STATISTICA, anno LXXV, n. 2, 2015

REREADING BERNOULLI

Corrado Gini (1946)

Lecture presented at the Statistical Seminar. of the Statistical Institute of Rome University (June 25, 1946). Published in "Metron", XV, 1-4, 1949.

It is my intention to refer to Jacob Bernoulli (1654-1705), the first and most famous of distinguished mathematicians who brought fame to the family[1]. His most important work - of which many speak, but very few have read - is *Ars Conjectandi*, published in 1713 posthumously by his nephew Nicholas[2]; the reading of this work must, however, be integrated with that of the letters exchanged with Leibniz in the last years of the author's life[3].

Reading classics is useful, not only in order to learn, but because it takes us back to the origins of the doctrine, it allows us to explain founded or unfounded positions assumed by authors who followed them, sometimes better than the same authors can explain. Such is the case for a reading of Jacob Bernoulli's work.

Ars Conjectandi consists of 4 parts.

The first part reproduces the treatise by Cristian Huygens *De Ratiociniis in Ludo Aleae*, followed by a note by Bernoulli. Huygens's treatise gives the solutions to 14 problems related to gambling games; for some of them, Bernoulli gives new proofs while broadening other problems and proposing and solving new ones.

The second part deals with the doctrine of permutations and combinations, already tackled by famous mathematicians, among whom the Author mentions Schooten, Leibniz, Wallis and Prestet, and to which he makes a substantial contribution.

---

[1] After that of Jacob, here are the names of the most important members of the family: Johann (1667-1748), Jacob's brother; Nicholas (1687-1759), son of one of Jacob's brothers, whose work he published posthumously; Johann's sons: Nicholas (1695-1726), Daniel (1700-1782) and Johann (1710-1790), and finally the sons of the latter: Johann (1744-1807) and Jacob (1758-1789).

[2] This is the full title: Jacobi Bernoulli, Profess. Basil. et utriusque Societ. Reg. Scientiar. Gall. et Pruss. Sodal. Mathematici Celeberrimi, *Ars Conjectandi*, Opus Posthumum, Accedit *Tractatus de Seriebus Infinitis*, et Epistola Gallice Scripta *De Ludo Pilae , Reticularis*, Basileae, Impensis Thurnisiorum, Fratrum. MDCCXIII.

[3] *Leibnizens Mathematische Schriften*, herausgegeben von G. I. Gerhardt, Erste, Abtheilung. Band III. Briefwechsel zwischen Leibniz, Jacob Bernoulli, Johann Bernoulli und Nicolaus Bernoulli, in *Leibnizens Gesammelte Werke aus den Handschriften der Konighlichen Bibliothek zu Hannover* herausgegeben von Georg Heinrich Pertz, 3, Folge: Mathematik, Bd. 3, Halle, Druck und Verlag H. W. Schmidt. 1855.

In the third part, Bernoulli takes into consideration 24 problems, all related to gambling games, aimed at explaining the application of the theory of permutations and combinations illustrated in the previous part.

Finally, as said in the title, the fourth part was supposed to deal with the use and application of the previous doctrine in civil, moral and economics matters. However incomplete it seems to be, as we will say later on, it is obviously the most important part of the publication, the one that laid down the foundations for all the successive developments of probability calculus, as well as its application to statistics.

It is divided into five chapters, whose titles clearly explain the context:

I) *Praeliminaria quaedam de Certitudine, Probabilitate, Necessitate, et Contingentia Rerum.*

II) *De Scientia et Conjectura. De Arte Conjectandi. De Argumentis Conjecturarum. Axiomata qaedam generalia huc pertinentia.*

III) *De variis argumentorum generibus, et quomodo eorum pondera aestimentur ad supputandas rerum probabilitates.*

IV) *De duplici Modo investigandi numeros casuum. Quid sentiendum de illo, qui instituitur per experimenta. Problema singulare eam in rem propositum, etc.*

V) *Solutio Problematis praecedentis.*

Let us look at the basis outlines.

Probability is conceived as a grade or fraction of certainty and the art of measuring it is called *Ars Conjectandi sive Stochastice.*

The probability of a phenomenon is derived from the number and weight (that is the evidential force) of matters demonstrating its existence. In turn, the weight of matters, hence the probability deriving from it, is derived from the ratio of only favorable cases to favorable and contrary cases, all of them being considered as equally possible, because, if they are not so, they can be made such by counting a case that has a greater opportunity to occur a number of times proportional to such opportunity. In other words, as Bernoulli clearly says, one proceeds in the same way as the gambling games considered in the previous part of the treatise.

However, practically speaking, except for such games set up so that the number of favorable and contrary cases can easily be determined, such a procedure is hardly ever followed. Anyway, it is sometimes possible to obtain *a posteriori* what was not possible to obtain *a priori*, that is from what, in similar cases, occurred in multiple observations. Such an empirical procedure is not new and it is also evident to anyone that it requires many observations and that the more these are, the less the danger of moving away from the true ratio. What remains to be searched for is whether, as the number of observations increases, the probability of attaining the true ratio continuously increases, differing less and less from certainty or, on the contrary, if there is a limit that cannot he exceeded.

Bernoulli states that there is no such limit: for instance, given an urn containing 3000 white balls and 2000 black balls, whoever does not know this may in any case repeat extractions so that the probability that the true ratio between white and black balls fails within certain limits - e.g., between 301 : 200 and 299 : 200, or between 3001 : 2000 and 2999 : 2000 - instead of outside these limits is greater than any predefined probability.

This – Bernoulli states – is the problem that I have undertaken to make public after having worked on it for 20 years, a problem whose novelty and great usefulness, along with its difficulty, are enough to add weight and value to the previous chapters of the Treatise.

The fifth and last chapter of the Treatise is entitled *Solutio Problematis praecedentis*. In it, after some lemmas, a proof is given of what is known historically as the "Bernoulli's theorem". Has Bernoully demonstrated his assumption by this?

Keynes denies it. On the basis of the correspondence that took place between Leibniz and Bernoulli, he maintains that, in effect, Bernoulli's intention was to demonstrate how probability can be valued *a posteriori*, by means of an inversion of the theorem which bears his name, demonstrated in *Ars Conjectandi*. It would be to this inverse theorem that Bernoulli refers in his letters to Leibniz. However, either because he was affected by Leibniz' s objections, or because he was prevented by death, he did not go any further and the work remained incomplete[4].

It is self-evident that *Ars Conjectandi* – published, as said before, posthumously by his nephew Nicholas – is incomplete. Nicholas himself states it clearly in the introduction. *IVtam Partem qua usum et applicationem praecedentium ad res civiles, morales et oeconomicas ostendere voluit, adversa diu usu valetudine tandemque ipsa morte praeventus imperfectam reliquit.*

But this sentence suggests what was actually still missing: applications in civil, social and economic matters.

Regarding the problem he had formulated, there appears to be no doubt, in the text of *Ars Conjectandi* itself, that Bernoulli thought he had solved it by the theorem to which his name is linked.

Indeed, in chapter four, after having said that he had been working on such a problem for 20 years, he adds that, before giving its solution, he will deal with some objections, which after all are the same objections presented to him by Leibniz and to which we shall later return on. The fifth chapter, including the proof of the theorem that took Bernoulli's name, is in fact entitled as we said before: *Solution of the previous problem.*

On the other hand, this conclusion is proven by an attentive reading of the correspondence between Leibniz and Bernoulli, on which it is worth spending some time.

It is Leibniz who gives Bernoulli the opportunity to speak about his work on probability.

He writes in April 1703, that you are seriously taking into consideration the theory of probability evaluation, which I think a great deal about. I would like someone to mathematically deal with the various games, offering good examples of such a theory. It would be, at the same time, an attractive and useful subject, and one not unworthy of you or of any other very serious mathematician.

---

[4] J. M. Keynes, *A Treatise on Probability*, London, Macmillan, 1921, pp. 368- 369.

Bernoulli showed to be highly enthusiastic. He profusely answered on the third of October of the same year.

For many years I have been dabbling in meditating about the theory of probability evaluation, so that I do not believe that anybody else has done more. In fact, I had in mind to write a treatise on the matter; but I often had to put it aside, as my laziness was enormously affected by illness. In any case, I have laid most of it out, although the important part is not yet complete, the one in which I teach how to apply the theory to civil, moral, and economics matters, after having solved a problem of some difficulty, but a very useful one, which my brother (Johann Bernoulli) has known about for 12 years.

I want to tell you about it. It is known that the probability of any event depends on the number of cases in which it may or may not occur: such a number of cases is known in gambling; in other examples it is not known, as when one has to calculate the probability that, between two people of different ages, the eldest one dies first. What I have asked myself is if that number of cases, eluding us *a priori*, cannot be determined *a posteriori* by many observations, on the basis of what happened in similar examples. So if out of 1000 cases where the eldest person dies first, 500 occur in which, on the contrary, the youngest dies first, we could say that the probability for the eldest to die first is double than that of the youngest.

Even the most stupid person knows – I do not know by which natural instinct and without any previous learning – that the more observations there are, the less danger there is of straying from the path of truth; anyway, to give an accurate mathematical demonstration of it is far from being despicable. Moreover, I have decided to investigate whether as the number of observations increases the probability of moving away from the true ratio asymptotically tends towards a limit inferior to unity – in which case the effort to empirically derive probability from the number of observed cases would be vain – or whether it tends towards certainty instead[5].

So I have found that this second case is the true one. Hence, I am able to determine which is the number of the observations to be made so that it will result to be 100, 1000, 10000 times more likely that the ratio between the number of cases thus determined corresponds to the true probability, being this sufficient for application in civil life so that our forecasts are directed to any contingency no less scientifically than in gambling games.

Nescio Vir Amplissime an speculationibus istis soliditatis aliquid inesse Tibi videtur Bernoulli adds. In which case I would be grateful if you could suggest some judicial topics for which you think they might be usefully applied. Perhaps, there is something I could use in the Pensionario De Witt, that I have seen quoted but do not know. If it is so, I would like a copy of it (pp. 77-78).

On the 3rd of December, Leibniz answers more skeptically than cautiously.

The estimation of probability is very useful, but, for judicial and political topics, an accurate listing of all circumstances is more important than the quibbles of calculation. You are looking for the possibility of empirically obtaining a perfect

[5] According to Bernoulli the inversion is implicitly correct.

evaluation of probability and you believe you have achieved it. Regarding this I see an intrinsic difficulty, because by finite experiments one cannot determine what derives from an infinite number of circumstances. Who can say that the following experiment does not differ in something from the law regulating all the previous ones? One cannot set limits to the nature of things so that it will not vary in the future. In fact, although empirically one cannot obtain a perfect evaluation, nevertheless the empirical evaluation will be practically less useful and sufficient. Regarding Pensionario De Witt, it is a small booklet of an informative nature – hence written in Belgium – dealing with the calculation of life annuities.

Slightly resentful, Bernoulli replies on 24^{th} April 1704.

I have learned from various problems related to insurance, life annuities, dowry agreements and so forth the fact that, in the area of judicial matters, the probability theory does not only need a listing of circumstances but reasoning and calculation as well as I shall explain later on. Your objections against my procedure for empirically determining the ratio between the number of cases actually concern, like this, the procedure of *a priori* determination. But, I have already told you that I am able to supply the proof of my assumption. My brother saw it more than 12 years ago and he did approve it.

I will provide you an example that will give you a better understanding of my thinking: I put 200 white balls and 100 black ones in an urn. You do not know this ratio and want to determine it experimentally. You will carry on doing successive extractions, making a note of the color of the extracted balls that you will then put back into the urn. I say that, assigned two limit ratios, however close, e.g., 201 to 100 and 199 to 100, one can scientifically set the number of extractions so that the unknown ratio falls between such limits with a probability 10, 100, 1000 times greater than the probability of falling outside such limits.

Similarly, one may determine the probability that between two people, the elder dies before the younger. Nor does it help to say that the number of causes of death is infinite, because even between two infinities, a finite ratio can be established. If new causes of death intervene later, new observations will be required.

From what you write to me - Bernoulli goes on - De Witt's booklet contains something that very much helps my purpose. As I have vainly searched for it in Amsterdam, I beg you to lend me your copy at the first opportunity.

In his answer on 28^{th} November of the same year, Leibniz says that he has been unable to find De Witt's booklet; he will search for it, although he does not believe that it contains anything that would be new to Bernoulli.

Concerning the substantial controversy, he notes that, in some *non satis colligatis* phenomena, it is not quite certain that, by increasing the number of observations (as when new years are added to the data on diseases), one gets nearer to the true medium ratio in the universe – even if caution suggests assuming so – while one always gets closer in series like the Ludolph's one.

In his letter dated 28^{th} February 1705, Bernoulli reminds Leibniz again to send him, whenever he happens to find it, De Witt's booklet, whatever its content is, will not fail in bringing him something new (in the following letter of the 22^{nd} March – the last written to Bernoulli by Leibniz – he had still not been able to find it).

Bernoulli does not answer Leibniz's objection - obviously not understanding its importance - while he categorically restates his viewpoint *Quod Verisimilitudines spectat, et earum augmentum pro aucto scil. observationum numero, res omnino se habet ut scripsi, et certus sum Tibi placituram demonstrationem, cum publicavero.*

It is not known (or at least I do not know) what Leibniz's reaction to the proof and, more generally to the whole Bernoulli's work was; but it is however right to suppose, from his silence, that he was not impressed enough.

In a letter dated 9^{th} September 1713, Nicholas - Jacob Bernoulli's nephew - who had edited the posthumous edition (p. 989 of the above quoted letters), and his brother Johann, who was very engaged in corresponding with Leibniz (p. 922), at the same time forewarned him of it being sent. On the following 28^{th} February, Nicholas, writing again, precisely that the work would have been sent, unless differently indicated, at the Frankfurt fair (p. 992). From Vienna, on 31^{st} March, Leibniz answered Johann asking that it should preferably be sent to the book seller Forster or to any other dealer in Hannover or Brunsvig (p. 930). On 23^{rd} May Johann sent Leibniz the receipt given to him by Forster for the parcel containing *Ars Conjectandi* and *Manuaria nautica*, which had just been published (p. 931). The following letter from Leibniz, written from Hannover on 30^{th} December, evidences that he has received the parcel as he is writing (p. 933) that he appreciates the *Manuaria Nautica*, although he has taken only a quick look at it. However, he does not mention *Ars Conjectandi*, nor does he mention it again in subsequent correspondence with Johann, that went on until Leibniz's death. Johann Bernoulli's last letter, written on the 11^{th} November 1716 from Basel, probably found Leibniz already dead; he died in Hannover three days after it had been written.

From all that has been said, it is evident that on Bernoulli's part it was not the case of a proof to be made, but of a well established proof that had been made long ago and had already been submitted to his brother Johann. All that Bernoulli was asking Leibniz was the suggestion of judicial and political topics for applications. Applications which he had not yet carried out, nor would he have had time to do, leaving his work incomplete.

It seems obvious that the theorem submitted to his brother for 12 years and to which Bernoulli refers in his letters to Leibniz was that very same which he had been working on for 20 years and of which, in chapter IV of the Fourth Part of *Ars Conjectandi*, foresaw the following explanation in order to give its solution in chapter V. This is also confirmed by the substantially identical qualifications given by the Author in *Ars Conjectandi.* (*"Problema singulare"*, p. 223 *"cujus tum novitas, tum summa utilitas cum pari coniuncta difficultate"*, p. 227) and in the letter to Leibniz of 3^{rd} October 1730 (*"soluto eum in finem singulari quodam Problemate, quod difficultatis commendationem non parvam, utilitatis longe maximam habet"*, p.77).

Nor does it seem that Bernoulli was at all affected by Leibniz's objections. This can be deduced, not only from the categorical repeated statement and the unshaken faith expressed in the letter of 28^{th} February 1705, with which he finishes the argument, but also from the answer he gives to Leibniz's objections, although his name is not mentioned, at the end of chapter IV of the Fourth Part of *Ars*

Conjectandi, before moving on to formulate the theorem.

He answers three of the objections:

I) *Objiciunt primo aliam esse rationem calculorum, aliam morborum aut mutationum aeris; illorum numerum determinatum esse, horum indeterminatum et vagum;*

II) *Objiciunt secundo, calculorum numerum finitum esse, morborum etc. infinitum;*

III) *Ajunt tertio, numerum morborum non manere constanter eundem, sed quotidie novos pullulare.*

We have already seen Bernoulli's answers to Leibniz in his letters regarding the last two objections: substantially he repeats them in *Ars Conjectandi*.
As for the first objection considered, I do not believe it is guessing if we see in it the last objection made by Leibniz in his letter dated 28^{th} November 1704, an objection to which Bernoulli had not yet given an answer. The answer given by Bernoulli is that, in the example of the balls in the urn *(calculi)*, as well as in the example of the illness and the climate, our knowledge is uncertain and indeterminate.

Such an answer, if I correctly understand Leibniz's objection, is not appropriate. Recalling what he had said in the previous letter concerning the possibility of new causes of death arising, I actually feel that Leibniz's objection must be interpreted in the sense that, when the probability of a phenomenon varies (as it is the case of mortality) it is not certain that the higher the number of observations, the higher the probability that an error remains between two given limits. Such an objection does not actually make sense, as it resulted by the theorem proved by Poisson; but Bernoulli could not have replied since his theorem is based on the hypothesis of a constant probability withrespect to the number of' the observations.

A more serious and basic objection, one that Leibniz has not made and Bernoulli had not thought of, is that calculating the unknown probability from the observed frequency represents a problem of inverse probability, while Bernoulli's theorem solves a problem of direct probability by teaching how to assess the observed frequency from the known *a priori* probability.

Bernoulli's theorem, hence, did not answer the practical problem he had in mind of empirically determining the probability by an approximation as large as one wishes. On this I agree with Keynes; but - and here I do not agree with Keynes - Bernoulli thought it did.

That improper passage from direct probability to inverse probability, sometimes emerging in Laplace's and Poisson's works, is, as I have explained[6], at the

[6] In particular, see the opening speeches of the I and VII scientific meeting of the Italian Statistical Society published in the Proceeding of the Society: *The Dangers of Statistics*, October 1939; *The Tests of Significance*, June 1943, and the summary of these and other papers, *On the logical bases and the gnoseologic importance of the statistical method* published in Italian in "Statistica". 1941-46, in Spanish in "Anales del Instituto

basis of the methods of the significance tests and confidence intervals proposed by the English School, thus finding its roots in Jacob Bernoulli; we could say that it represents the original sin of Probability calculus. Thus, one realizes how difficult it is to eradicate it.

The importance of such a logical error is such that one will never be warned enough against it. Indeed, it is at the basis not only of the previous methods, by which one assumes to measure the reliability of statistical data without resorting to any hypothesis but, of no less importance, it is implicit in reasoning which enforced all contemporary mystical orientation.

The extremely low probability that the phenomena we observe in nature derive from a random combination of natural elements, in fact leads us to consider as extremely improbable in theory, and to exclude in practice, that they occur due to chance; and this leads to the formulation of a supernatural regulating force. It is, for instance, observed that, in a chance combination, there would be a fading probability, less than 10^{-600}, that from the organic elements one of the simplest organic molecule, made of 2000 atoms, is produced. From this it may be concluded that it is practically certain that such a molecule cannot be derived from a chance combination of inorganic elements. So, from the solution of a problem of direct probability, one unduly feels allowed to draw a conclusion which, instead, implies the solution, for which one does not hold the elements, of a problem of inverse probability.

The absurd to which such a procedure may lead will be evident in another example.

The probability of winning with a set of four numbers is 1/511038: the reasoning suggested before leads to conclude that the win with a set of four numbers has only one probability over 511038 to be by chance and 511037 probabilities to be the result of artifice or cheating.

A caustic colleague from Padua used to say that, in Italy, it is not possible to play the lottery, because he who loses or wins a little is left in peace, but he who, unfortunately, makes a big win is thrown into prison, indeed, according to the specious excuse that the probability of the winning set is so small that its happening cannot be reasonably attributed to chance.

There are objections to this argument; nevertheless, I repeat, it is identical, from a logical viewpoint, the argument that, from Bernoulli, is genuinely taken into consideration even by the most serious experts of Probability calculus and stands at the basis of the modern statistical theories of the significance tests and confidence intervals as well as identical, from a logical viewpoint, is the argument by which the modern mystical currents believe to be in the position to strengthen their antimaterialistic reaction. That goes to show how different the critical sensitivity of the human mind is when dealing with theoretical speculations or, instead, matters directly regarding one's own vital interests.

Bernoulli's concept of probability is a subjective one. As I have said above, he conceives it as a fraction of certainty that depends on the number and on the

Actuarios Espanoles" Ano IV, 1946 and, with many addictions, in Portuguese "Revista Brasileira de Estatistica", 1948.

evidence of arguments for or against the existence of the phenomenon. He who rereads Bernoulli after having read Keynes, cannot but avoid feeling that Keynes has directly drawn from Bernoulli in order to build up his probability theory on a subjective basis, an ingenious and brilliant procedure, but one which does not convince me.

On the other hand, in my opinion, subjective and objective ideas of probability represent two different viewpoints, by no means in conflict since the results achieved on the basis of one concept are usually easily translated in terms of the other. The same holds for the distinction Bernoulli makes between necessity and contingency of the subject and necessity and contingency of its probative value.

He says that a subject may be necessary *(necessario existens)*, but not such that necessarily the phenomenon, of which one wants to measure the probability *(contingenter indicans)* follows. On the contrary, the subject may be contingent *(contingenter existens)*, but such that, if it exists, the phenomenon under consideration necessarily follows *(necessario indicans)*. Finally, the subject may be contingent *(contingenter existens)* and such that, because it exists, the phenomenon under consideration not necessarily follows *(contingenter indicans)*. Here is an example given by Bernoulli. It is quite sometime since I have received a letter from my lazy brother. What is the cause? I can take three circumstances into consideration: laziness, business, death. Laziness there certainly is, but it does not absolutely preclude that he might write to me *(necessario existens et contingenter indicans)*; death might or might not have intervened *(contingenter existens)*, but, if it did occur, it prevents him from writing *(necessario indicans)*. Business may or may not be involved, but, even if it is, it does not absolutely excludes the possibility of writing to me *(contingenter existens et contingenter indicans)*.

It is evident that the Bernoullian differentiation corresponds to that between *a priori* probability of the causes and the probability that, with one or the other cause intervening, the event occurs; a distinction on which, starting from an objective concept of probability, Bayes would base himself half a century later for the determination of *a posteriori* probabilities. Hence, in this area Bernoulli was a precursor; but, here too it appears evident that he did not know how to cross the gap between *a priori* and *a posteriori* probabilities. Obviously, the assessment of the probable cause for not receiving letters from his brother is a problem of *a posteriori* probability of the causes and, having separated the *a priori* probabilities of the causes from probabilities that each of them make the phenomenon to occur, Bernoulli had arranged all the elements ready to solve it: but he will leave the pride of the solution to Bayes.

Bayes's writings, published posthumously in 1764 and 1765 (in "Philosophical Transactions" of 1763 and 1764), by Price, were not valued in all of their importance; but this became evident in 1774, after Laplace gave to Bayes's theorem that precise and general formulation which was later to become traditional[7].

At that time, it was clear that the important problem which Jacob Bernoulli had solved did not really meet the purpose he had in mind. This was openly stated

[7] P. S. De Laplace, *Mémoire sur la probabilité des causes par les événements*, présentée à l'Academie des Sciences, Vol. 6, pp. 621-655, 1774.

by Prevost and Lhuilier[8] *"Et d'abord Jac. Bernoulli et tous ceux qui ont suivi sa marche, ont fait, on doit le dire, que de vaines tentatives pour arriver à cette estimation [des causes]. Leurs méthodes, quelque belles et utiles qu'elles fussent à d'autres égards, ne reposant point sur le principe étiologique*[9], *ne donnoient finalement que l'estimation des effets par la cause. C'est ce qu'on peut reconnoitre en jetant les yeux sur le grand problème, en apparence expérimental, qui est résolu à la fin de, l'Ars Conjectandi, repris par Moyvre (sic!) Bayer (sic!) et Price etc.*[10], *et traité par ces divers matématiciens d'une maniere plus exacte, mais non sur d'autres principes. Dans ce problème, s'agit de déterminer d'après la connoissance de la nature d'un dé, la probabilité qu'en jouant un très grand nombre de coups, on obtiendra des résultats contenus entre certaines limites, voisines d'un rapport qu'indiquent les faces du dé. Ainsi on conclut dans ce problème de la cause aux effets, et non des effets à la cause. Ces Stochasticiens se sont écartés du but, mais n'ont pas commis d'erreur"* (pp. 26-27).

Before reading Prevost's and Lhuilier's publication on *Ars Conjectandi's* analysis and Leibniz's correspondence, in contrast with Keynes, I had reached the conclusion that reassessed a truth that did not escape Bayes and Laplace's contemporaries, and it would have been strange if it were not so.

There is another part of the probability theory on which a reading of *Ars Conjectandi* may bring clarification: the origin of the so-called mathematical definition of probability, according to which the probability of a phenomenon is given by the ratio between the cases favorable to the phenomenon occurring and all the cases favorable or contrary to it, all cases being equally possible.

We have already seen how such a proposition is encountered in Bernoulli, although not as a definition of probability, but as a measure of the probability itself, of which one assumed to have the concept already. Actually, how does one measure a quantity, unless by determining how many units of measure, all assumed as equal among themselves, are included in it? And how does one measure the ratio between two quantities, unless by comparing the number of units of measure, all equal amongst themselves, included in one of the two quantities, to the number of units included in the other? In fact, the probability ratio can be considered to be a particular case of the ratio between two quantities: the possibility of an

[8] P. Prevost e S. A. Lhuilier, *Remarques sur l'utilité et l'etendue du principe par lequel on estime la probabilité des causes.* "Mem. Ac. Berlin" (1976), pp. 25-41, 1799.

[9] So Prevost and Lhuilier had, in a previous publication, called the well-known principle put by Laplace at the basis of determination of probability associated with the causes: "Si un événement peut etre produit par un nombre n de causes differentes, les probabilités de l'existence de ces causes prises de l'événement, sont entre elles, comme les probabilités de l'événement prises de ces causes". (see *Mémoire sur l'art d'éstimer la probabilité des causes par les effets.* "Mém. Ac. Berlin, 1976", Classe de philosophie speculative, p. 3–24, 1799).

[10] It must be noted how Prevost and Lhuilier did not realize that Bayes and Price, unlike Bernoulli and de Moivre, had dealt with the problem of probability associated with the causes. For the determination of probability associated with the causes, and exclusively resort to Laplace, in this as in all their other papers.

event and the possibility of a more comprehensive event, of which the first can be considered to be a modality. Having chosen a unit of measure of the possibility, to which the name chance is given, the above explained mathematical definition of probability follows.

It is not - as was expected - an idle tautology: indeed, like all measurements, it has an economic value, because it brings all the probability ranges back to one unit of measure, that is, that of the possible chance, hence, it does not exempt from defining the concept of possibility or probability[11].

In this regard, Bernoulli had a clearer perception than the majority of modern probabilists.

Jacob Bernoulli's fame is that of an analyst and is based on the demonstration of the theorem that took his name, but his logical intuition, as shown in *Ars Conjectandi*, as well as in his correspondence with Leibniz, is instead not properly appreciated. Even when his conclusions are not exhaustive - and two and a half centuries have not gone without leaving any sign - the brightness he proposes and distinguishes the problems with the sharpness of his reasoning and the appropriate choice of exemplifications are still striking.

It is strange how Part IV of *Ars Conjectandi* - without any doubt the most important from an analytical viewpoint and the only one of importance from a logical viewpoint is the one that has been translated less. While Parts I and II have, indeed, been separately translated in French and English, respectively; as far as I know there is only one translation in German of the whole book and no particular translation of Part IV.

If the Swiss Statistical Society, located in the town that had the honor of being the birthplace of Jacob Bernoulli, were to undertake the decision to translate Path IV in English and French, I believe it would add another title of merit to those it already has, supplying matter for admiration and reflection to many people who find it difficult to read in Latin and German.

Summary

Recalling the correspondence between Bernoulli and Leibniz, the Author stressed the logic problem of statistical inductions and the role of prior and posterior probabilities.

Keywords: Ars Conjectandi; Leibniz; Prior and posterior probabilities

[11] Regarding this topic, see the article *Concept et mesure de la prohabilité* in "Dialectica", Vol. III, N. 1-2, 1949.