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ON THE LOGICAL BASES AND GNOSIOLOGICAL IMPORTANCE OF THE STATISTICAL METHOD

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1. Introduction

The explorer about to discover the mysteries of a country, before setting foot in it, looks at the panorama and takes time to find his orientation and to structure his plan of action. After a long walk, he feels the need to gather his information in order to assess the part of the program that has been achieved and to value what he has learnt, for the remaining part of his journey. Similarly, when I left the University of Bologna and decided to dedicate my scientific activity mainly to statistics, I felt the need to form my personal opinion regarding its aims and relationships in regard to probability theory, as well as the concepts which are at its basis, and the importance of its applications by statistics and the meaning of the regularities which are defined by it[1].

After a very busy period of thirty years - spent, on one hand, organically developing statistical theory, so that it became more closely related to the aims of

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[1] The following publications are the result of my viewpoints of that time:
1. *Sul concetto di probabilità*, in "Atti del II Congresso della Società Filosofica Italiana" (Parma, 25-27 September 1907), Formiggini, Bologna-Modena, 1908.
2. *Che cosè la probabilità?*, in "Rivista di Scienza (Scientia)", vol. III, year II, 1908.
3. *Contributo alle applicazioni statistiche del calcolo delle probabilità*, in "Giomale degli Economisti", December 1907.
4. *Il sesso dal punto di vista statistico*, Roma, "Biblioteca del Metron", 1908, in particular chapters II *L'ufficio della statistica nella questione dei sessi*; IV *Misura della regolarita dell'eccedenza dei maschi nelle nascite umane*; V *Portata della regolarità dell'eccedenza dei maschi nelle nascite umane.*
5. *Intorno al metodo dei residui dello Stuart Mill e alle sue applicazioni alle scienze sociali*, in "Studi economico-giuridici della R. Universita di Cagliari", year II, 1910.
6. *Considerazioni sulle probabilità a posteriori e applicazioni al rapporto dei sessi nelle nascite umane*, in "Studi economico-giuridici della R. Università di Cagliari", year III, 1911. Other studies remained partially or totally unpublished, in particular a large manuscript on *La teoria logica e psicologica delle probabilità*, of which I have integrally reported the discussion *Sul concetto di casa* in the Communication presented in July 1941 at the Italian Statistical Society (see 7 of the quoted bibliography at the following note).

concrete research by new procedures and the improvement of those already known and, on the other hand, testing its suitability and usefulness by application to the most varied scientific fields, and, by doing so, sometimes finding suggestions for further developments - felt the urge to reconsider the critical examination of the logical bases and gnosiological importance of the statistical method.

The period of isolation, imposed by the war, was particularly useful for such attentive consideration.

Now that contacts with the international scientific world have been re-opened, it might be appropriate - as is suggested to me by the Editor of "Statistica"- to summarize the results of such a re-examination[2] for the benefits of Italians and

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[2] These are the titles of the publications in which such an examination was carried out. In the text of the paper the quotation will be replaced by the related order number.

1. *I pericoli della Statistica*, opening talk at the Italian Statistical Society (Pisa, 9^{th} October 1939), published in the Proceedings of the First Scientific Meeting of the Society and reprinted in "Rivista di Politica Econornica", November 1939.
2. *Sur La théorie de la dispersion et sur la vérification des schèmas théoriques*, report presented at the "Réunion d'etudes sur l'application du Calcul des probabilités" (Geneve, 12-15 July 1939) and published in "Metron", vol. XIV, n. 1, 15 June 1940.
3. *II principio della compensazione degli errori accidentali*, Communication presented at the II Congress of the Italian Mathematical Union (Bologna, 4-6 April 1940) published in the Proceedings of the Congress and printed in the "Statistical Supplement to Nuovi Problemi di Politica, Storia ed Economia", year VI, number I, 1940.
4. *Di alcune questioni fondamentali per la metodologia statistica*, Communication presented at the II Scientific Meeting of the Italian Statistical Society (Rome, 26-28 June 1940) and published in the Proceedings of the Meeting.
5. *Sulle basi del metodo statistico. II principio della compensazione degli errori accidentali e la legge dei grandi numeri*, in "Metron", vol. XIV, n. 2-3-4, 31 December 1941.
6. *Degli indici sintetici di correlazione e delle loro relazioni con l'indice interno di correlazione (intra class correlation coefficient) e con gli indici di correlazione tra serie di gruppi*, in "Metron" voL XIV, n. 2-3-4,31 December 1941.
7. *Sul concetto di caso*, Communication presented at the III Scientific Meeting of the Italian Statistical Society (Rome, June-July 1941) and published in the Proceedings of the Meeting.
8. *A proposito dei "testi di significativita"*, Communication presented at the VI Scientific Meeting of the Italian Statistical Society (January 1943) and published in the Proceedings of the Meeting.
9. *I testi di significativita*, opening speech at the VII Scientific Meeting of the Italian Statistical Society (Rome, 27-30 June 1943), and published in the Proceedings of the Meeting.
10. *Osservazioni alla comunicazione della Dott. Geppert sul valore dei così detti "testi di significativita"*, Communication presented at the VII Meeting of the Italian Statistical Society (Rome, 27-30 June 1943) and published in the Proceedings of the Meeting.
11. *Sulla probabilità inversa nel caso di grandezze a distribuzione costante*, Communication presented at the VII Meeting of the Italian Statistical Society (Rome, 27-30 June 1943) and published in the Proceedings of the meeting.
12. (In collaboration with Dr. Gregorio Livada) *Sulla probabilità inversa nel caso di grandezze intensive ed in particolare sulla sua applicazione a collaudi per masse a mezzo di campioni*, Communication presented at the VII Meeting of the Italian Statistical So-

foreigners. And to do this before moving on to tackle other important problems, which have yet to be solved for the complete settlement of methodological statistics from a logical viewpoint.

2. The necessity of statistical techniques

First of all, it is worth recalling what the circumstances are that make it necessary to resort to statistical technique. We are often interested in a quantitative study of phenomena whose characteristics cannot be detected by one observation alone, nor by a number of observations which are so limited that the normal human mental 1941. Faculties are unable to synthesize their result. In fact the human mind has limited power for numerical synthesis: this is a general limitation of our mental faculties, which matches the particular limitations of our senses. As, sometimes, one finds a remedy for these particular limitations by resorting to special techniques, microscopes, binoculars, megaphones, amplifiers, acoustic horns, etc, in the same way one finds a remedy for such a general limitation by resorting to statistical techniques[3]. Thus, statistics appears to be a suitable technique for the quantitative study of phenomena requiring collection or mass observation and therefore can be defined as collective or mass phenomena[4].

The reasons which make such a technique necessary for mass phenomena are various:

A) without the statistical technique, it becomes impossible to be aware of the phenomena under consideration, as for the sex ratios at births;

B) without the statistical technique it is possible to draw qualitative information

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ciety (Rome, 27-30 June 1943) and published in the Proceedings of the Meeting.
13. *Di alcune questioni attinenti al concetto di causalità e a concetti connessi ed affini*, forthcoming [see Analisi, III, IV, 1946].
14. *Rileggendo Bernoulli*, forthcoming [see Metron, XV, 1-4, 1949].
15. *Del passaggio dall'indice di variabilità di un campione all'indice di variabilità della massa*, forthcoming [see Metron, XVI, 1-2, 1951].

[3] For such a concept of Statistics see *Il sesso etc.*, op. cit., pp. 12-13, and for a wider explanation, the various editions of our Course in Statistics, held successively at the Universities of Cagliari (1909-1913), Padua (1914-1925), Rome (from 1926 onwards) and published as a lithography by students and colleagues. Finally it has been translated into Spanish by Dr. J. Vandellos, Curso de Estadistica, Editorial Labor, Barcelona (1935).

[4] Usually collective or mass phenomenon is defined as a phenomenon resulting from a mass or a collection of individual phenomena, instead of a phenomenon that, in order to be studied statistically, needs a mass or a collection of observations. However, such a definition is not correct: the male frequency among born and the male frequency among married couples both result, for instance, from a mass of individual phenomena (single born or single married): but, in order to be determined, the first needs a mass of observations, hence it belongs to the field of statistics and it must appropriately be called a collective phenomenon; not so for the second one, which we know a priori to be equal to 50%.

for mass phenomena, but not quantitative measurement, as for the frequency of high, average or low heights;

C) without the statistical technique it is possible to obtain a quantitative measurement of phenomena, but this is an approximate one, due to the errors to which it is subject[5].

This last necessity may occur in various circumstances:

a) when the intensity of the phenomenon one wishes to study is constant, but, in the observations we take of it, perturbing influences intervene, causing the results to be affected by errors that may be called measurement errors;

b) when the intensity of the phenomenon one wishes to study is always correctly detected, but it actually varies from one observation to another so that in order to know the phenomenon, it would be better to extend the observations to all the cases in which it occurs: wherever this is not done, in the observations carried out, the various intensity of the phenomenon will show (or they might show) with a different frequency from that in the total of the cases, and the results will be affected by errors that may be called frequency errors; when the intensity of the phenomenon one wishes to study;

c) when the intensity of the phenomenon one wishes to study varies from one observation to another and the observations are extended to the total cases of the phenomenon, but perturbing influences intervene in them, so that the results are affected by measurement errors;

d) when the intensity of the phenomenon one wishes to study varies from one observation to another, the observations do not extend to the total cases of the phenomenon and, in addition, perturbing circumstances intervene in them, so that results are affected at the same time by frequency and measurement errors.

Thus, the elimination of frequency and measurement errors is one of the main aims of statistics. As such errors depend on accidental perturbing circumstances, it is believed that their elimination is in fact obtained by resorting to mass observations, because the impact of accidental errors on the average intensity of the phenomenon would decrease with the increasing number of observations, until it becomes negligible when this is large enough. Regarding frequency errors, this thesis is expressed by the "law of large numbers" and, regarding measurement errors, by the "principle of random errors compensation".

In papers 3 and 4 and, more comprehensively, in 5 (and 6 for one subject related to this), the foundation of these two propositions has again been taken into serious examination, thus reaching the conclusions that are summarized and completed here.

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[5] Such a division in three parts of the aims of statistics has also been dealt with and developed in the publications quoted in note 4.

3. The law of large numbers

It was formulated by Poisson, who generalized Bernoulli's theorem, extending it to the case where the probability of the event is different in successive trials and likewise considering, besides the intensive quantities, hence relative frequencies, also extensive quantities, hence means of absolute values. The definition "Poisson's theorem" or, in order to keep to his words as much as possible, "theorem of large numbers", would have been well-suited for the generalization. Instead, Poisson, with an incredible logical jump, called it the "law of the large numbers", declaring in addition that it represents a general and indisputable fact, the result of experiences that cannot be contradicted. It was a logical jump because a theorem expressing necessary relationships among abstract entities is not necessarily a law expressing constant relationships among concrete phenomena. The jump creates a gap that cannot be filled, one which - in my viewpoint - Castelnuovo thought, although in vain, he would be able to fill by resorting to an "empirical law of chance" by which the experience would show that as the number of trials increases, the frequency of a phenomenon oscillates converging around a limit set by its mathematical probability. In vain, because, in order to define such a law, the trials should be increased to infinity, which is by itself humanly impossible and moreover the trials should be increased to infinity not only once, but as many times as it would be necessary to define a law.

It is true that in gambling games and other experiments set up so that they occur in the most uniform conditions possible - so as to assume that the probabilities of the various results remain constant - the (relative) frequency oscillations for such results go on decreasing while the observations increase. This might lead to the conclusion - with one of those inevitable generalisations in the inductive disciplines - that a limit frequency exists; but it remains to be demonstrated that such a limit frequency exactly corresponds to the mathematical probability. One cannot really see how this might be done without accepting that equally possible cases (*i.e.* equally probable cases), on which the mathematical probability is based, show the same limit frequency, that is accepting as demonstrated that correspondence between probability and limit frequency, which should instead be demonstrated.

Faced with this difficulty of going from Bernoulli's and Poisson's theorems to the law of large numbers, various authors have tried to follow the inverse path by defining probability not as the ratio of favorable cases to all the equally possible ones, but as the limit to which frequency tends as the number of observations increases, and deriving Bernoulli's and Poisson's theorems from such a definition, that implies the law of large numbers. This line of thinking obtained scientific formulation and organic development thanks to R. von Mises. However, I do not see how it is possible to avoid the following contradiction: if, analytically, probability is the limit of frequency (in the sense of calculus), it must always be possible to define a large enough number n of observations, such that, beyond it, the divergence between frequency and probability positively remains smaller than ε, a quantity however small. Instead, according to Bernoulli's and Poisson's theorems, however large n is, there is always a probability, however small, that a divergence greater than ε occurs[6].

On the other hand, how can one explain the widespread and indisputable trust in the law of large numbers or in an empirical law of chance, a trust that is at the basis of many scientific researches? I believe that it may be explained by the fact that the word "probability", occurring in the formulation of such laws, is in practice attributed a meaning which is different from the abstract one of "mathematical probability", which is at the basis of the probability calculus.

Indeed, as soon as in any statistical study one wants to specify the ideas, by defining the phenomenon one is interested in either for practical purposes or study, one immediately realizes that the number of cases within it may be more or less large, and sometimes very large, but always equal to a finite number. This is true whether we are dealing with gambling games, or the census of births and deaths in a population, or the stars in the sky, or the molecules in a gas. In this case at least, if not in all the cases, the idea of infinity arises unduly from the really different notion of indefinite[7]. As we will explain later, in relation to a finite number of cases of the phenomenon, the probability of one of its characteristics is represented by its frequency in the total number of cases. Hence, the law of large numbers states that when the number of observations of the phenomenon increases, getting nearer and nearer to their whole, the frequency of a characteristic of the phenomenon in the number of cases observed, comes closer while oscillating to its frequency in the total number of cases included in the phenomenon, until it coincides with it when all the cases have been observed[8]. Such a proposition, which may be empirically

[6] Von Mises answers an objection that might appear, but is not, similar and it is anyway formulated in such a way that the contradiction does not appear evident. He says that there is no contradiction between stating that, in a group of 11 observations, number m of times in which a phenomenon with a probability p $(0 < p < 1)$ occurs, may take all the values from 0 to 11 - as resulting from Bernoulli and Poisson's theorems - and the statement that the ratio of the number of times N_1 in which a phenomenon occurred, to the total number of observations N tends to probability p with the indefinite increasing of N_1 and N. The two frequencies m/n and N_1/N are - according to him - of a different type: between the two above mentioned statements there is no evident link. (See R. von Mises, Probability, Statistics and Truth, translated by J. Neyman, D. Sholl and E. Rabinowitsch, Hodge, Edirnburgh, 1939, pp. 127-128). One replies: in order to see the link and the contradiction between the two statements under consideration, let us say $n = N$, as there is no impediment to this. Indeed, if one can make N grow as much as one wishes, one can also make n grow in the same way. However large is the number N of the observations made, these may always be considered to be a group of the infinite possible observations. This is the essential point: so that m/n and N_1/N are not at all two frequencies of a different type and, for $n = N$, become the same frequency. But, however large $n = N$ is, Bernoulli and Poisson's theorems maintain their validity and all the values of m/n from 0 to 1 are possible, while, if it was true that N_1/N tends to limit p (in the true sense attributed to the expression in the analysis), there should be a large enough number $N = n$, so that the frequencies which diverge from p of more than a specific quantity would be excluded.

[7] Regarding this see what is said further.

[8] In publication 5 one may find the formulas which give the maximum possible devia-

verified and theoretically demonstrated, states precisely what is needed to give a basis to the mass observations.

4. The probability concept

In order to explain what is stated above, it is worth dwelling on the probability concept that has been just stated[9].

What do we ask to probability?

The word "probability" itself suggests that we ask the most plausible criterion in order to regulate our conduct. This may happen either when facing a single event, whose characteristics are uncertain, or a series of events that may show one or the other of some characteristics in single cases, without the possibility of foreseeing, from the definition of the category under consideration, which one will occur. In the first case of single events, when possible, the determination of probability is inevitably a subjective one, which will be explained further on. In the second case of series of events, or mass phenomena, the determination of probability may be objective. If we adopt as a more plausible criterion that which renders the errors sum nil and the sum of their squares minimum, we are led to conclude that the probability of a characteristics will be given exactly from its mean frequency in the single events which belong to the considered class. Except that we can exactly determine such a probability only for events entirely belonging to the past, while practically speaking its determination is of interest mainly for events partly or totally belonging to the future, or even purely hypothetical events. Thus, in practice, we are forced to resort to indirect paths, which are substantially only two: one based on the frequency of the characteristics observed in the past, leading to an approximate *a posteriori* determination of probability, and another based on the knowledge of the phenomenon mechanism, leading to an approximate

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tion; the standard deviation and the limit, within which a deviation is maintained with a given probability, when n of the N cases belonging to the phenomenon, have been observed.

[9] Such a concept has been dealt with since 1908. See the paper *Che cosè la probabilità?*, in "Rivista di Scienza (Scientia)", vol. III, year II, (1908), n. VI and the Communication *Sul concetto di probabilità, in Questioni Filosofiche*, "Atti del II Congresso della Societa Filosofica Italiana" (Parma, 25-27 September 1907), Formiggini, Bologna-Modena, 1908. Although I did not deal with it until 1941, it did not remain without a follow-up. See, in particular, L. G. Du Pasquier, *Sur les nouveaux fondements philosophiques et mathématiques du calcul des probabilités*, in "Atti del Congresso Internazionale dei Matematici", Bologna 3-10 September 1928, Vol. VI, page 5 and following; L. Galvani, *Punti di contatto e scambi di concetti tra la Statistica e la Matematica*, in "Giornale dell'Istituto Italiano degli Attuari", IV, n. 3, July 1933, pp. 413–414, and Introduzione matematica alto studio del metodo statistico, Giuffre, Milan, I Ed.1934, pp. 167–168, II Ed, 1945, pp. 261–263; G. Pietra, *Metodologia Statistica*, in Societa Italiana per il Progresso delle Scienze, Un secolo di progresso scientifico italiano, 1839-1939, vol. I, p. 305; *La Statistica Metodologica e la Scienza ltaliana, in "Supplemento Statistico ai Nuovi Problemi"*, V, series II, nn. 2–3–4, p. 137 and *Sur la statistique méthodologique italienne*, in "Revue de L'Institute Internationale de Statistique", 8, 3/4, 1940, p. 187.

a priori determination of probability.

The approximate *a posteriori* determination leads to a definition of the so-called empirical probability, according to which the probability of a characteristic of a phenomenon is given by the ratio of the number of times the characteristic has occurred in a large number of observations, to the number of times it might have occurred. The approximate *a priori* determination leads to a definition of the socalled mathematical probability, according to which the probability of the characteristic of a phenomenon is given by the ratio between the cases favourable to the characteristic occurring and the possible cases, all cases being equally possible.

The approximation of the empirical determination of probability depends on the hypothesis that the frequency of the characteristic in the observed cases of the phenomenon is equal to its frequency in the cases yet to be observed. This hypothesis can reasonably be made as an approximation, when there are no circumstances making the two frequencies systematically different. On the other hand, even when a systematic difference exists, it is not excluded that it might be approximately evaluated, hence allowing us to define a correction coefficient to be applied to the frequency of the characteristic for the observed cases, in order to calculate its frequency for future cases or the total cases where the phenomenon occurs. The more the number of observations increases, getting nearer to the total cases of the phenomenon, the more the empirical probability, determined on the basis of the observations carried out, approaches the exact probability of the characteristic of the phenomenon.

The approximation of the determination of the mathematical probability depends on the impossibility of judging *a priori* when two or more cases rigorously have the same chance. The inevitable approximation, by which the equi-possibility of cases is judged, is reflected in the approximation of the mathematical probability. Rigorously, the equal possibility of the various possible cases might be deduced only *a posteriori*, from their frequency in the total observations of the considered phenomenon, in which case the mathematical probability of a characteristic would coincide with its frequency in the said total.

It is not always possible to determine the empirical probability, or the mathematical probability. The determination of the empirical probability assumes a phenomenon that, although not exhausted (in which case one could use the direct determination of true probability), has however occurred many times. The determination of mathematical probability assumes that, in the mechanism by which the phenomenon occurs, various hypotheses can be differentiated, some of which are favorable and some contrary to the presence of a specific characteristic and all considered as equally possible, a fact that practically does not occur except for a limited number of phenomena, artificially set up by us, as is the case of gambling games.

My viewpoint is that both the determinations of empirical and mathematical probabilities have been wrongly identified with the exact definition of probability. In particular, this has happened for the determination of mathematical probability.

Unknown to the first modern authors who dealt with the concept of probability and derived probability from experience[10], it was introduced by Jacob Bernoulli[11], and became the basis of all the subsequent construction of probability calculus. As the attention of those who followed probability calculus was finally drawn to gambling games, for which the determination of mathematical probability is particularly suitable, it became easier to adopt. On the other hand, for practical purposes, it offers the advantage of generality, so it may be applied to an indefinite group of experiences related to a specific object, instrument or phenomenon. For instance, one may speak of the mathematical probability of a specific result of a dice game, roulette, baccarat, no matter which are the dice, the roulette, or, respectively, the cards used, no matter who the players are, and so on. However, this generality is only obtained at the cost of some approximation, which in fact represents the approximation of probability determination through mathematical probability. It is not at all true that the probability of a result is always the same for all the roulettes. Actually, each roulette has its own probability for the various numbers, a fact of which a group of Monte Carlo gamblers cleverly took advantage. They had studied all the trends of the various roulettes and by playing, no longer on the basis of mathematical probability, but on empirical probability, instead, they were able to make enormous profits, until the Casino management, suspicious of the losses, found out the trick and defeated it by changing the roulette plates every night. This is also the reason why in card games, where the stakes are very high, a new pack of cards is used for each game. It cannot be excluded that accurate studies of large number of observations might verify that the game results for dice or roulettes are sensibly different, not only depending on the roulettes or dice being used, but also on the person who turns the roulette or throws the dice.

Hence, it is not that the frequency represents an approximation of exact probability, given by the mathematical probability, it is instead the mathematical probability that represents an approximate *a priori* determination of the true probability represented by its frequency. It is however necessary to agree on the word "frequency". It must be understood as a frequency of a phenomenon characteristic in all the cases within the concept of the phenomenon itself; which we might call its totalitarian frequency[12]. A partial frequency, even if based on very many cases,

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[10] Regarding the concept of probability according to *Port Royal Logic* (1662) and according to Locke, *An Essay concerning Human understanding* (1690), see J. M. Keynes, *A Treatise on Probability*, Macmillan, London, 1921, p. 80.

[11] Reality, Bernoulli did not consider the ratio of cases favorable to a phenomenon, to all the favorable or opposite cases, as a definition of probability, but only as its measurements. A probability of a phenomenon is defined by him as a level or fraction of the certainty of its existence. Such a level or fraction is obtained from the number and the probatory strength of the arguments demonstrating the existence of the phenomenon and the probatory strength is derived from the ratio of the cases favourable to the phenomenon, to all the favorable or opposite cases. Regarding this see publication 14. In Bernoulli, the subjective concept of probability, with which we will deal later, appears to be clearly formulated.

[12] This expression must not be confused with that of "total frequency" used by Borel and, following his example, by other French experts in probability calculus, for the def-

can only lead to an approximate *a posteriori* determination of probability.

Several modern mathematicians base the definition of probability on frequency, but on limit frequency instead of totalitarian frequency. Let us find out if and how such a concept may be justified.

One may note that there are phenomena occurring under the same conditions at various circumstances of time and place, so that one sees no reason for admitting that they have a different probability: for instance, like the sex ratios at births for one year and those for the following year, tonight's roulette game and tomorrow's. On the other hand, even when the various phenomena are exactly defined by stating the time and place of their occurrence, when they do not entirely refer to the past, the number of cases in which they occur remains generally undefined. Thus, may we say that a probability concept, such as the one derived from limit frequency, one which is not linked to a finite number of cases, nor to particular circumstances of time and space, does not represent an advantage?

It is easy to answer that even if we cannot perceive the difference between the probabilities of some characteristics in various phenomena differentiated according to time and place (for instance, the probability of certain results in roulette games carried out today or tomorrow), it does not mean that differences between such probabilities cannot exist for circumstances unperceived by us, as in effect it will show when the phenomena will have exhausted itself in all its cases and it will belong to the past; nor the fact that the number of cases, part of each phenomenon, is indefinite, in the sense of "impossible to be determined a priori", must be confused with the statement that it is not finite, but infinite.

Hence, from a theoretical viewpoint, to calculate probability from the limit frequency does not represent any advantage, so it might be only justified from a practical aspect in order to supply a uniform criterion for cases where we cannot in practice differentiate the different probabilities of the phenomenon. However, practically speaking, the limit frequency cannot be determined and it would instead be worth resorting either to an *a priori* approximate determination of it, by mathematical probability, or to an *a posteriori* approximate determination of it, by empirical probability. As we stated above, these procedures are already justifiable as approximate measurements of totalitarian frequency, without the need to resort to the concept of limit frequency.

One might still point out that, if it is true that in controlling our behavior we practically find ourselves facing phenomena limited in time and space, and including a finite number of cases, nevertheless nothing prevents us from grouping such phenomena in wider phenomena, for which we may as well determine the probabilities. For instance, we may decide to determine the probabilities of obtaining certain results in roulette games, not tonight or tomorrow night and for a specific

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inition of "limit frequency", with which we will deal later. For example, regarding this, see M. Fréchet, in Exposé et discussion de quelques recherches récentes sur les fondements du Calcul des probabilités, Hermann, Paris 1938, p. 25. But, actualy, I do not see the advantage of substituting such a clear expression and one already adopted by the scientific language, like that of "limit frequency" with another that may be misleading, as this one of "total frequency".

table, but for all the tables where the game is played, has been played or will be played. Similarly, we may decide to determine the sex ratios at births, not of a specific population, in one year or the other, but for all the human population, for all time.

However, this would lead to a very large number of observations, yet always a finite number, and on the other hand by widening the number of cases beyond those which directly interest us, probability would lose its function of supplying the most plausible criterion for the control of our behavior.

Nor, would it be worth insisting on this, saying that one might think of roulette games or other similar events considered in an abstract way, structuring the various hypotheses allowed by its results, independently of actual trials or observations and with the reference to the general possibility that these repeat themselves as many times as one wishes, because, in such a case, the determination of probability would loose the *a posteriori* characteristics of limit frequency returning to mathematical probability, *a priori* determined on the basis of cases theoretically possible.

On the other, one must keep in mind, as it has been previously pointed out with regard to the law of large numbers, that the probability definition as limit frequency leads to a contradiction, likely to be unavoidable, with Bernoulli's and Poisson's theorems, so that such a definition seems to have to be discarded.

Before leaving this topic, I will say a few words regarding the so called "subjective theory of probabilities", a theory that, according to my viewpoint, is wrongly judged by many people to be incompatible with the probability concept derived from frequency and set as the basis of the theory, which in antithesis is said "objective".

Some authors assume that, when probability is applied, not to a series of cases (e.g. death probability in the people of a country, of a certain age, and in the course of a specific interval of time; probability of a shipwreck, in the course of a certain trip, for the ships insured with a specific company), but to a single case (e.g., the sex of an as yet unborn child, the condition of a historical event), it cannot be based on frequency - that for a single case is necessarily equal to 0 or equal to 1- but it only expresses the subjective confidence level that in the single case one condition, rather than the other, occurs. Because usually (in fact "always", as some say, purposely exaggerating) probability is applied to the single cases, this should be the definition to be adopted for the term probability.

When the topic is further investigated, one realizes that, even when probability is attributed to a single case, it is still always defined for a series of cases, to which the considered single case belongs. We do not know enough about the antecedents of the phenomenon (or, even in the hypothesis of a phenomenon not belonging to the future, of the circumstances occurring or following it) which are related to the event we are interested in, to decide if this occurs or not in the case under consideration. Hence, we must reckon that these antecedents might occur either in the case in which the event occurs or, in the case in which it does not occur, with the real or hypothetical cases forming the category for which the defined probability is valid. The confidence level that the event occurs in the single case under consideration is just the frequency by which, on the basis of *a priori* and *a posteriori* considerations, we assume that the event occurs in the conceptual class, to which the single case under consideration belongs. It is such a conformity that makes it reasonable to bet on the occurrence or not, of the event in the single case. But not always can such a frequency be numerically expressed; often we can only say that it is high, low or medium. On the other hand, the definition of the class in which the considered single case may be placed, does not represent a datum of the problem, but it depends on the more or less vast knowledge or in the more or less sophisticated studies conducted by he who wishes to define the probability and also on the major or minor importance that the person assigns to one or the other of the various circumstances that might affect the occurrence of the event. In this manner the probability determination for single cases may well be called subjective[13].

5. The concept of chance

To the contrast between "objective theory" and "subjective theory" of probability, from a certain similar viewpoint a contrast corresponds between "chance in the objective sense" and "chance in the subjective sense". The two contrasts are similar because, like the subjective concept of probability, so the subjective concept of chance refers to single events, while the objective concept of chance, like the objective concept of probability, refers to classes of events.

We call a single event "random" when we cannot foresee it. If we suppose that all events have a cause, the conclusion is that only our ignorance allows us to speak of chance. For a being of superior intelligence, who, as such, would know all the causes of phenomena, nothing would be unforeseen and chance would not exist. Such is the subjective concept of chance, as found in the classics of probability theory, but it can be found much earlier, at least in Spinoza (1677).

To this, Cournot explicitly opposed the objective concept of chance, that, on the other hand, is already formulated in a treatise by Jean de Laplacette (1714) and, according to Cournot himself, it had been foreseen by S. Thomas in the Middle Ages and, even before that, by Boethius. Having defined a class of phenomena B, when we cannot foresee the occurrence of a phenomenon A, from the occurrence of B, we will say that the occurrence of A is random in relation to B. Cournot says that this happens whenever A and B derive from independent causes. But this does not occur only in the case of such a hypothesis: if a negative relationship exists between A and B, it is even more likely that the occurrence of A appears as random in relation to the occurrence of B. Hence, in relation to B, a phenomenon A which, in its occurrence, is not at all consistent with B, may be defined as random. It results as such, not because of the ignorance of this or that person, but for all people, independently of their knowledge. Thus, it is objective.

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[13] The thesis that each probability is determined in relation to a class of cases and that the definition of probability in relation to such class is objective, while the size of the class is subjective, has finally been supported by Fréchet, by modifying his previously expressed opinion (see Exposé quoted above, p. 50). We have dealt with it since 1908 (see the quoted article Che cos'è la probabilità?, pp. 7-11). We do not know if Fréchet knew about our thesis of so many years before.

It results as such because, of the circumstances preceding or following phenomenon A, one assumes knowing only those defining phenomenon B, to which A is related. In relation to the knowledge of B, A is casual. There might be those who know and those who do not know the reasons, why a person became a delinquent: anyway, it must declare as casual the fact that the delinquent was born on one day of the week, rather than another. Hence, the event which is casual in the objective sense, may be called so in the relative sense, because it is such in relation to some of our information, while an event, casual in the subjective sense, may in contrast be called so in an absolute sense, because it is so in relation to all our information.

It may be observed that, even in the objective sense, the concept of chance implies some ignorance, the ignorance of those circumstances preceding or concomitant with A, which are not those defining phenomenon B, with which A is compared: but it is an hypothetical ignorance, not an actual one, artificial, not real.

The actual ignorance of causes, from which chance derives in the subjective sense, may or may not be inevitable. This second alternative occurs in gambling games, purposely set up so that the result will be unpredictable. In relation to them one may speak of chance in an almost objective sense.

Does an antithesis really exist between the concepts of chance and cause? Nowadays there is a lot of talk about it, especially by physicists, who oppose deterministic laws to statistical laws. What can one say regarding this?

A cause is always followed by its effect, so that between the former and the latter there is a frequency ratio equal to 1, but often the effect is determined, instead of by only one cause, by a complex of causes, which can be split into various "concomitant causes". Therefore between the concomitant cause and the effect, there is a probability ratio that translates into a frequency smaller than 1, but greater than 1/2. A frequency equal to 1/2 is a sign of independence between the event and the circumstances to which it is related, and, finally, a frequency smaller than 1/2 shows, among the said circumstances, the presence of some counter-effecting factor, we might say of a "counter-cause". An event appears to be casual to us when the circumstances to which it is related do not include either its cause, or one of its concomitant causes.

Thus the antithesis between cause and chance is without foundation, whenever, by it, one means that a phenomenon, which is attributed to chance, evades the principle of casuality; but it is right, if one means that the phenomenon is called casual in relation to a circumstance or a set of circumstances, which does not represents, nor includes, the cause or a concomitant cause of the phenomenon[14].

6. Principle of random error compensation

Some people define as random those errors whose probable value is equal to zero, that is to say, errors that tend to compensate themselves. If understood in this

[14] Regarding the concept of case, see publications 5 and 7; regarding the causality principle in relation to the so called indetermination principle, see publication 13.

way, the principle of random errors compensation becomes a mere tautology[15]. Random errors may be otherwise defined, intending as random errors those which are the result of perturbing circumstances that tend to compensate themselves. Then, the problem of whether random errors tend to compensate themselves as the number of observations increases, acquires a different meaning which is as follows: provided that a phenomenon is observed for the cases in which it occurs under the influence of a specific circumstance (or of a specific group of circumstances), whose effects we are interested in studying, and, at the same time, under the influence of other perturbing circumstances which we consider as independent from the first, hence, accidental in relation to it, so that, equally, they tend to compensate themselves, can we say that the effects of these accidental circumstances also tend to compensate as the number of observations increases? This is precisely the meaning attributed to the question by statisticians, since Quetelet [16].

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[15] In a very recent article *Sulla teoria della media tipica* ("Analisi", I, 2 quarter 1945) Boldrini - obviously referring to our publication 5 - states that the "classic definition of random errors", according to which observation errors are called random if their sum tends to 0 as the number of observations increases, "has recently been declared as tautological". And he dwells upon the evaluation of such an accusation of tautology (see pp. 10–12). The fact is, anyway, that I never dreamt of stating that such a definition - with which I will deal later (see note 16) - is tautological. What I stated instead was that the compensation principle becomes tautological when such an assumption is adopted for the random errors. These are my actual words: "Allowing such a definition for random errors, the principle of compensation for random errors is obviously true; but it becomes a tautology: that is, it becomes the statement that, as the number of observations increases, those errors whose influence, as a definition, tends to disappear as the number of observations increases, tend to compensate." (pp. 11–12). I feel that such a statement is, on one hand, indisputable and, on the other, so clear that it should not have led to misunderstanding. Such an article by Boldrini represents the first part of a publication of the same title, presented at the meeting of the Accadermia Pontificia delle Scienze, 5^{th} April 1945. At this very moment, while I am correcting the proofs, I am receiving an abstract, that will be considered in the following notes.

[16] Quetelet differentiated the causes of statistical phenomena in constant, variable and accidental and assigned to statistics the task of finding the effects of the systematic causes (constant or variable), eliminating those of accidental causes [see Lettres sur La Théorie des probabilités appliquée aux sciences morales et politiques (Bruxelles, Hayez, 1846)]. He defines the accidental causes as: "Les causes accidentelles ne se manifestent que fortuitement, et agissent indifferemment dans l'un or l'autre sens" (p. 159). Instead, he does not define accidental errors; but it is clear that he thinks of them as the effects of accidental causes.
Regarding this Gauss was clearer. In the first paragraph of the famous *Theoria Combinationis Observationum Erroribus Minimis Obnoxiae* (1821), he said: "Quaedam errorum causae ita sunt comparatae, ut ipsarum effectus in qualibet observatione a circumstantiis variabilibus pendeat, inter quas et ipsam observationem nullus nexus essentialis concipitur: errores hinc oriundi irregulares seu fortuiti vocantur, quatenusque illae circumstantiae calculo subiici nequeunt, idem etiam de erroribus ipsis valet". (Karl Friedrick Gauss Werke, Vierter Band, Zweiter Abdruck, Göttingen, 1880).
Regarding this Gauss was clearer. In the first paragraph of the famous. Theoria Combi-

Hence, the principle of random error compensation admits that - being equal to 0 the probable value of the differences between the intensities of some circumstances and their arithmetical mean, while another circumstance (or a group of circumstances) remains constant the probable value of the effects of such differences is also equal to 0.

Having defined in such a way the content of the principle of random errors compensation, it becomes evident that such a principle is not necessarily true. It is in fact obvious that, if the expected value of a variable - in our case represented by the whole of perturbing circumstances - is equal to 0, it is not necessarily the expected value of any of its functions. For the value of one of its function to be equal to 0, this function must satisfy certain conditions. Several of the conditions, sufficient for the above, were taken into consideration:

a) linear relationship between the intensity of the perturbing circumstances and the intensity of the resulting errors;

b) symmetry, either in the frequency curve of the intensity of perturbing circumstances, or in the relationship between the intensity of these, and the intensity of the resulting errors;

c) concordance between mean and median, either for the perturbing circumstances and the resulting errors, and monotonic relationship between the intensity of the perturbing circumstances and the intensity of the resulting errors, so that greater errors correspond to stronger perturbances;

d) coincidence between the arithmetical mean and the mode of the observed values and inverse relationship between frequency and intensity of the errors[17].

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nationis Observationum Erroribus Minimis Obnoxiae (1821), he said: "Quaedam errorum causae ita sunt comparatae, ut ipsarum effectus in qualibet observatione a circumstantiis variabilibus pendeat, inter quas et ipsam observationem nu!lus nexus essentialis concipitur: errores hinc oriundi irregulares seu fortuiti vocantur, quatenusque i!lae circumstantiae calculo subiici nequeunt, idem etiam de erroribus ipsis valet". (Karl Friedrick Gauss Werke, Vierter Band, Zweiter Abdruck, Gi:ittingen, 1880).

It is obvious that both Gauss and Quetelet understand random errors as the effects of causes acting independently of the constant quantity one wishes to observe. This concept well suits the definition we have given for accidental errors; a definition that in itself does not have as essential the condition of errors compensation.

Instead, Boldrini says that, according to the classic concept of random errors, "numerous errors of unknown origin are called by that name, usually small, which inevitably are made when attentively measuring the same statistical quantity several times, the sum of which tends to zero with the increasing number of observations" (see Sulla teoria della media tipica p. 9 of the paper in "Analisi" p. 2 of the extract from "Atti della Pontificia Accademia delle scienze"), a definition that I would suggest to refuse in order to present one of my own. I now have a really strong wish that Boldrini would quote from the classics - as among these neither Gauss nor Quetelet are present -who give of random errors the definition that he attributes to them.

[17] By following Quetelet, many statisticians assumed as a sufficient condition for the ap-

From the detailed examination carried out, the result was that not always one of the above-described condition occurs. It is concluded that the application of the principle of compensation of the effects of accidental perturbing circumstances (a procedure which forms the basis for so many statistical methods) will not be legitimate. For it to be so, it is worth examining each individual case if there is reason to assume that one of the above listed conditions is occurring, if not exactly, at least with the approximation required by the nature of the studies. It is true though that, even if none of the above-mentioned condition occurs, one cannot rule out that the application of the said principle might lead to exact conclusions. Some more complicated hypotheses occur, which make the application of the said principle proper, but, until one has demonstrated that a more complicated hypothesis corresponds to reality, the application of the principle will not be justified.

7. Prevalence principle of constant causes

We would therefore ask ourselves if we cannot indicate a principle for more general applications than that of the accidental errors compensation, a principle on which to base the elimination of measurement errors. In this regard, one may observe that, when the errors are rare, the more intense they are, the maximum frequency of the observed values will correspond to an error equal to 0, that is, the mode of the observed values will correspond to the true value of the quality. The validity of this condition is more general than that of the above-mentioned condition *d)*, because it does not require the hypothesis of correspondence between mode and

plication of the random errors principle, the fact that the curve of the observed quantities follows the normal or Gaussian distribution, so that the arithmetic mean of a normal distribution would give the value of the phenomenon, that would occur due to the systematic cause or the causes, apart from the intervention of perturbing circumstances.
However, as I pointed out in publication 5, the statement is not proved and in fact everything seems to make us believe that it is without foundation, at least until we refer to the approximately normal distributions met in reality. In such a publication I exactly supplied an example of distribution - that practically would certainly be judged as normal - for quantities resulting from the combination of a constant quantity and of the disturbances brought in by two circumstances whose intensities compensate but whose effects do not compensate. The mean of the observed quantities for such a distribution is equal to 17.42, considerably different from the intensity of the constant quantity equal to 16.58.
In the quoted publication *Sulla teoria della media tipica*, presented at the "Accademia Pontificia delle Scienze", Boldrini insists on Quetelet's theory, although he introduces limitations that, according to him, would represent points of contact with my objections; but, according to my viewpoint, unsuccessfully. In fact, he does not exclude, and he does not even take into consideration the possibility, that I had exemplified too, that perturbing circumstances with non-constant effect and intensity which do not compensate for one another, might cause the mean of the observed quantities to diverge from the constant quantity, without the distribution of these essentially diverging from the normal type. When one does not take into consideration hypotheses contrary to one's own thesis, one may pretend to demonstrate anything one wishes.

arithmetic mean of the observed values. It must be added that it does not even imply the hypothesis that perturbing causes have an accidental aspect. Such a proposition may be called prevalence principle of constant causes. It leads us to resort to mode instead of arithmetic mean as the most plausible value of the true intensity of the phenomenon, independently of the influence of the perturbing factors[18].

8. Analysis of variance

Besides the influence of random errors on the mean intensity, their influence on variability must be taken into consideration.

It is known that the probable value of the total variance of a series of measurements affected by accidental errors is equal to the variance of the exact quantities (systematic variance) plus the probable value of the variance, to which the single observed measurements are subjected due to the accidental errors (accidental variance).

When extended to the subjective means, this result leads us to consider the variance of a phenomenon under the influence of two groups of circumstances A and B, as the sum of the variance V_{ma} which the mean values of the phenomenon show with the variation of circumstance (or a group of circumstances) A, and of the variance ${}_aV_b$, that, with the same A, the single values of the phenomenon show as B varies (procedure α); or also as the sum of the variance V_{mb}, which the mean values of the phenomenon show with the variation of circumstance (or group of circumstances) B and of the variance ${}_bV_a$ that, with the same B, the single values of the phenomenon show as A varies (procedure β). One or the other of the procedures is used to split the total variance of the phenomenon into two parts, respectively, due to the two groups of circumstances A and B.

An objection which does not seem to be avoidable is that the results of the two symmetrical procedures (α and β) do not coincide, except for particular cases, and in fact often differ considerably. For instance, one procedure applied to the analysis of the variance of the number of *Drosophila's* ocelli according to the genetic constitution and temperature, derived from Krafka's experiments, leads to

[18] In publication 5 it was noted that "the principle of random errors compensation has the advantage of the arithmetic mean, to which it leads, being less affected, in relation to the mode to which the principle of constant causes prevalence leads, by the influence of the limited number of observations. Even when its probable error is not negligible, this may nonetheless be determined, while the probable error of the mode cannot yet be determined" (p. 207).
The probable error of a mode had really been determined - as I verified later - by Kazutaro Yasukawa, in the laboratory of Prof. K. Pearson (*On the probable Error of the Mode of Skew Frequency Distributions*, in "Biometrika", vol. XVIII, Parts III IV, 1926), assuming hyper geometric distribution. As it is known, starting from this distribution, Pearson deduced his well known types of curves. Recently, the study of this problem has again been undertaken at the Institute of Statistics of Rome University, either regarding the single types of curves studied by Pearson, or starting from more general hypotheses.

attributing, to the two groups of circumstances, respectively the weight of 55 and 45% and the other, instead, of 68 and 32%.

It seems more acceptable to stick to a third procedure, by comparing the variance ${}_bV_a$, shown by the phenomenon under a group of circumstances A, when the group of circumstances B is kept constant, or the variance ${}_aV_b$ shown by the phenomenon under a group of circumstances B, when the group of circumstances A is kept constant, to the sum of the two variances ${}_bV_a$ and ${}_aV_b$. It corresponds to the procedure followed in the experimental method, where each factor's influence is isolated by observing how things develop when other factors remain constant. In our example by such procedure the influence of the genetic constitution and that of temperature on the variance of the number of *Drosophila's* ocelli is 60 and 40% respectively[19].

9. The relationships of casuality and probability cannot be inverted

Given that by increasing the number of observations, the frequency of accidental errors can practically be eliminated and measurement errors can compensate themselves when specific conditions occur, one is motivated to look for the relationships between the number of observations and the intensity of such errors - another fundamental problem of statistical methodology for the assessment of reliability of the available data.

When the probability of a phenomenon, corresponding to its frequency in the total observations, is known, from Bernoulli's theorem it is easy to calculate the probability of a random frequency error and, when the true value and the standard deviation of an absolute quantity are known, it is easy to calculate the probability of a measurement error from the curve of accidental errors.

Except that, usually, nor the probability in the first case, nor the true value and the standard deviation of the absolute quantity, in the second case, are known. Therefore, such values are usually substituted by those of frequency and, respectively, of the mean value and of the standard deviation, obtained from a large number of observations. Such substitutions - and particularly that of the observed standard deviation for the theoretical standard deviation - lead to some reservations (see paper 1); but the main difficulty is not related to this. In order to move - however approximately - from the observed quantity to the probable value of the true quantity, are the same known facts, which are necessary in order to move from the true quantity to the probable value of the observed quantity, enough? This is the crucial point of the matter. This is the problem around which all the discussions on inverse probability, tests of significance, confidence levels, gravitate[20].

In order to solve the problem, it is worth starting from the undisputed proof of non reversibility of the relationships of casuality. After it rains, roads are wet;

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[19] For the analysis of variance see publications 3, 4, 5, 6.

[20] In the paper *Considerazioni sulle probabiltà a posteriori e applicazioni al rapporto dei sessi nelle nascite umane*, in "Studi economico–gioridici" of the R. Università of Cagliari, year III, 1911

but if a road is wet, this does not prove that it did rain; the road might have been watered, or some flooding might have occurred, or further, the water might be the cause of the burst of a pipeline. Besides, if we do not have some information regarding the frequency of rain, watering, flooding and water pipeline bursting, we will never be able to assess with which probability the wetness of the roads is due to one or the other of the above-mentioned causes.

Probability relationships differ from those of causality because the same antecedent or the same system of antecedents does not always cause the same effect, but sometimes one type of effect and some other times another, each with a specific probability; but for them as well it might occur that the same effect might derive from a system of antecedents and it is impossible to determine the probability that an effect is due to an antecedent rather than another without some information about the frequency by which these various antecedents occur. This information is not required, when, an antecedent being known, one wants to determine the probability of its various possible effects. For instance, it is easy to calculate what is the probability that, at one or the other game of cards, a player holds four aces in his hands; but, if you see a player with four aces in his hands, you cannot calculate what is the probability that one game, rather than the other, is being played, if you do not know what are the games being played and with what frequency.

When keeping in mind this obvious considerations, it appears clear that the task of the probability theorem inversion - and particularly of building up the tests of significance and assessing confidence intervals - without having, or assuming to have, any information regarding the frequency of the possible causes of the results, is a desperate attempt. Only by a misunderstanding could someone have thought to have managed to do so.

Let us examine where the misunderstanding might have come from.

10. Probability that a result occurs in a random combination and probability that a result depends on a random combination

Let us consider a family with 10 daughters. The probability that such a combination occurs due to chance is very small: as less females than males are born, the result is smaller than $1/2^{10} = 1/1024$. From this some assume that there is a probability greater than 1023/1024 that the parents had a tendency to generate females.

Let us consider a genealogy where grandfather, father and four children have died of a specific illness. For such an illness the probability of death is 1/200. Assuming that it is not the case of a contagious disease, $1/200^6$ will be the probability that such a combination occurs due to chance. Many people draw the conclusion that the considered illness, or at least a predisposition to it, had hereditary characteristic, as there is only one possibility out of 200^5 favorable to the opposite hypothesis. This type of debate is often met among geneticians, to support the importance of inheritance in the assessment of some characteristics.

The following debate, which aims at demonstrating the practical impossibility that organic matter is derived from inorganic matter, is one of greater impact. It is said that the probability of one of the simplest organic molecules, made of only just 2000 atoms, to be originated by a casual combination of the considered atoms, is less than $1/10^{600}$. Hence, there is less than one possibility out of 10^{600} that it does not originate from a predetermined plan. The mystical reaction to the materialistic concept of the beginning of life, tries to find an objective foundation in this and other similar arguments.

Many people believe that the validity of these arguments cannot be contested; but their fallacy is clearly perceived when one resorts to a similar argument interfering with our interests, because then our logical sensibility is awakened and stimulated.

Think, you have won the lottery with a set of four winning numbers and call to collect your winnings. You find that at the lottery shop there is a policeman who stops you and says: "The probability that your numbers were extracted as a pure chance is 1/511038. Hence, there are 511037 probabilities to one that your winning is the result of some cheating. This is enough for me to declare that you are under arrest". I am quite sure that this reasoning does not persuade you!

In order to be sure of the fallacy of this and the previous reasoning it is sufficient to consider that, among the 1024 sex combinations that may occur for 10 births, among the 200^6 combinations for death causes that may occur in the sequence of two ancestors and four brothers, among the 10^{600} or more combinations for 2000 atoms, among the 511038 combinations for four of the five extracted numbers, one combination had to occur anyway, and the one which did occur was as probable as any other. The same reasoning could have been repeated for any other combination that happened to occur, thus concluding, with the same basis, that it could not have been casual.

Where then is the mistake? The mistake lies in confusing the probability that a specific result occurs in a series of combinations by chance with the probability that it does occur by a casual combination, rather than by one due to systematic causes. Some authors, and among them some very esteemed ones, ran into such a misunderstanding that appears to be a banal one. The misunderstanding is, for instance, obvious in a paper by Karl Pearson, to which the modem formulation of the tests of significance, provided by the English School, can be traced back. The paper is entitled: *On the Criterion that a given System of Deviations from the Probable in the Case of a Correlated System of Variables is such that it can be reasonably supposed to have arisen from Random Sampling* ("Philosophical Magazine", 1900). The title says *"From random sampling"*; but soon enough in the text the expression is substituted by the other *"on a random selection"* and it is this expression which corresponds to the demonstration that is actually given in the paper. Therefore Pearson aimed to supply a criterion so that it could reasonably be assumed that a specific system of deviations was derived from a random selection and, instead, he demonstrated a criterion so that in a random selection the desired system of deviation is obtained.

A very simple relationship exists between the probability ${}_aP_e$, that a specific event e occurs in a casual combination and the probability ${}_e\pi_a$ that the same event, which has occurred, derives from a casual combination. The relationship, that I recalled in paper 1 and to which I returned to in paper 9, is

$$
{}_e\pi_a = \frac{{}_aP_e}{{}_aP_e + \frac{1-P_a}{P_a}\ {}_sP_e} \tag{1}
$$

where ${}_aP_e$ and ${}_e\pi_a$ have the meanings explained above, P_a is the probability that accidental causes intervene, $1 - p_a$, the complementary probability that non-accidental causes intervene and ${}_sP_e$ the probability that, if non-accidental causes have intervened, the considered event occurs.

Let us apply this formula to the four winning numbers. As we have seen, in such a case, it is ${}_aP_e = 1/511038$; but, suppose that the lottery game is carried out with total impartiality. In it only accidental causes intervene; that is $P_a = 1$ hence $(1 - P_a)/P_a = 0$ and consequently ${}_e\pi_a = 1$. There is therefore the certainty (and not a probability equal to $1/511038$) that the winning is due to casual combination. Naturally, there would be a different conclusion if the game was not carried out with the necessary impartiality.

Again, let us apply the formula to the formation of the organic molecule. In this case it is ${}_aP_e < 1/10^{600}$; but which are the values of P_a and ${}_sP_e$? The misbeliever refuses to take into consideration other causes rather than the natural ones and he denies the possibility of a preestablished end in nature. For him it is $P_a = 1$ hence ${}_e\pi_a = 1$. Instead the believer is convinced of the contrary: for him it is $1 - p_a = 1$ and also ${}_sP_e$ assumes a high value, so that ${}_e\pi_a$ acquires a fading value.

It must be observed that when non-accidental factors intervene, so that it is $P_a < 1$, $(1 - P_a)/P_a > 0$, the value of ${}_sP_e$ and hence of ${}_e\pi_a$ is not the same for all the possible combinations, for which the value ${}_aP_e$ is instead the same. For those who admit that the process of nature is directed towards an aim, there is a strong probability ${}_sP_e$ (if not perhaps the certainty) that the pre-set combination of the atoms did originate the organic molecule, but there is not the same probability that it might have originated any other combination. In a hypothetical country, where the lottery game is not carried out honestly, there will be a more or less strong probability that a large win is the result of some cheating, while there is no equivalent reason for assuming that the extraction of some numbers, of which nobody can take advantage, is the result of that cheating.

Regarding this, it is also worth considering that the usefulness of formula (1) is not just to allow the numerical determination of ${}_e\pi_a$, that in reality can only be done exceptionally, but it is to show the relationship between ${}_aP_e$ and ${}_e\pi_a$, which is useful from a dual viewpoint From a negative viewpoint, because it enlightens how arbitrary it is, and even resulting as dangerous, to go from ${}_aP_e$ to ${}_e\pi_a$ without any other information. From a positive viewpoint, because it shows how in particular cases, when one has this type of knowledge, actually concerning the values of P_a (hence of $1 - p_a$) and of ${}_sP_e$, it is possible to reach founded conclusions, however not always numerically expressible, about the value of ${}_e\pi_a$.

However, the misunderstanding between direct and inverse problems of probability did not occur for the first time in the modern English Statistical School. It actually dates much further back.

It is commonly known that Jacob Bernoulli has demonstrated that when the probability ν of a phenomenon is known, it is possible to calculate the probability that in a given number of observations its frequency f deviates from ν of less than a given quantity. This is the theorem - a theorem of direct probability - known as Bernoulli's theorem. But, indeed, he tackled a problem of inverse probability, that of the *a posteriori* determination, from the observed frequency f of the phenomenon, of its unknown probability ν with an error smaller than a given intensity, when it might not have been possible to determine *a priori* such a probability as the ratio of favorable cases to the possible-cases of the phenomenon.

The contrast between the assumption and its demonstration is obvious for anybody who attentively reads *Ars Conjectandi* and, if it is usually not noticed, I believe this happens because many speak of Bernoulli, but very few have read him. Keynes, who read him, noticed the contrast and thought that Bernoulli actually had in mind - after having demonstrated the direct theorem - to demonstrate the inverse related theorem, but he had been unable to do so because of his sudden death, thus leaving his work unfinished. There is now no doubt that *Ars Conjectandi* is an incomplete work, but what is missing is the application to civil, moral and economic subjects, while, with regard to the theoretical part, from a careful reading of the book and of the letters exchanged between the Author and Leibniz there is no doubt that Bernoulli thought he had successfully demonstrated his assumption (see paper 14). Hence, he openly misunderstood a direct probability theorem and an inverse probability theorem. Bernoulli is rightly regarded as the founder of Probability theory. Hence, the misunderstanding dates back to the same origin. It might be called the "original sin of probability theory". Thus one understands why it has been so difficult to eliminate it.

11. The inversion of Bernoulli's theorem carried out by Laplace with the hypothesis of equiprobable causes. Proposal of a more general scheme and its application to batch testing by samples

The above recalled formula (1) is only one particular case of a more general formula used in the probability theory to determine the *a posteriori* probability on the basis of the probability of causes. In this particular case the causes are grouped in two categories: accidental and systematic causes. Instead, when the considered causes are many (theoretically infinite) and they all intervene with the same probability and lead to different probabilities for the event, which are uniformly distributed between 0 and 1, Laplace demonstrated that Bernoulli's theorem can be inverted, that means to say that probability ${}_{\overline{\nu}}P_{+ef}$ that in n cases a frequency f occurs, which differs from the corresponding true value (that is from the corresponding probability) $\overline{\nu}$ of more than

$$e\sqrt{\frac{2\overline{\nu}(1-\overline{\nu})}{n}}$$

in such an hypothesis, is equal to the probability ${}_{\overline{f}}\Pi_{+e\nu}$ at the unknown probability ν differs from frequency $\overline{f}$, which occurred in n cases, of more than

$$e\sqrt{\frac{2\overline{f}(1-\overline{f})}{n}}$$

The hypothesis of equiprobable causes, which the different values from 0 to 1 of the event *a priori* probabilities imply, cannot generally be verified, and many times it clearly does not correspond to truth: so, one understands why so many authors tried to demonstrate that such an hypothesis is not necessary. They believe to have done so, provided that the number n of observations is large enough that the terms of order $1/n$ can be neglected; but their demonstrations are not exempt from objections, as I have demonstrated in paper 9. Indeed, in paper 12 (prepared in collaboration with Dr.G. Livada), reconsidering and developing a scheme already proposed in 1911[21], I demonstrated that, when the causes corresponding to *a priori* probabilities of the event gather around a mode value $k/(k+h)$ and they have a value included between x and $x+dx$ with probability

$$p_x = \frac{(k+h+1)!}{k!h!}\, x^k(1-x)^h dx$$

the values of ${}_{\overline{f}}\Pi_{+e\nu}$ depend on k and h, as well as on the number $\overline{f}n$ of times in which the event occurred and on the number of times $(1-\overline{f})n$ in which it did not occur. As there is no restriction on the values k and h, it appears clear that, even when the number n of observations is so large that terms of order 1/n can be neglected, the results essentially depend on the values k and h, that is, on the distribution of causes.

This scheme is more general than that considered by Laplace, which can be obtained in the particular case when $k=0$, $h=0$. Practically it can also be applied, and in fact I did apply it, to the testing of product batches by sampling, when the firms calling for the testing had previously submitted other samples from which the values k and h can be calculated. I demonstrated that by making various hypothesis on the percentages of faulty elements resulting from previous samples, the probability that in the submitted batch the percentage of faulty elements does not go beyond a specific limit may result to be very different, depending on the percentages of faulty elements resulting from previous samples and, nonetheless, very different than that which one would have obtained on the basis of Laplace's hypothesis of equiprobability of causes. In the example I gave, according to this hypothesis, one would have expected a probability equal to 0.1438 that the percentage of faulty elements in the hatch was not greater than 8%, while, according to the various hypothesis considered in the application of our scheme, the respective probability was 0.8773; 0.2996; 0.0004; 0.3107.

If I am not wrong, the introduction of this more general scheme, applied to tests by sampling, represents a substantial progress in dealing with inverse probability problems, either from a theoretical viewpoint and a practical one.

[21] In the paper *Considerazioni sulle probabilità a posteriori e applicazioni al rapporto dei sessi nelle nascite umane*, in "Studi economico-giurdici" of the R. Universita of Cagliari, III, 1911.

12. Inversion of error probability for absolute quantity. estimation theory

R. A. Fisher gave to the study of the tests of significance a different trend than that given by Laplace. He did so in a 1930 publication, in which he showed that he considered as intuitive - because he did not give a demonstration - that, if for instance there is a probability of 95% or 997‰that the observed value t of an extensive or intensive known quantity $\overline{\theta}$ or of one of its functions is greater than limit $\bar{t}$, there is also the probability of 95%, or respectively of 997‰, that, having observed a value $\bar{t}$, the true value of quantity θ remains smaller than $\overline{\theta}$. Fisher does not think any other hypothesis necessary except the continuity of the quantity t. It is substantially on such a thesis that the formulation of the confidence intervals method, later developed by J. Neyman and E. S. Pearson[22] is based.

By recalling the subject in paper 11, I demonstrated that such a thesis is without foundation and that the legitimacy of inversion is subordinated to two hypotheses:

a) that the true value of quantity θ might take *a priori*, with the same probability, any of the values smaller or greater than $\overline{\theta}$ from which the observed value $\bar{t}$ may derive.

b) that the distribution of the observed values t does not vary with the variation of θ, at least within the limits from which the observed value $\bar{t}$ may derive.

Instead, it is not indispensable that neither the continuity of quantity t, nor the continuity of θ occurs.

The mentioned hypotheses are not identical to those on the basis of which Laplace demonstrated the inversion of Bernoulli's theorem for the relative frequencies. While hypothesis a) in fact matches the similar hypothesis of Laplace on equiprobable causes implying *a priori* probabilities between 0 and 1, hypothesis b) cannot occur in Laplace scheme regarding relative frequencies, because the binomial model, according to which these are distributed, notoriously varies its shape depending on whether the binomial terms are the same or different, and, in this second case, depending on whether they are more or less different. Hypothesis b) can only occur for absolute quantities.

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[22] Neyman himself, writing about it in 1934 ("Journal of the R. Statistical Soc." Vol. XCVII) treated this method as an extension of the previous results by Fisher. From a paper which appeared during the war, ("Biometrika" Vol. XXXII, Part II, October 1941), of which only now, while I am correcting the proofs, I am able to have knowledge - I realize that later Neyman radically changed his opinion and now reckons that between the two theories - that by Fisher and the other by himself and E. S. Pearson - there is no relationship. However, the groundlessness of the method by Neyman and E. S. Pearson has been demonstrated by me - independently of its derivation from that by Fisher - either in publication 9, on the basis of the article by Clopper and E. S. Pearson in "Biometrika" (VoL XXVI, Part III-VI) in 1934, or in publication 10, on the basis of the presentation made for such a method, in 1943, at the Italian Statistical Society, by Dr. Geppert

By an appropriate example I have also separately examined, in both cases, the sensible impact that may affect the results if hypothesis a) does not occur or if hypothesis b) does not occur.

The above-mentioned system of hypotheses, if sufficient, is not however necessary, because it may be substituted by different systems of hypotheses, which would also result as sufficient.

Indeed, such an inversion is equally allowed when the following two hypotheses occur:

α) the logarithm of the true value of quantity θ may *a priori*, with the same probability, take any of the values smaller or greater than $\overline{\theta}$ from which the observed value $\bar{t}$ may derive;

β) the distribution of the logarithms of the observed values does not vary with the variation of value θ, at least within the limits from which the observed value $\bar{t}$ may derive.

These two hypotheses may be assumed as verified (while the previous hypotheses a) and b) may not be assumed verified) when the true quantity θ is represented by the variability index of a collective phenomenon and the observed quantity $\bar{t}$ by the variability index of one of its samples. The inference from the variability index of the sample to the variability index of the collective phenomenon is authorized by the two hypotheses α) and β) (see paper 15).

In order for the estimation theory - which in fact aims at arriving at the corresponding characteristics of the collective phenomenon from the sample characteristics - to undertake a rigorous aspect, it is necessary:

1. to be absolutely clear that it is not possible to arrive at the collective phenomenon characteristics, from the sample characteristics, without formulating hypotheses;

2. to define - generally or from time to time - the hypotheses sufficient for such an inference to be allowed.

The problems mentioned in 2) can present two forms:

A) to define which are the hypotheses sufficient for a given inference to be allowed, e.g. to define the hypotheses sufficient for Bernoulli's theorem inversion to be allowed or for the inversion from the standard deviation of the collective phenomenon to the standard deviation of one of its samples to be allowed;

B) given some hypotheses, to define which is the allowed inference, e.g. to define the probable value of probability p after a frequency m/n has occurred in n observations, in the hypotheses that, before the n observations, all the values of p were equally possible. As known, in such a hypothesis, the probable value of p is equal to $(m+1)/(n+2)$.

13. Probatory value of an event favouring a hypothesis and reliability of the hypothesis after the event has occurred

The importance of the conclusions reached in publications 8, 9, 10, 11, 12, 15 really goes beyond the applications of probability theory. Indeed, the problem put forward in such publications is only a particular case of a more general problem, one which covers the very theory of knowledge.

Let us assume that we have several hypotheses A, B, C, ..., Z, all assumed as admissible at a specific stage of our knowledge. A fact α occurs. We ask for the probatory value of fact a favoring hypothesis A. Generally speaking, the problem may be solved.

The probatory value for hypothesis A (rather than for the alternative hypotheses B, C, ..., Z) indeed depends on the compatibility of α with A, rather than with B, C, ..., Z, that is on the probability that, assuming that hypothesis A is true event α occurs in comparison to the probability that, assuming one of the alternative hypothesis B, C, ..., Z to be true, such an event α occurs.

Rightly so, but one must not confuse the probatory value, that has just been determined, of α for hypothesis A, with the reliability of hypothesis A, after event α.

The reliability of hypothesis A, after event α, evidently depends, not only on the probatory value of event α for hypothesis A, but also on the reliability of hypothesis A, before event α. The pretence to determine the reliability of hypothesis A after event α is absurd if one does not know (or assumes not to know) the reliability of hypothesis A before such an event α.

Only if, before event a, the reliability for the various hypotheses A, B, C, ..., Z, was the same, the reliability of any one of these, after such an event, would result as proportional to the probatory value of event α for the considered hypothesis. However, many times the reliability for the various hypotheses A, B, C, ..., Z, before event α, is different from one hypothesis to another and, in such a case, their reliability after event α does not only depend on the probatory value of event α, and sometimes depends on this only a lesser extent[23].

These conclusions might appear to be obvious: one must believe that none of the unconditional supporters of the tests of significance and of the confidence intervals will refuse his agreement. This shows how sometimes, in logical problems, instead of clarifying ideas, mathematics might confuse them. This occurs when operations, which are expressed by the same or similar symbols, have a different logical meaning, as in fact occurs for the direct and inverse theorems of probability calculus[24].

[23] Particularly, regarding this, see publication 8.

[24] It could be said that the misunderstandings are not due to mathematics but to the mismanagement of it. Naturally, they can be avoided by introducing new symbols, which allow us to properly differentiate different operations. Proposals of this type will be found in our forthcoming publication: *Di alcuni simboli che sarebbe opportuno impiegare nella trattazione matematica dei fenomeni statistici.*

14. The importance of the applications of the dispersion theory and of theoretical schemes in general

Let us go back to the problem from which we started: the problem of deciding whether or not an observed combination depends on accidental causes. As long as one deals with the only observation in which the combination occurred, it remains but to resort to formula (1), but when, besides such an observation others have been made, in a sufficiently large number, one might face the problem of whether the frequency of the combination under consideration is more or less higher than it should be for the effect of chance. For instance, this study was carried out extensively for sex combinations in families. This is the case of one of those inductive applications of probability theory, which belong to the dispersion theory by Dormoy and Lexis.

Now, in this regard some considerations should be made, which limit the purport of such a comparison.

The first is that such a comparison tells us nothing about the accidental or systematic characteristic of a single observed combination. If we find, as in fact it was found, that the sex combinations in single families depend for one tenth or a little more on the systematic tendencies of the parents to conceive one rather than the other sex, and for the rest they are the result of chance[25], this does not mean that the same can be said for any single family. For instance, if a family has 10 daughters in ten births, this might totally depend on chance, with the family really not having any particular tendency to give birth to females and even having a tendency to conceive males, and it might instead depend exclusively on the inability of the parents to conceive males, without any accidental inf1uence.

The second observation is that, even considering all the observations, the comparison of the observed results with the forecast derived from the probability theory cannot lead to certain conclusions. There is always the possibility that a coincidence of actual data with theoretical data represents the effect of chance, while groups of consecutive observations might show that in reality the phenomenon has a dispersion greater or smaller than the theoretical one. Equally, there is the possibility that a difference between observed results and theoretical forecasts depends on chance, while subsequent groups of observations might reveal that the phenomenon actually has a normal dispersion. It has often happened that in statistics a seeming regularity, that was calculated from a number of observations which seemed large enough, was later on denied as such by further observations. Therefore, one must calculate the probable error for the result of the comparison between actual data and theoretical forecasts, expressed by the dispersion index; but the calculation of the probable error is, on one hand, subordinated to the above stated hypotheses, implicit in the probability inversion and, on the other hand, it cannot itself be free from the influence of chance. In conclusion, it must never be forgotten that the applications of probability theory lead to conclusions which are often hypothetical and always more or less probable, never certain.

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[25] *Il sesso dal punto di vista statistico*, 1908, Rome, Biblioteca del "Metron", X: La variabilità individuale nella tendenza a produrre i due sessi.

Finally, a third consideration must be made regarding the cognitive importance of such comparisons. The observation stretches to all the theoretical schemes, to which the dispersion theory belongs as a particular case.

A theoretical scheme is a logical construction that generally implies several hypotheses. The ascertainment that the results of the observation correspond to the data of the theoretical scheme does not at all allow for the conclusion that the hypotheses related to the scheme must correspond to reality, but only that they might correspond to reality. Indeed, the correspondence may also be explained by different hypotheses. In order to conclude that the said hypotheses - assumed in a numbers - implied in the scheme, correspond to reality, it must be proved that not only the global result, foreseen on the basis of the theoretical scheme, but also $s-1$ hypotheses, implied in the scheme, correspond to reality.

In the case of dispersion, the Lexis scheme assumes that the probability of the considered event is constant and that the occurrence of an event in one case is independent of its occurring in previous cases; but a normal dispersion may still be obtained when there is compensation among the subsequent events, while the probability of the event is subjected to variations in the course of the observation. So the normal dispersion - assessed by several studies - of sex ratios in human births for various territorial districts or consecutive time intervals, did not by itself justify Lexis' conclusion that the probability of a male birth or a female one did not vary from the territorial district to another and from one considered time interval to another. Such a conclusion became justified only after the compensating tendency - stated by many authors - among the sex ratios for consecutive groups of births was excluded. It then allowed us to drop several theories on sex determination, which by such constancy resulted as being incompatible[26].

Similarly, the genetic uniformity for the various individuals of a population, in relation to a characteristic, dependent for its phenotype manifestations on the perturbing influence of the environment, leads to a Gauss distribution of the characteristic intensities; but it is wrong to derive, as Quetelet did, from such a Gauss

[26] For all this see the quoted publication *II sesso dal punto di vista statistico* (1908), in which two chapters are dedicated to the application of the dispersion theory to sex ratios at births for various territorial districts and consecutive time intervals (Cap. IV: *Misura della regolarità dell'eccedenza dei maschi nelle nascite umane*; Cap. V: *Portata della regolarità dell'eccedenza dei maschi nelle nascite umane*). In chapter V, the substantial meaning of the normal dispersion is examined, and the conclusions which can be drawn regarding the theory of sex determination are enlightened. Also explained are the ways in which a probability of a phenomenon may vary during the observations or may be influenced by the frequency of the phenomenon, in the previous cases, and of each the influence which affects dispersion tending to make it hypemormal or hyponormal is shown. We take into consideration the positive or vice versa negative interdependencies, among the probabilities assumed by the phenomenon in the single cases which are part of each term of the series, an interdependence that I am now realising was vastly elaborated by Hilda Griringer in an article that appeared during the war *A New Explanation of Non–normal Dispersion in the Lexis Theory*, in "Econometrica" (January 1942). Obviously, the author, who speaks of "New Explanation", ignored the explanation of the subject given by me 34 years before.

distribution the genetic uniformity of the population in relation to the characteristic under consideration.

The problem - examined in publication 1 and more extensively in 2 - of the cognitive value of the influence of theoretical schemes is very important today, when such schemes multiply, often without taking care of the necessary checks. The necessity to carry out such checks must be stressed, and not only regarding the forecasts to which the scheme leads, but also regarding the single hypotheses implied in the scheme.

15. The claimed contradictions of statistics

Another problem examined in publication 1 regards the contradictions that are claimed to exist between the conclusions on the same phenomena arrived at by several statisticians, sometimes all of them skilled. Generally, such contradictions are only apparent. They often depend on the fact that, in reality, the diverging conclusions do not refer to the same phenomenon. Thus, comparing the mortality of two populations, different statisticians may arrive at different conclusions depending on whether they compare the raw coefficients of mortality or, instead, they eliminate the influence of age, or also that of sex, of marital status, or of profession, or wealth, and so on. Actually, the conclusions made by the various authors refer to different phenomena; those by one to the total mortality; those by another to the total mortality the age composition of the populations being equal; those by a third author the sex composition of the populations being equal; those by a fourth the age and profession composition of the populations being equal and so on. On the other hand, it must be acknowledged that the many factors influencing the statistical phenomena and the need to carry out a sufficient number of observations for each of them, make difficult to eliminate all the factors of no interest; nor all the researchers, because of the material available to them, are able to do so for the same factors and in the same amount, so that such seeming contradictions are actually more frequent in statistics than in any other area.

Other seeming contradictions arise from the fact that the temporary effects of a factor may be different - sometimes opposite - from its permanents effects, as occurs for the influence of economic conditions on birth–rate. Similarly, the effects of the same factor may be different depending on whether its intensity remains within specific limits or it, instead, overcomes them, as, for instance, is the case of tobacco or alcohol consumption, or meat consumption. Still, one must consider that the indirect effects are sometimes in contrast with direct ones, and eventually prevail over them, as it is in the case of the effect of a favorable environment influencing mortality, because it prolongs the life of people who enjoy it, but, on the other hand, by diminishing the effect of natural selection, it makes future generations less strong. In their studies, some authors consider one or another type of effects and, when such a diversity of objectives does not appear clear enough, their diverging conclusions may appear wrongly incompatible.

16. The dangers of statistics

These seeming contradictions and the acknowledged facility to run into them for particularly complex phenomena such as those dealt with by statistics; the possibility to achieve only probabilistic conclusions and never certain ones, when one wants to extend the results beyond the observed data; the required need to carry out checks, not always possible or satisfactory, of the various statements, besides the forecasts of the theoretical schemes; the inevitable hypotheses and which often do not correspond to reality, to which the tests of significance of statistical data and the elimination of accidental errors are subordinated, all surround statistical applications with dangers, of which one must be aware without t, statistics is an *avant garde* discipline and as such it is normal that it implies particular risks; but, on the other hand, it must be reckoned that often it would not be possible to reach where it reaches, to see or perceive what it sees or perceives, to reap the crop (however, sometimes not ripe) that it gathers; in other words, nothing could replace it. Nietzsche said that to live dangerously is an essential condition for obtaining maximum benefit and maximum satisfactions: perhaps in no other field better than in statistics does his quotation apply.

Summary

The Author deals with a number of arguments inherent statistical inference and probability as the frequency limit by von Mises, the correlation between probability and chance, the concepts of casualty and causality, the inversion of Bernoullis theorem and the tests of significance.

Keywords: Empirical and mathematical probability; subjective and objective probability; subjective and objective chance.