Review by Augustus De Morgan

ART. IV.—*Théorie Analytique des Probabilités*. Par M. le Marquis de Laplace, &c. &c. 3ème edition. Paris 1820.

MONTUCLA remarked, that if any subject might be expected to baffle the mathematicians, it would be *chance*. The same might have been said of the motions of the heavenly bodies; not at the time when the first rude theories sufficiently well represented the results of still ruder observations, but while successive improvements in the latter department were overwhelming the successive attempts at improvement of the former. In truth, the notion of chance, probability, likelihood, or by whatever name it may be called, is as much of its own nature the object of mathematical reasoning, as force or colour: it contains in itself a distinct application of the notion of relative magnitude: it is *more* or *less*, and the only difficulty (as in many other cases) lies in the assignment of the test of quantity, *how much* more or less.

Worse understood than any of the applications of mathematics, a science has been growing for a century and a half, which must end by playing an even a more important part in the adjustment of social relations, than astronomy in international communication. We make this assertion most deliberately and most positively, to be controverted by some who are at least as well able to judge as ourselves, to be looked upon with derision by others, and with doubt by most educated men. If the public mind has not yet been made to feel that the preceding prophecy is actually in process of fulfilment, it is because one primary agent has not yet been awakened to a sense of the importance of his share of the work. The mathematician has done his part, and a more difficult task he never had; the statesman is only just awakened to so much as a disposition to accumulate *some* of the data which are necessary. We speak especially of England, and by the English statesman we now begin to understand all the monied and educated part of the English public. Among the liberties in which we pride ourselves, is that of refusing to the executive all the information which is necessary to *know the country*, or at least a very considerable portion of the statistics necessary for large legislation. And yet we expect ministers to be accurately informed upon the bearings of every measure they propose, at a period when it is demonstrable that the greater portion of the community neither knows, nor has the means of being told, within twelve per cent., what its stake in the national property amounts to. This is a curious assertion, and we proceed to make it good.

All who hold life incomes, whether salaries of professional emoluments, and all who expect reversions, have a tenure which depends for its value upon two things—the average duration of life at *every* age (the mathematician will understand that we do not fall into the common error), and the rate of interest which money will obtain. As to the second, who will undertake to say what rate of interest actually *is* made, not by large companies, always ready with means of investment, or by clever men of business, who live in the metropolis, but by the average transactions of all who use money throughout the country. Let us only suppose it to be a question of one per cent.: that is, that it lies somewhere, say between 3 and 4 per cent. (if $3\frac{1}{2}$ and $4\frac{1}{2}$ be taken, it will hardly affect the final result). Now, with regard to the first point, all are agreed that the Northampton Tables are below the general average at present existing, and the Government Tables absolve it. The latter are so near the Carlisle Tables, that, for our present rough purpose, the two can hardly be distinguished. What stress we are to lay on the following circumstances we hardly know, but if we take the results of a neighbouring country, Belgium, where statistical enquiries are in a state of rapid prosecution, we find the general average of the whole country to be extremely near the mean between the Northampton and Carlisle Tables. As follows:

Age.	Mean duration of life in years		
	On the average of the Carlisle and	M. Quetelet's Belgian Tables.	Difference
0	31.95	32.15	$+.20$
5	46.04	45.72	$-.32$
10	44.30	43.86	$-.44$
15	40.26	40.50	$+.24$
20	37.45	37.34	$-.11$
25	34.35	34.72	$+.37$
30	31.31	31.96	$+.65$
35	28.34	28.93	$+.59$
40	25.35	25.84	$+.49$
45	22.49	22.68	$+.19$
50	19.55	19.48	$-.07$
55	16.58	16.44	$-.34$
60	13.78	13.44	$-.34$
65	11.34	10.76	$-.49$
70	8.89	8.40	$-.49$
75	6.78	6.39	$-.39$
80	5.13	5.04	$-.09$
85	3.75	3.83	$+.08$
90	2.85	3.12	$+.27$
95	2.14	2.13	$-.01$

This agreement is remarkably close, but it is useless. We have not the means of forming an opinion as to whether life in Belgium much exceeds of falls short of that of England. By a rough calculation for the age of 40, made from M. Ansell's Table[1], we find $24\frac{1}{2}$ for the mean duration of life at that age: but we are not prepared to go any farther into the subject. Incidentally, we may press upon those who are actually engaged in such matters, the propriety of taking steps to ascertain what is the proper mean between the Carlisle and Northampton tables which represents the grand average.

Resuming our subject, we conceive the great extremes of the question to be represented by the Carlisle table at 3 per cent.: and the Northampton table at 4 per cent. That is to say, 13 years and 17 years' purchase are the limits of the remaining value of a life income in the hands of a person aged 40, supposed to have just received a year's income. Taking 15 years as the mean, we think he must be a bold man who will undertake to pronounce, for the whole country, where between 14 and 16 years the truth

[1]This table refers entirely to the labouring classes, members of Friendly Societies. It is from work published by the Society for the Diffusion of Useful Knowledge.

would lie. Or, one man with another throughout England, the value of existing interests cannot be pronounced upon within 12 per cent.

That the preceding will be denied from all quarters, only favours our assertion. For some will lean to one table, some to another. We remain uncontradicted, so long as authorities differ to the amount which we have stated. The insurance offices, which deal in select lives, now stand upon a proper basis of knowledge, the permanence of which, however, rests upon their demanding what would be called, if means of information were more extensive, enormous premiums. The reason why they are not to be so styled, shows the consequences which result to the country from insufficient statistics. Let us take the instance of the Equitable Society, an insurance office which has accumulated enormous wealth. How was this accumulation made? By demanding the ignorance of those who came to insure. We deny the correctness of this view, but it was an ignorance which was common both to the office and to its customers; and known to the former, who were therefore obliged to make such charges as would cover, not only the risk which their tables showed with regard to the individual, but also the danger of attempting insurance at all with such limited knowledge. Having demanded premiums of which in the first instance, it was only known that they were *safe*, the result has been that they were *much more than safe:* the profit really belongs to those who now possess it, and it has been bought and paid for. The consequence to the public is, that the want of foresight in an existing government, whether blameable or not we will not undertake to pronounce, has caused a large body of subjects of the realm to make a provision for their families at the expense of some millions sterling more than was necessary for the purpose.

The same indifference to statistical information on the part of the government, or fear of the disinclination of the people to inform it, still exists and produces its effects. We see it in every large financial measure which is proposed to Parliament. The roughness of the guesses on which such plans are built, is only exceeded by the boldness of the mathematical steps by which the results are to be deduced. It is hard to say where the wedge is to be introduced into the massy obstacles which are to be cleft asunder. Shall we point to the good effects which have resulted, and do result, form the application of sound principles to actual measurements of facts? The inertness of the legislative power never attempts to originate utility, unless acted on by pressure from without. Shall we address ourselves to the individual inhabitant of the country, and endeavour to show him that the power which governs his interests is not knowledge? We shall find him so busily employed in watching the intentions of his rulers, that he has no time to think about their fitness to carry their intentions into useful effect. All the countries of Europe, which are in a state of commercial progress, are searching for information; while with us it is much that the government should compile for the use of the legislature, just that information which the collection of the revenue brings with itself, or which the demand of individual members of the House of Commons has caused to be furnished. The tables now produced by the Board of Trade, useful as they are compared with any thing which had previously existed, are but a poor provision for the growing wants of the community. We hail them, nevertheless, as harbingers of better things.

Whenever a demand arises for the creation of some new method of meeting the uncertainty of individual prospects, a process takes place which we may briefly describe as follows. To meet the chance of fundamental error, arising from ignorance of the subject, a large capital must be subscribed as an insurance fund in case the whole speculation should fail. The subscribers of this capital are of course indemnified for the risk, by receiving a return in the success of the undertaking. Those who receive this benefit must pay for it; and hence the ant of statistical knowledge is an immediate and direct tax upon the people, in favour of those who may be called the *ignorance-insurers*. But some extreme and enormous case of failure must be anticipated and provided for. And through the principle of *mutual* insurance, so little understood, yet there remains the evil of requiring those who would stipulate for a fixed sum, and have perhaps better means of employing their money, to buy, not what they want, but a something between that and half as much again, without any very definite means of saying what it shall be.

Among the various projects of this kind, we observe one recently established, which promises to be of great utility. It is a society for providing fixed sums to be paid on account of each child of a marriage, on his attaining a given age, in consideration of a life premium to be paid by the father, to date from the time of the marriage. We see here, firstly, the apparatus of an insuring capital; secondly, the construction of tables. superintended, it is said, by a great mathematician. Now this mathematician, be he great or small, could not make bricks without straw, or tables without data. Doubtless he enquired, what is the average age of marriage? What are the relative numbers of such contracts made by parties at different ages? What is the number of children produced by each, on an average, and what is the average interval between their births? He need be no conjurer to see, that all this and more, was necessary for his purpose; but we must confess, we should think him one, if he found the answers to all these questions. No doubt his province was to investigate the premiums which should be paid on some supposition which the most cautious theoriser would admit to be above the mark, and require the office to adopt them. Both the office and the insurers may thus be made safe; but neither party can undertake to say what the other buys and the other sells. The nominal \$100 must be something between \$100 and \$150, a part of the surplus being deducted in favour of the owners of the subscribed capital.

Such is the state of our commercial relations in regard to the employment of life interests for the creation of certainties. And yet we see daily variations of such interests, to which even the courts of law are continually obliged to appeal. The office of actuary has received a legal character, though what constitutes an actuary is not defined. Without the diploma of a college, or the initiation of an apprenticeship, a class of professional arithmeticians has arisen, whose verdicts are, in fact, as binding upon our courts as those of a jury where they agree, without any distinct rule as to what is to be done when they disagree. The statute relating Friendly Societies requires that their rules should be certified by an "actuary or person skilled in calculation." Is the second necessarily in the first, or the first necessarily the second? We do not at all quarrel with the legal uncertainty, because the consequence is, that an actuary is in fact he who is shown to be one, by proof that men will pay him money to have his opinions. No class of men, taken as a whole, has acted with more judgment in the multifarious and important questions which have been submitted to them. They seem to have been fully aware, that in the absence of perfect information, it was at least desirable to throw the difficulties of the subject entire upon the data, and to make everything sure from that point. Apply to one actuary for the value of a contingent reversion, and he answers boldly, say \$2539. 14*s*. $7\frac{3}{4}d$. Apply to another and his answer is as ready, say \$2092. 16*s*. $\frac{1}{2}d$. Whence arises this difference between two mean, each of whom might almost be supposed to contend for that last farthing? *There is a theory in dispute between them*, about which the public knows nothing, and each avoids distressing the mercantile man by using round numbers. For the latter, in common with the rest of the world, has got a notion that the mathematical process must give exact results, whatever the nature of the *data* may be. But both, the trader and actuary employ a course of proceeding which, as far as it goes, is one of safety. The first is generally no mathematician, and the second very often not more so than is absolutely required for his purpose. Now, to know what to throw away without thereby rendering the result more imperfect than the data, is the more difficult and delicate part of the province of the mathematician. It is, therefore, most desirable that such abbreviation should not be handled by any one who is not fully competent, as well by experience in this as in other branches of practical applications.

We have said that it may frequently happen, that two actuaries differ in their results, by differing on a point of theory, and it may be useful to the general reader, to know the general characteristics of this difference. The table know by the name of *Northampton Tables*, were published by Dr. Price in 1771, by means of registers kept in the town of Northampton, from the year 1741. This table (with some theoretical alterations, for the sake of introducing equality of decrements,) is formed form 4,700 deaths at various ages, of which, however, only 2,700 occurred above the age of 20. It was formed with that degree of caution, in such matters, for which Dr. Price was distinguished; and to which, we have no doubt whatever, the community is indebted for this, that no insurance office has ever failed, nor so far as we know, ever been generally believed to be close to failure. A bolder theorist might very easily, and upon sufficiently plausible grounds, have hazarded tables. which would have retarded this important social improvement for fifty years at least. The Northampton Tables were made the basis of the transactions of all the insurance offices; and, considered as a whole, must be looked upon as a great commercial benefit to the country. But it was soon supposed that they contained defects which made them unfit to adjust the relative interests of parties at different ages, and it was frequently affirmed, that while the younger lives were represented as too low in value, the older lives were made too high.

The *Carlisle Tables* were published in 1815, by Mr. Milne, then and now actuary to the Sun Life Assurance Society. They exhibit (with theoretical alterations as before,) the results of 1,840 deaths, which took place at Carlisle between 1779 and 1787: and 861 of these were above the age of twenty. With reference, therefore, to numbers of deaths, they are inferior in authority to the Northampton Tables, but not so much as would be generally supposed. For it is a principle *perfectly* demonstrable, but not *easily*, that when chance selections are used for the purpose of constructing a probable general law, the degree of confidence which is to be placed in the superior numbers of one selection, does not increase with the numbers, *but with their square roots*. Thus, to construct a table which should be *twice* as good as another, *ceteris paribus*, *four* times as many deaths must be recorded; for *thrice*, *nine* times as many, and so on. Exclusive, therefore of every circumstance except mere numbers, the goodness of the Carlisle and Northampton tables is not (for above 20 years of age) as 861 to 2400, but as 29 to 49, or thereabouts. In every circumstance except mere numbers, Mr. Milne had the advantage of Dr. Price: and he used it with an energy which deserved distinguished success, and, as it turned out, obtained it. For there can now be no question, that the Carlisle tables, represent the state of life among the better classes (in wealth) of this country with an approach towards precision which is remarkable, considering the scanty character of the materials.

Within the last few years, the two insurance offices which possessed the largest amount of experience, the Amicable and the Equitable, have published their results. The first of these dates back for more than a century, the second for more than fifty years. The selection of its lives, in the first, was, for a long time, anything but rigorous, as we are informed: the latter has always been distinguished by more than usual care in this respect. Taking the mean durations of life at different ages, a test which have several reasons for preferring to the one in more common use, we subjoin the following table:–

Age	Mean Duration according to the			
	Northampton	Carlisle	Amicable	Equitable
20	33.4	41.5	36.1	41.7
30	28.3	31.3	31.1	34.5
40	23.1	27.6	24.4	27.4
50	18.0	21.1	17.9	20.4
60	13.2	14.3	12.5	13.9
70	8.6	9.2	7.8	8.7
80	4.8	5.5	5.0	4.8

The Amicable Table contains 2800 deaths above the age of 20 and the Equitable 5100. On looking at these tables, we see not only a remarkable connexion between the Northampton and the Amicable, and between the Carlisle and Equitable, but also some similarity between the circumstances under which each pair was made. The Northampton table is older than the Carlisle; the Amicable is on the whole older than the Equitable. The town of Northampton is shown, by the documents of Dr. Price, to be less healthy than Carlisle, by those of Mr. Milne; the election of the Amicable, on the whole term of its existence, was believed, before their tables appeared, to be inferior to that of the Equitable. And in both there is the same anomaly with regard to the older lives; the difference between the Amicable and the Equitable, which is very great at 20 years of age, is materially lessened as we approach the older ages. But the particular point on which the Northampton Tables were long suspected, appears even from the comparison with its own companion; for whereas at 20 years of age, the Northampton gives considerably less than the Amicable, at 60 years and upwards the case is reversed. We do not speak of various other tables, as we only wish to convey to the reader who is entirely new to the question, some slight notion of the state in which we stand with respect to the results of tables.

Now the question among actuaries is this: which are the tables to be actually used in the computation of money results, those of long or short life, the Carlisle of the Northampton. There are great authorities, so far as authorities go, on both sides of the question: and we apprehend that some would use one table in one set of circumstances, and another in another. Discretion must decide; but in the meanwhile it is of importance that the public in general, and the courts of law in particular, should distinctly know, that the actuary does not merely deduce a result of pure arithmetic: for he has not only to use the tables, but to settle which of the conflicting tables he shall use. And this alone is frequently a question of two or three years' purchase in the value of contingency. It has happened more than once, that litigation has been rendered more complicated, by the opposing values producing very different opinions upon the estimated value of life interests. On what principles the judges settle the matter in such a case, we are not aware; but it most unquestionably belongs to them to enquire *what tables have been used, and why?* For the question, whether a given individual shall be considered a good or a bad life, is one which admits of being determined by the evidence, and it would be much better that the court, acting upon information, should decide whether one of the other table should be used, or whether any and what mean between them should be taken, than permit such a matter to be settled by the actuaries consulted,—the point in dispute having considerable authorities on both sides. It is also to be remembered, that even the professional men consulted are not always in possession of the information necessary to decide: a case may begin, "*A person*, aged fifty," &c. without the least information as to what the class and habits of this person may be; and parties, interested in the result, may wilfully put such a case, with *algebraical* description only, for the purpose of taking into court such an opinion as may suit their purpose. We are convinced, that, in the process of time, and as the eyes of the public become open to the very extensive character of *life interests* in this country, an officer will be appointed, a new species of *Master in Chancery*, whose duty it will be to decide those points which are now settled by reading the opinions given upon *cases before caused by parties*.

Among all the confusion which unfortunately exists in the ramifications of an extensive branch of the subject we are considering, there seems to us but one point which is very clear; namely, that though such progress has been made as secures safety as to those who are interested *en masse*, the equitable apportionment of the relative claims of the different parts of the whole, is by no means in the same state of forwardness.

The state of probability in general, as applied to the preceding questions, may be divided into two parts; of which the knowledge of the first is easily attainable, in comparison with that of the second. The latter of the two is the guide of the formed, and often the method of checking too hasty conclusions drawn from it. The mathematical analysis of the former is easy, while that of the latter is almost as complicated as the planetary theory, perhaps even more so length for length. We need hardly add, that we refer to those extensions of the subject which were first struck out by De Moivre, and which have been raised to a high degree of development by La Place. Of all the masterpieces of analysis, this is perhaps the best known; if does not address its powers to the consideration of a vast and prominent subject, such as astronomy or optics, but confines itself to a branch of enquiry of which the first principles are so easily mastered (in appearance), that the student who attempts the higher parts feels almost deprived of this rights when he begins to encounter the steepness of the subsequent ascent. The *Théorie des Probabilités* is the Mont Blanc of mathematical analysis; but the mountain has this advantage over the book, that there are guides always ready near the former, whereas the student has been left to his own method of encountering the latter.

The genius of Laplace was a perfect sledge hammer in bursting purely mathematical obstacles; but, like that useful instrument, it gave neither finish nor beauty to the results. In truth, in truism if the reader please, Laplace was neither Lagrange nor Euler, as every student is made to feel. The second is power and symmetry, the third power and simplicity. But, nevertheless, Laplace never attempted the investigation of a subject without leaving upon it the marks of difficulties conquered; sometimes clumsily, sometimes indirectly, always without minuteness of design or arrangement of detail; but still his end is obtained, and the difficulty is conquered. There are several circumstances connected with the writings of this great mathematician, which indicate vices peculiar to himself, and others which are common to his countrymen in general, we shall begin with one of the latter.

The first duty of a mathematical investigator, in the manner of sating his results, is the most distinct recognition of the rights of others; and this is a duty which he owes as much to himself as to others. He owes it to himself, because the value of every work diminishes with time, so far as it is a statement of principles or development of methods; others will in time present all such information in a shape better suited to the habits of a succeeding age. But the *historical* value of a work never diminished, but rather increases, with time; theory may be overthrown, processes may be simplified, but historical information remains, and becomes an authority which renders it necessary to preserve and refer to any work in which it exists. No one now thinks of consulting the work of the erudite Longomontanus; while that of his contemporary Riccoli is esteemed and sought after. The reason is, that the first contains little or nothing of history, while the second is full of it. That such attention to the rights of others, is due to those others, need hardly be here insisted on. Now, what we assert is, that there runs throughout most of the writings of the French national school, a thorough and culpable indifference to the necessity of clearly stating how much has been done by the writer himself, and how much by his predecessors. We do not by any means charge them with nationality; on the contrary, they are most impartially unfair both to their own countrymen and to foreigners; we may even say, that, to a certain extent, they behave properly to the latter, while of each other they are almost uniformly neglectful. Laplace himself set the most striking example of this disingenuous practice. For instance, Lagrange, proceeding on a route suggested by a theorem of Lambert, discovered the celebrated method of expansion, which all foreigners call *Lagrange's theorem*. Other and subordinate methods (in generality, only, not in utility) had been given by Taylor and Maclaurin, and are sufficiently well known by their names. Now, Laplace has occasion to demonstrate these theorems in the *Méchanique Céleste*, and how does he proceed? "Nous donnerons sur la réduction des fonctions en series, quelques théorèmes généraux qui nous seront utilisé dans la suite." (Book II, No. 20.) Would not any one imagine that these were some theorems which Laplace was producing for the first time? In the sequel, the theorem which is know to the mere beginner, as Taylor's Theorem, is described as "la formule (i) du numéro 21." Let us even grant that it is natural to refer back throughout any one work to any fixed part of it, and we have not done with this strange determination not to mention the writings of any other mathematician. For in *Théorie des Probabilités*, a work totally unconnected with the one just mentioned. Lagrange's theorem has no other designation than "la formule (p) du numéro 21 du second Livre de la *Méchanique Céleste*." And the exception only of professedly historical summaries upon points which have for the most part no connexion with his own researches, a studied suppression of the names of his predecessors and contemporaries, insomuch that had he had occasion to cite a proposition of Euclid, we have little doubt that it would have appeared as "le théoreme que j'ai demonstré dans un tel numéro." The consequence is, that the student of the *Méchanique Céleste* begins by forming an estimate of the author, which is too high, even for Laplace; and ends by discovering that the author has frequently, even where he appears most original, been only using the materials, and working nupon the track, of Lagrange, or some other. If the reaction be greater than it should be, and if the estimate formed of Laplace should be lower than it really ought to be, it would be no more than a proper lesson for living analysts of the same country, who, as we could easily show, if we were concerned with their writings, have closely copied the not very creditable example of Laplace.

The preceding remarks have a particular bearing upon the *Théorie des Probabilités*, for it is in this work that the author has furnished the most decided proof of grand originality and power. It is not that the preceding fault is avoided; for to whatever extent De Moivre, Euler, or any other, had furnished either isolated results, of hints as to method of proceeding, to precisely the same extent have their names been suppressed. Nevertheless, since less had been done to master the difficulties of this subject than in the case of the theory of gravitation, it is here that Laplace most shines as a creator of resources. It is not for us to say that, failing such predecessors as he had (Newton only excepted), he would not by his won genius have opened a route for himself. Certainly, if the power of any one man would have sufficed for the purpose, that man might have been Laplace. As it is, we can only, looking at the *Théorie des Probabilités*, in which he is most *himself*, congratulate the student upon the fact of more symmetrical heads having preceded him in his *Méchanique Céleste*. Sharing, as does the latter work, in the defects of the former, what would its five volumes have presented if Laplace had no forerunner?

It might appear to be our intention to decry the work which we have placed at the head of this article. We cannot but demur to such a charge, because to *decry* is, we presume, to try to alter the tone of a cry already existing. Now, even meaning by the world the mathematical world, there is not a sufficient proportion of that little public which has read the work in question, to raid any such collective sound as a cry either on one side or the other. The subject of the work is, in its highest parts, comparatively isolated and detached, though admitted to be of great importance in the sciences of observation. The pure theorist has no immediate occasion for the results, as results, and therefore contents himself in many instances with a glance at the processes, sufficient for admiration, though hardly so for use. The practical observer and experimenter obtains a knowledge of results and nothing more, will knowing in most cases, that the analysis is above his reach. We could number upon the finders of one hand, all the men we know *in Europe* who have *used* the results in their *published* writings in a manner which makes it clear that they could both *use* and *demonstrate*.

In pointing out, therefore, the defects of the work in question—in detaching them form the subject and laying them upon the author—taking care at the same time to distinguish between the high praise which is due to the originality and invention of the latter, and the expression of regret that he should, like Newton, have retarded the progress of his most original views by faults of style and manner—we conceive that we are doing good service, not only to the subject itself, but even to the fame of its investigator. If, at the same time, we can render it somewhat more accessible to the student, and help to create a larger class of readers, we are forwarding the creation of the opinion that the results of this theory, in its more abstruse parts, may and should be made both practical and useful, even in the restricted and commercial sense of the former term. Such must be the impression of all who have examined the evidence for this theory.

It is not our intention to conclude the subject in the present number: the length of this article (for such articles should not be very long) warns us to conclude for the present by finishing our account of the difficulties which have been placed in the way of the student previously entering upon the consideration of the subject matter of the treatise.

The *Théorie des Probabilités* consists of three great divisions. 1. An introductory essay, explanatory of general principles and results, without any appearance of mathematical symbols. 2. A purely mathematical introduction, developing the analytical methods which are finally to be employed. 3. The application of the second part to the details of the solution of questions connected with probabilities. The first of these has also been published in a separate form, under the title of *Essai Philosophique*, &c., and is comparatively well known. Our business here is mostly with the second and third. The arrangement will seem simple and natural, but there is a secret which does not appear immediately, and refers to a point which distinguishes this and several other works from most of the same magnitude. The work is not an independent treatment of the subject, but a collection of memoirs taken *verbatim* from those which the author had previously inserted in the Transactions of the Academy of Sciences. Thus in the volume for 1782, appears a paper on the valuation of functions of very high numbers, with an historical and explanatory introduction. Now this introduction being omitted, the rest of the memoir is, substantially, and the most part word for word, inserted in the work we are now describing. And the same may be said of other memoirs published at a later period: so that the *Théorie des Probabilités*, first published in 1812, may be considered as a collection of the various papers which had appeared in the Transactions cited from 1778 up to 1812.

This materially alters the view which must be taken of the treatise, considered as intended for the mathematical student. It also makes a change in the idea which must be formed of the real difficulty of the subject, as distinguished from that which is actually found in reading Laplace. The course taken has both its advantages and disadvantages: on which it may be worth while to say a few words.

Of the highest and most vigorous class of mathematical students, it may be easily guessed that they are most benefitted by the works which are least intended for them. Complete digestion and arrangement, so far from being essential to aid them in the formation power, are rather injurious. The best writer is he who shows most clearly by his process where the difficulty lies,. and who meets it in the most direct manner. All the artifice by which the road is smoothed and levelled, all the contrivance by which difficulty is actually overcome without perception of its existence, though a desirable study for the proficient, and most useful with reference to the application of science, is a loss of advantageous prospect to the student who wishes to become an original investigator. An officer who has never seen any but well-drilled soldiers, may *command* an army of them; but he who would *raise* an army must have been used to the machine he wishes to create in every stage of its process of creation, from a disorderly assembly of clowns up to a completely organised force. It is on such a ground as this that we take our stand, when we say that Euler, from the almost infinite simplicity with which he presents the most difficult subjects, and Lagrange, from the unattainable combination of power and generality which he uses *for* (more than *through*) the student, are not the best guides for one who would practise investigation. It is Laplace whose writings we should recommend for this purpose, for those very reasons which induce us to point him out as one of the most rough and clumsy of mathematical writers. A student is more likely *pro ingenio suo*, to be able to imitate Laplace by reading Laplace, than Lagrange by Lagrange, or Euler by Euler.

In the next place, of all the works which any one has produced, the most effective for the formation of original power are those which lie nearest to his own source of invention. All the difference between analysis and synthesis will exist, for the most part, between the memoir in which the discoverer first opened his views, and the ultimate method which he considered as most favourable for their deduction from his first principles. Hence we should recommend to the student to leave the elementary works and the arranged treatises as soon as possible, and betake himself to the original memoirs. He will find them not only absolutely more clear than compilations from them, but what is of much more importance, they state with distinctness what has been done on each particular point, and what is attempted to be done. If there should arise confusion from the student not perceiving that he is employed upon an isolated part of the whole which is not yet complete, there are safeguards in the *Memoir* which do not exist in the *Treatise*. Take any work on the differential calculus, from the time of Leibnitz downwards, and the formality of chapters, distinctions of subjects, and treatment of nothing but what is complete, or appears so, will leave the impression that the whole is exhausted, and that all apparent difficulty arises from the student not being able to see all that is presented to him. Now the fact is that in many cases the obstacle is of another kind, namely, that the reader is not made aware that there is more to be looked for than is presented. The assertion, *je n'en sais rien*, by which Lagrange frequently astonished those who imagined that a grand mathematician knew every thing, is frequently embodied in the spirit, or enspirited into the body, of a memoir, but seldom into that of a formal treatise. It happened to us not long ago to be very much puzzled with the account of a process given in the great work of Lacroix, one of the best of methodical writers. Chance threw in our way the original memoir of Legendre, from which the process was taken, and we found that, word for word nearly, the former writer agreed with the latter, so far as he went. But a few sentences of omission in which the original writer had limited himself were, it should seem, inconsistent with the vastness of the general design indicated in the excellent compiler's chapter. The difficulty vanished at once, since it merely arose form venturing to hint to ourselves, in the way of doubt, precisely what the original writer had proposed as a limitation.

So far then, as the great work before us preserves the actual contents of the original memoirs, it must be looked upon as very wholesome exercise for the student. But there are still some defects, arising from not completing the plan. The short historical notice and general explanation is omitted, in consequence, we suppose, of the humiliation which the writer of a treatise would feel, were he compelled to name another man. The extravagance of an original memoir lights the candle at both ends: not merely is an author permitted to say clearly where he ends, but also where he began. Did Stirling give a result which might have afforded a hint as to the direction in which more was to be looked for? Laplace may and does confess it in the Transactions of the Academy. But the economy of a finished work will not permit such freedoms; and while on the one hand the student has no direct reason for supposing that there ever *will* be any body but Laplace, he has, on the other, no means of knowing that there ever *was* any body but Laplace.

In the next place, the difficulty of the subject is materially increased by the practice of placing general descriptions at the beginning, instead of the end. Our present work begins with a tremendous account of the theory of generating functions, which we doubt not has deterred many a reader, who has imagined that it was necessary to master this first part of the work before there was an old memoir ready to reprint from. And where in the subsequent part of the work is it used? In some isolated problems connected with gambling, which in the first place might be omitted without rendering the material part of the work more difficult; and in the second place there are applications of the theory of generating functions of so simple a character, that the preliminaries connected with it might be discussed in two pages. And in what future part of the work do the very tedious (though skilful) methods of development become useful which are formally treated in the introductory chapter? Nowhere.

Here the reader may begin to suspect that the difficulty of this work does not lie entirely in the subject, but is to be attributed in great part to the author's method. That such difficulty is in part wholesome, may be very true; but it is also discouraging. Believing as we do that in spite of all we have said, the *Théorie des Probabilités* is one of the points to which the attention of the future analyst should be directed, as the subject is any way within his power, we shall here finish what we have to say on the character of the work, and proceed in a future article with that of its results.

ART. IV.—*Théorie Analytique des Probabilités*. Par M. le Marquis de Laplace, &c. &c. 3ème edition. Paris 1820.

IN continuing our remarks upon the work of which the title is now before the reader's eye, we may remind him that we have not room to enter at length upon the subject. We have already discussed considerations of a practical character, tending to shew that upon several questions, in which recourse is actually had to to the theory of probabilities, insufficiency of information produces effects prejudicial to the pecuniary interests of those concerned. This is indeed a strong point: we might urge in any plan or prospective utility upon the English public, till we were tired, and without awakening the least attention. Nor would there be any reason to complain of such a result; for the present is an age of suggestions, and every person who can read and write has some scheme in hand, by which the community is to be advantage: no wonder, then, that so few of the speculations in question have more than one investigator. But when we speak of the theory of probabilities, we bring forward a something upon which, right or wrong, many tens of millions of pounds sterling depend. The insurance offices, the friendly societies, all annuitants, and all who hold life interests of any species—again, all who insure their goods from fire, or their ships from wreck—are visibly and immediately interested in the dissemination of correct principles upon probability in general. So much for that which actually is invested: now with regard to that which might be, let it be remembered, that whenever money is hazarded in commerce or manufactures, by those who would resign the possibility of more than average profit, if they might thereby be secured from the risk of disastrous loss, the desired arrangement is rendered impossible, by want of knowledge how to apply the theory of probabilities, combined with the defect of methodized information upon the contingencies in question.

The name of the *theory of probabilities* is odious in the eyes of many, for, as all the world knows, it is the new phrase for the computation of chances, the instrument of gamblers, and, for a long time, of gamblers only; meaning, by that word, not the people who play with stocks and markets, but with cards, dice, and horses. Such an impression was the inevitable consequence of the course pursued by earlier writers on the subject, who filled their books entirely with problems related to games of chance. This was not so much a consequence of the nature of the subject, as of the state of mathematical knowledge at the time: games of chance, involving a given and comparatively small number of cases, are of easy calculation, and require only the application of simple methods; while questions of natural philosophy, or concerning the common affairs of life, involve very large numbers of cases, and require a more powerful analysis. Consequently, the older works abound with questions upon games of chance, while later writings begin to display the power of applying the very same principles to wider as well as more useful inquiries.

This objection to the tendency of the theory of probability, or the doctrine of chances, is as old as the time of De Moivre; who was not, however, able to meet it, by extending the subject matter of his celebrated treatise. In the second edition, published in 1738. he writes thus, in his dedication to a Lord Carpenter: "There are many people in the world who are prepossessed with an opinion, that the doctrine of chances has a tendency to promote play; but they soon will be undeceived, if they think fit to look into the general design of this book. In the mean while, it will not be improper to inform them, that your lordship is pleased to espouse the patronage of this second edition," &c. &c. The general design of De Moivre's work appears to be, the analysis of every game of chance which prevailed in his time; and the author seems to have imagined that he could not attract attention to any other species of problem.

In reviewing the *general design* of the work of Laplace, we desire to make the description of a book mark the present state of a science. In any other point of view, it would be superfluous to give an account of a standard treatise, which is actually in the hands of a larger number of persons than are able to read it.

In considering the simple questions of chances, we place ourselves, at the outset, in hypothetical possession of a set of circumstances, and attribute to ourselves exact and rigorous knowledge. We assume that we positively know every case that can arrive, and also that we can estimate the relative probabilities of the several causes. This of itself has a tendency to mislead the beginner, because the known circumstances are generally expressed by means of some simple gambling hypothesis. A set of balls which have been drawn, 83 white and 4 black, places ourselves in the same position with regard to our disposition to expect black or white for the future, as that in which we should stand if we had observed 83 successful and 43 unsuccessful speculations in a matter of business: it matters nothing as to the amount of chances for the future, whether the observed even be called the drawing of a white ball, or the acquirement of a profit. Nevertheless, the abstraction of the idea of probability from the circumstances under which it is presented, sometimes throws a difficulty in the way.

The science of probability has also this in common with others, that the problems which most naturally present themselves are of an inverse character, as compared with those which an elementary and deductive course first enables the student to solve. If we know that out of 1000 infants born, 900 live a year, it is sufficiently easy to understand why we say it is nine to one any specified individual of them will live a year. But seeing that we can only arrive at such knowledge by observation, and also that such observation must be limited, there arises this very obvious preliminary question—Having registered a certain thousand infants, and found that, *of that thousand*, nine hundred were alive at the end of a year, what presumption would arise from thence that something like the same proportion would obtain if a second thousand were registered? For instance, would it be wise to lay an even bet that the the results of the second trial would exhibit something between 850 and 950, in place of 900? Or, to generalize the form of the question, let us imagine a thousand balls to have been drawn from a lottery containing an infinite number; of which it is found that there are 721 white, 116 red, and 163 black. We may then ask, what degree of presumption ought to be considered as established—1. That the contents of the lottery all white, red, and black, and not other colour? 2. That the white and red balls are distributed throughout the whole mass, nearly in the proportion of 721 white to 116 red? This is a question which must present itself previously to the definition of any inference upon the probable results of future drawings: but at the same time, it is not of the most and direct easy class, requiring, in fact, the previous discussion of many methods which are subsequent in order of application.

It is common to assume that any considerable number of observations will give a result nearly coinciding with the average of the whole. The constructors of the Northampton and Carlisle tables (see the last number, p.344) did not think it necessary to ask whether 2,400 and 861 cases of mortality would themselves furnish a near approximation to the law which actually prevails in England. It had long been admitted, or supposed, that a considerable number of deaths (no definite number being specified) would present a table of mortality. such as might be depended on for pecuniary transactions. It is true that such is the case; but the proposition is one requiring that sort of examination and demonstration which Laplace has given. We shall not stop to rebut any conclusion which might be drawn against the utility of the theory, from the circumstances of common sense having felt for and attained some of its most elaborate results: but we *shall* stop to remark, that in the case of a speculation, so very delicate, so very liable to be misunderstood, and, above all, accessible to so small a part of the educated world, it is a great advantage that there exist such landmarks, as propositions which, though distant results of theory, yet coincide with notions of the world at large, and are supposed to have evidence of their own.

When we have learnt that the result of analysis agrees with general opinion, in admitting the safety of relying upon a comparatively small number of cases to determine a general average, we then become disposed to rely on the same analysis for correctly determining the probable limits of accidental fluctuation.

The two-fold object of the theory is, then, firstly, to determine the mean, or average state of things; secondly, to ascertain what degree of fluctuation may be reasonably expected. Let it be remarked, that the common theory of chances applies itself almost entirely to the first-mentioned problem; when we say that we determined the probability of an event to be two-sevenths, we mean, that, taking every possible case in which the said event can happen, we shall find it happen twice out of seven times. Such is the general average; but, supposing that we select 700 possible cases out of the whole, it does not therefore become probable, or more likely than not, that the event shall happen precisely 200 times, and fail precisely 500 times. All that becomes very likely is, that the number of arrivals shall be nearly 200, and of non-arrivals nearly 500; and if is one of the most important objects of the theory, to ascertain within what limits there is a given amount of probability that the departure from the general average shall be contained.

The question thus enunciated is of no small practical importance, and to the neglect of it we must attribute the supposed necessity for the large capitals with which many undertakings are commenced. (See last Number, p.342.) Let us imagine an insurance office to be founded, and, for the sake of simplicity, let it take no life except at the age of 30. Let the materials for its management consist in the examination of a register of 1,000 lives, which have been found to drop in the manner pointed out, say by the Carlisle table. The premium which should be demanded is then easily ascertained; but its security depends upon two circumstances—1. That the 1,000 lives so recorded, shall represent the general mortality. 2. That the amount of business obtained by the office, shall be so large as to render their actual experience another representation of the same general average. Neither of these conditions can be precisely attained; some small allowance must be made for both; and the question is, what amount of additional premium is necessary to cover the risk of fluctuation?—what number of insured lives will be sufficient to begin with?—or, supposing that all risks are to be taken, what is the smallest capital upon which a commencement can prudently be made, without any security for a large amount of business?

Perhaps we could not in fewer words convey an idea of the different states of the science in the times of De Moivre and Laplace, than by stating, that the former could have ascertained the requisite premium, and the latter could have made the necessary additions for the fluctuation, &c.

We now pass from matters of business,—as to which we can only say what might be done,—to questions connected with the sciences of observation and inquiry which involves the actual use of our physical senses, the repetition of a process will always afford a series of discordances, varying in amount with the method used, the skill of the observed, and the nature of the observation. If the observed discordances present anything like uniformity of character, we are naturally led to conclude, that they are not, properly speaking, the results of errors of observation, but of some unknown law, by which the predicted or expected result than might have been expected, and sometimes smaller. Now, having noticed a set of observations which do not agree, it is one of the first objects of the theory to settle what presumption should exist that the variations are accidental (that is, totally unregulated by apparent or discoverable law), or that they follow a law which then becomes the object on investigation. The case taken by Laplace, as an illustration, will do for the same purpose here. It was suspected that, independently of local fluctuations, the barometer was always a little higher in the morning than in the afternoon. To settle this point, four hundred days were chosen, in which the barometer was remarkably steady, not varying four millimetres in any one day. This was done to avoid the large fluctuations, which would have rendered the changes in question, if such there were, imperceptible. It was found that, the sum of the heights at four in the afternoon, by four hundred millimetres, or, one day with another, by a millimetre a day. But what can we infer from such a circumstance, is the first suggestion? A millimetre, or about one twenty-fifth part of an inch, is so very small a variation, that considering the nature of observation, and the imperfections of the instrument, that mere instrumental error might have occasioned such a discrepancy. The theory of probabilities gives an entirely different notion: it appears that it is many millions to one against such a phenomenon presenting itself, upon the supposition that it was produced by nothing but the casual imperfections of the instrument. A very great probability was therefore given to the supposition, that there really exists a diurnal variation of the barometer, in virtue of which, *ceteris paribus*, it is a little higher at one particular part of the day than at another.

In this way, Laplace actually used the theory of probabilities as a method of discovery. He expressly affirms (p.355), that the irregularity of the lunar motion, which he afterwards showed to depend on the figure of the earth, was pointed out to him as not being of a merely casual character, by having "soumis son existence au calcul des probabilités." Of another of his most brilliant results, he says as distinctly (p.356), "L'Analyse des probabilités m'a conduit pareillement a la cause des grands irregularités de Jupiter et de Saturne." There is much in these assertions which will appear not a little singular, even to those versed in the subject. But there are two circumstances which afford presumption, not only of the good faith of Laplace, but of his freedom from a mistaken bias for a favourite subject. In the first place, it somewhat lowers the opinion which the world at large entertains of a philosopher, when he is found using means, instead of penetrating mysteries by pure thought. The Newton of the world at large sat down under a tree, saw an apple fall, and after an immense reveries, the length of which is not stated, got up, with the theory of gravitation will planned, if not fit to print. It is painful to be obliged to add, that the Newton of Trinity College Cambridge, of whom there is no manner of doubt that he was the hero of the preceding myth, not only was to a large extent indebted to the perusal of what his predecessors had written, but went through years of deduction and comparison,—abandoned his theory, on account of its non-agreement with some existing observations,—took it up again upon trial when new sets of observations had been made,—and, in point of fact, went through a detail which was a great deal more like a book-keeping operation, than the poetical process of the fable. Partial as Laplace might be wedded to it, as to wish it should appear that he had used a method, instead of unassisted sagacity. The fault of discoverers generally lies in the opposite extreme; they conceal the simple suggestions which led them on the road, and by presenting a finished and elaborate results, as well as establish them. One of the most difficult and original inquiries in which he engaged, was the question of *tides in the atmosphere*, answering to those in the ocean, and produced by the same causes. That such tides must exist, to some degree or other, cannot be questioned by any one who admits the theory of gravitation: the point was to ascertain whether corresponding appearances could be detected to any *sensible* extent. Laplace investigated the deduction of the law in a brilliant manner,—and carefully examined barometrical observations, which of course exhibited a mixed amount of error and actually prevailing law. But upon submitting the result to the test of the theory of probabilities, there was not found to be strong presumption that any part of the diurnal variation arose from such a law as was shewn by theory to be a consequence of the luni-solar action: and the theory, beautiful as it is, was honestly abandoned. We assume then, that Laplace did not deceive himself, when he attributed a part of this success in the explanation of the phenomena, to his use of the theory of probabilities; and we pass to another division of the subject.

All observations are liable to error; if we were to take, for instance, all the altitudes which had ever been measured by a given theodolite and a given observer,—and if we could ascertain what the correct truth was in each instance, we should find many observations wrong by half-a-minute or less; but much fewer in number wrong by something more than half a minute. The *law of facility* of error, is a term we use to express the chance of an error under a given amount; to speak mathematically, let ϕx express the chance, that the error of a single observation is not so great as x is called the law of facility. Nothing can be more obvious than that the law of facility may vary with the phenomenon to be observed, the general character of the observer, his state of mind for the time, &c. &c.

At the same time, there is one conclusion on which all the scientific world was agreed, on every subject, for every instrument, &c.; namely, that when a number of observations disagreed with each other, the way of determining their most probable result, was to take the *average* of all the observations. But it must be obviously proper to ask, can this method be true, whatever might have been the qualities of the observer, the instrument, &c.? Is it likely that the same rule for deducing the probable truth would apply to the bungler and the practised observer, the near and the far-sighted,—to Hipparchus without a telescope, missing whole degrees and Bradley, with his zenith sector, missing seconds? There never was perhaps a case, in which the applications of strict investigation was more likely to play havoc with the prevailing opinion of preceding ages. Such was not, however, the case; and we have here a striking instance of the manner in which existing notions have been confirmed by the march of science.

The theory of probabilities draws a remarkable distinction between observations which have been made, and those which are to be made. Suppose it required of an experimenter, that he should choose his method of treating results previously to obtaining them, and then, whatever his tendency to err may be, provided only that he is not more likely to measure too much than too little,—or in technical language, that positive and negative errors are equally likely,—the method of averaging is the best which he can take. But let him be allowed to defer his choice of a process until the observations are finished, and the process of averaging is not then the best which can be chosen, unless it can be shown that one particular law of facility, pointed out by the theory, is the one to which he is really subject. Some little account of the reason of this paradox may be easily given. The probability of any event is not a quality of the event itself, but an impression of the mind, depending upon our state of knowledge with regard to the causes of the event. If A feel certain that an urn contains nothing but white balls, and B that half its contents are black, the two are really in different circumstances, and the probability of a drawing being white is not the same to both. Now *before* the observations are made, there is no presumption to guide the observer in suspecting any law of facility; but afterwards, the observations themselves furnish an imperfect knowledge of the law of facility. For instance, this much at least will be seen, that if the results of observation be near to each other, the tendency to error is small, and if they differ very much, the same tendency is considerable. Now since it is always competent to the observer to choose his method of proceeding when he pleases; it follows, that the common notion cannot be strictly applicable to the results of any case.

But at the same time it appeared, singularly enough, that whatever the law of facility may be, the more numerous the observations, the more nearly does their average present the most probable result. And more than this, the approximation implied in the preceding sentence takes place so rapidly, that a moderate number of observations is sufficient to allow of its application. There is another consideration, which cannot be explained to any but the mathematician; namely, that the law of facility, under which the average is strictly the most probable result, contains an arbitrary constant, by means of which a particular case of it may be made a sufficient approximation to any law of facility which can be believed to exist. Practically then, the method of averaging, as universally used, has that tendency to promote correctness, as compared with other methods, which it has always been thought to have.

As it is rather our object to shew the bearings of the science on the notions of mankind, than to make a digest of results, we shall here take notice of another theorem, in which propositions, generally admitted, but apparently wholly unconnected, are shewn to be dependent, so that one of them cannot be true without the other. It has not been noticed by Laplace, but has been deduced by ourselves form the principles employed by him and others.

Firstly,—the value of any sum of money is always considered as dependent upon the whole of which it forms a part. A guinea is *nothing* to a rich man, but *a great deal* to a poor man and, on the same principle, no trader contemplates the gain or loss of a given sum, otherwise than with reference to the whole capital which is invested to produce it. Among the various ways in which a part may be compared with the hole, the simple proportion, per centage, or whatever it may be called, is that which is universally adopted; we shall say, then, that the value of any piece of money is to be measured by its proportion to the whole sum of which it is considered to be a part.

Secondly,—the effect of life assurance is considered, in a point of view imported by its name: it is not called the insurance of a certain sum of money *at death*, but the insurance *of life*. It is then taken as placing every person who avails himself of it, in the position of being sure to live a certain time. But, if we consider that those who live long must pay more than they receive, in order that those who die before their time may receive more than they pay, it is clear, that life insurance amounts to an equalization of life, or the assigning to each person the average share of life. Thus the effect of guaranteeing sums of money at death, for premiums properly calculated, is equivalent to insuring the average term of life.

These two propositions, both, to all appearances, highly reasonable to themselves, are not visibly connected with each other: either might be true, it should seem, without the other. But this is not the fact; for it can be shown, that if either of them be false, the other falls with it. If, for instance, a person should affirm, that a guinea to a man who is insured for a hundred, is to be considered as precisely the same thing as the same sum is to another person insured for a thousand, then it can be proved that he contradicts himself, if he imagines that the effect of life insurance is equivalent to the equalization of life in all persons who begin at the same age. There is great analogy between the dependence just explained, and that which prevails between the method of averaging, and the existence of one particular law of facility; and many common notions, examined by the test of the theory of probability, will either confute or confirm each other.

The crowning proposition in the application of the theory to natural philosophy, is undoubtedly that known as the *method of least squares*, to which astronomy, in particular, lies under very great obligations. In fact, we may safely say, that the time must have arrived, when, but for this aid, additional observation would have ceased to carry additional accuracy into our knowledge of the celestial motions. It will somewhat diminish the effect of the technical term "method of least squares," if we state, that the method of averaging is a particular case of it, so that a farmer, who calculates his probable crop by taking an average bushel from several soils, proceeds by the method of least squares, as much as an astronomer, who uses it to determine the elements of a comet's orbit. We remember having heard the following problem proposed, which is an ingenious illustration of the cases to which the method applies. A large target is erected, with a small chalk mark, (not necessarily in the middle) and a number of persons, all of whom are tolerably certain of hitting the target, and all of whom are equally likely to miss the chalk in any direction from it, fire in succession, say with sharp-pointed arrows. The chalk is then rubbed out, and the target, with all the arrows sticking in it, is presented to a mathematician, who is required to say what point, judging from the position of the arrows, is the one which was fired at. His investigation will lead them to the following result; he must ascertain that point in the target, from which, if lines were drawn to all the points of the arrows, the sum of the squares of those lines would be the least possible. From the answer to such questions always requiring the sum of certain squares to be made the least possible, the method derives its name. It is not of course asserted, that the process described would infallibly discover the place where the chalk mark existed; but if the same person were to try the method upon a hundred such targets, losing at the rate of a given sum for every inch by which he was wrong, he would certainly lose less by acting in the manner described than by any other process.

Singularly enough, it was not as a result of the theory of probabilities, but as a convenient and easily practicable process, that the method of least squares first appeared. Legendre and Gauss, independently of each other (though the former first published it) saw the utility of such an addition to astronomical computation. It is to Laplace that we owe its introduction as the best theoretical mode of ascertaining the *most probable* result of discordant observations. His investigation wants clearness and elegance; but is in other respects one of his most brilliant labours. The beauty, generality, and simplicity of the result secured for it an immediate admission into every process, though the demonstration is of a kind which there are not many to understand; the process is one which has the air of being highly probable, and seems in itself to be free from objections which might be proposed against any other method. But at the same time it appears to us, that many have used it without a thorough comprehension of its meaning; and just as we now say that astronomy must have stopped its career of increasing accuracy, if the method of least squares had not been introduced, so we will venture to hope that the time must come when the same remark shall be made upon an improved and extended way of using it.

The difficulty of admitting several points connected with the theory of probabilities arises from the neglect to make an important distinction; namely, between the correctness or incorrectness of the hypothesis assumed, and that of the inferences which are drawn from it. Let it be proposed to apply mathematical reasoning to the valuation of the credibility of evidence, and the answer appears to be simple—namely, that such a proposition must be the result of an overheated imagination. That would be a fair answer if it were required to apply calculation to the character and actions of a given man, with a view of ascertaining whether he was likely or not to tell the truth in a particular case. Mathematics will not tell us whether A and B are credible witnesses, not whether, supposing them credible, their evidence will be as much as should in prudence be considered sufficient for the establishment of any particular point. Nor will mathematics enable us to measure a length in feet, or to reason upon it, unless we first know by other than mathematical means, what is that length which it is agreed to call a foot. But let a foot be known, and we can then assign lines, areas, and solids, by means of numbers; and, in like manner, let the credibility of one witness be given, and we can then determine that which results from the evidence of any number, contradicted by any other number. By the credibility of a witness, we are supposed to mean the probability that an assertion advanced by him will be correct, the moment before the assertion is made.

For instance, suppose it admitted that a jury of twelve men, all equally likely to be correct in any particular verdict, decide wrongly once out of fifty times. It is a matter of pure algebra to find out how often each of them, using his own unassisted judgement, would come to erroneous decisions. It is also the province of algebra to determine how often a jury would err, if, upon the preceding hypothesis as to the correctness of twelve men, the number were reduced or increased. Laplace, and others before him, have made extensive applications of analysis to such questions; but their labours in this respect have been misunderstood, and always must be, until the province of mathematical reasoning is better understood by the world at large.

We have now, we believe, briefly touched upon the principle subjects which are to be found in the Théorie des Probabilités. The subject is one which must make its way slowly, having to extricate itself from its old connexion with games of chance, before it can take its proper place as an agent in statistical and political enquiry. One of our principal objects in writing the present articles has been to show that the nature of probability may be treated, and its results applied, without mention of dice or cards. Laplace himself has introduced a few problems connected with common gambling, in some instances on account of their historical notoriety, in others because they afforded easy and striking examples of the applications of generating functions, the theory of which was introduced in his work. But the greater part of the treatise is full of such questions as those which have been alluded to in the preceding pages, bearing in the most direct manner on the way to draw correct inferences from physical and statistical facts.

If we can make a few reflecting individuals understand, that, be the theory of probabilities true or false, valuable or useless, its merits must be settled by reference to something more than the consideration of a few games at cards, we shall have done all which we ventured to propose to ourselves.

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