ART. III. ESSAI PHILOSOPHIQUE *sur les Probabilités.* Par M. LE COMTE LAPLACE, Chancelier, &c. Paris, 1814.

IT is to the imperfection of the human mind, and not to any irregularity in the nature of things, that our ideas of chance and probability are to be referred. Events which to one man seem accidental and precarious, to another, who is better informed, or who has more power of generalization, appear to be regular and certain. Contingency and verisimilitude are therefore the offspring of human ignorance, and, with an intellect of the highest order, cannot be supposed to have any existence. In fact, the laws of the material world have the same infallible operation on the minute and the great bodies of the universe; and the motions of the former are as determinate as those of the latter. There is not a particle of water or of air, of which the condition is not defined by rules as certain as that of the sun or the planets, and that has not described from the beginning a trajectory determined by mechanical principles, subjected to the law of continuity, and capable of being mathematically defined. This trajectory is therefore in itself a thing *knowable*, and would be an object of science to a mind informed of all the original conditions, and possessing an analysis that could follow them through their various combinations. The same is true of every atom of the material world; so that nothing but information sufficiently extensive, and a calculus sufficiently powerful, is wanting to reduce all things to certainty, and, from the condition of the world, at any one instant to deduce its condition at the next; nay, to integrate the formula in which those momentary actions are included, and to express all the phenomena that ever have happened, or ever will happen, in a *function* of duration reckoned from any given instant. This is in truth the nearest approach that we can make to the idea of OMNISCIENCE; of the Wisdom which presides over the least as well as the greatest things; over the falling of a stone as well as the revolution of a planet; and which not only numbers and names the stars, but even the atoms that compose them.

The farther, accordingly, that our knowledge has extended, the more phenomena have been brought from the dominion of Chance, and placed under the government of physical causes; and the farther off have the boundaries of darkness been carried. It was, says M. LAPLACE, of the phenomena not supposed to be subjected to the regulation of fixed laws, that superstition took hold, for the purpose of awakening the fears and enslaving the minds of men. The time, adds he, is not far distant, when unusual rains, or unusual drought, the appearance of a comet, of an eclipse, of an aurora borealis, and, in general, of any extraordinary phenomenon, was regarded as a sign of the anger of heaven; and prayers were put up to avert its dangerous consequences. Men never prayed to change the course of the sun or of the planets, as experience would have soon taught them the inefficacy of such supplications. But those phenomona of which the order was not clearly perceived, were thought to be a part of the system of nature which the Divinity had not subjected to fixt laws, but had left free, for the purpose of punishing the sins of the world, and warning men of their danger. The great comet of 1456 spread terror over all Europe, at that time alarmed by the rapid successes of the Turks, and the fall of the Greek empire; and the Pope directed public prayers to be said on account of the appearance of the comet, no less than the progress of MAHOMET.

It is curious to remark how different the sensations have been which, after four revolutions, this same comet has excited in the world. HALLEY having recognized its identity with the comets of 1531, 1607, 1682, showed it to be a body revolving round the sun in 75 years nearly; he foretold its return in 1758, or the beginning of 1759, and the event has verified the most remarkable prediction in science. Comets have since ceased to be regarded as signs of the Divine displeasure; and every body must have remarked, with satisfaction, how far the comet of 1811 was from being viewed with terror, (in this country at least), even by the least instructed of the people, and from exciting any sentiment but admiration of its extraordinary beauty. The dominion of Chance is thus suffering constant diminution; and the *Anarch Old* may still complain, as in MILTON, of the encroachments that are continually making on his empire.

Probability and chance are thus ideas relative to human ignorance. The latter means a series of events not regulated by any law that we perceive. Not perceiving the existence of a law, we reason as if there were none, or no principle by which one state of things determines that which is to follow. The axiom, or, as it may be called, the definition, on which the doctrine of Probability is founded, is, that when any event may fall out a certain number of ways, all of which, to our apprehension, are equally possible, the probability that the event will happen with certain conditions accompanying it, is expressed by a fraction, of which the numerator is the number of the instances favourable to those conditions, and the denominator the number of possible instances. Thus, the probability of throwing an ace with one die is denoted by $\frac{1}{6}$, as there are six ways that the event may turn out, and only one in which it can be an ace. With two dice, the chance of throwing 2 aces is $\frac{1}{36}$; as each face of the one ace may be combined with any face of the other. Certainty is denoted here by unity; it is what happens when all the cases are favourable to the condition expected, and when the numerator and denominator of the fraction are the same. It were absurd to say, that the sentiment of belief produced by any probability, is proportional to the fraction which expresses that probability; but it is so related, or ought to be so related to it, as to increase when it increases, and to diminish when it diminishes.

The calculation of *Probability* is therefore a very ingenious application of mathematical reasoning, in order to substitute for that certainty which is quite beyond our reach, the degree of evidence that the case admits of, and to reduce this to a system of accurate reasoning. The thing obtained is only probability; but we have a certainty as to the degree in which it exists.

The invention of this calculus does not go far back. It is true, that wherever there have been games of chance, and they have been in all countries from the rudest to the most civilized, there must have been some numerical estimate formed of the probability of certain events, by which the stakes and the expectations of the gamesters must have been regulated. The principle just stated, must therefore with more or less distinctness have been long recognized; but nothing like a system of reasoning founded on it is to be found before the time of FERMAT and PASCAL. HUYGHENS was the next after these two illustrious men who treated of this matter in a treatise, *De Ratiociniis in Ludo Aleæ.* Several other mathematicians, HUDDES and DE WITT in Holland, HALLEY in England, applied the same calculus to the probabilities of human life, and the latter published the first tables relative to that object. JAMES BERNOUILLI, about the same time, proposed and resolved many problems concerning probabilities, and composed the treatise entitled *Ars Conjectandi*, which was not published till 1713, some years after his death. This work is worthy of the high reputation of the author, who treats in it of the probability which a succession of the same events, at any time, gives of its continuance; and he was the first to demonstrate a proposition concerning the indefinite multiplication of casual events, to which we shall again have occasion to advert. MONMORT published an estimable work on the same subject, *Essai sur les Jeux de Hasard*; and DEMOIVRE followed with his treatise *on the Doctrine of Chances*, which first appeared in the Philosophical Transactions for 1711, but was afterwards published in three editions, which the author successively improved. This work is the first that mentioned the theory of *recurring series*, a subject of such importance in algebra, and connected with so many of the discoveries which have since been made in the calculus of *Finite Differences*. LA PLACE does great justice to it, and has entered into an analysis of the part that relates to series. DEMOIVRE gives a demonstration of the theorem of BERNOUILLI, just referred to, which, in a series of events, serves to connect the future and the past. Several other mathematicians, and particularly LA GRANGE, have been attracted by the results which this theory offered, and by the difficulty of the problems it suggested, which seemed in many respects to require a new application of analysis. The last who has treated of it, is our author himself, in a large work in quarto, entitled, *Theorie Analytique des Probabilités*, published at Paris in 1812. The essay now under review, is an abstract of this last, containing an account of the more important conclusions deduced in it, together with many general and profound remarks on the principles of the calculus, and their application to the researches of philosophy, as well as to the affairs of life.

The analytical work contains some valuable improvements in this branch of the mathematics. We have adverted to the use made by DEMOIVRE, in his work on Chances, of the series, called Recurring, in which the coefficient of each term is formed in the same manner from the coefficients of a certain number of the preceding terms. The generalization of this property led LA PLACE to consider all those series in which the coefficients are formed by substituting the exponents, every where, in the same formula; or where, in every term, the coefficient is the same function of the exponent. A series of this kind being supposed, a function of the variable quantity may be found, from the development of which the series might be derived; and this function is what LA PLACE calls the *Generating Function* (*Fonction Generatrice*) of the coefficients in the supposed series, or rather of the function in which all those series are included. This gives rise to a new branch of analysis, the calculus of *Generating Functions*, the principles of which he first explained in the Memoires of the Academy of Sciences for 1779. From these series, by applying the method of finite and partial differences, he has extracted results that throw great light on the doctrine of Chances, and readily afford demonstrations of many propositions that cannot but with the greatest difficulty be proved by any other means. It must not seem surprising that the doctrine of Series is thus intimately connected with the theory of Probabilities; for it should be remembered, that the first considerable improvement in that theory came from the same quarter. The numbers of combinations that can be formed of a given number of things, taking them two and two, three and three, &c. are given by the successive coefficients of a binomial raised to the power denoted by the number of things in question. Such combinations are evidently much concerned in the laws of Chance; and BERNOUILLI deduced from them a great number of conclusions concerning those laws. DE MOIVRE went farther than BERNOUILLI, and LA PLACE much farther than either; but to give any adequate idea of the analytical methods which he has employed, is not to be expected in an abstract like the present. For a general view of the analytical methods applied to the calculation of Probabilities, we may refer the reader to the conclusion of the *Essai Philosophique*, p. 90. &c. and to the beginning of the *Theorie Analytique*. To a passage in the latter, however, we cannot but advert, and with much less satisfaction than we have generally felt in pointing out any of the remarks of this celebrated writer to the attention of our readers. '*Il paraît que FERMAT, le veritable inventeur du calcul differentiel, a consideré ce calcul comme une dérivation de celui des differences finies,*' &c. Against the affirmation that FERMAT is the real inventor of the Differential Calculus, we must enter a strong and solemn protestation. The age in which that discovery was made, has been unanimous in ascribing the honour of it either to NEWTON or LEIBNITZ; or, as seems to us much the fairest and most probable opinion, to both; that is, to each independently of the other, the priority in respect of time being somewhat on the side of the English mathematician. The writers of the history of the mathematical sciences have given their suffrages to the same effect;—MONTUCLA, for instance, who has treated the subject with great impartiality, and BOSSUT, with no prejudices certainly in favour of the English philosopher. In the great controversy, to which this invention gave rise, all the claims were likely to be well considered; and the ultimate and fair decision, in which all sides seem to have acquiesced, is that which has just been mentioned. It ought to be on good grounds, that a decision, passed by such competent judges, and that has now been in force for a hundred years, should all at once be reversed.—FERMAT has strong claims undoubtedly on the gratitude of posterity; and we do not believe that there exists, either among the productions of antient or modern science, a work of the same size with his *Opera Varia*, that contains so many *traits* of original invention. He had certainly approached very near to the differential or fluxionary calculus, as his friend ROBERVAL had also done. He considered the infinitely small quantities introduced in his method of drawing tangents, and of resolving *maxima* and *minima*, as derived from finite differences; and, as LAPLACE remarks, he has extended his method to a case, when the variable quantity is irrational. He was, therefore, very near to the method of fluxions; with the principle of it, he was perfectly acquainted;—and so at the same time were both ROBERVAL and WALLIS, though men much inferior to FERMAT. The truth is, that the discovery of the new calculus was so gradually approximated, that more than one had come quite near it, and were perfectly acquainted with its principles, before any of the writings of NEWTON or LEIBNITZ were known. That which must give, in such a case, the right of being considered as the true inventor, is the extension of the principle to its full range; connecting with it a new calculus, and new analytical operations; the invention of a new algorithm with corresponding symbols. These last form the public acts, by which the invention becomes known to the world at large, the judge by which the matter must be finally decided. Great, therefore, as is the merit of FERMAT, which no body can be more willing than ourselves to acknowledge; and near as he was to the greatest invention of modern times, we cannot admit that his property in it is to be put on a footing with that of NEWTON or of LEIBNITZ;—we should fear, that in doing so, we were violating one of the most sacred and august monuments that posterity ever raised in honour of the dead.

It has been already stated, that when all the different ways in which an event can fall out, are equally possible and independent of one another, the fraction which expresses the probability, that the event may have certain conditions, is one which has for its numerator all the cases favourable to such conditions, and for its denominator all the cases possible. But when the event that happens affects that which is to follow, the question becomes sometimes of considerable difficulty. M. LAPLACE mentions one case, simple indeed, but important in its application. Suppose a fact to be transmitted through 20 persons;—the first communicating it to the second, the second to the third, &c.; and let the probability of each testimony be expressed by $\frac{9}{10}$, (that is, suppose that of 10 reports made by each witness, 9 only are true), then at every time the story passes from one witness to another, the evidence is reduced to $\frac{9}{10}$ of what it was before.—Thus, after it has passed through the whole 20, the evidence will be found to be less than $\frac{1}{8}$.

'The diminution of evidence by this sort of transmission may,' says LAPLACE, 'be compared to the extinction of light by the interposition of several pieces of glass; a small number of pieces will be sufficient to render an object entirely invisible which a single piece allowed to be seen very distinctly. Historians do not seem,' he adds, 'to have paid sufficient attention to this degradation of the probability of facts when seen across a great number of successive generations.'

It does not appear, however, that the diminution of evidence here supposed is a necessary consequence of transmission from one age to another. It may hold in some instances; but in those that most commonly occur, no sensible diminution of evidence seems to be produced by the lapse of time. Take any antient event that is well attested, such, for example, as the retreat of the Ten Thousand, and we are persuaded it will be generally admitted that the certainty of that event having taken place is as great at this moment as it was on the return of the Greek army, or immediately after Xenophon had published his narrative. The calculation of chances may indeed be brought to depose in favour of it; for the probability will be found to be very small, that any considerable interpolation or change in the supposed narrative of Xenophon could have taken place without some historical document remaining to inform us of such a change. The combination of the chances necessary to produce and to conceal such an interpolation is in the highest degree improbable; and the autherity of Xenophon remains, on that account, the same at this moment that it was originally. The ignorance of a transcriber, or the presumption of a commentator, may vitiate and alter a passage; but there is a virtue in sense and consistency by which they restore themselves. The greatest danger that an antient author runs is when a critic like Bentley is turned loose upon his text. Yet there is no fear but that, in the arguments by which he would recommend his alterations, he will leave a sufficient security against their being received.

There is an error on the subject of chance, and of cases that are equally possible, against which it is necessary to guard.

Some writers argue as if regular events were less possible than irregular, and that in the game, for example, of Cross and Pile, a combination in which Cross would happen twenty times in succession, is less easy for nature to produce than one in which Cross and Pile are mixt together without regularity. This however is not true; for it is to suppose that the events which have already taken place, affect those that are to follow; and this, in what relates to chance, cannot be admitted. The regular combinations happen more rarely than the irregular, only because they are less numerous. If we look for a particular cause as acting in the cases where symmetry occurs, it is not because we suppose the symmetrical arrangement to be less possible than any other; but it is improbable that chance has produced it, because the symmetrical arrangements are few and the asymmetrical may be without number. We see on a table, for instance, letters so disposed as to make the word *Constantinople*; and we immediately conclude that this arrangement is not the effect of chance: not that it is less possible for chance to produce it, than any other given arrangement of the same fourteen letters—for if it were not a word in any language, we would never suspect the existence of design—but because the word being in use amongst us, it is incomparably more probable that this arrangement of the letters is the work of design, than of chance.

'Events may be so extraordinary that they can hardly be established by testimony. We would not give credit to a man who would affirm that he saw an hundred dice thrown in the air, and that they all fell on the same faces. If we had ourselves been spectators of such an event, we would not believe our own eyes, till we had scrupulously examined all the circumstances, and assured ourselves that there was no trick nor deception. After such an examination, we would not hesitate to admit it, notwithstanding its great improbability; and no one would have recourse to an inversion of the laws of vision in order to account for it. This shows that the probability of the *continuance* of the laws of nature is superior, in our estimation, to every other evidence, and to that of historical facts the best established. One may judge therefore of the weight of testimony necessary to prove a suspension of those laws, and how fallacious it is in such cases to apply the common rules of evidence.'

It sometimes happens, however, that a prevailing opinion, or a prejudice, may so diminish the natural improbability of an event, that it shall appear easily overcome by the force of testimony.

'This has happened with men of the first abilities; and in the age of Lewis XIV, Racine and Pascal were two remarkable examples of it. It is humiliating to see with what complacency Racine, that admirable painter of the human heart, *and the most perfect poet who has ever been*, relates as a miraculous event, the cure of Mademoiselle Perrier, the niece of Pascal, and *pensionnaire* of the Abbey of Port-Royal: It is no less painful to read the reasonings by which Pascal endeavours to prove that this miracle had become necessary to the cause of religion, in order to justify the doctrine of the Nuns of that Abbey, at that time persecuted by the Jesuits. The young Mademoiselle Perrier, who was then about three years and a half old, was afflicted with a *fistula lachrymalis*; she touched her sore eye with a relique which professed to be one of the thorns of the crown placed by the Jews on the head of our Saviour, and she believed herself cured from that instant. Some days after, the physicians and surgeons attested the cure, and gave it as their opinion (in which probably they were perfectly correct) that the medicines had had no effect in bringing it about. This event, which happened in 1656, made a great noise: All Paris,' says Racine, 'flocked to Port-Royal. The crowd increased from day to day; and God seemed to take pleasure in authorizing the devotion of the people, by the number of miracles worked in that church.'

The question here touched on, how far the evidence of testimony is able to overcome that which arises from our experience of the course of nature, is one of the most delicate and important which the doctrine of Probability presents. That testimony itself derives all its force from experience, seems very certain. This, however, has sometimes been disputed; and it has been urged, that there is a natural tendency to believe in the testimony of others, independent of experience. That such a tendency really exists, we are willing to allow. A man who feels in himself a propensity to speak the truth, readily supposes a like propensity in others; and therefore, previous to all experience, may be disposed to believe in their testimony. He soon learns, however, that he cannot trust safely to this principle; for he perceives, that though men have a tendency to speak the truth, they have often motives that lead to do the contrary, that tempt them to conceal and even to pervert it; and how much these opposite motives may counteract one another, is a matter only to be collected from experience and observation. Indeed, it is quite evident, that whatever propensity we *naturally* have to believe in testimony, it must be in itself extremely fallacious, as bearing no proportion to the probability of the thing believed, or the likelihood that it will happen.

It is useless, therefore, in treating of probability, to talk of a tendency to believe, which, confessedly not being regulated by the experience of the past, cannot be depended on for its anticipation of the future. Such a tendency, whether natural or acquired, is evidently no better than a mere prejudice, and is as likely to lead to error as to truth. The evidence of testimony, then, is measured in the same way with other probabilities, and is expressed by the number of instances in which men, circumstanced in a particular way, have been known to speak true, divided by the number of cases in which they have given evidence whether true or false. It is true that the strict arithmetical value of this fraction is hardly possible, in any case, to be assigned. But a certain coarse and loose estimate of it may be formed, sufficient for directing the judgment and the conduct, on ordinary occasions.

The first author, we believe, who stated fairly the connexion between the evidence of testimony and the evidence of experience, was HUME, in his Essay on *Miracles*, a work full of deep thought and enlarged views; and, if we do not stretch the principles so far as to interfere with the truths of religion, abounding in maxims of great use in the conduct of life, as well as in the speculations of philosophy.

Conformably to the principles contained in it, and also to those in the Essay now before us, if we would form some general rules for comparing the evidence derived from our experience of the course of nature with the evidence of testimony, we may consider physical phenomena as divided into two classes, the one comprehending all those of which the course is known from experience to be perfectly uniform; and the other comprehending those of which the course, though no doubt regulated by general laws, is not perfectly conformable to any law with which we are acquainted; so that the most general rule that we are enabled to give, admits of many exceptions. The violation of the order of events among the phenomena of the former class, the suspension of gravity for example,—the deviation of any of the stars from their places, or their courses in the heavens, &c.—these are facts of which the improbability is so strong, that no testimony can prevail against it. It will always be more wonderful that the violation of such order should have taken place, than that any number of witnesses should be deceived themselves, or should be disposed to deceive others.

It is here very well worth attending to, how much the extension of our knowledge tends to give us confidence in the continuance of the general laws of nature, and to increase the improbability of their violation. Suppose a man not at all versed in astronomy, who considers the moon merely as a luminous circle that, with certain irregularities, goes round the earth from east to west nearly in 24 hours, rising once and setting once in that interval. Let this man be told, from some authority that he is accustomed to respect, that on a certain day it had been observed at London, that the moon did not set at all, but was visible above the horizon for 24 hours:—there is little doubt that, after making some difficulty about it, he would come at last to be convinced of the truth of the assertion. In this he could not be accused of any extraordinary and irrational credulity. The experience he had of the uniform setting and rising of the moon was but very limited; and, the fact alleged, might not appear to him more extraordinary, than many of the irregularities to which that luminary was subject. Let the same thing be told to an astronomer, in whose mind the rising and setting of the moon were necessarily connected with a vast number of other appearances; who knew, for example, that the supposed fact could not have happened, unless the moon had deviated exceedingly from that orbit in which it has always moved; or the position of the earth's axis had suddenly changed; or the atmospherical refraction had been increased to an extent that was never known. Any of all these events must have affected such a vast number of others, that, as no such thing was perceived, an incredible body of evidence is brought to ascertain the continuance of the moon in her regular course. The barrier that generalization and the explanation of causes thus raises against credulity and superstition,—the way in which it multiplies the evidence of experience, is highly deserving of attention, and is likely to have a great influence on the future fortunes of the human race.

Against the uniformity, therefore, of such laws, it is impossible for testimony to prevail. But with those laws that are imperfectly known, and that admit of many exceptions, the violations are not so improbable, but that testimony may be sufficient to establish them. In our own time it has happened, that the testimony produced in support of a set of extraordinary facts, has been confirmed by a scrupulous examination into the natural history of the facts themselves. When the stones which were said to have fallen from the heavens came to be chemically analyzed, they were found to have the same characters, and to consist of the same ingredients, nearly in the same proportions. Now, let us suppose two such instances:—the first person gives the stones into the hands of a naturalist, and their characters are ascertained; the second does so likewise, and the stones have the same character. Now if this character were one which, like that of sandstone, or of limestone, belongs to a numerous class, the chance of the agreement might be considerable, because the chance that the second observer should fall on a stone exactly of the same species with the first, would be as the number of the stones existing of that species, divided by the whole number of stones, of all different species existing on the face of the earth. This, with regard to sandstone or limestone, might be a large fraction; and the coincidence of the two testimonies in a falsehood might not be extremely improbable. But if the species is a very rare one, the probability of the coincidence becomes extremely small. Suppose, for example, that it is a species, numerous in a medium degree; and as there are reckoned about 261 species, let us suppose that the individuals of the species to which the meteoric stones belong amount to $\frac{1}{261}$th part of all the stones on the surface of the earth. The accidental coincidence of the second witness with the first is denoted by the fraction $\frac{1}{261}$; of a third with the other two, by $\frac{1}{261} \times \frac{1}{261} = \frac{1}{68121}$; of a fourth with the other three, by $\frac{1}{(261)^3}$; and so on. As there are more than ten such cases, the chance of deceit or imposture is not more than $\frac{1}{(261)^9}$: that is, 1 divided by the 9th power of 261, or by a number so large as to consist of 22 places. This fraction, though extremely small, is vastly greater than the truth. The individuals of this species, instead of making a 261th part of all the stones on the surface of the earth, make, so far as we know, no part of them at all. Here, therefore, we have a testimony confirmed, and rendered quite independent of our previous knowledge of the veracity of the witnesses.

The truth of the descent of these stones on the evidence of testimony alone, would have been long before it gained entire credit; and scepticism with respect to it would have been just and philosophical. In certain states of their information, men may, on good grounds, reject the truth altogether.

The way in which probability is affected by the indefinite multiplication of events, is a remarkable part of this theory. If out of a system of events governed by chance (or by no perceivable law) you take a small number, you will find great irregularity, and nothing that looks like order, or obedience to a general rule. Increase the number of events, or take in a larger extent of the domain over which you suppose chance to preside, you will find the irregularities bear a much less proportion to the whole; they will in a certain degree compensate for one another; and something like order and regularity will begin to emerge. In proportion as the events are farther multiplied, this convergency will become more apparent; and in summing up the total amount, the events will appear adjusted to one another, by rules, from which hardly any deviation can be perceived.

Thus, in considering the subject of life and death; if we take a small extent of country, or a few people, a single parish for instance, nothing like a general rule will be discovered. The proportion of the deaths to the numbers alive, or to the numbers born; of those living at any age to those above or below that age,—all this will appear the most different in one year, compared with the next; or in one district compared with another. But subject to your examination the parish registers of a great country, or a populous city, and the facts will appear quite different. You will find the proportion of those that die annually out of a given number of inhabitants fixt with great precision, as well as of those that are born, and that have reached to the different periods of life. In the first case, the irregularities bear a great proportion to the whole: in the second, they compensate for one another; and a rule emerges, from which the deviations on opposite sides appear almost equal.

This is true not only of natural events, but of those that arise from the institutions of society, and the transactions of men with one another—Hence insurance against fire, and the dangers of the sea. Nothing is less subject to calculation, than the fate of a particular ship, or a particular house, though under given circumstances. But let a vast number of ships, in these circumstances, or of houses, be included, and the chance of their perishing, to that of their being preserved, is matter of calculation founded on experience, and reduced to such certainty, that men daily stake their fortunes on the accuracy of the results.

This is true, even where chance might be supposed to predominate the most; and where the causes that produce particular effects, are the most independent of one another.

Laplace observes, that at Paris, in ordinary times, the number of letters returned to the Post-Office, the persons to whom they were directed not being found, was nearly the same from one year to another. We have heard the same remark stated of the Dead Letter Office, as it is called, in London.

Such is the consequence of the multiplication of the events least under the controul of fixt causes: And the instances just given, are sufficient to illustrate the truth of the general proposition; which Laplace has thus stated.—

'The recurrences of events that depend on chance, approach to fixt ratios as the events become more numerous, in such a manner that the probability of the mean results not differing from those ratios by any given quantity, may come nearer to certainty than the smallest limit that can be assigned.'

Thus, if in an urn, the number of white balls to that of black, have the ratio of p to q, the number of white balls brought out if the whole number drawn be n, will approach to $\frac{p}{p+q} \times n$, the more nearly the greater that the number n is taken.

This proposition is deducible *a priori* from the theory of Probability. It was first demonstrated by Bernouilli, in the *Ars Conjectandi*, by a method that is very elaborate, and confessedly the work of much thought and study. A more simple demonstration was given by Demoivre, in his doctrine of Chances. Our author, in his *Theorie Analytique*, has given one much preferable to either, deduced from his theory of *Generating Functions*.

The solution of another curious problem which LAPLACE has given, is closely connected with the preceding. An event having happened a certain number of times in succession, what is the probability that it will happen once more?

When the number of times the event has happened is small, the formula that contains the answer to this question is considerably complicated; when the number is very great, it is extremely simple. Suppose the number to be n, the chance that the same event will again occur, is $\frac{n+1}{n+2}$, which, if n be great, is very near to unity, and may express a probability not sensibly inferior to certainty.

Thus, supposing with M LAPLACE, that the greatest antiquity to which history goes back is 5000 years, or 1826213 days, the probability that the sun will rise again to-morrow, is, according to this rule, $\frac{1826214}{1826215}$; or there is 1826214 to 1, to wager in favour of that event. This, therefore, may be considered as affording a measure of the probability that the course of nature will continue the same in future that it has been in time past. It is not however on the refined principles of this calculus, that the universal belief of mankind in such continuance is founded. The above theorem was first given by BERNOUILLI Our author's demonstration of it in the *Essai Analytique*, we believe to be new and more simple than any other.

The same multiplication of events enables us to employ the theory of probability in the discovery of causes. On this subject LAPLACE has made a number of very important observations. The phenomena of nature are for the most part enveloped in such a number of extraneous circumstances, and so many disturbing causes unite their influence, that it is very difficult, when they are small, to separate them from one another. The best way to discover them is to multiply observations, that the accidental effects may destroy one another, and leave a mean result containing only what is essential to the phenomenon. The entire removal of the accidental part is not to be expected, as has just appeared, without an infinite number of observations: the greater the number of observations, however, the more nearly is this mean result approximated.

Of this application of the doctrine of Probabilities, a number of examples are then given. The first relates to the diurnal variation of the barometer, as found from the observations of that instrument made at the Equator, where it is least subject to the action of irregular causes. From these, there appears to be a small diurnal oscillation, of which one *maximum* takes place about 9 in the morning, and a *minimum* about 4 in the evening: a second *maximum* at 11 at night, and a second *minimum* about 4 in the morning. The oscillations of the day are greater than those of the night, and amount to about $\frac{1}{18}$th of an inch. The inconstancy of the weather does not allow this variation to be immediately observeable without the tropics, or within the range of the variable winds. Nevertheless, by applying the calculus of Probabilities to a great number of accurate barometrical observations made by RAMOND during several successive years, M. LAPLACE has found such indications of the same oscillation, as to leave no doubt of its existence, though concealed under the irregular action of many accidental causes. This oscillation having its period equal to a solar day, must arise from the sun's action, most probably, in the heating and cooling of the atmosphere.

To the same calculus, in what regards the irregularities of the planetary system, our author professes to be greatly indebted. The difficulty in such cases is, often, to know whether a certain small irregularity, combined as it is with many other irregularities, has an existence or not. If it has an existence, it will give a certain determination to all the results one way more than another; and by comparing a great number of results, the reality of the determination may be discovered. It is just as if a *die* were thrown a great number of times, and it was required to find whether it had a bias to a certain side or not. After a vast number of throws, if there is no bias, each face must have turned up nearly the same number of times. If this is not found to hold; if there be one face which has turned up considerably oftener than the rest, it may safely be concluded that there is a bias to that side; and from the calculus of probabilities, the amount of the bias may be estimated.

In this way, the calculus may be applied to several astronomical phenomena, and may be considered as a means of discovering from *induction*, some conclusions that could hardly be otherwise obtained. M. LAPLACE gives an instance of this in his own researches, concerning the diminution of a certain inequality in the precession of the equinoxes, relatively to the moon only, which was suspected by MAYER, but rejected by most astronomers as not being explained on the principle of gravitation. A scrupulous examination of observations, and the application of the calculus, convinced M. LAPLACE, that the existence of the inequality was highly probable; so that he began to look out for the cause of it. It was not long before he perceived that it must arise from the spheroidal figure of the earth, which must change a little the laws of gravity towards that body, and produce of consequence an inequality in the lunar motions. This cause had hitherto been neglected by astronomers; but, when taken into account, it explained with precision the irregularity in question, and the magnitude which, by the rules of probability, he had been led to assign to it Other instances are given in the irregularities of Jupiter and Saturn, the satellites of Jupiter, &c. We shall only mention one result, and it is a very remarkable one, deduced from the motions of the planets being all in the same direction.

'One of the most remarkable phenomena in the solar system, is, that the motions of rotation and of revolution in the planets and satellites are all in the same direction, viz. in that of the sun's rotation, and not far from the plane of his equator. A phenomenon so remarkable cannot be the effect of chance: and it obviously indicates one *general cause*, which has determined all these motions. To estimate the probability with which this cause is pointed out, it must be considered, that the planetary system, such as we now see it, is composed of eleven planets and eighteen satellites; and that the rotation of the sun, of six planets, of the satellites of Jupiter, of the ring of Saturn, and of one of his satellites, are all known. These movements, taken in conjunction with those of revolution, make a total of forty-three—all in the same direction. Now, by the calculation of probabilities, it will be found that there are more than 4 millions of millions to wager against one, that this disposition is not the effect of chance; a probability much superior to that of the historical events about which we entertain the least doubt. We must therefore believe at least with equal confidence, that ONE *Primitive Cause* has directed all the planetary motions; especially when we consider, that the greater part of these motions are also nearly in the same plane.'

Our Author proceeds, then, to offer some conjectures concerning the *physical cause* to which these motions are to be ascribed. He brings together a great number of facts, from Dr HERSCHELL's observations concerning the *nebulæ* which, combined with the preceding, seem to point out the solar atmosphere as the most probable cause. But where the facts lie so far out of the reach of accurate observation as many of these do, and when the supposed cause has ceased so entirely to act, the evidence we can have is so slight, and the difficulties so many, that even the AUTHOR of the *Mécanique Céleste* must fail in giving weight and durability to his system.

In those sciences which are in a great measure conjectural, such as medicine, agriculture and politics, the *calculus* of probabilities may be employed for discovering the value of the different methods that are had recourse to. Thus, to find out the best of the treatments in use in the cure of a particular disease, the comparison of a number of cases, where the circumstances have been as much alike as possible, will enable us to judge of the accidental causes that in each particular case assisted or impeded the cure: these last will make a compensation for one another; and if the number of cases is sufficiently great, will leave the efficacy or inefficacy of the remedies distinctly visible.

'The same,' he adds, 'may be applied to political economy; with respect to which, the operations of governments are so many experiments, made on a great scale, and calculated to throw light on the conduct to be pursued on similar occasions. So many unforeseen, concealed, and inappreciable causes, have an influence on human institutions, that it is impossible to judge *a priori* of their effects.—Nothing but a long series of experiments can unfold these effects, and point out the means of counteracting those that are hurtful. It would conduce much to this object, if, in every branch of the administration, an exact register were kept of the trials made of different measures; and of the results, whether good or bad, to which they have led.'

He concludes with a maxim, which the circumstances of the times in which he has lived, must have but too deeply engraven on the mind of every Frenchman.

'Ne changeons qu'avec une circonspection extrême nos anciennes institutions et usages auxquels nos opinions et nos habitudes se sont depuis long-tems pliées. Nous connaissons bien par l'expérience du passé les inconveniens qu'ils nous presentent; mais nous ignorons quelle est l'etendue des maux que leur changement peut produire.'

These are safe and just maxims; and we are glad to think that he who expresses them holds a high situation in the government of his country. There is, however, another maxim grounded also on the doctrine of Probability, which we should think hardly less necessary than this, viz. that the rulers of mankind, in order to remove as much as possible all chance of sudden and great revolutions, would strike at the roots of the causes which so often render them inevitable, by taking care that all political institutions are gradually and slowly corrected, as their errors are found out, or as new circumstances in the situation of the world render them inapplicable. The negative precept, of not changing things but slowly, is not alone sufficient; it is necessary to add the affirmative precept, of changing them slowly, but readily, when reason for such change appears. In this way the causes that tend to disturb the public order are prevented from accumulating, so as to create, or even to justify, the spirit of revolution; and by gradual reformations, which may be made without danger, those great changes are avoided which cannot happen without incalculable mischief.

One of the most important applications of the doctrine of Probability, is to determine the most probable *mean*, or average, among a number of observations. The most accurate experiments and observations are liable to errors, which must affect the truth of the results obtained from them. To make these disappear as much as possible, observations must be greatly multiplied, in order that the errors in defect and in excess may destroy one another, and the mean, of consequence, become nearly correct. Still, however, the manner of striking this mean to the greatest advantage, remains to be examined, as also the degree of error to which, after all, it must be liable.

For a long time mathematicians were contented with taking the arithmetical mean as the true result of the observations; that is, they added them altogether, and divided the sum by the number of observations. This was sufficient when the observations appeared to be all equally good, and entitled to equal weight in the determination of the result. This, however, was far from being always the case; and COTES was the first, as M. LAPLACE remarks, who thought of a method by which each observation should have an influence in the determination of the results proportioned to its real value. Suppose that it is the position of an object that is required to be found by astronomical observation; let the place given by each individual observation be found, and at each of these conceive a weight to be placed proportional to the accuracy, or inversely as the error which it is reasonable to assign to that particular observation; the centre of gravity of all these weights is the true, or the most probable place of the object. This was in fact a generalization of the common method of taking an arithmetical mean; for it is only conceiving, that if one observation *A*, was twice as good as another observation *B*, then, instead of *A*, there should be accounted two observations of the same value with *B*, and giving the same result with *A*, and so on in any other proportion, even if the proportion were expressed by a fraction. The principle here is, that after a great number of observations, the errors in opposite directions (the positive and negative errors) must be equal. This is true, if the number were infinitely great; and, in all cases, affords a probable approximation to the truth.

The above theorem, which COTES has given at the end of his *Estimatio Errorum*, admits of a simple analytical expression, but does not appear, as is remarked by LAPLACE, to have been made use of till EULER, in his tract on the Inequalities of Jupiter and Saturn, employed equations of condition, for the first time, in determining the elements of the orbits of these two planets. Much about the same time, TOBIAS MAYER employed similar methods in his Inquiry into the Libration of the Moon, and afterwards in his Lunar Tables.

The method of COTES, when there is but one result to be determined, is of most easy application; but when there are more than one, and, of consequence, as many equations as there are observations, it is not obvious how it can be applied, and how the equations are to be combined to the best advantage. The idea occurred to LE GENDRE to introduce another equation, by supposing the sums of the squares of the errors of the observations to be a *minimum.* * This is a very happy generalization of the method of the centre of gravity, and applicable to cases to which it could not easily be accommodated. The same idea occurred to M. GAUSS about the same time. It was not demonstrated, however, till it was done in the THEORIE ANALYTIQUE of M. LAPLACE, that the result thus obtained is the best of all, that which leaves the least probable error, the limits of which are assigned at the same time.

The mean result being determined, the following rule for the limit of the accuracy is given. *Take the difference between the mean result of all the observations, and the result of each particular observation. The mean error, or the greatest that is to be feared, (and it may be either positive or negative), is a fraction, having for its numerator the square root of the sum of the squares of the differences above obtained, and for its denominator the number of observations multiplied into the square root of the number which denotes the ratio of the circumference to the diameter.*

Thus, if the differences between the mean of the observations and the observations themselves be a, b, c, d, and if n be their number, the mean error is $\frac{\sqrt{a^2 + b^2 + c^2 + \&c.}}{n\sqrt{\pi}}$

It would be unsafe to wager that the error was less than this quantity.

It will no doubt appear singular, that a quantity $\sqrt{\pi}$ having apparently no connexion with the matters in hand, should enter into the above expression. It is introduced there by the operation of integration; by means of which, it is often brought into expressions, where it was not expected. BERNOUILLI was the first who found the quantity π enter into the expressions of probability; and he appears to have thought it very remarkable.

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* Nouvelles Méthodes pour la Determination des Orbites des Cometes. Paris, 1806.

The preceding conclusion may be useful in many cases of practical astronomy, and in other parts of natural philosophy; or indeed, when any thing is to be determined in quantity or position from a great number of observations; and especially when the things to be found are represented by the co-efficients of the terms of an algebraic formula.

As an instance:—Suppose it were required having two sorts of lunar tables; and, having compared them with observations, to determine which is the best. The common way is to add together the errors of observation, and to take the arithmetical mean: the tables to which the least mean error belongs, are accounted the best. This, however, is not the way in which the question ought to be decided. The sums of the squares of the differences between the observed and the calculated places should be added together: that set in which the square root of the sum divided by the number of observations is least, is the most exact. If the number of the terms be the same, the mere comparison of the sums of the squares decides on which side the preference lies. This instance of the utility of the method of finding the mean, is given by M. LAPLACE himself. Another of the same kind may be added.—Suppose that two chronometers have been compared with the sun at noon, for a certain number of days running, and from the register kept of their errors it is required to find which of them is the best. This ought to be done by taking the squares of the differences of the errors of the chronometer for every day: that in which the sum of these squares is the least, is the preferable time-keeper. If it is required to compute the error that might be found, if either of them were applied to find the longitude, it will be determined by the formula above, and will be very considerably different from the result that would arise from a mere arithmetical mean.

We have here an instance of a problem, to which, in this country, very frequent recourse has been had in the trials of chronometers for the longitude. The only method of resolving it, has hitherto been by finding the arithmetical mean, which, however, the late Astronomer-Royal did in a particular way, which, though not the same with this, was probably the best then known. It is, however, certain, that the true going of a clock, or the measure of its merit, cannot be accurately determined, but by means of the rule which has just been explained.

We shall conclude our extracts from this small, but comprehensive volume, with one from the article on Population, which we have great pleasure in laying before our readers.

'The ratio of the population to the number of births would be increased if we could diminish or destroy any disease that is dangerous and common. This has been done, happily, in the case of the small-pox, first by the common inoculation for the disease itself, and afterwards in a much more complete manner by the vaccine inoculation, the inestimable discovery of JENNER, who has rendered himself, by that means, one of the greatest benefactors of the human race.'

'The most simple way of calculating the advantage which the extinction of a disease would produce, consists in determining from observation the number of individuals of a given age who die of it yearly, and in subtracting the amount from the total number of deaths of persons of that same age. The ratio of the difference to the total number alive at the same age would be the probability of dying at that age if the disease did not exist. By summing up all these probabilities from the beginning of life to a given age, and taking the sum from unity, the remainder will be the probability of living to that age, on the hypothesis of the disease in question being extinguished. From the series of these probabilities, the mean duration of life on the same supposition may be computed, according to rules that are well known. M. DUVILARD has found that the mean duration of human life is increased at least *three years* by the vaccine inoculation.' p. 69.

But as this review is now in danger of becoming longer than the book reviewed, we shall conclude, with recommending to our readers the perusal of the work itself; and with assuring them, that they will find in it much valuable and important matter, which has not fallen within the scope of this analysis.

ART. IV. *A Voyage round the World, in the Years* 1803, 4, 5, & 6: *Performed by Order of his Imperial Majesty Alexander the First, Emperor of Russia, in the Ship Neva.* By UREY LISIANSKY, Captain in the Russian Navy, and Knight of the Orders of St George and St Vladimer. London. Booth, Longman & Co. 4to. pp. 388. 1814.

A COUNTRY butcher makes his customers take a certain proportion of gravy beef, when he serves them with what are denominated the *prime parts.* In vain the carnivorous purchaser may plead, that he wants only to roast, and has not the most distant thought of stewing: the cunning slaughterer of horned cattle holds him fast in the chains of sensuality, and loads him with lean, and useless flank, before he allows him to enjoy the flavour of the rib, or to pasture on the obesity of the rump. Travellers are as bad as butchers. Instead of coming at once to