and obtains an exact estimate that a white ball will recur in consequence of this hypothesis. But as this hypothesis is only one out of many possible ones, since the ratio of white and black balls may be 98 to 2, or 97 to 3, or 96 to 4, and so on, he has to repeat the estimate for every such possible hypothesis. To make the method of solving the problem perfectly evident, I will describe in the next section a very simple case of the problem, originally devised for the purpose by Condorcet, which was also adopted by Lacroix[a], and has passed into the works of De Morgan, Lubbock, and others.

Simple Illustration of the Inverse Problem.

Suppose it to be known that a ballot-box contains only four black or white balls, the ratio of black and white balls being unknown. Four drawings having been made with replacement, and a white ball having appeared on each occasion but one, it is required to determine the probability that a white ball will appear next time. Now the hypotheses which can be made as to the contents of the urn are very limited in number, and are at most the following five :—

[a] 'Traité élémentaire du Calcul des Probabilités,' 3rd ed. (1833), p. 148.

4	white	and	0	black	balls
3	,,	,,	1	,,	,,
2	,,	,,	2	,,	,,
1	,,	,,	3	,,	,,
0	,,	,,	4	,,	,,

The actual occurrence of black and white balls in the drawings renders the first and last hypotheses out of the question, so that we have only three left to consider.

If the box contains three white and one black, the probability of drawing a white each time is $\frac{3}{4}$, and a black $\frac{1}{4}$; so that the compound event observed, namely, three white and one black, has the probability $\frac{3}{4} \times \frac{3}{4} \times \frac{3}{4} \times \frac{1}{4}$, by the rule already given (p. 233). But as it is indifferent to us in what order the balls are drawn, and the black ball might come first, second, third, or fourth, we must multiply by four, to obtain the probability of three white and one black in any order, thus getting $\frac{27}{64}$.

Taking the next hypothesis of two white and two black balls in the urn, we obtain for the same probability the quantity $\frac{1}{2} \times \frac{1}{2} \times \frac{1}{2} \times \frac{1}{2} \times 4$, or $\frac{16}{64}$, and from the third hypothesis of one white and three black we deduce likewise $\frac{1}{4} \times \frac{1}{4} \times \frac{1}{4} \times \frac{3}{4} \times 4$, or $\frac{3}{64}$. According, then, as we adopt the first, second, or third hypothesis, the probability that the result actually noticed would follow is $\frac{27}{64}$, $\frac{16}{64}$, and $\frac{3}{64}$. Now it is certain that one or other of these hypotheses must be the true one, and their absolute probabilities are proportional to the probabilities that the observed events would follow from them (see p. 279). All we have to do, then, in order to obtain the absolute probability of each hypothesis, is to alter these fractions in a uniform ratio, so that their sum shall be unity, the expression of certainty. Now since $27 + 16 + 3 = 46$, this will be effected by dividing each fraction by 46 and multiplying by 64. Thus the probabilities of the first, second, and third hypotheses are respectively—

$$\frac{27}{46}, \quad \frac{16}{46}, \quad \frac{3}{46}.$$

The inductive part of the problem is now completed, since we have found that the urn most likely contains three white and one black ball, and have assigned the exact probability of each possible supposition. But we are now in a position to resume deductive reasoning, and infer the probability that the next drawing will yield, say a white ball. For if the box contains three white and one black ball, the probability of drawing a white one is certainly $\frac{3}{4}$; and as the probability of the box being so constituted is $\frac{27}{46}$, the compound probability that the box will be so filled and will give a white ball at the next trial, is

$$\frac{27}{46} \times \frac{3}{4} \text{ or } \frac{81}{184}.$$

Again, the probability is $\frac{16}{46}$ that the box contains two white and two black, and under those conditions the probability is $\frac{1}{2}$ that a white ball will appear; hence the probability that a white ball will appear in consequence of that condition, is

$$\frac{16}{46} \times \frac{1}{2} \text{ or } \frac{32}{184}.$$

From the third supposition we get in like manner the probability

$$\frac{3}{46} \times \frac{1}{4} \text{ or } \frac{3}{184}.$$

Now since one and not more than one hypothesis can be true, we may add together these separate probabilities, and we find that

$$\frac{81}{184} + \frac{32}{184} + \frac{3}{184} \text{ or } \frac{116}{184}$$

is the complete probability that a white ball will be next drawn under the conditions and data supposed.

General Solution of the Inverse Problem.

In the instance of the inverse method described in the last section, a very few balls were supposed to be in the ballot-box for the purpose of simplifying the calculation. In order that our solution may apply to natural phenomena, we must render our hypothesis as little arbitrary as possible. Having no *à priori* knowledge of the conditions of the phenomena in question, there is no limit to the variety of hypotheses which might be suggested. Mathematicians have therefore had recourse to the most extensive suppositions which can be made, namely, that the ballot-box contains an infinite number of balls; they have thus varied the proportion of white balls to black balls continuously, from the smallest to the greatest possible proportion, and estimated the aggregate probability which results from this comprehensive supposition.

To explain their procedure, let us imagine that, instead of an infinite number, the ballot-box contained a large finite number of balls, say 1000. Then the number of white balls might be 1 or 2 or 3 or 4, and so on, up to 999. Supposing that three white and one black ball have been drawn from the urn as before, there is a certain very small probability that this would have occurred in the case of a box containing one white and 999 black balls; there is also a small probability that from such a box the next ball would be white. Compound these probabilities, and we have the probability that the next ball really will be white, in consequence of the existence of that proportion of balls. If there be two white and 998 black balls in the box, the probability is greater, and will increase until the balls are supposed to be in the proportion of those drawn. Now 999 different hypotheses are possible, and the calculation is to be made for each of these, and their aggregate taken as the final result. It is apparent that as the number of balls in the box is increased, the absolute probability of any one hypothesis concerning the exact proportion of balls is decreased, but the aggregate results of all the hypotheses will assume the character of a wide average.

When we take the step of supposing the balls within the urn to be infinite in number, the possible proportions of white and black balls also become infinite, and the probability of any one proportion actually existing is infinitely small. Hence the final result that the next ball drawn will be white is really the sum of an infinite number of infinitely small quantities. It might seem, indeed, utterly impossible to calculate out a problem having an infinite number of hypotheses, but the wonderful resources of the integral calculus enable this to be done with far greater facility than if we supposed any large finite number of balls, and then actually computed the results. I will not attempt to describe the processes by which Laplace finally accomplished the complete solution of the problem. They are to be found described in several English works, especially De Morgan's 'Treatise on Probabilities,' in the 'Encyclopædia Metropolitana,' and Mr. Todhunter's 'History of the Theory of Probability.' The abbreviating power of mathematical analysis was never more strikingly shown. But I may add that though the integral calculus is employed as a means of summing infinitely numerous results, we in no way abandon the principles of combinations already treated. We calculate the values of infinitely numerous factorials, not, however, obtaining their actual products, which would lead to an infinite number of figures, but obtaining the final answer to the problem by devices which can only be comprehended after study of the integral calculus.

It must be allowed that the hypothesis adopted by Laplace is in some degree arbitrary, so that there was some opening for the doubt which Boole has cast upon it[b]. But it may be replied, (1) that the supposition of an infinite number of balls treated in the manner of Laplace is less arbitrary and more comprehensive than any other that could be suggested. (2) The result does not differ much from that which would be obtained on the hypothesis of any very large finite number of balls. (3) The supposition leads to a series of simple formulæ which can be applied with ease in many cases, and which bear all the appearance of truth so far as it can be independently judged by a sound and practiced understanding.

Rules of the Inverse Method.

By the solution of the problem, as described in the last section, we obtain the following series of simple rules.

1. *To find the probability that an event which has not hitherto been observed to fail will happen once more, divide the number of times the event has been observed increased by one, by the same number increased by two.*

If there have been m occasions on which a certain event might have been observed to happen, and it has happened on all those occasions, then the probability that it will happen on the next occasion of the same kind is $\frac{m+1}{m+2}$. For instance, we may say that there are nine places in the planetary system where planets might exist obeying Bode's law of distance, and in every place there is a planet obeying the law more or less exactly, although no reason is known for the coincidence. Hence the probability that the next planet beyond Neptune will conform to the law is $\frac{10}{11}$.

2. *To find the probability that an event which has not hitherto failed will not fail for a certain number of new occasions, divide the number of times the event has hap-*

[b] 'Laws of Thought,' pp. 368–375.

pened increased by one, by the same number increased by one and the number of times it is to happen.

An event having happened m times without fail, the probability that it will happen n more times is $\frac{m+1}{m+n+1}$. Thus the probability that three new planets would obey Bode's law is $\frac{10}{13}$, but it must be allowed that this, as well as the previous result, would be much weakened by the fact that Neptune can barely be said to obey the law.

3. *An event having happened and failed a certain number of times, to find the probability that it will happen the next time, divide the number of times the event has happened increased by one, by the whole number of times the event has happened or failed increased by two.*

Thus, if an event has happened m times and failed n times, the probability that it will happen on the next occasion is $\frac{m+1}{m+n+2}$.

Thus, if we assume that of the elements yet discovered 50 are metallic and 14 non-metallic, then the probability that the next element discovered will be metallic is $\frac{51}{66}$.

Again since of 37 metals which have been sufficiently examined only four, namely, sodium, potassium, lanthanum and lithium, are of less density than water, the probability that the next metal examined or discovered will be less dense than water is $\frac{4+1}{37+2}$ or $\frac{5}{39}$.

We may state the results of the method in a more general manner thus,—If under given circumstances certain events A, B, C, &c., have happened respectively m, n, p, &c., times, and one or other of these events must happen, then the probabilities of these events are proportional to $m+1$, $n+1$, $p+1$, &c., so that the probability of A will be $\frac{m+1}{m+1+n+1+p+1+\&c.}$. But if new events may happen in addition to those which have been observed, we must assign unity for the probability of such new event. The proportional probabilities then become 1 for a new event, $m+1$ for A, $n+1$ for B, and so on, and the absolute probability of A is $\frac{m+1}{1+m+1+n+1+ \&c.}$[c]

It is very interesting to trace out the variations of probability according to these rules under diverse circumstances. Thus the first time a casual event happens it is 1 to 1, or as likely as not that it will happen again; if it does happen it is 2 to 1 that it will happen a third time; and on successive occasions of the like kind the odds become 3, 4, 5, 6, &c., to 1. The odds of course will be discriminated from the probabilities which are successively $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, &c. Thus on the first occasion on which a person sees a shark, and notices that it is accompanied by a little pilot fish, the odds are 1 to 1, or the probability $\frac{1}{2}$, that the next shark will be so accompanied.

When an event has happened a very great number of times, its happening once again approaches nearly to certainty. Thus if we suppose the sun to have risen demonstratively one thousand million times, the probability that it will rise again, on the ground of this knowledge merely, is $\frac{1,000,000,000+1}{1,000,000,000+1+1}$. But then the probability that it will continue to rise for as long a period as we know it to have risen is only $\frac{1,000,000,000+1}{2,000,000,000+1}$, or almost exactly $\frac{1}{2}$. The probability that it will continue so rising a thousand times as long is only about $\frac{1}{1001}$. The lesson which we may draw from these figures is quite that which we should adopt on other grounds, namely that experience never affords certain knowledge, and that it is exceedingly improbable that events will always happen as we observe

[c] De Morgan's 'Essay on Probabilities,' Cabinet Cyclopædia, p. 67.

them. Inferences pushed far beyond their data soon lose any considerable probability. De Morgan has said[d], 'No finite experience whatsoever can justify us in saying that the future shall coincide with the past in all time to come, or that there is any probability for such a conclusion.' On the other hand, we gain the assurance that experience sufficiently extended and prolonged will give us the knowledge of future events with an unlimited degree of probability, provided indeed that those events are not subject to arbitrary interference.

It must be clearly understood that these probabilities are only such as arise from the mere happening of the events, irrespective of any knowledge derived from other sources concerning those events or the general laws of nature. All our knowledge of nature is indeed founded in like manner upon observation, and is therefore only probable. The law of gravitation itself is only probably true. But when a number of different facts, observed under the most diverse circumstances, are found to be harmonized under a supposed law of nature, the probability of the law approximates closely to certainty. Each science rests upon so many observed facts, and derives so much support from analogies or direct connections with other sciences, that there are comparatively few cases where our judgment of the probability of an event depends entirely upon a few antecedent events, disconnected from the general body of physical science.

Events may often again exhibit a regularity of succession or preponderance of character, which the simple formula will not take into account. For instance, the majority of the elements recently discovered are metals, so that the probability of the next discovery being that of a metal, is doubtless greater than we calculated (p. 298). At the more distant parts of the planetary system, there

[d] 'Treatise on Probability,' Cabinet Cyclopædia, p. 128.

are symptoms of disturbance which would prevent our placing much reliance on any inference from the prevailing order of the known planets to those undiscovered ones which may possibly exist at great distances. These and all like complications in no way invalidate the theoretic truth of the formulæ, but render their sound application much more difficult.

Erroneous objections have been raised to the theory of probability, on the ground that we ought not to trust to our *à priori* conceptions of what is likely to happen, but should always endeavour to obtain precise experimental data to guide us[e]. This course, however, is perfectly in accordance with the theory, which is our best and only guide, whatever data we possess. We ought to be always applying the inverse method of probabilities so as to take into account all additional information. When we throw up a coin for the first time, we are probably quite ignorant whether it tends more to fall head or tail upwards, and we must therefore assume the probability of each event as $\frac{1}{2}$. But if it shows head, for instance, in the first throw, we now have very slight experimental evidence in favour of a tendency to show head. The chance of two heads is now slightly greater than $\frac{1}{4}$, which it appeared to be at first[f], and as we go on throwing the coin time after time, the probability of head appearing next time constantly varies in a slight degree according to the character of our previous experience. As Laplace remarks, we ought always to have regard to such considerations in common life. Events when closely scrutinized will hardly ever prove to be quite independent, and the slightest preponderance one way or the other is some evidence of connexion, and in the absence of better evidence should be taken into account.

[e] J. S. Mill, 'System of Logic,' 5th Edition, bk. iii. chap. xviii. § 3.

[f] Todhunter's 'History,' pp. 472, 598.

The grand object of seeking to estimate the probability of future events from past experience, seems to have been entertained by James Bernouilli and De Moivre, at least such was the opinion of Condorcet; and Bernouilli may be said to have solved one case of the problem[g]. The English writers Bayes and Price are, however, undoubtedly the first who put forward any distinct rules on the subject[h]. Condorcet and several other eminent mathematicians advanced the mathematical theory of the subject; but it was reserved to the immortal Laplace to bring to the subject the full power of his genius, and carry the solution of the problem almost to perfection. It is instructive to observe that a theory which arose from the consideration of the most petty games of chance, the rules and the very names of which are in many cases forgotten, gradually advanced, until it embraced the most sublime problems of science, and finally undertook to measure the value and certainty of all our inductions.

Fortuitous Coincidences.

We should have studied the theory of probability to very little purpose, if we thought that it would furnish us with an infallible guide. The theory itself points out the possibility, or rather the approximate certainty, that we shall sometimes be deceived by extraordinary, but fortuitous coincidences. There is no run of luck so extreme that it may not happen, and it may happen to us, or in our time, as well as to other persons or in other times. We may be forced by all correct calculation to refer such coincidences to some necessary cause, and yet we may be deceived. All that the calculus of probability

[g] Todhunter's 'History,' pp. 378, 79.

[h] 'Philosophical Transactions' [1763], vol. liii. p. 370, and [1764], vol. liv. p. 296. Todhunter, pp. 294–300.

pretends to give, is *the result in the long run*, as it is called, and this really means in *an infinity of cases*. During any finite experience, however long, chances may be against us. Nevertheless the theory is the best guide we can have. If we always think and act according to its well interpreted indications, we shall have the best chance of escaping error; and if all persons, throughout all time to come, obey the theory in like manner, they will undoubtedly thereby reap the greatest advantage.

No rule can be given for descriminating between coincidences which are casual and those which are the effect of law or common conditions. By a fortuitous or casual coincidence, we mean an agreement between events, which nevertheless arise from wholly independent and different causes or conditions, and which will not always so agree. It is a fortuitous coincidence, if a penny thrown up repeatedly in various ways always falls on the same side; but it would not be fortuitous if there were any similarity in the motions of the hand, and the height of the throw, so as to cause or tend to cause a uniform result. Now among the infinitely numerous events, objects, or relations in the universe, it is quite likely that we shall occasionally notice casual coincidences. There are seven intervals in the octave, and there is nothing very improbable in the colours of the spectrum happening to be apparently divisible into the same or similar series of seven intervals. It is hardly yet decided whether this apparent coincidence, with which Newton was much struck, is well founded or not[i], but the question will probably be decided in the negative.

It is certainly a casual coincidence which the ancients noticed between the seven vowels, the seven strings of the lyre, the seven Pleiades, and the seven chiefs at Thebes[k].

[i] 'Nature,' vol. i. p. 286.

[k] Aristotle's 'Metaphysics,' xiii. 6. 3.

The accidents connected with the number seven have misled the human intellect throughout the historical period. Pythagoras imagined a connection between the seven planets, and the seven intervals of the monochord. The alchemists were never tired of drawing inferences from the coincidence in numbers of the seven planets and the seven metals, not to speak of the seven days of the week.

A singular circumstance was pointed out concerning the dimensions of the earth, sun, and moon; the sun's diameter was almost exactly 110 times as great as the earth's diameter, while in almost exactly the same ratio the mean distance of the earth was greater than the sun's diameter, and the mean distance of the moon from the earth was greater than the moon's diameter[1]. The agreement was so close that it might have proved more than casual, but its fortuitous character is sufficiently shown by the fact, that the coincidence ceases to be remarkable when we adopt the amended dimensions of the planetary system.

A considerable number of the elements have atomic weights, which are apparently exact multiples of that of hydrogen. If this be not a law to be ultimately extended to all the elements, as supposed by Prout, it is a most remarkable coincidence. But, as I have observed, we have no means of absolutely discriminating accidental coincidences from those which imply a deep producing cause. A coincidence must either be very strong in itself, or it must be corroborated by some explanation or connection with other laws of nature. Little attention was ever given to the coincidence concerning the dimensions of the sun, earth, and moon, because it was not very strong in itself, and had no apparent connexion with the

[1] Chambers's 'Astronomy,' 1st. ed. p. 23.

principles of physical astronomy. Prout's Law bears more probability because it would bring the constitution of the elements themselves in close connexion with the atomic theory, representing them as built up out of a simpler substance.

In historical and social matters, coincidences are frequently pointed out which are due to chance, although there is always a strong popular tendency to regard them as the work of design, or as having some hidden cause. It has been pointed out that if to 1794, the number of the year in which Robespierre fell, we add the sum of its digits, the result is 1815, the year in which Napoleon fell; the repetition of the process gives 1830, the year in which Charles the Tenth abdicated. Again, the French Chamber of Deputies, in 1830, consisted of 402 members, of whom 221 formed the party called, 'La queue de Robespierre,' while the remainder, 181 in number, were named 'Les honnêtes gens.' If we give to each letter a numerical value corresponding to its place in the alphabet, it will be found that the sum of the values of the letters in each name exactly indicates the number of the party[m].

A number of such coincidences, often of a very curious character, might be adduced, and the probability against the occurrence of each may be enormously great. They must be attributed to chance, because they cannot be shown to have the slightest connexion with the general laws of nature; but persons are often found to be greatly influenced by such coincidences, regarding them as evidence of fatality, that is of a system of causation governing human affairs independently of the ordinary laws of nature. Let it be remembered that there are an infinite number of opportunities in life for some strange coincidence to present itself, so that it is quite to be expected that remarkable conjunctions will sometimes happen.

[m] S. B. Gould's 'Curious Myths,' p. 222.

In all matters of judicial evidence, we must bear in mind the necessary occurrence from time to time of unaccountable coincidences. The Roman jurists refused for this reason to invalidate a testamentary deed, the witnesses of which had sealed it with the same seal. For witnesses independently using their own seals might be found to possess identical ones by accident[n]. It is well known that circumstantial evidence of apparently overwhelming completeness will sometimes lead to a mistaken judgment, and as absolute certainty is never really attainable, every court must act upon probabilities of a very high amount, and in a certain small proportion of cases they must almost of necessity condemn the innocent victims of a remarkable conjuncture of circumstances[o]. Popular judgments usually turn upon probabilities of far less amount, as when the palace of Nicomedia, and even the bedchamber of Diocletian, having been on fire twice within fifteen days, the people entirely refused to believe that it could be the result of accident. The Romans believed that there was a fatality connected with the name of Sextus.

'Semper sub Sextis perdita Roma fuit.'

The utmost precautions will not provide against all contingencies. To avoid errors in important calculations, it is usual to have them repeated by different computers, but a case is on record in which three computers made exactly the same calculations of the place of a star, and yet all did it wrong in precisely the same manner, for no apparent reason[p].

[n] Possunt autem omnes testes et uno annulo signare testamentum. Quid enim si septem annuli una sculptura fuerint, secundum quod Pomponio visum est?—'Justinian,' ii. tit. x. 5.

[o] See Wills on 'Circumstantial Evidence,' p. 148.

[p] 'Memoirs of the Royal Astronomical Society,' vol. iv. p. 290, quoted by Lardner, 'Edinburgh Review,' July 1834, p. 278.

Summary of the Theory of Inductive Inference.

The theory of inductive inference adopted in this and the previous chapter, was chiefly suggested by the study of the Inverse Method of Probabilities, but it also bears much resemblance to the so-called Deductive Method described by Mr. J. S. Mill, in his well known 'System of Logic[q].' Mr. Mill's views concerning the Deductive Method, probably form the most original and valuable part of his treatise, and I should have ascribed the doctrine entirely to him, had I not found that the opinions put forward in other parts of his work are entirely inconsistent with the theory here upheld. As this subject is the most important and difficult one with which we have to deal, I will try to remedy the imperfect manner in which I have treated it, by giving a brief recapitulation of the views adopted.

All inductive reasoning is but an inverse application of deductive reasoning. Being in possession of certain particular facts or events expressed in propositions, we imagine some more general proposition expressing the existence of a law or cause; and, deducing the particular results of that supposed general proposition, we observe whether they agree with the facts in question. Hypothesis is thus always employed, consciously or unconsciously. The sole conditions to which we need conform in framing any hypothesis is, that we both have and exercise the power of inferring deductively from the hypothesis, to the particular logical combinations or results, which are to be compared with the known facts. Thus there are but three steps in the process of induction :—

(1) Framing of some hypothesis as to the character of the general law.

[q] Book iii. chap. 11.

(2) Deducing consequences from that law.

(3) Observing whether the consequences agree with the particular facts under consideration.

In very simple cases of inverse reasoning, hypothesis may sometimes seem altogether needless. Thus, to take numbers again as a convenient illustration, I have only to look at the series,

1, 2, 4, 8, 16, 32, &c.,

to know at once that the general law is that of geometrical progression; I need no successive trial of various hypotheses, because I am familiar with the series, and have long since learnt from what general formula it proceeds. In the same way a mathematician becomes acquainted with the integrals of a number of common formulæ, so that we have no need to go through any process of discovery. But it is none the less true that whenever previous reasoning does not furnish the knowledge, hypotheses must be framed and tried. (See p. 142.)

There naturally arise two different cases, according as the nature of the subject admits of certain or only probable deductive reasoning. Certainty, indeed, is but a singular case of probability, and the general principles of procedure are always the same. Nevertheless, when certainty of inference is possible the process is simplified. Of several mutually inconsistent hypotheses, the results of which can be certainly compared with fact, but one hypothesis can ultimately be entertained. Thus in the inverse logical problem, two logically distinct conditions could not yield the same series of possible combinations. Accordingly in the case of two terms we had to choose one of seven different kinds of propositions, or in the case of three terms, our choice lay among 192 possible distinct hypotheses (pp. 154–164). Natural laws, however, are often quantitative in character, and the possible hypotheses are then infinite in variety.

When deduction is certain, comparison with fact is needed only to assure ourselves that we have rightly selected the hypothetical conditions. The law establishes itself, and no number of particular verifications can add to its probability. Having once deduced from the principles of algebra that the difference of the squares of two numbers is equal to the product of their sum and difference, no number of particular trials of its truth will render it more certain. On the other hand, no finite number of particular verifications of a supposed law will render that law certain. In short, certainty belongs only to the deductive process, and to the teachings of direct intuition; and as the conditions of nature are not given by intuition, we can only be certain that we have got a correct hypothesis when, out of a limited number conceivably possible, we select that one which alone agrees with the facts to be explained.

In geometry and kindred branches of mathematics, deductive reasoning is conspicuously certain, and it would often seem as if the consideration of a single diagram yields us certain knowledge of a general proposition. But in reality all this certainty is of a purely hypothetical character. Doubtless if we could ascertain that a supposed circle was a true and perfect circle, we could be certain concerning a multitude of its geometrical properties. But geometrical figures are physical objects, and the senses can never assure us as to their exact forms. The figures really treated in Euclid's 'Elements' are imaginary, and we never can verify in practice the conclusions which we draw with certainty in inference; questions of degree and probability enter.

Passing now to subjects in which deduction is only probable, it ceases to be possible to adopt one hypothesis to the exclusion of the others. We must entertain at the same time all conceivable hypotheses, and regard each with the degree of esteem proportionate to its probability. We go through the same steps as before.

(1) We frame an hypothesis.

(2) We deduce the probability of various series of possible consequences.

(3) We compare the consequences with the particular facts, and observe the probability that such facts would happen under the hypothesis.

The above processes must be performed for every conceivable hypothesis, and then the absolute probability of each will be yielded by the principle of the inverse method (p. 279). As in the case of certainty we accept that hypothesis which certainly gives the required results, so now we accept as most probable that hypothesis which most probably gives the results; but we are obliged to entertain at the same time all other hypotheses with degrees of probability proportionate to the probabilities that they would give the results.

So far we have treated only of the process by which we pass from special facts to general laws, that inverse application of deduction which constitutes induction. But the direct employment of deduction is often combined with the inverse. No sooner have we established a general law, than the mind rapidly draws other particular consequences from it. In geometry we may almost seem to infer that *because* one equilateral triangle is equiangular, therefore another is so. In reality it is not because one is that another is, but because all are. The geometrical conditions are perfectly general, and by what is sometimes called *parity of reasoning* whatever is true of one equilateral triangle, so far as it is equilateral, is true of all equilateral triangles.

Similarly, in all other cases of inductive inference, where we seem to pass from some particular instances to a new instance, we go through the same process. We form an hypothesis as to the logical conditions under which the given instances might occur; we calculate inversely the probability of that hypothesis, and compounding this with the probability that a new instance would proceed from the same conditions, we gain the absolute probability of occurrence of the new instance in virtue of this hypothesis. But as several, or many, or even an infinite number of mutually inconsistent hypotheses may be possible, we must repeat the calculation for each such conceivable hypothesis, and then the complete probability of the future instance will be the sum of the separate probabilities. The complication of this process is often very much reduced in practice, owing to the fact that one hypothesis may be nearly certainly true, and other hypotheses, though conceivable, may be so improbable as to be neglected without appreciable error. But when we possess no knowledge whatever of the conditions from which the events proceed, we may be unable to form any probable hypotheses as to their mode of origin. We have now to fall back upon the general solution of the problem effected by Laplace, which consists in admitting on an equal footing every conceivable ratio of favourable and unfavourable chances for the production of the event, and then accepting the aggregate result as the best which can be obtained. This solution is only to be accepted in the absence of all better means, but like other results of the calculus of probabilities, it comes to our aid where knowledge is at an end and ignorance begins, and it prevents us from over-estimating the knowledge we possess. The general results of the solution are in accordance with common sense, namely, that the more often an event has happened the more probable, as a general rule, is its subsequent occurrence. With the extension of experience this probability indefinitely increases, but at the same time the probability is slight that events will long continue to happen as they have previously happened.

We have now pursued the theory of inductive inference, as far as can be done with regard to simple logical or numerical relations. The laws of nature deal with time and space, which are indefinitely, or rather infinitely, divisible. As we passed from pure logic to numerical logic, so we must now pass from questions of discontinuous, to questions of continuous quantity, encountering fresh considerations of much difficulty. Before, therefore, we consider how the great inductions and generalizations of physical science illustrate the views of inductive reasoning just explained, we must break off for a time, and review the means which we possess of measuring and comparing magnitudes of time, space, mass, force, momentum, energy, and the various manifestations of energy in motion, heat, electricity, chemical change, and the other phenomena of nature.

CHAPTER XXIII.

THE USE OF HYPOTHESIS.

If the views of induction upheld in this work be correct, all inductive investigation consists in a marriage of hypothesis and experiment. When facts are already in our possession, we frame an hypothesis to explain their mutual relations, and by the success or non-success of this explanation is the value of the hypothesis to be entirely judged. In the framing and deductive treatment of such hypotheses, we must avail ourselves of the whole body of scientific truth already accumulated, and when once we have obtained a probable hypothesis, we must not rest until we have verified it by comparison with new facts. By deductive reasoning and calculation, we must endeavour to anticipate such new phenomena, especially those of a singular and exceptional nature, as would necessarily happen if the hypothesis be true. Out of the infinite number of observations and experiments which are possible at every moment, theory must lead us to select those few critical ones which are suitable for confirming or negativing our anticipations.

This work of inductive investigation cannot be guided by any system of precise and infallible rules, like those of deductive reasoning. There is, in fact, nothing to which we can apply rules of method, because the laws of nature to be treated must be in our possession before we can treat them. If, indeed, there were any single rule of inductive method, it would direct us to make an exhaustive arrangement of facts in all possible orders. Given a certain number of specimens in a museum, we might arrive at the best possible classification by going systematically through all possible classifications, and, were we endowed with infinite time and patience, this would be an effective method. It doubtless is the method by which the first few simple steps are taken in every incipient branch of science. Before the dignified name of science is applicable, some coincidences will chance to force themselves upon the attention. Before there was a science of meteorology, or any comprehension of the true conditions of the atmosphere, all observant persons learned to associate a peculiar clearness of the atmosphere with coming rain, and a colourless sunset with fine weather. Knowledge of this kind is called *empirical*, as seeming to come directly from experience; and there is doubtless a considerable portion of our knowledge which must always bear this character.

We may be obliged to trust to the casual detection of coincidences in those branches of knowledge where we are deprived of the aid of any guiding notions; but a very little reflection will show the utter insufficiency of haphazard experiment, when applied to investigations of a complicated nature. At the best, it will be the simple identity, or partial identity, of classes, as illustrated in pp. 146–154 of the first volume, which can be thus detected. It was pointed out that, even when a law of nature involves only two circumstances, and there are one hundred distinct circumstances which may possibly be connected, there will be no less than 4950 pairs of circumstances between which a coincidence may exist. When a law involves three or more circumstances, the possible number of coincidences becomes vastly greater still. When considering, again, the subject of combinations and permutations, it became apparent that we could never cope with the possible variety of nature. An exhaustive examination of the metallic alloys, or chemical compounds which can be formed, was found to be out of the question (vol. i. p. 218). It is on such considerations that we can explain the very small additions made to our knowledge by the alchemists. Many of them were men of the greatest acuteness, and their indefatigable labours were pursued through many centuries. A few of the more common compounds and phenomena were discovered by them, but a true insight into the principles of nature, now enables chemists to discover far more useful facts in a single year than were yielded by the alchemists during many centuries. There can be no doubt that Newton was really an alchemist, and often spent his days and nights in laborious experiments. But in trying to discover the secret by which gross metals might be rendered noble, his lofty powers of deductive investigation were wholly useless. Deprived of all guiding clues, his experiments must have been, like those of all the alchemists, purely tentative and haphazard. While his hypothetical and deductive investigations have given us the true system of nature, and opened the way in almost every one of the great branches of natural philosophy, the whole results of his tentative experiments are comprehended in a few happy guesses, given in his celebrated 'Queries.'

Even when we are engaged in apparently passive observation of a phenomenon, which we cannot modify experimentally, it is advantageous that our attention should be guided by some theoretical anticipations. A phenomenon which seems simple is, in all probability, really complex, and unless the mind is actively engaged in looking for particular details, it is quite likely that the most critical circumstances will be passed over. Bessel regretted that no distinct theory of the constitution of comets had guided his observations of Halley's comet[a]; in attempting to verify or refute any good hypothesis, not only would there have been a chance of establishing a true theory, but if confuted, the very confutation would probably have involved a large store of useful observations.

It would be an interesting work, but one which I cannot undertake, to trace out the gradual reaction which has taken place in recent times against the purely empirical, or Baconian, theory of induction. Francis Bacon, seeing the futility of the scholastic logic, which had long been predominant, asserted that the accumulation of facts and the careful and orderly abstraction of axioms, or general laws from them, constituted the true method of induction. This method, as far as we can gather its exact nature from Bacon's writings, would correspond to the process of exhaustive examination and classification to which I have just alluded. The value of this method might be estimated historically by the fact that it has not been followed by any of the great masters of science. Whether we look to Galileo, who preceded Bacon, to Gilbert, his contemporary, or to Newton and Descartes, his successors, we find that discovery was achieved by the exactly opposite method to that advocated by Bacon. Throughout Newton's works, as I shall more fully show in succeeding pages, we find deductive reasoning wholly predominant, and experiments are employed, as they should be, to confirm or refute hypothetical anticipations of nature. In my 'Elementary Lessons in Logic' (p. 258), I stated my belief that there was no kind of reference to Bacon in Newton's works. I have since found that Newton does once or twice employ the

[a] Tyndall, 'On Cometary Theory,' Philosophical Magazine, April, 1869. 4th Series, vol. xxxvii. p. 243.

expression *experimentum crucis* in his 'Opticks,' but this is the only expression, so far as I am aware, which could indicate on the part of Newton direct or indirect acquaintance with Bacon's writings[b].

Other great physicists of the same age were equally prone to the use of hypotheses rather than the blind accumulation of facts in the Baconian manner. Hooke emphatically asserts in his posthumous work on Philosophical Method, that the first requisite of the Natural Philosopher is readiness at guessing the solution of many phenomena and making queries. 'He ought to be very well skilled in those several kinds of philosophy already known, to understand their several hypotheses, suppositions, collections, observations, &c., their various ways of ratiocinations and proceedings, the several failings and defects, both in their way of raising, and in their way of managing their several theories: for by this means the mind will be somewhat more ready at guessing at the solution of many phenomena almost at first sight, and thereby be much more prompt at making queries, and at tracing the subtlety of Nature, and in discovering and searching into the true reason of things.'

We find Horrocks, again, than whom no one was more filled with the scientific spirit, telling us how he tried theory after theory in order to discover one which was in accordance with the motions of Mars[c]. It might readily be shown again that Huyghens, who possessed one of the most perfect philosophical intellects, followed the deductive process combined with continual appeal to experiment, with a skill closely analogous to that of Newton. As to Descartes and Leibnitz, their investigations were too much opposed to the Baconian rules, since they too often

b See 'Philosophical Transactions,' abridged by Lowthorp. 4th edit. vol. i. p. 130.

c Horrocks, 'Opera Posthuma' (1673), p. 276.

adopted hypothetical reasoning to the exclusion of experimental verification. Throughout the eighteenth century science was supposed to be advancing by the pursuance of the Baconian method, but in reality hypothetical investigation was the main instrument of progress. It is only in the present century that physicists began to recognise this truth. So much opprobrium had been attached by Bacon to the use of hypotheses, that we find Young speaking of them in an apologetic tone. 'The practice of advancing general principles and applying them to particular instances is so far from being fatal to truth in all sciences, that when those principles are advanced on sufficient grounds, it constitutes the essence of true philosophy [d]'; and he quotes cases in which Sir Humphry Davy trusted to his theories rather than his experiments. The late Sir John Herschel, who was both a practical physicist and an abstract logician, always entertained the deepest respect for Bacon, and made the 'Novum Organum' as far as possible the basis of his admirable 'Discourse on the Study of Natural Philosophy.' Yet we find him in Chapter VII fully recognising the part which the formation and verification of theories forms in the higher and more general investigations of physical science. The late Mr. J. S. Mill carried on the reaction by recognising as a distinct method the Deductive Method in which Ratiocination, that is, deductive reasoning, is employed for the discovery of new opportunities of testing and verifying a hypothesis. His main error consisted in the fact that throughout the other parts of his system he inveighed against the value of the deductive process, and even asserted from time to time that every process of reasoning is inductive. In fact Mill fell into much confusion in the use of the words induction and deduction, because he

[d] Young's Works, vol. i. p. 593.

failed to observe that the inverse use of deduction constitutes induction.

Even Francis Bacon was not wholly unaware of the value of hypothetical anticipation. In one or two places he incidentally acknowledges it, as when he remarks that the subtlety of nature surpasses that of reason, adding that 'axioms abstracted from particular facts in a careful and orderly manner, readily suggest and mark out new particulars.'

The true course of inductive procedure is that which has yielded all the more lofty and successful results of science. It consists in *Anticipating Nature*, in the sense of forming hypotheses as to the laws which are probably in operation; and then observing whether the combinations of phenomena are such as would follow from the laws supposed. The investigator begins with facts and ends with them. He uses such facts as are in the first place known to him in suggesting probable hypotheses; deducing other facts which would happen if a particular hypothesis is true, he proceeds to test the truth of his notion by fresh observations or experiments. If any result prove different from what he expects, it leads him either to abandon or to modify his hypothesis; but every new fact may give some new suggestion as to the laws in action. Even if the result in any case agrees with his anticipations, he does not regard it as finally confirmatory of his theory, but proceeds to test the truth of the theory by new deductions and new trials.

The investigator in such a process is assisted by the whole body of science previously accumulated. He may employ analogy, as I shall point out, to guide him in the choice of hypotheses. The manifold connexions between one science and another may give him strong clues to the kind of laws to be expected, and he thus always selects out of the infinite number of possible hypotheses those which are, as far as can be foreseen at the moment, most probable. Each experiment, therefore, which he performs is that most likely to throw light upon his subject, and even if it frustrate his first views, it probably tends to put him in possession of the correct clue.

Requisites of a Good Hypothesis.

There will be no difficulty in pointing out to what conditions, or rather to what condition an hypothesis must conform in order to be accepted as valid and probable. That condition, as I conceive, is the single one of enabling us to infer the existence of phenomena which occur in our experience. *Agreement with fact is the one sole and sufficient test of a true hypothesis.*

Hobbes, indeed, has named two conditions which he considers requisite in an hypothesis, namely, (1) That it should be conceivable and not absurd; (2) That it should allow of phenomena being necessarily inferred. Boyle, in noticing Hobbes' views, proposed to add a third condition, to the effect that the hypothesis should not be inconsistent with any other truth or phenomenon of nature[e]. Of these three conditions, I am inclined to think that the first cannot be accepted, unless by *inconceivable* and *absurd* we mean self-contradictory or inconsistent with the laws of thought and nature. I shall have to point out that some of the most sure and satisfactory theories involve suppositions which are wholly *inconceivable* in a certain sense of the word, because the mind cannot sufficiently extend its ideas to frame a notion of the actions supposed to exist. That the force of gravity should act instantaneously between the most distant parts of the planetary system, or that a ray of violet light should consist of

[e] Boyle's 'Physical Examen,' p. 84.

about 700 billions of vibrations in each second, are statements of an inconceivable and absurd character in one sense; but they are so far from being opposed to fact that we cannot on any other suppositions account for the phenomena observed. But if an hypothesis involve self-contradiction, or is inconsistent with known laws of nature, it is so far self-condemned. We cannot even apply processes of deductive reasoning to a self-contradictory notion; and being entirely opposed to the most general and certain laws known to us, the primary laws of thought, it thereby conspicuously fails to agree with facts. Since nature, again, is never self-contradictory, we cannot at the same time accept two theories which lead to contradictory results. If the one agrees with nature, the other cannot. Hence if there be a law which we believe with high probability to be verified in observation, we must not frame an hypothesis in conflict with it, otherwise the hypothesis will necessarily be in disagreement with observation. Since no law or hypothesis is proved, indeed, with absolute certainty, there is always a chance, however slight, that the new hypothesis may displace the old one; but the greater the probability which we assign to that old hypothesis, the greater must be the evidence required in favour of the new and conflicting one. A decisive *experimentum crucis* to negative the one, and establish the other, will probably be requisite to allay the strife.

I am inclined to assert, then, that there is but one test of a good hypothesis, namely, *its conformity with observed facts;* but this condition may be said to involve, at the same time, three minor conditions, nearly equivalent to those suggested by Hobbes and Boyle, namely:—

(1) That it allow of the application of deductive reasoning and the inference of consequences.

(2) That it do not conflict with any laws of nature, or of mind, which we hold as true.

(3) That the consequences inferred do agree with facts of observation.

The First Requisite—Possibility of Deductive Reasoning.

As the truth of an hypothesis is to be proved by its conformity with fact, the first condition is that we be able to apply methods of deductive reasoning, and learn what should happen according to such an hypothesis. Even if we could imagine an object acting according to laws wholly unknown in other parts of nature, it would be useless to do so, because we could never decide whether it existed or not. We can only infer what would happen under supposed conditions by applying what knowledge we possess of nature to those conditions. Hence, as Boscovich truly said, we are to understand by hypotheses 'not fictions altogether arbitrary, but suppositions conformable to experience or analogy.' It follows that every hypothesis worthy of consideration must suggest some likeness, analogy, or common law, acting in two or more things. If, in order to explain certain facts, a, a', a'', &c., we invent a cause A, then we must in some degree appeal to experience as to the mode in which A will act. As the objects and laws of nature are certainly not known to the mind intuitively, we must point out some other cause B, which supplies the requisite notions, and all we do is to invent a fourth term to an analogy. As B is to its effects b, b', b'', &c., so is A to its effects a, a', a'', &c. When, for instance, we attempt to explain the passage of light and heat radiations through space unoccupied by matter, we imagine the existence of the so-called *ether*. But if this ether were wholly different from anything else known to us, we should in vain try to reason about it. We must at least apply to it the laws of motion, that is, we must so far liken it to matter. And as when applying those laws to the elastic medium air, we are able to infer the phenomena of sound, so by arguing in a similar manner concerning ether we are able to infer the existence of light phenomena corresponding to what do occur. All that we do is to take a material elastic substance, increase its elasticity in an almost indefinite degree, and denude it of gravity and some others of the ordinary properties of matter, but we must retain sufficient likeness to matter to allow of deductive calculations.

The force of gravity is in some respects an almost incomprehensible existence, but in other respects entirely conformable to experience. We can distinctly observe that the force is proportional to mass, and that it acts in entire independence of the other matter which may be present or intervening. The law of the decrease of intensity as the square of the distance increases, may be observed to hold true of light, sound, and any other influences emanating from a point, and spreading uniformly through space. The law is doubtless connected at this point with the primary properties of space itself, and is so far conformable to our necessary ideas.

It may well be said, however, that no hypothesis can be so much as framed in the mind unless it be more or less conformable to experience. As the material of our ideas is undoubtedly derived from sensation, so we cannot figure to ourselves any existence or agent, but as endowed with some of the properties of matter. All that the mind can do in the creation of new existences is to alter combinations, or by analogy to alter the intensity of sensuous properties. The phenomenon of motion is familiar to sight and touch, and different degrees of rapidity are also familiar: we can pass beyond the limits of sense, and suppose the existence of rapid motion, such as our senses could not measure or observe. We know what is elasticity, and we can therefore in a certain sense figure to ourselves elasticity a thousand or a million times greater than any which is sensuously known to us. The waves of the ocean are many times higher than our own bodies; other waves, we may observe, are many times less; continue the proportion, and we may ultimately arrive at waves as small as those of light. Thus it is that from a sensuous basis the powers of mind enable us to reason concerning agents and phenomena different in an unlimited degree. If no hypothesis then can be absolutely opposed to sense, accordance with experience must always be a question of degree.

In order that an hypothesis may allow of satisfactory comparison with experience, it must possess a certain definiteness, and, generally speaking, a certain mathematical exactness allowing of the precise calculation of results. We must be able to ascertain whether it does or does not agree with facts.

The theory of vortices, on the contrary, did not present any mode of calculating the exact relations between the distances and periods of the planets and satellites; it could not, therefore, undergo that rigorous testing to which Newton scrupulously submitted his theory of gravity before its promulgation. Vagueness and incapability of precise proof or disproof often enables a false theory to live; but with those who love truth, such vagueness should excite the highest suspicion. The upholders of the ancient doctrine of Nature's abhorrence of a vacuum, had been unable to anticipate the important fact that water would not rise more than 33 feet in a common suction pump. Nor when the fact was pointed out could they explain it, except by introducing a special alteration of the theory to the effect that Nature's abhorrence of a vacuum was limited to 33 feet.

The Second Requisite—Consistency with established Laws of Nature.

In the second place an hypothesis must not be contradictory to what we believe to be true concerning Nature. It must not involve self-inconsistency which is opposed to the highest and simplest laws, namely, those of Logic. Neither ought it to be irreconcileable with the simple laws of motion, of gravity, of the conservation of energy, or any parts of physical science which we consider to be established beyond reasonable doubt. Not that we are absolutely forbidden to adopt such an hypothesis, but if we do so we must be prepared to disprove some of the best demonstrated truths in the possession of mankind. The fact that conflict exists means that the consequences of the theory are not verified if previous discoveries are correct, and we must therefore show that previous discoveries are incorrect before we can verify our theory.

An hypothesis will be exceedingly improbable, not to say invalid, if it supposes a substance or agent to act in a manner unknown in other cases; for it then fails to be verified in our knowledge of that substance or agent. Several physicists, especially Euler and Grove, have supposed that we might dispense with any ethereal basis of light, and infer from the interstellar passage of rays that there was some kind of rare gas occupying space. But if so, that gas must be excessively rare, as we may infer from the apparent absence of an atmosphere around the moon, and from many other facts and laws known to us concerning gases and the atmosphere; and yet at the same time it must possess an elastic force at least a billion times as great as atmospheric air at the earth's surface, in order to account for the extreme rapidity of the light rays. Such an hypothesis then is inconsistent with the main body of our knowledge concerning gases.

Provided that there be no clear and absolute conflict with known laws of nature, there is nothing so improbable or apparently inconceivable that it may not be rendered highly probable, or even approximately certain, by a sufficient number of concordances. In fact the two best founded and most conspicuously successful theories in the whole range of physical science involve the most absurd suppositions. Gravity is a force which appears to act between bodies through vacuous space; it is in positive contradiction to the old dictum that nothing could act but through some intervening medium or substance. It is even more puzzling that the force acts in perfect indifference to all intervening obstacles. Light in spite of its extreme velocity, shows much respect to matter, for it is almost instantaneously stopped by opaque substances, and to a considerable extent absorbed and deflected by transparent ones. But to gravity all media are, as it were, absolutely transparent, nay non-existent; and two particles at opposite points of the earth affect each other exactly as if the globe were not between. To complete the apparent impossibility, the action is, so far as we can observe, absolutely instantaneous, so that every particle of the universe is at every moment in separate cognizance, as it were, of the relative position of every other particle throughout the universe at that same moment of absolute time. Compared with such incomprehensible conditions, the theory of vortices deals with common-place realities. Newton's celebrated saying, *hypotheses non fingo*, bears the appearance of pure irony; and it was not without apparent grounds that Leibnitz and the greatest continental philosophers charged Newton with re-introducing occult powers and qualities.

The undulatory theory of light presents almost equal difficulties of conception. We are asked by physical philosophers to give up all our ordinary prepossessions, and believe that the interstellar space which seemed so empty is not empty at all, but filled with *something* immensely more solid and elastic than steel. As Dr. Young himself remarked[f], 'the luminiferous ether, pervading all space, and penetrating almost all substances, is not only highly elastic, but absolutely solid!!!' Sir John Herschel has calculated the amount of force which may be supposed, according to the undulatory theory of light, to be exerted at each point in space, and finds it to be 1,148,000,000,000 times the elastic force of ordinary air at the earth's surface, so that the pressure of the ether upon a square inch of surface must be about 17,000,000,000,000, or seventeen billions of pounds[g]. Yet we live and move without appreciable resistance through this medium, indefinitely harder and more elastic than adamant. All our ordinary notions must be laid aside in contemplating such an hypothesis; yet they are no more than the observed phenomena of light and heat force us to accept. We cannot deny even the strange suggestion of Dr. Young, that there may be independent worlds, some possibly existing in different parts of space, but others perhaps pervading each other unseen and unknown in the same space[h]. For if we are bound to admit the conception of this adamantine firmament, it is equally easy to admit a plurality of such. We see, then, that mere difficulties of conception must not in the least discredit a theory which otherwise agrees with facts, and we must only reject hypotheses which are inconceivable in the sense of breaking distinctly the primary laws of thought and nature.

[f] Young's 'Works,' vol. i. p. 415.

[g] 'Familiar Lectures on Scientific Subjects,' p. 282.

[h] Young's 'Works,' vol. i. p. 417.

The Third Requisite—Conformity with Facts.

Before we accept a new hypothesis, it must furnish us with distinct credentials, consisting in the deductive anticipation of a series of facts, which are not already connected and accounted for by any equally probable hypothesis. We cannot lay down any precise rule as to the number of accordances which can establish the truth of an hypothesis, because the accordances will vary much in value. While, on the one hand, no finite number of accordances will give entire certainty, the probability of the hypothesis will increase very rapidly with the number of accordances. Seldom, indeed, shall we have a theory free from difficulties and apparent inconsistency with facts. Though one real and undoubted inconsistency would be sufficient to overturn the most plausible theory, yet there is usually some probability that the fact may be misinterpreted, or that some supposed law of nature, on which we are relying, may not be true. Almost every problem in science thus takes the form of a balance of probabilities. It is only when difficulty after difficulty has been successfully explained away, and decisive *experimenta crucis* have, time after time, resulted in favour of our theory, that we can venture to assert the falsity of all objections.

The sole real test of an hypothesis is its accordance with fact. Descartes' celebrated system of vortices is exploded and rejected, not because it was intrinsically absurd and inconceivable, but because it could not give results in accordance with the actual motions of the heavenly bodies. The difficulties of conception involved in the apparatus of vortices, are mere child's play compared with those of gravitation and the undulatory theory already described. The vortices are on the whole plausible suppositions; for the planets and satellites bear at first sight much resemblance to objects carried round in whirlpools, an analogy which doubtless suggested the theory. The failure was in the first and third requisites; for, as already remarked, the theory did not allow of any precise calculation of planetary motions, and was so far incapable of rigorous verification. But so far as we can institute a comparison, facts are entirely against the vortices. Newton carefully pointed out that the Cartesian theory was inconsistent with the laws of Kepler, and would represent the planets as moving more rapidly at their aphelia than at their perihelia[i]. Newton did not ridicule the theory as absurd, but showed[k] that it was 'pressed with many difficulties.' The rotatory motions of the sun and planets on their own axes are in striking conflict with the revolutions of the satellites carried round them; and comets, the most flimsy of bodies, calmly pursue their courses in elliptic paths, altogether irrespective of the vortices which they intersect. We may now also point to the interlacing orbits of the minor planets as a new and insuperable difficulty in the way of the Cartesian ideas.

Newton, though he established the best of theories, was also capable of proposing one of the worst; and if we want an instance of a theory decisively contradicted by facts, we have only to turn to his views concerning the origin of natural colours. Having analysed, with incomparable skill, the origin of the colours of thin plates, he suggests that the colours of all bodies and substances are determined in like manner by the size of their ultimate particles. A thin plate of a definite thickness will reflect a definite colour; hence, if broken up into fragments it will form a powder of the same colour. But, if this be a sufficient explanation of coloured substances, then every coloured fluid ought to reflect the complementary colour of that which it transmits. Colourless transparency arises,

[i] 'Principia,' bk. II. Sect. ix. Prop. 53.

[k] Ibid. bk. III. Prop. 43. General Scholium.

according to Newton, from all the particles being too minute to reflect light; but if so, every transparent substance should appear perfectly black by reflected light, and, *vice versâ*, every black substance should be transparent. Newton himself so acutely felt this last difficulty as to suggest that true blackness is due to some internal refraction of the rays to and fro, and an ultimate stifling of them, which he did not attempt further to explain. Unless some other process came into operation, neither refraction nor reflection, however often repeated, would destroy the energy of light. The theory gives no account, therefore, as Brewster shows, of 24 parts out of 25 of the light which falls upon a black coal, and the $\frac{1}{25}$th part which is reflected from the lustrous surface is equally inconsistent with the theory, because fine coal-dust is almost entirely devoid of reflective power[1]. It is now generally believed that the colours of natural bodies are due to the unequal absorption of rays of light of different refrangibility.

Experimentum Crucis.

As we deduce more and more conclusions from a theory, and find them verified by trial, the probability of the theory increases in a most rapid manner; but we never escape the risk of error altogether. Absolute certainty is beyond the power of inductive investigation, and the most plausible suppositions may ultimately be proved false. Such is the groundwork of similarity in nature, that two very different conditions may often give closely similar results. We sometimes find ourselves therefore in possession of two or more hypotheses which both agree

[1] Brewster's 'Life of Newton,' 1st edit. chap. vii.

with so many experimental facts as to have great appearance of truth. Under such circumstances we have need of some new experiment, which shall give results agreeing with one hypothesis but not with the other.

Any such experiment which decides between two rival theories may be called an *Experimentum Crucis*, an Experiment of the Finger Post. Whenever the mind stands, as it were, at cross-roads, and knows not which way to select, it needs some decisive guide, and Bacon therefore assigned great importance and authority to instances or facts which serve in this capacity. The name given by Bacon has become exceedingly familiar; it is perhaps almost the only one of Bacon's figurative expressions which has passed into common use. We even find Newton, as I have already mentioned, using the name (vol. ii. p. 134).

I do not think, indeed, that the common use of the word at all agrees with that intended by Bacon. Sir John Herschel says that 'we make an experiment of the crucial kind when we form combinations, and put in action causes from which some particular one shall be deliberately excluded, and some other purposely admitted[m].' This, however, seems to be the description of any special experiment not made at haphazard. Pascal's experiment of causing a barometer to be carried to the top of the Puy-de-Dôme has often been considered as a perfect *experimentum crucis*, if not the first distinct one on record[n]; but if so, we must dignify the doctrine of Nature's abhorrence of a vacuum with the position of a rival theory. A crucial experiment must not simply confirm one theory, but must negative another; it must decide a mind which is in equilibrium, as Bacon says[o],

[m] 'Discourse on the Study of Natural Philosophy,' p. 151.

[n] Ibid. p. 229.

[o] 'Novum Organum,' bk. II. Aphorism 36.

between two equally plausible views. 'When in search of any nature, the understanding comes to an equilibrium, as it were, or stands suspended as to which of two or more natures the cause of nature inquired after should be attributed or assigned, by reason of the frequent and common occurrence of several natures, then these Crucial Instances show the true and inviolable association of one of these natures to the nature sought, and the uncertain and separable alliance of the other, whereby the question is decided, the former nature admitted for the cause, and the other rejected. These instances, therefore, afford great light, and have a kind of overruling authority, so that the course of interpretation will sometimes terminate in them, or be finished by them.'

The long continued strife between the Corpuscular and Undulatory theories of light forms the best possible illustration of the need of an Experimentum Crucis. It is highly remarkable in how complete and plausible a manner both these theories agreed with the ordinary laws of geometrical optics, relating to reflection and refraction.

A moving particle, according to the first law of motion, proceeds in a perfectly straight line, when undisturbed by extraneous forces. If the particle, being perfectly elastic, strike a perfectly elastic plane, it will bound off in such a path that the angles of incidence and reflection will be equal. Now a ray of light proceeds in a perfectly straight line, or appears to do so, until it meets a reflecting body, when its path is altered in a manner exactly similar to that of the elastic particle. Here is a remarkable correspondence which probably suggested to Newton's mind that light consisted of minute elastic particles moving with excessive rapidity in straight lines. The correspondence was found to extend also to the law of simple refraction; for if these particles of light be supposed capable of attracting matter, and being attracted by it at insensibly small distances, then a ray of light, falling on the surface of a transparent medium, will suffer an increase in its velocity of motion perpendicular to the surface, and the familiar law of sines is the necessary consequence. This remarkable explanation of the law of refraction had doubtless a very strong effect in leading Newton to entertain the corpuscular theory, and he appears to have thought that the analogy between the propagation of the rays of light and the motion of bodies was perfectly exact, whatever might be the actual nature of light[p]. It is highly remarkable, again, that Newton was able to give, by his corpuscular theory, a plausible explanation of the inflection of light as discovered by Grimaldi. The theory would indeed have been a very probable one could Newton's own law of gravity have been applied; but this was excluded, because the particles of light, in order that they may move in straight lines, must be assumed devoid of any influence upon each other.

The Huyghenian or Undulatory theory of light was also able to explain the same phenomena, but with one remarkable difference. If the undulatory theory be true, light must move more slowly in a dense refracting medium than in a rarer one; but the Newtonian theory assumed that the attraction of the dense medium caused the particles of light to move more rapidly than in the rare medium. On this point, then, there was a complete discrepancy between the two theories, and observation was required to show which theory was to be preferred. Now by simply cutting a uniform plate of glass into two pieces, and slightly inclining one piece so as to increase the length of the path of a ray passing through it, experimenters have been able to show that the light does move

[p] 'Principia,' bk. I. Sect. xiv. Prop. 96. Scholium, 'Opticks,' Prop. VI. 3rd edit. p. 70.

more slowly in glass than in air[q]. More recently, in 1850, Fizeau and Foucault independently measured the velocity of light in air and water by a revolving mirror, and found that the velocity is greater in air[r]. There are indeed a number of other points at which experience decides against Newton, and in favour of Huyghens and Young. Euler rejected the Corpuscular theory because particles of matter moving with the immense velocity of light must possess great momentum, of which there is no evidence in fact[s]. Bennet concentrated the light and heat of the sun upon a body so delicately suspended that an exceedingly small amount of momentum must have been rendered apparent, but there was no such effect[t]. This experiment, indeed, is of a negative kind, and is not absolutely conclusive, unless we could estimate the momentum which Newton's theory would require to be present (see vol. ii. p. 45); but there are other difficulties. Laplace pointed out that the attraction supposed to exist between matter and the corpuscular particles of light, would cause the velocity of light to vary with the size of the emitting body, so that if a star were 250 times as great in diameter as our sun, its attraction would prevent the emanation of light altogether[u]. But so far as experience shows, the velocity of light is uniform, and independent of the magnitude of the emitting body, as it should be according to the undulatory theory. Lastly, Newton's explanation of diffraction or inflection fringes of colours was only *plausible*, and not true; for Fresnel ascertained that the dimensions of the fringes are not what they would be according to Newton's theory.

[q] Airy's 'Mathematical Tracts,' 3rd edit. pp. 286–288.

[r] Jamin, 'Cours de Physique,' vol. iii. p. 372.

[s] Euler's 'Letters,' vol. ii. Letter XIX. p. 69.

[t] Balfour Stewart, 'Elementary Treatise on Heat,' p. 161.

[u] Young's 'Lectures on Natural Philosophy' (1845), vol. i. p. 361.

Although the Science of Light presents us with the most beautiful examples of crucial experiments and observations, instances are not wanting in other branches of science. Copernicus asserted in opposition to the ancient Ptolemaic theory that the earth and planets moved round the sun, and he predicted that if ever the sense of sight could be rendered sufficiently acute and powerful, we should see phases in Mercury and Venus. Galileo with his telescope was able, in 1610, to verify the prediction as regards Venus, and subsequent observations of Mercury lead to a like conclusion. The discovery of the aberration of light added a new proof, still further strengthened by the more recent determination of the parallax of fixed stars. Hooke proposed to prove the existence of the earth's diurnal motion by observing the deviation of a falling body, an experiment successfully accomplished by Benzenberg; and Foucault's pendulum has since furnished an additional indication of the same motion, which is indeed also apparent in the direction of the trade winds. All these are crucial facts in favour of the Copernican theory.

Davy's discovery of potassium and sodium in 1807 was a good instance of a crucial experiment; for it decisively confirmed Lavoisier's views, and at the same time negatived the ancient notions of phlogiston.

Descriptive Hypotheses.

There are some, or probably many, hypotheses which we may call *descriptive hypotheses*, and which serve for little else than to furnish convenient names. When a certain phenomenon is of an unusual and mysterious kind, we cannot even speak of it without using some analogy. Every word implies some resemblance between the thing to which it is applied, and some other thing, which fixes the meaning of the word. Thus if we are to speak of what constitutes electricity, we must search for the nearest analogy, and as electricity is chiefly characterised by the rapidity and facility of its movements, the notion of a fluid of a very subtle character presented itself as most appropriate. There is the single fluid and the double fluid theory of electricity, and a great deal of discussion has been uselessly spent upon them. The fact is that if these theories be understood as more than convenient modes of describing the phenomena, they are grossly invalid. The analogy extends only to the rapidity of motion, and the fact that a phenomenon occurs successively at different points of the body. The so-called electric fluid adds nothing to the weight of the conductor, and to suppose that it really consists of particles of matter would be even more absurd than to reinstate the Corpuscular theory of light. An infinitely closer analogy exists between electricity and light undulations, which are about equally rapid in propagation; and while we shall probably continue for a long time to talk of the electric fluid, there can be no doubt that this expression merely represents some phase of molecular motion, some wave of disturbance propagating itself at one time through material conductors, at another time through the ethereal basis of light. The invalidity of these fluid theories is moreover shown in the fact that they have not led to the invention of a single new experiment. When we speak of heat as *flowing* from one body to another, we likewise use a descriptive hypothesis merely; for Lambert's theory of the fluid motion of heat is no better than the Corpuscular theory of light.

Among these merely descriptive hypotheses I should be inclined to place Newton's theory of Fits of Easy Reflection and Refraction. That theory has been since exploded by actual discordance with fact, but even when really entertained it did not do more than describe what took place. It involved no deep analogy to any other phenomena of nature, for Newton could not point to any other substance which went through these extraordinary changes. We now know that the true analogy would have been the waves of sound, of which Newton had acquired in other respects so complete a comprehension. But though the notion of interference of waves had distinctly occurred to Hooke, Newton had failed to see how the periodic phenomena of light could be connected with the periodic character of waves. His hypothesis fell because it was out of analogy with everything else in nature, and it therefore did not allow him, as in other cases, to descend by mathematical deduction to consequences which could be verified or refuted.

We are always at freedom again to imagine the existence of a new agent or force, and give it an appropriate name, provided there are phenomena incapable of explanation from known causes. We may speak of *vital force* as occasioning life, provided that we do not take it to be more than a name for an undefined something giving rise to inexplicable facts, just as the French chemists called Iodine the Substance X, while they were unaware of its real character and place in chemistry[y]. Encke was quite justified in speaking of the *resisting medium* in space so long as the retardation of his comet could not be otherwise accounted for. But such hypotheses will do much harm whenever they divert us from attempts to reconcile the facts with known laws, or when they lead us to mix up entirely discrete things. We have no right, for instance, to confuse Encke's supposed resisting medium with the ethereal basis of light. The name protoplasm, now so familiarly used by physiologists, is doubtless legitimate so long as we do not mix up different sub-

[y] Paris, 'Life of Davy,' p. 274.

stances under it, or imagine that the name gives us any knowledge of the obscure origin of life. To name a substance protoplasm no more explains the infinite variety of forms of life which spring out of the substance, than does the *vital force* which may be supposed to reside in the protoplasm. Both expressions appear to me to be mere names for an unknown and inexplicable series of causes which out of apparently similar conditions produce the most diverse results.

Hardly to be distinguished from descriptive hypotheses are certain imaginary objects or conditions which we often frame for the more ready investigation or comprehension of a subject. The mathematician, in treating abstract questions of probability, finds it convenient, to represent the conditions to his own or other minds by a concrete analogy in the shape of a material ballot-box. The fundamental principle of the inverse method of probabilities upon which depends the whole of our reasoning in inductive investigations is proved by Poisson, who imagines a number of ballot-boxes, of which the contents are afterwards supposed to be mixed in one great box (vol. i. p. 280). Many other such devices are also used by mathematicians. When Newton investigated the nature of waves, he employed the pendulum as a convenient mode of representing the nature of the undulation. Centres of gravity, oscillation, &c., poles of the magnet, lines of force, are other imaginary existences solely employed to assist our thoughts (vol. i. p. 422). All such creations of the mind may be called *Representative Hypotheses*, and they are only permissible and useful so far as they embody analogies. Their further consideration properly belongs either to the subject of Analogy, or to that of language and representation, founded upon analogy.