

Sur un principe que M. Poisson avait cru découvrir et qu'il avait appelé Loi des grands nombres.*

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It is around twenty years ago, Mr. Poisson believed to have demonstrated a new law destined to regulate all the statistical observations and all the results of same specie in all the parts of the sciences, either physics, or morals and politics. He called it *Law of large Numbers*, and he made it known to the Academy of Sciences.¹

Since the first communications of the illustrious geometer, he made me easy to recognize that he had arrived not at all to the end that he seemed to be proposed: and after the publication made in 1837 of his *Recherches sur la probabilité des jugements*, I indicated how he had had to undergo one of those illusions to which Laplace has not disdained to dedicate a chapter of his *Essai sur le calcul des probabilités*. But the state of the health of Mr. Poisson did not permit to give to my remarks perhaps the necessary publicity. Science had soon after the misfortune of the loss prematurely; the negation of the Law of Large Numbers has remained hidden in the unprinted part of the procès-verbaux² of the Philomatic Society to which I belong; and today my critique is scarcely known but by some geometers.

Many times one has reproached me for not having reproduced it, and one has seemed to conclude that perhaps I judged no longer in the same manner a work that I have left so long ago in obscurity, while many works or memoirs published in this interval are supported on the Law of Mr. Poisson.

I come, in consequence, to demand by this Academy the permission to repeat before it that the *Law of Large Numbers* does not exist, and to enter into this subject in some indispensable explications. One will see thence that I feel no doubt on the exactitude of my denial; and at the same time it will appear more surely to the persons who are occupied in some observations and some experiences so multiplied which do not return precisely into the domain of the physical or mathematical sciences.

In order to be well understood, it is good to repeat that the word *cause*, when the concern is probabilities, has received a special sense. The authors, by employing it, do

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¹See the Comptes-Rendus de l'Académie des Sciences, session of 14 December 1835, p. 478, and 11 April 1836, p. 377, etc.

²Procès-verbal of the session of 16 April 1842.

not hear of that which produces an effect or an event, of that which assures the arrival of it; they wish only to speak of the state of things, of the set of circumstances during which this event has a determined probability. Thus, for example, if the mathematical probability of the birth of a boy, in a certain country, remained numerically the same, one said that the cause or the causes of the birth of a boy are constants. This which would prevent not at all that a pregnancy taken at random brought forth a girl, since the constant probability of the birth of a boy permits the contrary probability to persist, then constant also, of the birth of a girl.

This word thus defined, one sees without difficulty that which signifies the theorem of Jacques Bernoulli, out of the events subject to some constant probabilities. Here is the statement of it: when the causes are constants, and when hence the probability of an event or of any fact remains the same for each trial or each observation, the number of repetitions of this event out of a great number of trials, is very nearly proportional to the fraction which expresses its probability; in other terms, the ratio of the numbers of repetitions of an event and of the contrary event, deviates little from the ratio of their respective probabilities.

That does not wish to say that this small difference of the ratios is necessarily realized. It is only very probable. If the number of the observations or trials is excessively great, it becomes excessively probable. But it will never be certain. It is always thus in each calculation of probabilities; and it is thence often that which renders so difficult to follow a long reasoning on some probable things, and to escape from each illusion.

From the theorem of Bernoulli there remains a quite remarkable consequence, it is that the mean results or constant causes could offer most often only very small deviations. So that the part of chance is very small: it is necessary that the cause changes in order that the mean values of the observed facts in great number come to undergo great variations.

For example: out of one million trials subject to the same mathematical probability, the deviation between the observed mean, and the calculated mean according to this probability, will not exceed one thousand probably. But out of 10,000 trials a deviation of 100 would have the same probability, at least very nearly.

The smallness of the extent of these deviations remains the same, whatever be the nature of the observed facts; and it remains further the same if one represents the constant probability no longer as absolutely fixed, but as being the constant mean value of a certain number of probabilities which result from variable causes, of which each is able to be presented at each trial indifferently, according to one law of possibility assigned in advance.

One imagines easily that the two cases return absolutely to the same. There is no need to demonstrate it; and without doubt those who have taken the pain have seen only an exercise of calculation. The identity of a constant probability and of the mean probability of a certain number of probabilities which are able always to regulate any trial, had appeared to these last times with a complete evidence. It is even thus that Jacques Bernoulli has understood his unique probability. One is able to be assured by reading that which he has said of it in the preamble to his theorem.³ He gives as example the probability resulting from multiple causes of mortal maladies; and which

³*Ars conject.* pars 4, p. 224 and 226.

more is the constant probability deduced from meteorological causes: *Innumerabili complexionum verietate industriam nostram aeternum lusuris.*

One knows that the posthumous work of Bernoulli is suddenly interrupted after the demonstration of his theorem. So that there subsists no trace of the real applications of which he promised the execution, and that it is impossible to know how he would have directed and understood them. But when one makes some scientific researches truly serious, when one is not limited to small numbers of experiences or of observations, and when one has to compare the facts of many years, it is difficult that one not perceive that the deviations assigned by the theorem of Bernoulli, are further from equaling the considerable differences which are encountered between the ratios of the numbers of natural phenomena gathered with the most exactitude. There is especially a fact which must strike an attentive mind: it is that the number of observations necessary in order to obtain a mean offering a certain degree of precision, is not the same, by a great deal, in all the kinds of researches. In certain researches some relatively small numbers suffice in order that the mean results differ little from one another. For the others, this specie of constancy of the mean results requires some numbers of observations much greater.

Thus the counts of the criminal justice attest that the mean ratio of the condemnations to the acquittals, have little variation from one year to the other (under the same legislation), although the total number of accusations does not exceed 7 or 8,000.

On the contrary, in order that the ratio of the births of boys to the births of girls, takes the same degree of steadfastness, there is necessary numbers 10 or even 100 times more considerable.

It would be permitted, to a certain point, to attribute these contrasts, to this that the constants vary much more for one class of facts than for another. And it is this which results from the simple application of the theorem of Bernoulli. But this explication is not at all satisfying, because by studying the best possible the circumstances of the things, one does not see clearly that they had had really much to change from one collection of facts to another. Often even, one has place to record some considerable deviations then that one is nearly persuaded to the constancy of the set of causes which has regulated the diverse series of researches.

This discordance had not escaped Mr. Poisson, and he had sensed the necessity of a new explication of the manner by which the probabilities are combined in order that the mean results finish by becoming constants, by requiring immediately numbers of observations very different.

It is thence at least that which one is able to conclude from the diverse passages where he has spoken of his law of large numbers.⁴

If therefore he had published another calculation, another formula, although his ideas do not appear to have been arrested very cleanly, and if he seems to have changed sometimes in point of view, one must presume that he had given the name of *Law of large numbers* to some new theorem. One would be able without doubt to think that this name offers a greater sense than the possible sense of a new theorem: for it is quite clear that there are some very great numbers which do not offer constant results, while the words *Law of large numbers* must express some rule applicable to all numbers, since

⁴See the passages of the Comptes-Rendus of the Académie des Sciences, cited above, p. 379, and the *Recherches sur la probabilité des jugements*, introd. and ch. 2 and 4.

they would have the character to be *great*: save to be understood of their magnitude.

But the calculations of Mr. Poisson permit no doubt. After having read his *Recherches sur la probabilité*, if one has followed the analysis that he developed and the formulas that result from it, one acquires the certitude that he has simply demonstrated the theorem of Jacques Bernoulli, under the hypothesis where the constant probability is the mean value of a set of variable probabilities which are able to be offered each to all trials: a hypothesis so evident that it is not necessary to demonstrate it.

The formulas of Mr. Poisson assign indeed the same very small deviations which are deduced from the rule of Bernoulli. One does not find that which he had announced in the introduction to his work, and previously, namely: of the different deviations for the numbers of equal magnitude, when the phenomena differ; nothing marks no more the necessity of numbers of observations greater in certain cases than in others, in order to arrive to some mean results very nearly fixed. If Mr. Poisson had examined well the extent so restricted of the deviations of the formula of Jacques Bernoulli or of his own, he would have without doubt abandoned each idea of a real discovery. In this regard, he is remained under a singular illusion. For he has well been obliged to recognize⁵ that the mean probability played in his formula the role of the unique probability in that of Bernoulli, in a manner that the forms were veritably identical. But at the same moment where he admits this identity of form, he sustains that there is a difference at base. And the explication that he believes to give of it is not exact, since it reposes only on the diversities of the possible succession of the causes, and since he has carefully pointed out himself that this succession is at each trial equally possible under all senses.

All his analysis, likewise as his results, establishes that it is at base as of the form: and, according to his calculations, nothing necessary, in order to arrive to some constant means, with greater numbers than the formula of Bernoulli requires.

One is therefore right to affirm that the *Law of large numbers* has no real existence; and that it is no longer necessary to endorse these words in the sense that Mr. Poisson had given to them.

Good sense sufficed besides to give birth to some serious doubts on the claimed discovery. How was he able to make that the magnitude of the numbers dispensed from the constancy of the causes? One does not comprehend the existence of constant results, without carrying up again to the existence of some constant reunion of circumstances which produce them. One has besides some rather frequent examples of very great numbers of which the relations not cease to vary. Finally, Mr. Poisson himself admitted that the causes should not vary in a progressive manner. But by the fact, as he took only the mean, he made them not vary at all.

I have suppressed here all calculation on this subject: I will say only that all the algebra in the world adds only very little clarity.

I will cite a particular result to which I had arrived a long time ago, although I have published it only in 1839.⁶ By seeking to explicate how from a constant set of fixed causes, there is able to result some considerable deviations in the mean results, I have found the theorem that is here: when some causes forming an invariable set are able

⁵See page 146, *des Recherches sur les Jugements*.

⁶See the procès-verbaux of the Societé Philomatique, of 4 May 1839, and the journal *l'Institut*, of 6 June 1839.

to be presented indifferently, but when the probability due to that which comes to be offered, regulates a very small number of successive trials, three or four, for example, the extent of the probable deviations above or below the mean probability is able to become double of that which it was in the formula of Bernoulli. So that then a number of observations four times greater is then necessary, in order to arrive to some means which enjoy the same degree of probable stability. One imagines that if the variable probability from one cause to another, endured during more than three or four trials, the extent of the deviations would be able to be very great. This would be then a true variation of the causes.

Thus the formula which expresses this special law, be it able to serve to explicate the irregular march of certain phenomena. Such is, for example, the mean quantity of rain which falls each year. As the causes which bring forth the rain endure during many consecutive years, as besides the number of the days of rain attain scarcely that of 100 to 200 per year in our climates, a very long time is necessary in order to obtain a mean which had a certain character of stability. Until the present the observations comprehend scarcely more than a century; it has been possible to form only two or three means of around 30 or 40 years each; and the knowledge of the laws that the quantities of rain follow in the diverse seasons, is very little advanced.

There are some years, for example, one placed Poitiers and its environs under the regime of the rains of summer, after ten years of observations that a physician, Mr. de La Mazière, had sent to Père Cotte, who gathered all the statistical notions which he was able to procure. But he agrees, to the contrary, until more amply informed at least, to rank Poitiers in the zone of rains of Autumn. This is a consequence which results from the extract that I have made ten years ago, from a manuscript of the same physician, Mr. de La Mazière. This manuscript, which has been communicated to me by the librarian of the city of Poitiers, contained 40 years of painstaking observations, of which 30 posterior to those of Mr. de La Mazière were sent to Père Cotte.⁷ Thus 10 years would have furnished a mean opposed to the true mean of the quantity of water fallen in the different seasons. Of them 40 is necessary in order to make evident this true mean. And it would be necessary 40 or 80 others in order to confirm it.

Despite the fortunate application which has been able to be made in this case, of the theorem of which I just spoke, I considered it not as always capable of explicating the great deviations which some daily statistical facts present. I think that there is no point at all to seek here a general law, as it was a veritable illusion as to propose such a goal. There is manifestly variation in the causes when the results vary: I believe therefore that it will be necessary to study each result in order to guess in some way by what variable combination of constant causes, one will be able to explicate them. The infinite variety of the arrangements employed by nature in order to produce very simply some very complex events, makes presume that one would discover some particular theorems which would regulate diverse classes of facts. But it does not seem that a

⁷This consequence is much more important than it appeared at the first arrival. The restocking of the woods after the cutting, if easy in Germany and in certain parts of France, appeared to me to result in this that these countries have the *maximum* of the rains in summer. In a very great part of France, the *maximum* is in the autumn. There results from it that very often young recruits are grilled by the sun, and perish for lack of water. There is therefore some special precautions to take then from the cutting of woods, in order to avoid by this natural accident which increases the chances of depopulation.

formula analogous to that of Jacques Bernoulli is able to embrace all the circumstances possible.

I limit myself to this indication in order to not be deviated further from the sole point that I wished to record here, the non-existence of that which had been called: *Law of Large Numbers*. I will say in ending that, if some able statisticians have demonstrated on this subject a just defiance and are not mistaken, it is no less regrettable that this claimed theorem has received a name, and especially a name so proper to satisfy the ear. When some idea is named it seems that it exists, to such point that in Germany one has sustained that the denial was some thing, uniquely because it is quite necessary to designate it by a word. I hope that the word which just occupied me will not be conserved in the scientific applications, and that one will hold to the theorem of Jacques Bernoulli, under the form so remarkable and so simple that Moivre had given first, and which as been understood so happily by Laplace.