CHAPTER XXV.

OF THE GROUNDS OF DISBELIEF.

Sec. 1. The method of arriving at general truths, or general propositions fit to be believed, and the nature of the evidence on which they are grounded, have been discussed, as far as space and the writer's faculties permitted, in the twenty-four preceding chapters. But the result of the examination of evidence is not always belief, nor even suspension of judgment; it is sometimes disbelief. The philosophy, therefore, of induction and experimental inquiry is incomplete, unless the grounds not only of belief, but of disbelief, are treated of; and to this topic we shall devote one, and the final, chapter.

By disbelief is not here to be understood the mere absence of belief. The ground for abstaining from belief is simply the absence or insufficiency of proof; and in considering what is sufficient evidence to support any given conclusion, we have already, by implication, considered what evidence is not sufficient for the same purpose. By disbelief is here meant, not the state of mind in which we form no opinion concerning a subject, but that in which we are fully persuaded that some opinion is not true; insomuch that if evidence, even of great apparent strength, (whether grounded on the testimony of others or on our own supposed perceptions,) were produced in favour of the opinion, we should believe that the witnesses spoke falsely, or that they, or we ourselves if we were the direct percipients, were mistaken.

That there are such cases, no one is likely to dispute. Assertions for which there is abundant positive evidence are often disbelieved, on account of what is called their improbability, or impossibility. And the question for consideration is what, in the present case, these words mean, and how far and in what circumstances the properties which they express are sufficient grounds for disbelief.

Sec. 2. It is to be remarked in the first place, that the positive evidence produced in support of an assertion which is nevertheless rejected on the score of impossibility or improbability, is never such as amounts to full proof. It is always grounded on some approximate generalization. The fact may have been asserted by a hundred witnesses; but there are many exceptions to the universality of the generalization that what a hundred witnesses affirm is true. We may seem to ourselves to have actually seen the fact: but, that we really see what we think we see, is by no means an universal truth; our organs may have been in a morbid state; or we may have inferred something, and imagined that we perceived it. The evidence, then, in the affirmative being never more than an approximate generalization, all will depend on what the evidence in the negative is. If that also rests on an approximate generalization, it is a case for comparison of probabilities. If the approximate generalizations leading to the affirmative are, when added together, less strong, or in other words, farther from being universal, than the approximate generalizations which support the negative side of the question, the proposition is said to be improbable, and is to be disbelieved provisionally. If however an alleged fact be in contradiction, not to any number of approximate generalizations, but to a completed generalization grounded on a rigorous induction, it is said to be impossible, and is to be disbelieved totally.

This last principle, simple and evident as it appears, is the doctrine which, on the occasion of an attempt to apply it to the question of the credibility of miracles, excited so violent a controversy. Hume's celebrated doctrine, that nothing is credible which is contradictory to experience, or at variance with laws of nature, is merely this very plain and harmless proposition, that whatever is contradictory to a complete induction is incredible. That such a maxim as this should either be accounted a dangerous heresy, or mistaken for a great and recondite truth, speaks ill for the state of philosophical speculation on such subjects.

But does not (it may be asked) the very statement of the proposition imply a contradiction? An alleged fact, according to this theory, is not to be believed if it contradict a complete induction. But it is essential to the completeness of an induction that it shall not contradict any known fact. Is it not then a *petitio principii* to say, that the fact ought to be disbelieved because the induction opposed to it is complete? How can we have a right to declare the induction complete, while facts, supported by credible evidence, present themselves in

opposition to it?

I answer, we have that right whenever the scientific canons of induction give it to us; that is, whenever the induction *can* be complete. We have it, for example, in a case of causation in which there has been an *experimentum crucis*. If an antecedent A, superadded to a set of antecedents in all other respects unaltered, is followed by an effect B which did not exist before, A is, in that instance at least, the cause of B, or an indispensable part of its cause; and if A be tried again with many totally different sets of antecedents and B still follows, then it is the whole cause. If these observations or experiments have been repeated so often, and by so many persons, as to exclude all supposition of error in the observer, a law of nature is established; and so long as this law is received as such, the assertion that on any particular occasion A took place, and yet B did not follow, *without any counteracting cause*, must be disbelieved. Such an assertion is not to be credited on any less evidence than what would suffice to overturn the law. The general truths, that whatever has a beginning has a cause, and that when none but the same causes exist, the same effects follow, rest on the strongest inductive evidence possible; the proposition that things affirmed by even a crowd of respectable witnesses are true, is but an approximate generalization; and--even if we fancy we actually saw or felt the fact which is in contradiction to the law--what a human being can see is no more than a set of appearances; from which the real nature of the phenomenon is merely an inference, and in this inference approximate generalizations usually have a large share. If, therefore, we make our election to hold by the law, no quantity of evidence whatever ought to persuade us that there has occurred anything in contradiction to it. If, indeed, the evidence produced is such that it is more likely that the set of observations and experiments on which the law rests should have been inaccurately performed or incorrectly interpreted, than that the evidence in question should be false, we may believe the evidence; but then we must abandon the law. And since the law was received on what seemed a complete induction, it can only be rejected on evidence equivalent; namely, as being inconsistent not with any number of approximate generalizations, but with some other and better established law of nature. This extreme case, of a conflict between two supposed laws of nature, has probably never actually occurred where, in the process of investigating both the laws, the true canons of scientific induction had been kept in view; but if it did occur, it must terminate in the total rejection of one of the supposed laws. It would prove that there must be a flaw in the logical process by which either one or the other was established: and if there be so, that supposed general truth is no truth at all. We cannot admit a proposition as a law of nature, and yet believe a fact in real contradiction to it. We must disbelieve the alleged fact, or believe that we were mistaken in admitting the supposed law.

But in order that any alleged fact should be contradictory to a law of causation, the allegation must be, not simply that the cause existed without being followed by the effect, for that would be no uncommon occurrence; but that this happened in the absence of any adequate counteracting cause. Now in the case of an alleged miracle, the assertion is the exact opposite of this. It is, that the effect was defeated, not in the absence, but in consequence of a counteracting cause, namely, a direct interposition of an act of the will of some being who has power over nature; and in particular of a Being, whose will being assumed to have endowed all the causes with the powers by which they produce their effects, may well be supposed able to counteract them. A miracle (as was justly remarked by Brown[41]) is no contradiction to the law of cause and effect; it is a new effect, supposed to be produced by the introduction of a new cause. Of the adequacy of that cause, if present, there can be no doubt; and the only antecedent improbability which can be ascribed to the miracle, is the improbability that any such cause existed.

All, therefore, which Hume has made out, and this he must be considered to have made out, is, that (at least in the imperfect state of our knowledge of natural agencies, which leaves it always possible that some of the physical antecedents may have been hidden from us,) no evidence can prove a miracle to any one who did not previously believe the existence of a being or beings with supernatural power; or who believes himself to have full proof that the character of the Being whom he recognises, is inconsistent with his having seen fit to interfere on the occasion in question.

If we do not already believe in supernatural agencies, no miracle can prove to us their existence. The miracle

itself, considered merely as an extraordinary fact, may be satisfactorily certified by our senses or by testimony; but nothing can ever prove that it is a miracle: there is still another possible hypothesis, that of its being the result of some unknown natural cause: and this possibility cannot be so completely shut out, as to leave no alternative but that of admitting the existence and intervention of a being superior to nature. Those, however, who already believe in such a being, have two hypotheses to choose from, a supernatural and an unknown natural agency; and they have to judge which of the two is the most probable in the particular case. In forming this judgment, an important element of the question will be the conformity of the result to the laws of the supposed agent, that is, to the character of the Deity as they conceive it. But, with the knowledge which we now possess of the general uniformity of the course of nature, religion, following in the wake of science, has been compelled to acknowledge the government of the universe as being on the whole carried on by general laws, and not by special interpositions. To whoever holds this belief, there is a general presumption against any supposition of divine agency not operating through general laws, or in other words, there is an antecedent improbability in every miracle, which, in order to outweigh it, requires an extraordinary strength of antecedent probability derived from the special circumstances of the case.

Sec. 3. It appears from what has been said, that the assertion that a cause has been defeated of an effect which is connected with it by a completely ascertained law of causation, is to be disbelieved or not, according to the probability or improbability that there existed in the particular instance an adequate counteracting cause. To form an estimate of this, is not more difficult than of other probabilities. With regard to all *known* causes capable of counteracting the given causes, we have generally some previous knowledge of the frequency or rarity of their occurrence, from which we may draw an inference as to the antecedent improbability of their having been present in any particular case. And neither in respect to known or unknown causes are we required to pronounce on the probability of their existing in nature, but only of their having existed at the time and place at which the transaction is alleged to have happened. We are seldom, therefore, without the means (when the circumstances of the case are at all known to us) of judging how far it is likely that such a cause should have existed at that time and place without manifesting its presence by some other marks, and (in the case of an unknown cause) without having hitherto manifested its existence in any other instance. According as this circumstance, or the falsity of the testimony, appears more improbable, that is, conflicts with an approximate generalization of a higher order, we believe the testimony, or disbelieve it; with a stronger or a weaker degree of conviction, according to the preponderance: at least until we have sifted the matter further.

So much, then, for the case in which the alleged fact conflicts, or appears to conflict, with a real law of causation. But a more common case, perhaps, is that of its conflicting with uniformities of mere coexistence, not proved to be dependent on causation: in other words, with the properties of Kinds. It is with these uniformities principally, that the marvellous stories related by travellers are apt to be at variance: as of men with tails, or with wings, and (until confirmed by experience) of flying fish; or of ice, in the celebrated anecdote of the Dutch travellers and the King of Siam. Facts of this description, facts previously unheard of but which could not from any known law of causation be pronounced impossible, are what Hume characterizes as not contrary to experience, but merely unconformable to it; and Bentham, in his treatise on Evidence, denominates them facts disconformable *in specie*, as distinguished from such as are disconformable *in toto* or in *degree*.

In a case of this description, the fact asserted is the existence of a new Kind; which in itself is not in the slightest degree incredible, and only to be rejected if the improbability that any variety of object existing at the particular place and time should not have been discovered sooner, be greater than that of error or mendacity in the witnesses. Accordingly, such assertions, when made by credible persons, and of unexplored places, are not disbelieved, but at most regarded as requiring confirmation from subsequent observers; unless the alleged properties of the supposed new Kind are at variance with known properties of some larger kind which includes it; or in other words, unless, in the new Kind which is asserted to exist, some properties are said to have been found disjoined from others which have always been known to accompany them; as in the case of Pliny's men, or any other kind of animal of a structure different from that which has always been found to coexist with animal life. On the mode of dealing with any such case, little needs be added to what has been

said on the same topic in the twenty-second chapter.[42] When the uniformities of coexistence which the alleged fact would violate, are such as to raise a strong presumption of their being the result of causation, the fact which conflicts with them is to be disbelieved; at least provisionally, and subject to further investigation. When the presumption amounts to a virtual certainty, as in the case of the general structure of organized beings, the only question requiring consideration is whether, in phenomena so little understood, there may not be liabilities to counteraction from causes hitherto unknown; or whether the phenomena may not be capable of originating in some other way, which would produce a different set of derivative uniformities. Where (as in the case of the flying fish, or the ornithorhynchus) the generalization to which the alleged fact would be an exception is very special and of limited range, neither of the above suppositions can be deemed very improbable; and it is generally, in the case of such alleged anomalies, wise to suspend our judgment, pending the subsequent inquiries which will not fail to confirm the assertion if it be true. But when the generalization is very comprehensive, embracing a vast number and variety of observations, and covering a considerable province of the domain of nature; then, for reasons which have been fully explained, such an empirical law comes near to the certainty of an ascertained law of causation: and any alleged exception to it cannot be admitted, unless on the evidence of some law of causation proved by a still more complete induction.

Such uniformities in the course of nature as do not bear marks of being the results of causation, are, as we have already seen, admissible as universal truths with a degree of credence proportioned to their generality. Those which are true of all things whatever, or at least which are totally independent of the varieties of Kinds, namely, the laws of number and extension, to which we may add the law of causation itself, are probably the only ones, an exception to which is absolutely and permanently incredible. Accordingly, it is to assertions supposed to be contradictory to these laws, or to some others coming near to them in generality, that the word impossibility (at least *total* impossibility) seems to be generally confined. Violations of other laws, of special laws of causation for instance, are said, by persons studious of accuracy in expression, to be impossible *in the circumstances of the case*; or impossible unless some cause had existed which did not exist in the particular case.[43] Of no assertion, not in contradiction to some of these very general laws, will more than improbability be asserted by any cautious person; and improbability not of the highest degree, unless the time and place in which the fact is said to have occurred, render it almost certain that the anomaly, if real, could not have been overlooked by other observers. Suspension of judgment is in all other cases the resource of the judicious inquirer; provided the testimony in favour of the anomaly presents, when well sifted, no suspicious circumstances.

But the testimony is scarcely ever found to stand that test, in cases in which the anomaly is not real. In the instances on record in which a great number of witnesses, of good reputation and scientific acquirements, have testified to the truth of something which has turned out untrue, there have almost always been circumstances which, to a keen observer who had taken due pains to sift the matter, would have rendered the testimony untrustworthy. There have generally been means of accounting for the impression on the senses or minds of the alleged percipients, by fallacious appearances; or some epidemic delusion, propagated by the contagious influence of popular feeling, has been concerned in the case; or some strong interest has been implicated--religious zeal, party feeling, vanity, or at least the passion for the marvellous, in persons strongly susceptible of it. When none of these or similar circumstances exist to account for the apparent strength of the testimony; and where the assertion is not in contradiction either to those universal laws which know no counteraction or anomaly, or to the generalizations next in comprehensiveness to them, but would only amount, if admitted, to the existence of an unknown cause or an anomalous Kind, in circumstances not so thoroughly explored but that it is credible that things hitherto unknown may still come to light; a cautious person will neither admit nor reject the testimony, but will wait for confirmation at other times and from other unconnected sources. Such ought to have been the conduct of the King of Siam when the Dutch travellers affirmed to him the existence of ice. But an ignorant person is as obstinate in his contemptuous incredulity as he is unreasonably credulous. Anything unlike his own narrow experience he disbelieves, if it flatters no propensity; any nursery tale is swallowed implicitly by him if it does.

Sec. 4. I shall now advert to a very serious misapprehension of the principles of the subject, which has been

committed by some of the writers against Hume's Essay on Miracles, and by Bishop Butler before them, in their anxiety to destroy what appeared to them a formidable weapon of assault against the Christian religion; and the effect of which is entirely to confound the doctrine of the Grounds of Disbelief. The mistake consists in overlooking the distinction between (what may be called) improbability before the fact, and improbability after it; or (since, as Mr. Venn remarks, the distinction of past and future is not the material circumstance) between the improbability of a mere guess being right, and the improbability of an alleged fact being true.

Many events are altogether improbable to us, before they have happened, or before we are informed of their happening, which are not in the least incredible when we are informed of them, because not contrary to any, even approximate, induction. In the cast of a perfectly fair die, the chances are five to one against throwing ace, that is, ace will be thrown on an average only once in six throws. But this is no reason against believing that ace was thrown on a given occasion, if any credible witness asserts it; since though ace is only thrown once in six times, *some* number which is only thrown once in six times must have been thrown if the die was thrown at all. The improbability, then, or in other words, the unusualness, of any fact, is no reason for disbelieving it, if the nature of the case renders it certain that either that or something equally improbable, that is, equally unusual, did happen. Nor is this all: for even if the other five sides of the die were all twos, or all threes, yet as ace would still on the average come up once in every six throws, its coming up in a given throw would be not in any way contradictory to experience. If we disbelieved all facts which had the chances against them beforehand, we should believe hardly anything. We are told that A. B. died yesterday: the moment before we were so told, the chances against his having died on that day may have been ten thousand to one; but since he was certain to die at some time or other, and when he died must necessarily die on some particular day, while the preponderance of chances is very great against every day in particular, experience affords no ground for discrediting any testimony which may be produced to the event's having taken place on a given day.

Yet it has been considered, by Dr. Campbell and others, as a complete answer to Hume's doctrine (that things are incredible which are *contrary* to the uniform course of experience), that we do not disbelieve, merely because the chances were against them, things in strict *conformity* to the uniform course of experience; that we do not disbelieve an alleged fact merely because the combination of causes on which it depends occurs only once in a certain number of times. It is evident that whatever is shown by observation, or can be proved from laws of nature, to occur in a certain proportion (however small) of the whole number of possible cases, is not contrary to experience; though we are right in disbelieving it, if some other supposition respecting the matter in question involves on the whole a less departure from the ordinary course of events. Yet, on such grounds as this have able writers been led to the extraordinary conclusion, that nothing supported by credible testimony ought ever to be disbelieved.

Sec. 5. We have considered two species of events, commonly said to be improbable; one kind which are in no way extraordinary, but which, having an immense preponderance of chances against them, are improbable until they are affirmed, but no longer; another kind which, being contrary to some recognised law of nature, are incredible on any amount of testimony except such as would be sufficient to shake our belief in the law itself. But between these two classes of events, there is an intermediate class, consisting of what are commonly termed Coincidences: in other words, those combinations of chances which present some peculiar and unexpected regularity, assimilating them, in so far, to the results of law. As if, for example, in a lottery of a thousand tickets, the numbers should be drawn in the exact order of what are called the natural numbers, 1, 2, 3, &c. We have still to consider the principles of evidence applicable to this case: whether there is any difference between coincidences and ordinary events, in the amount of testimony or other evidence necessary to render them credible.

It is certain, that on every rational principle of expectation, a combination of this peculiar sort may be expected quite as often as any other given series of a thousand numbers; that with perfectly fair dice, sixes will be thrown twice, thrice, or any number of times in succession, quite as often in a thousand or a million throws, as any other succession of numbers fixed upon beforehand; and that no judicious player would give

greater odds against the one series than against the other. Notwithstanding this, there is a general disposition to regard the one as much more improbable than the other, and as requiring much stronger evidence to make it credible. Such is the force of this impression, that it has led some thinkers to the conclusion, that nature has greater difficulty in producing regular combinations than irregular ones; or in other words, that there is some general tendency of things, some law, which prevents regular combinations from occurring, or at least from occurring so often as others. Among these thinkers may be numbered D'Alembert; who, in an Essay on Probabilities to be found in the fifth volume of his *Melanges*, contends that regular combinations, though equally probable according to the mathematical theory with any others, are physically less probable. He appeals to common sense, or in other words, to common impressions; saying, if dice thrown repeatedly in our presence gave sixes every time, should we not, before the number of throws had reached ten, (not to speak of thousands of millions,) be ready to affirm, with the most positive conviction, that the dice were false?

The common and natural impression is in favour of D'Alembert: the regular series would be thought much more unlikely than an irregular. But this common impression is, I apprehend, merely grounded on the fact, that scarcely anybody remembers to have ever seen one of these peculiar coincidences: the reason of which is simply that no one's experience extends to anything like the number of trials, within which that or any other given combination of events can be expected to happen. The chance of sixes on a single throw of two dice being 1/36, the chance of sixes ten times in succession is 1 divided by the tenth power of 36; in other words, such a concurrence is only likely to happen once in 3,656,158,440,062,976 trials, a number which no dice-player's experience comes up to a millionth part of. But if, instead of sixes ten times, any other given succession of ten throws had been fixed upon, it would have been exactly as unlikely that in any individual's experience that particular succession had ever occurred; although this does not *seem* equally improbable, because no one could possibly have remembered whether it had occurred or not, and because the comparison is tacitly made, not between sixes ten times and any one particular series of throws, but between all regular and all irregular successions taken together.

That (as D'Alembert says) if the succession of sixes was actually thrown before our eyes, we should ascribe it not to chance, but to unfairness in the dice, is unquestionably true. But this arises from a totally different principle. We should then be considering, not the probability of the fact in itself, but the comparative probability with which, when it is known to have happened, it may be referred to one or to another cause. The regular series is not at all less likely than the irregular one to be brought about by chance, but it is much more likely than the irregular one to be produced by design; or by some general cause operating through the structure of the dice. It is the nature of casual combinations to produce a repetition of the same event, as often and no oftener than any other series of events. But it is the nature of general causes to reproduce, in the same circumstances, always the same event. Common sense and science alike dictate that, all other things being the same, we should rather attribute the effect to a cause which if real would be very likely to produce it, than to a cause which would be very unlikely to produce it. According to Laplace's sixth theorem, which we demonstrated in a former chapter, the difference of probability arising from the superior *efficacy* of the constant cause, unfairness in the dice, would after a very few throws far outweigh any antecedent probability which there could be against its existence.

D'Alembert should have put the question in another manner. He should have supposed that we had ourselves previously tried the dice, and knew by ample experience that they were fair. Another person then tries them in our absence, and assures us that he threw sixes ten times in succession. Is the assertion credible or not? Here the effect to be accounted for is not the occurrence itself, but the fact of the witness's asserting it. This may arise either from its having really happened, or from some other cause. What we have to estimate is the comparative probability of these two suppositions.

If the witness affirmed that he had thrown any other series of numbers, supposing him to be a person of veracity, and tolerable accuracy, and to profess that he took particular notice, we should believe him. But the ten sixes are exactly as likely to have been really thrown as the other series. If, therefore, this assertion is less credible than the other, the reason must be, not that it is less likely than the other to be made truly, but that it is

more likely than the other to be made falsely.

One reason obviously presents itself why what is called a coincidence, should be oftener asserted falsely than an ordinary combination. It excites wonder. It gratifies the love of the marvellous. The motives, therefore, to falsehood, one of the most frequent of which is the desire to astonish, operate more strongly in favour of this kind of assertion than of the other kind. Thus far there is evidently more reason for discrediting an alleged coincidence, than a statement in itself not more probable, but which if made would not be thought remarkable. There are cases, however, in which the presumption on this ground would be the other way. There are some witnesses who, the more extraordinary an occurrence might appear, would be the more anxious to verify it by the utmost carefulness of observation before they would venture to believe it, and still more before they would assert it to others.

Sec. 6. Independently, however, of any peculiar chances of mendacity arising from the nature of the assertion, Laplace contends, that merely on the general ground of the fallibility of testimony, a coincidence is not credible on the same amount of testimony on which we should be warranted in believing an ordinary combination of events. In order to do justice to his argument, it is necessary to illustrate it by the example chosen by himself.

If, says Laplace, there were one thousand tickets in a box, and one only has been drawn out, then if an eye-witness affirms that the number drawn was 79, this, though the chances were 999 in 1000 against it, is not on that account the less credible; its credibility is equal to the antecedent probability of the witness's veracity. But if there were in the box 999 black balls and only one white, and the witness affirms that the white ball was drawn, the case according to Laplace is very different: the credibility of his assertion is but a small fraction of what it was in the former case; the reason of the difference being as follows.

The witnesses of whom we are speaking must, from the nature of the case, be of a kind whose credibility falls materially short of certainty: let us suppose, then, the credibility of the witness in the case in question to be 9/10; that is, let us suppose that in every ten statements which the witness makes, nine on an average are correct, and one incorrect. Let us now suppose that there have taken place a sufficient number of drawings to exhaust all the possible combinations, the witness deposing in every one. In one case out of every ten in all these drawings he will actually have made a false announcement. But in the case of the thousand tickets these false announcements will have been distributed impartially over all the numbers, and of the 999 cases in which No. 79 was not drawn, there will have been only one case in which it was announced. On the contrary, in the case of the thousand balls, (the announcement being always either "black" or "white,") if white was not drawn, and there was a false announcement, that false announcement *must* have been white; and since by the supposition there was a false announcement once in every ten times, white will have been announced falsely in one tenth part of all the cases in which it was not drawn, that is, in one tenth part of 999 cases out of every thousand. White, then, is drawn, on an average, exactly as often as No. 79, but it is announced, without having been really drawn, 999 times as often as No. 79; the announcement therefore requires a much greater amount of testimony to render it credible.[44]

To make this argument valid it must of course be supposed, that the announcements made by the witness are average specimens of his general veracity and accuracy; or, at least, that they are neither more nor less so in the case of the black and white balls, than in the case of the thousand tickets. This assumption, however, is not warranted. A person is far less likely to mistake, who has only one form of error to guard against, than if he had 999 different errors to avoid. For instance, in the example chosen, a messenger who might make a mistake once in ten times in reporting the number drawn in a lottery, might not err once in a thousand times if sent simply to observe whether a ball was black or white. Laplace's argument therefore is faulty even as applied to his own case. Still less can that case be received as completely representing all cases of coincidence. Laplace has so contrived his example, that though black answers to 999 distinct possibilities, and white only to one, the witness has nevertheless no bias which can make him prefer black to white. The witness did not know that there were 999 black balls in the box and only one white; or if he did, Laplace has taken care to make all the

999 cases so undistinguishably alike, that there is hardly a possibility of any cause of falsehood or error operating in favour of any of them, which would not operate in the same manner if there were only one. Alter this supposition, and the whole argument falls to the ground. Let the balls, for instance, be numbered, and let the white ball be No. 79. Considered in respect of their colour, there are but two things which the witness can be interested in asserting, or can have dreamt or hallucinated, or has to choose from if he answers at random, viz. black and white: but considered in respect of the numbers attached to them, there are a thousand: and if his interest or error happens to be connected with the numbers, though the only assertion he makes is about the colour, the case becomes precisely assimilated to that of the thousand tickets. Or instead of the balls suppose a lottery, with 1000 tickets and but one prize, and that I hold No. 79, and being interested only in that, ask the witness not what was the number drawn, but whether it was 79 or some other. There are now only two cases, as in Laplace's example; yet he surely would not say that if the witness answered 79, the assertion would be in an enormous proportion less credible, than if he made the same answer to the same question asked in the other way. If, for instance, (to put a case supposed by Laplace himself,) he has staked a large sum on one of the chances, and thinks that by announcing its occurrence he shall increase his credit; he is equally likely to have betted on any one of the 999 numbers which are attached to black balls, and so far as the chances of mendacity from this cause are concerned, there will be 999 times as many chances of his announcing black falsely, as white.

Or suppose a regiment of 1000 men, 999 Englishmen and one Frenchman, and that of these one man has been killed, and it is not known which. I ask the question, and the witness answers, the Frenchman. This was not only as improbable *a priori*, but is in itself as singular a circumstance, as remarkable a coincidence, as the drawing of the white ball: yet we should believe the statement as readily, as if the answer had been John Thompson. Because though the 999 Englishmen were all alike in the point in which they differed from the Frenchman, they were not, like the 999 black balls, undistinguishable in every other respect; but being all different, they admitted as many chances of interest or error, as if each man had been of a different nation; and if a lie was told or a mistake made, the misstatement was as likely to fall on any Jones or Thompson of the set, as on the Frenchman.

The example of a coincidence selected by D'Alembert, that of sixes thrown on a pair of dice ten times in succession, belongs to this sort of cases rather than to such as Laplace's. The coincidence is here far more remarkable, because of far rarer occurrence, than the drawing of the white ball. But though the improbability of its really occurring is greater, the superior probability of its being announced falsely cannot be established with the same evidence. The announcement "black" represented 999 cases, but the witness may not have known this, and if he did, the 999 cases are so exactly alike, that there is really only one set of possible causes of mendacity corresponding to the whole. The announcement "sixes *not* drawn ten times," represents, and is known by the witness to represent, a great multitude of contingencies, every one of which being unlike every other, there may be a different and a fresh set of causes of mendacity corresponding to each.

It appears to me, therefore, that Laplace's doctrine is not strictly true of any coincidences, and is wholly inapplicable to most: and that to know whether a coincidence does or does not require more evidence to render it credible than an ordinary event, we must refer, in every instance, to first principles, and estimate afresh what is the probability that the given testimony would have been delivered in that instance, supposing the fact which it asserts not to be true.

With these remarks we close the discussion of the Grounds of Disbelief; and along with it, such exposition as space admits, and as the writer has it in his power to furnish, of the Logic of Induction.

FOOTNOTES:

[1] *Cours de Philosophie Positive*, ii. 656.

[2] Vide supra, book iii. ch. xi.

[3] *Philosophy of Discovery*, pp. 185 et seqq.

[4] *Philosophie Positive*, ii. 434-437.

[5] As an example of legitimate hypothesis according to the test here laid down, has been justly cited that of Broussais, who, proceeding on the very rational principle that every disease must originate in some definite part or other of the organism, boldly assumed that certain fevers, which not being known to be local were called constitutional, had their origin in the mucous membrane of the alimentary canal. The supposition was indeed, as is now generally admitted, erroneous; but he was justified in making it, since by deducing the consequences of the supposition, and comparing them with the facts of those maladies, he might be certain of disproving his hypothesis if it was ill founded, and might expect that the comparison would materially aid him in framing another more conformable to the phenomena.

The doctrine now universally received, that the earth is a natural magnet, was originally an hypothesis of the celebrated Gilbert.

Another hypothesis, to the legitimacy of which no objection can lie, and which is well calculated to light the path of scientific inquiry, is that suggested by several recent writers, that the brain is a voltaic pile, and that each of its pulsations is a discharge of electricity through the system. It has been remarked that the sensation felt by the hand from the beating of a brain, bears a strong resemblance to a voltaic shock. And the hypothesis, if followed to its consequences, might afford a plausible explanation of many physiological facts, while there is nothing to discourage the hope that we may in time sufficiently understand the conditions of voltaic phenomena to render the truth of the hypothesis amenable to observation and experiment.

The attempt to localize, in different regions of the brain, the physical organs of our different mental faculties and propensities, was, on the part of its original author, a legitimate example of a scientific hypothesis; and we ought not, therefore, to blame him for the extremely slight grounds on which he often proceeded, in an operation which could only be tentative, though we may regret that materials barely sufficient for a first rude hypothesis should have been hastily worked up into the vain semblance of a science. If there be really a connexion between the scale of mental endowments and the various degrees of complication in the cerebral system, the nature of that connexion was in no other way so likely to be brought to light as by framing, in the first instance, an hypothesis similar to that of Gall. But the verification of any such hypothesis is attended, from the peculiar nature of the phenomena, with difficulties which phrenologists have not shown themselves even competent to appreciate, much less to overcome.

Mr. Darwin's remarkable speculation on the Origin of Species is another unimpeachable example of a legitimate hypothesis. What he terms "natural selection" is not only a *vera causa*, but one proved to be capable of producing effects of the same kind with those which the hypothesis ascribes to it: the question of possibility is entirely one of degree. It is unreasonable to accuse Mr. Darwin (as has been done) of violating the rules of Induction. The rules of Induction are concerned with the conditions of Proof. Mr. Darwin has never pretended that his doctrine was proved. He was not bound by the rules of Induction, but by those of Hypothesis. And these last have seldom been more completely fulfilled. He has opened a path of inquiry full of promise, the results of which none can foresee. And is it not a wonderful feat of scientific knowledge and ingenuity to have rendered so bold a suggestion, which the first impulse of every one was to reject at once, admissible and discussable, even as a conjecture?

[6] Whewell's *Phil. of Discovery*, pp. 275, 276.

[7] What has most contributed to accredit the hypothesis of a physical medium for the conveyance of light, is the certain fact that light *travels*, (which cannot be proved of gravitation,) that its communication is not instantaneous, but requires time, and that it is intercepted (which gravitation is not) by intervening objects. These are analogies between its phenomena and those of the mechanical motion of a solid or fluid substance.

But we are not entitled to assume that mechanical motion is the only power in nature capable of exhibiting those attributes.

[8] *Phil. of Disc.* p. 274.

[9] P. 271.

[10] P. 251 and the whole of Appendix G.

[11] In Dr. Whewell's latest version of his theory (*Philosophy of Discovery*, p. 331) he makes a concession respecting the medium of the transmission of light, which, taken in conjunction with the rest of his doctrine on the subject, is not, I confess, very intelligible to me, but which goes far towards removing, if it does not actually remove, the whole of the difference between us. He is contending, against Sir William Hamilton, that all matter has weight. Sir William, in proof of the contrary, cited the luminiferous ether, and the calorific and electric fluids, "which," he said, "we can neither denude of their character of substance, nor clothe with the attribute of weight." "To which," continues Dr. Whewell, "my reply is, that precisely because I cannot clothe these agents with the attribute of Weight, I *do* denude them of the character of Substance. They are not substances, but agencies. These Imponderable Agents, are not properly called Imponderable Fluids. This I conceive that I have proved." Nothing can be more philosophical. But if the luminiferous ether is not matter, and fluid matter too, what is the meaning of its undulations? Can an agency undulate? Can there be alternate motion forward and backward of the particles of an agency? And does not the whole mathematical theory of the undulations imply them to be material? Is it not a series of deductions from the known properties of elastic fluids? *This* opinion of Dr. Whewell reduces the undulations to a figure of speech, and the undulatory theory to the proposition which all must admit, that the transmission of light takes place according to laws which present a very striking and remarkable agreement with those of undulations. If Dr. Whewell is prepared to stand by this doctrine, I have no difference with him on the subject.

Since this chapter was written, the hypothesis of the luminiferous ether has acquired a great accession of apparent strength, by being adopted into the new doctrine of the Conservation of Force, as affording a mechanism by which to explain the mode of production not of light only, but of heat, and probably of all the other so-called imponderable agencies. In the present immature stage of the great speculation in question, I would not undertake to define the ultimate relation of the hypothetical fluid to it; but I must remark that the essential part of the new theory, the reciprocal convertibility and interchangeability of these great cosmic agencies, is quite independent of the molecular motions which have been imagined as the immediate causes of those different manifestations and of their substitutions for one another; and the former doctrine by no means necessarily carries the latter with it. I confess that the entire theory of the vibrations of the ether, and the movements which these vibrations are supposed to communicate to the particles of solid bodies, seems to me at present the weakest part of the new system, tending rather to weigh down than to prop up those of its doctrines which rest on real scientific induction.

[12] Thus, water, of which eight-ninths in weight are oxygen, dissolves most bodies which contain a high proportion of oxygen, such as all the nitrates, (which have more oxygen than any others of the common salts,) most of the sulphates, many of the carbonates, &c. Again, bodies largely composed of combustible elements, like hydrogen and carbon, are soluble in bodies of similar composition; rosin, for instance, will dissolve in alcohol, tar in oil of turpentine. This empirical generalization is far from being universally true; no doubt because it is a remote, and therefore easily defeated, result of general laws too deep for us at present to penetrate; but it will probably in time suggest processes of inquiry, leading to the discovery of those laws.

[13] Or (according to Laplace's theory) the sun and the sun's rotation.

[14] Supra, book iii. ch. v. Sec. 7.

[15] Supra, book iii. ch. x. Sec. 2.

[16] In the preceding discussion, the *mean* is spoken of as if it were exactly the same thing with the *average*. But the mean for purposes of inductive inquiry, is not the average, or arithmetical mean, though in a familiar illustration of the theory the difference may be disregarded. If the deviations on one side of the average are much more numerous than those on the other (these last being fewer but greater), the effect due to the invariable cause, as distinct from the variable ones, will not coincide with the average, but will be either below or above the average, whichever be the side on which the greatest number of the instances are found. This follows from a truth, ascertained both inductively and deductively, that small deviations from the true central point are greatly more frequent than large ones. The mathematical law is, "that the most probable determination of one or more invariable elements from observation is that in which *the sum of the squares* of the individual aberrations," or deviations, "*shall be the least possible*." See this principle stated, and its grounds popularly explained, by Sir John Herschel, in his review of Quetelet on Probabilities, *Essays*, pp. 395 *et seq.*

[17] *Essai Philosophique sur les Probabilites*, fifth Paris Edition, p. 7.

[18] It even appears to me that the calculation of chances, where there are no data grounded either on special experience or on special inference, must, in an immense majority of cases, break down, from sheer impossibility of assigning any principle by which to be guided in setting out the list of possibilities. In the case of the coloured balls we have no difficulty in making the enumeration, because we ourselves determine what the possibilities shall be. But suppose a case more analogous to those which occur in nature: instead of three colours, let there be in the box all possible colours: we being supposed ignorant of the comparative frequency with which different colours occur in nature, or in the productions of art. How is the list of cases to be made out? Is every distinct shade to count as a colour? If so, is the test to be a common eye, or an educated eye, a painter's for instance? On the answer to these questions would depend whether the chances against some particular colour would be estimated at ten, twenty, or perhaps five hundred to one. While if we knew from experience that the particular colour occurs on an average a certain number of times in every hundred or thousand, we should not require to know anything either of the frequency or of the number of the other possibilities.

[19] *Prospective Review* for February 1850.

[20] "If this be not so, why do we feel so much more probability added by the first instance, than by any single subsequent instance? Why, except that the first instance gives us its possibility (a cause *adequate* to it), while every other only gives us the frequency of its conditions? If no reference to a cause be supposed, possibility would have no meaning; yet it is clear, that, antecedent to its happening, we might have supposed the event impossible, *i.e.*, have believed that there was no physical energy really existing in the world equal to producing it.... After the first time of happening, which is, then, more important to the whole probability than any other single instance (because proving the possibility), the *number* of times becomes important as an index to the intensity or extent of the cause, and its independence of any particular time. If we took the case of a tremendous leap, for instance, and wished to form an estimate of the probability of its succeeding a certain number of times; the first instance, by showing its possibility (before doubtful) is of the most importance; but every succeeding leap shows the power to be more perfectly under control, greater and more invariable, and so increases the probability; and no one would think of reasoning in this case straight from one instance to the next, without referring to the physical energy which each leap indicated. Is it not then clear that we do not ever" (let us rather say, that we do not in an advanced state of our knowledge) "conclude directly from the happening of an event to the probability of its happening again; but that we refer to the cause, regarding the past cases as an index to the cause, and the cause as our guide to the future?"--*Ibid.*

[21] The writer last quoted says that the valuation of chances by comparing the number of cases in which the event occurs with the number in which it does not occur, "would generally be wholly erroneous," and "is not

the true theory of probability." It is at least that which forms the foundation of insurance, and of all those calculations of chances in the business of life which experience so abundantly verifies. The reason which the reviewer gives for rejecting the theory, is that it "would regard an event as certain which had hitherto never failed; which is exceedingly far from the truth, even for a very large number of constant successes." This is not a defect in a particular theory, but in any theory of chances. No principle of evaluation can provide for such a case as that which the reviewer supposes. If an event has never once failed, in a number of trials sufficient to eliminate chance, it really has all the certainty which can be given by an empirical law: it *is* certain during the continuance of the same collocation of causes which existed during the observations. If it ever fails, it is in consequence of some change in that collocation. Now, no theory of chances will enable us to infer the future probability of an event from the past, if the causes in operation, capable of influencing the event, have intermediately undergone a change.

[22] Pp. 18, 19. The theorem is not stated by Laplace in the exact terms in which I have stated it; but the identity of import of the two modes of expression is easily demonstrable.

[23] For a fuller treatment of the many interesting questions raised by the theory of probabilities, I may now refer to a recent work by Mr. Venn, Fellow of Caius College, Cambridge, "The Logic of Chance;" one of the most thoughtful and philosophical treatises on any subject connected with Logic and Evidence, which have been produced in this or any other country for many years. Some criticisms contained in it have been very useful to me in revising the corresponding chapters of the present work. In several of Mr. Venn's opinions, however, I do not agree. What these are will be obvious to any reader of Mr. Venn's work who is also a reader of this.

[24] There was no greater foundation for this than for Newton's celebrated conjecture that the diamond was combustible. He grounded his guess on the very high refracting power of the diamond, comparatively to its density; a peculiarity which had been observed to exist in combustible substances; and on similar grounds he conjectured that water, though not combustible, contained a combustible ingredient. Experiment having subsequently shown that in both instances he guessed right, the prophecy is considered to have done great honour to his scientific sagacity; but it is to this day uncertain whether the guess was, in truth, what there are so many examples of in the history of science, a farsighted anticipation of a law afterwards to be discovered. The progress of science has not hitherto shown ground for believing that there is any real connexion between combustibility and a high refracting power.

[25] Hartley's *Observations on Man*, vol. i. p. 16. The passage is not in Priestley's curtailed edition.

[26] I am happy to be able to quote the following excellent passage from Mr. Baden Powell's *Essay on the Inductive Philosophy*, in confirmation, both in regard to history and to doctrine, of the statement made in the text. Speaking of the "conviction of the universal and permanent uniformity of nature," Mr. Powell says (pp. 98-100),

"We may remark that this idea, in its proper extent, is by no means one of popular acceptance or natural growth. Just so far as the daily experience of every one goes, so far indeed he comes to embrace a certain persuasion of this kind, but merely to this limited extent, that what is going on around him at present, in his own narrow sphere of observation, will go on in like manner in future. The peasant believes that the sun which rose to-day will rise again to-morrow; that the seed put into the ground will be followed in due time by the harvest this year as it was last year, and the like; but has no notion of such inferences in subjects beyond his immediate observation. And it should be observed that each class of persons, in admitting this belief within the limited range of his own experience, though he doubt or deny it in everything beyond, is, in fact, bearing unconscious testimony to its universal truth. Nor, again, is it only among the *most* ignorant that this limitation is put upon the truth. There is a very general propensity to believe that everything beyond common experience, or especially ascertained laws of nature, is left to the dominion of chance or fate or arbitrary intervention; and even to object to any attempted explanation by physical causes, if conjecturally thrown out

for an apparently unaccountable phenomenon.

"The precise doctrine of the *generalization* of this idea of the uniformity of nature, so far from being obvious, natural, or intuitive, is utterly beyond the attainment of the many. In all the extent of its universality it is characteristic of the philosopher. It is clearly the result of philosophic cultivation and training, and by no means the spontaneous offspring of any primary principle naturally inherent in the mind, as some seem to believe. It is no mere vague persuasion taken up without examination, as a common prepossession to which we are always accustomed; on the contrary, all common prejudices and associations are against it. It is pre-eminently *an acquired idea*. It is not attained without deep study and reflection. The best informed philosopher is the man who most firmly believes it, even in opposition to received notions; its acceptance depends on the extent and profoundness of his inductive studies."

[27] Supra, book iii. ch. iii. Sec. 1.

[28] It deserves remark, that these early generalizations did not, like scientific inductions, presuppose causation. What they did presuppose, was *uniformity* in physical facts. But the observers were as ready to presume uniformity in the coexistences of facts as in the sequences. On the other hand, they never thought of assuming that this uniformity was a principle pervading all nature: their generalizations did not imply that there was uniformity in everything, but only that as much uniformity as existed within their observation, existed also beyond it. The induction, Fire burns, does not require for its validity that all nature should observe uniform laws, but only that there should be uniformity in one particular class of natural phenomena: the effects of fire on the senses and on combustible substances. And uniformity to this extent was not assumed, anterior to the experience, but proved by the experience. The same observed instances which proved the narrower truth, proved as much of the wider one as corresponded to it. It is from losing sight of this fact, and considering the law of causation in its full extent as necessarily presupposed in the very earliest generalizations, that persons have been led into the belief that the law of causation is known *a priori*, and is not itself a conclusion from experience.

[29] Book ii. chap. iii.

[30] One of the most rising thinkers of the new generation in France, M. Taine (who has given, in the Revue des Deux Mondes, the most masterly analysis, at least in one point of view, ever made of the present work), though he rejects, on this and similar points of psychology, the intuition theory in its ordinary form, nevertheless assigns to the law of causation, and to some other of the most universal laws, that certainty beyond the bounds of human experience, which I have not been able to accord to them. He does this on the faith of our faculty of abstraction, in which he seems to recognise an independent source of evidence, not indeed disclosing truths not contained in our experience, but affording an assurance which experience cannot give, of the universality of those which it does contain. By abstraction M. Taine seems to think that we are able, not merely to analyse that part of nature which we see, and exhibit apart the elements which pervade it, but to distinguish such of them as are elements of the system of nature considered as a whole, not incidents belonging to our limited terrestrial experience. I am not sure that I fully enter into M. Taine's meaning; but I confess I do not see how any mere abstract conception, elicited by our minds from our experience, can be evidence of an objective fact in universal Nature, beyond what the experience itself bears witness of; or how, in the process of interpreting in general language the testimony of experience, the limitations of the testimony itself can be cast off.

[31] Book i. chap. vii.

[32] In some cases, a Kind is sufficiently identified by some one remarkable property: but most commonly several are required; each property considered singly, being a joint property of that and of other Kinds. The colour and brightness of the diamond are common to it with the paste from which false diamonds are made; its octohedral form is common to it with alum, and magnetic iron ore; but the colour and brightness and the

form together, identify its Kind; that is, are a mark to us that it is combustible; that when burnt it produces carbonic acid; that it cannot be cut with any known substance; together with many other ascertained properties, and the fact that there exist an indefinite number still unascertained.

[33] This doctrine of course assumes that the allotropic forms of what is chemically the same substance are so many different Kinds; and such, in the sense in which the word Kind is used in this treatise, they really are.

[34] Mr. De Morgan, in his *Formal Logic*, makes the just remark, that from two such premises as Most A are B, and Most A are C, we may infer with certainty that some B are C. But this is the utmost limit of the conclusions which can be drawn from two approximate generalizations, when the precise degree of their approximation to universality is unknown or undefined.

[35] *Rationale of Judicial Evidence*, vol. iii. p. 224.

[36] Supra, vol. i. p. 115.

[37] Supra, book i. ch. v. Sec. 1, and book ii. ch. v. Sec. 5.

[38] The axiom, "Equals subtracted from equals leave equal differences," may be demonstrated from the two axioms in the text. If $A = a$ and $B = b$, $A - B = a - b$. For if not, let $A - B = a - b + c$. Then since $B = b$, adding equals to equals, $A = a + c$. But $A = a$. Therefore $a = a + c$, which is impossible.

This proposition having been demonstrated, we may, by means of it, demonstrate the following: "If equals be added to unequals, the sums are unequal." If $A = a$ and B not $= b$, $A + B$ is not $= a + b$. For suppose it be so. Then, since $A = a$ and $A + B = a + b$, subtracting equals from equals, $B = b$; which is contrary to the hypothesis.

So again, it may be proved that two things, one of which is equal and the other unequal to a third thing, are unequal to one another. If $A = a$ and A not $= B$, neither is $a = B$. For suppose it to be equal. Then since $A = a$ and $a = B$, and since things equal to the same thing are equal to one another, $A = B$: which is contrary to the hypothesis.

[39] Geometers have usually preferred to define parallel lines by the property of being in the same plane and never meeting. This, however, has rendered it necessary for them to assume, as an additional axiom, some other property of parallel lines; and the unsatisfactory manner in which properties for that purpose have been selected by Euclid and others has always been deemed the opprobrium of elementary geometry. Even as a verbal definition, equidistance is a fitter property to characterize parallels by, since it is the attribute really involved in the signification of the name. If to be in the same plane and never to meet were all that is meant by being parallel, we should feel no incongruity in speaking of a curve as parallel to its asymptote. The meaning of parallel lines is, lines which pursue exactly the same direction, and which, therefore, neither draw nearer nor go farther from one another; a conception suggested at once by the contemplation of nature. That the lines will never meet is of course included in the more comprehensive proposition that they are everywhere equally distant. And that any straight lines which are in the same plane and not equidistant will certainly meet, may be demonstrated in the most rigorous manner from the fundamental property of straight lines assumed in the text, viz. that if they set out from the same point, they diverge more and more without limit.

[40] *Philosophie Positive*, iii. 414-416.

[41] See the two remarkable notes (A) and (F), appended to his *Inquiry into the Relation of Cause and Effect*.

[42] Supra, pp. 119, 120.

[43] A writer to whom I have several times referred, gives as the definition of an impossibility, that which there exists in the world no cause adequate to produce. This definition does not take in such impossibilities as these--that two and two should make five; that two straight lines should inclose a space; or that anything should begin to exist without a cause. I can think of no definition of impossibility comprehensive enough to include all its varieties, except the one which I have given: viz. An impossibility is that, the truth of which would conflict with a complete induction, that is, with the most conclusive evidence which we possess of universal truth.

As to the reputed impossibilities which rest on no other grounds than our ignorance of any cause capable of producing the supposed effects; very few of them are certainly impossible, or permanently incredible. The facts of travelling seventy miles an hour, painless surgical operations, and conversing by instantaneous signals between London and New York, held a high place, not many years ago, among such impossibilities.

[44] Not, however, as might at first sight appear, 999 times as much. A complete analysis of the cases shows that (always assuming the veracity of the witness to be 9/10) in 10,000 drawings, the drawing of No. 79 will occur nine times, and be announced incorrectly once; the credibility therefore of the announcement of No. 79 is 9/10; while the drawing of a white ball will occur nine times, and be announced incorrectly 999 times. The credibility therefore of the announcement of white is 9/1008, and the ratio of the two 1008:10; the one announcement being thus only about a hundred times more credible than the other, instead of 999 times.

BOOK IV.

OF OPERATIONS SUBSIDIARY TO INDUCTION.

"Clear and distinct ideas are terms which, though familiar and frequent in men's mouths, I have reason to think every one who uses does not perfectly understand. And possibly it is but here and there one who gives himself the trouble to consider them so far as to know what he himself or others precisely mean by them; I have, therefore, in most places, chose to put determinate or determined, instead of clear and distinct, as more likely to direct men's thoughts to my meaning in this matter."--LOCKE'S *Essay on the Human Understanding*; Epistle to the Reader.

"Il ne peut y avoir qu'une methode parfaite, qui est la *methode naturelle*; on nomme ainsi un arrangement dans lequel les etres du meme genre seraient plus voisins entre eux que ceux de tous les autres genres; les genres du meme ordre, plus que ceux de tous les autres ordres; et ainsi de suite. Cette methode est l'ideal auquel l'histoire naturelle doit tendre; car il est evident que si l'on y parvenait, l'on aurait l'expression exacte et complete de la nature entiere."--CUVIER, *Regne Animal*, Introduction.

"Deux grandes notions philosophiques dominent la theorie fondamentale de la methode naturelle proprement dite, savoir la formation des groupes naturels, et ensuite leur succession hierarchique."--COMTE, *Cours de Philosophie Positive*, 42me lecon.