

## ON THE PROBABILITY CONCEPT

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1. Every person makes conjectures and on the bases of their probability knows how to judge, reason, control his actions and interests: this is, amongst our logical faculties, the one which is more constantly and more essentially useful to us in our practical way of life. I believe nobody will deny this, neither the economist, the statistician, or the financier, particularly if one does not limit oneself to a detached observation and listing of facts, but studies them passionately and knows how to listen to the voices of life.

For instance, what is a budget? It is a calculation made on the basis of conjectures, forecasts, evaluations which appear as probable to us. If this essential psychological element is removed, only a useless arithmetical exercise will remain, with no other value but that of a mere example. Nothing but calculations will remain, proving that, assuming this as an example, a factory which is being planned, might cost so much for plant installations, might produce so much and at such a cost, and that if the sales will be a certain amount at a defined price, would yield a specific profit. But nobody would rely, for a budget, on an example which has been made up for nothing else but for giving an example: it is without doubt that anybody who would know how to build a car in one hour at a cost of 30 liras, and would find immediately to sell it for 100,000, could become a millionaire within one day; but it is equally certain that none of the readers will be attracted by an explanatory budget of such a kind to begin to be a car builder. While, instead, a similar calculation, but one where data will appear as realistic, has a fundamental importance for one who wishes to undertake a business.

Nor can it be said that the likelihood, or lack of it, are just a consequence of the major or minor distance of the conditions assumed as hypotheses, from the real conditions found in business and similar circumstances. Such an objection would be essential because, if it were right, the determining reason of a budget value would have an empirical positive aspect, instead of a psychological one. But, such a positive criterion would evidently be too simplistic: the fact that hypotheses are close to the real conditions of similar business, is not important by itself but, if ever, because it appears verisimilar. To carry out the crucial experiment, let us assume in fact that for a new business at a specific moment, developing conditions are thought verisimilar, which are very different from those of the pre-existing similar business: there is no doubt that for a budget one would rely on the verisimilar, although unusual, conditions and not on those which, although usual, are non-realistic for the situation under consideration.

Practically, this case is very frequent, and the reasons leading to this likelihood or probability evaluation may be very, very different and they do not need to derive from a precise positive criterion: their value derives from the given psychological appreciation.

Such a psychological concept of probability not only has not been invented by mathematicians, but in fact has not even been examined and studied in all of its depths and richness: the mathematicians, some more than others, always limit the area of probability theory, almost as they feared that the introduction of such a live and genuine element in their formulae would bring goodness knows what a confusion.

So, for a long time the definition of probability was limited to the area of those artificial and schematic problems, where it being possible to recognize a division in a finite number of possible cases which, for symmetry reasons, are considered as equally probable - the well-known definition of probability is introduced as the ratio between a number of favorable and possible cases. The extension to problems external to such a special area, an extension appearing by itself as a spontaneous one, was introduced more or less tacitly, with more or less vague justifications inspired by criteria of analogy, with more or less vague limitations related to its legitimacy.

Successively, various attempts were made to create a foundation of probability calculation on more convincing bases, but keeping intact the purely arithmetic and at least apparently totally extra psychological character of the practical definition. In my opinion, any attempt made with such intentions is necessarily going to be a failure, because its aim is to pervert the considered concept, to eradicate it from the only ground from which it absorbs life and vigor and from which we cannot think of it as detached, without the withering of the concept or without it being devoid of any meaning. We can realize this when considering two of the more serious and recent attempts: I refer to the theory which has been widely developed by Kamke <sup>1</sup> and the theory which has been outlined by Cantelli <sup>2</sup>.

2. Kamke starts from the empirical observation that, when submitted to repeated trials, some phenomena create frequencies which ordinarily become closet to a determinate value, and he admits that such a value is the limit of the sequence of frequencies. So, he defines the probability of the phenomenon as the limit of frequency when the number of trials tends to infinity.

Fundamentally, it is the same starting point as von Mises' <sup>3</sup>, but which has been cured of the intimate contradiction, by the abandonment of *Regellosigkeitsaxiom* (axiom of non-regularity, of lacking of a rule in the occurrence of favorable and unfavorable trials). The interesting fact is that even a follower of von Mises' viewpoint felt the need to eliminate the *Regellosigkeitsaxiom*, but even more interesting is to note how, having been dismissed (from the system of the starting

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<sup>1</sup> E. KAMPKE, *Einführung in die Wahrscheinlichkeitstheorie*, Hirzel, Leipzig, 1932.

<sup>2</sup> F. P. CANTELLI, *Una teoria astratta del calcolo delle probabilità*, "Giorn. Ist. It. Attuari", III, n. 2, 1932.

<sup>3</sup> R. VON MISES, *Wahrscheinlichkeitstheorie*, Deuticke, Wien, 1931; *Wahrscheinlichkeit, Statistik und Wahrheit*, Springer, 1929.

axioms), it comes back (in the formulation of any practical problem), and with this the absurdity, which was eliminated in the theoretical field, reappears as soon as concrete applications are attempted. Thus, in his book, in order to be able to salve the contradictions of von Mises' system, what Kamke does is just to use the theory of Cesaro's sums for sequences formed exclusively of zeroes and ones, arbitrarily using expressions taken from probability theory.

As long as we remain in the mathematical field, for instance by defining the probability that an integer is divisible by 7 as the limit (evidently equal to  $1/7$ ) of the frequency of the multiples of 7 in the first  $n$  integers, for  $n \rightarrow \infty$ , there is no doubt that everything is fine, and eventually one might only discuss the appropriateness of such a definition. In fact, if one abandons the usual meaning with which, one way or another, one thinks of probability as a guidance to forecasting, if one limits oneself only to theorizing about the occurred facts, it would be even better to avoid the word probability and to use that one of frequency or limit frequency, which is beyond any possible doubt or misunderstanding. This is obviously the case for arithmetic problems, while in concrete problems it would be the case, only in the (practically impossible) hypothesis that all the trials of the infinite succession, which is under consideration, have already been carried out.

However, if one is really dealing with probability, the case is very different; let us consider a *roulette* game and let us see what it means to say that the probability is  $p = 1/2$  for somebody who follows such a definition. It means stating that, in a preset series of trials, the frequency tends to the limit  $1/2$ , which is justified by the empirical observation that if a very large number of experiments is carried out, ordinarily the frequency is near to  $1/2$ , and the approximation is the better, the bigger the number of trials. In particular, it means to affirm the impossibility that the result will always be indefinitely "black". One observes spontaneously that, if on the one hand one cannot deny the extremely, unlikely situation that in a very large number of trials, and even worse in infinite trials, "black" will always appear, it is on the other hand equally undeniable that one can't see any reason why to exclude such an eventuality as an *a priori* impossible one, which certainly does not include anything in itself contradictory. Hence, the equally spontaneous doubt arises that the assimilation of the "infinitely small truth likeness" to the "impossible", contains that same error, an infinitely small one in itself, but an essential one as related to substance and method, made by the person who would confuse, for instance the "limiting point of a set" with the "point of a set". In particular, this person could say that all numbers are rational (because they can be approximated by rational numbers with whatever precision grade), and, by putting  $\sqrt{2} = \frac{m}{n}$  he would reach the absurd relationship  $2n^2 = m^2$ .

In Kamke's case, an absurdity cannot be derived, in a similar way, from his postulates themselves, because, as I said, the formal absurdity, which is intrinsic of von Mises' system, has been solved, but it reappears when one undertakes any concrete application, which is based on the same criteria for which Kamke believes he is allowed to call his theory *Wahrscheinlichkeitsrechnung*. In fact, the empirical considerations, from which one would like to derive that in a series of *roulette* trials the frequency-limit will be  $1/2$ , should evidently be valid for all the sequences of trials or for none (as they do not include any element varying from one sequence of trials to another). For instance, if I participate in the

game irregularly and if I take into consideration only the trials made when I was present, I have no motive (not to search for it in logical or psychological reasons, as I would do, nor to turn to the principle of insufficient reason, as others would consider convenient) to reach a different conclusion from the previous one. That is, the frequency-limit, in the cases I have observed, will be  $1/2$  and, particularly, it will be possible for me to see always the small ball stopping on “black” (what does it in fact matter if, during my absence, the *roulette* is working or has stopped?). Generally, the same I can say for any pre-set sequence of trials, and, if for each sequence of trials it is impossible that “black” is always obtained, it means that “black” can appear only in a finite number of trials, which contradicts the hypothesis that the frequency-limit is  $1/2$ . Synthetically, one could say that the addition of the principle of insufficient reason to Kamke’s axioms, in whatever form, is equivalent to the addition of *Regellosigkeitsaxiom*, hence it leads to absurdity.

Another form to enlighten the weak point of the system is the following, which is less suitable for making the criticism in itself, given that (as a difference from the previous one where its own ammunition is used) in its considerations appear which are alien to the system itself and which a defender might reject on principle, but perhaps more intuitive for the unprejudiced reader. Why do we assume as practically impossible that an infinite sequence of *roulette* results will always appear as “black”? Because this appears to us as practically impossible already in a very large number  $n$  of trials and because in  $n$  trials we have only one sequence of all “black”, over an enormous number ( $2^n$ ) of possible sequences judged as equally probable. There is no other reason: no one, except perhaps some cabalist, will think of a superhuman law forcing exceptional cases not to happen, anymore than they are being forced by their own rarity. Also, among the infinite sequences, the one made up of all “black” has no particular reason to be assumed as impossible; it is simply one among infinite possible ones, hence it is only considered as infinitely little probable. But then, also any other pre-set sequence of “blacks” and “reds” could be assumed, with equal reason, as impossible, because it is equally improbable that the one who makes any well determined forecast guesses the result of all the throws, as it is for the one who forecasts all “blacks”. Hence, no sequence will be possible!

3. Thus we have demonstrated that Kamke’s theory, which is perfect as long as one remains in the mathematical field (examples where the frequency-limit can be observed), cannot be applied without contradictions in the probability field (examples where the frequency limit constitutes a prophecy). But one should ask: would it be actually an advantage for probability theory if one were able (apart from the fact whether this is *a priori* possible or impossible) to found it according to a viewpoint of the von Mises-Kamke’s type, that is one based on the frequency-limit? One could start doubting that what is found are flaws, flaws which annoy the pedantic purist, but which the practical man cannot avoid surpassing, while waiting for somebody else to deal with them, in order to enjoy the important and persuasive results. In the same way it is understandable that many do not wish to give up the integrity of Cantor’s suggestive construction, in spite of objections on the validity of Zermelo’s principle. If we were in a similar case, all the criticism so far made would certainly have less impact, while still

keeping the same theoretical value. Is this the case?

We will see it is not: precisely we will see that consideration, determination and knowledge of the frequency-limit are neither necessary nor sufficient in any reasoning about probability, and that its unique function, as its supporters express it, is to shift the reasoning from probability theory to a formal area, linked to it by incorrect considerations, but in such a way that, anyway, by repeating such incorrect considerations either in the formulation of the problem or in the practical interpretation of the results, it leads to the correct results that one would have obtained directly. Substantially one ends up arbitrarily substituting the concept of probability with that of frequency-limit: the calculations on the frequency-limit are carried out, and afterwards one returns to the interpretation of the frequency-limit as probability. But neither the first, nor the second step are right, and only the fact that the two steps are one the opposite of the other induces a reciprocal correction and leads to correct results, provided that on the frequency-limit operations have been made which could have been made on the probability. Now, the operations which are required in order to deal directly with the probability are sum and multiplication (theorems of total probability and of conditional probability); because the related theorems are also valid for Cesaro's summation, it is certain that formally one reaches the same results for all the problems which can be formulated without contradiction, but equally certain that nothing more can be obtained. Hence, nothing justifies a preference of the system under discussion from the viewpoint of the mathematical possibilities of the development of the theory.

Does it at least clarify the meaning of the problems? Perhaps to someone it will appear of great value to translate probability theory into a mathematical theory which does not explicitly call for subjective concepts. But it is obvious that there are two cases: either I limit myself to develop the formal theory of the relationships which will be necessarily verified by the frequency-limit, a theory which does not involve contradictions but is irrelevant for all practical purposes, thus, for a purely formal layout, I can find clearer and more suitable schemes, as the one of Cantelli about which we will soon speak. Or else, instead, I believe it is possible to forecast the frequency-limit and then, not only will remain within the subjective field, but I will fall in an area which is subjective and incomprehensible at the same time, because I will not have to judge any more the likelihood of a practically demonstrable event (such as, for instance, the frequency on a finite number of trials), but of an imaginary event which is practically uncontrollable and, worse, absolutely indeterminate. Also, what is it that would be determined, provided something can be determined? The frequency-limit in a sequence, in a *Kollektiv*, of which we are practically interested in the first thousand, hundred thousand trials, or even a million or a thousand million, or a thousand billion, but always a finite number. Now, a finite number of events may anyway be altered in a *Kollektiv* without the frequency-limit changing. This means that, to form a *Kollektiv* we could formally have  $n$  events (trials) of real interest, followed by other events, from the  $(n + 1)$ -th onwards, of an arbitrary *Kollektiv* which has frequency-limit equal to any preset number  $p$  that has nothing to do with the probability of the trials which are of real interest. But could it be said that a *Kollektiv* cannot be formed by grouping arbitrary events, but only events of the same nature? This restriction is so vague that it merits more distrust than the intuitive concept of probability. But let it be so. One will admit that

observations on mortality regard events of the same nature; let us then form the *Kollektiv* where we will consider, in a chronological order, all those born in a country and let us observe if they are still alive at one year of age. Let us assume that the mortality rate is indefinitely improving (it tends to zero); the frequency-limit will be zero. Anyway, for a good number of years, mortality might even be high and nobody, for instance an insurance company, can ignore it, nor will be able to base himself on the frequency-limit, even though he accepts that is impossible to know with certainty that it is null. Then, perhaps, one would like to restrict the *Kollektiv* even more: one can say that children born each year form a different *Kollektiv*; and if they unfortunately are born in a finite number, this inconvenience must be corrected by resorting to an infinity of other imaginary born people among which an infinite number will reach the age of one year and some (still an infinite number) will not, and by asking them to arrange themselves in such an order as to give the frequency-limit a meaning that, in my opinion, does not differ from probability in an intuitive sense and that certainly is not defined by the reasoning on the basis of the imaginary born people. But can it be said that the probability in a *Kollektiv* must keep constant in every single trial? I believe there is no other way out, and this would mean the total collapse of the system. Because if I speak (obviously in the intuitive sense) of the probability of the single trials, it means that all the elaborations made in order to define probability as frequency-limit, by getting rid of the intuitive concept, have failed. As we wished to demonstrate.

4. All that can be said in order to bring the theory developed by Kamke closer to probability theory, is only this: that for a large category of the problems for probability theory (but not for all, as it is shown by the absurdities found and by the ones which could easily be found), by imagining an infinite sequence of similar experiences, one can build up an example of a possible course of results in a way as to obtain a limit frequency equal to probability, for each sequence of similar events.

But then, only a formal scheme will remain and, as a formal scheme, the much simpler one proposed by Cantelli is certainly preferable (as outlined), which represents events using plane figures and their probabilities by the respective areas. Because the essential thing is that the problem of total probabilities exist, each image, where a suitable additive quantity intervenes (bodies and their weights, solids and their volumes, etc.) can be used as a formal scheme of a probability theory: in particular Cantelli's, which certainty is among the most intuitive ones. In comparison to von Mises-Kamke's, it mainly has the advantage of not leaving any possible illusion on the existence of a link which is not the purely formal one with the concept and the theory of probability, and also it represents directly the probability of an event, avoiding complications such as the consideration of a *Kollektiv*. Besides, all the properties, depending on the theorem of total probability, become immensely clear. Less good is, instead, the representation regarding the composite probabilities, because independent events are represented by multipliable areas (such that the common part is equal to the product of the single areas, naturally in relation to a preset unit of measure) and such a property is not geometrically very significant, although it is not obscure nor complicated.

Anyway, as a geometric image for the illustration by a schematic figure of this or that problem, procedure or result, Cantelli's method is certainly advisable, particularly for didactic purpose, but if one wants to make it a system, even here one encounters unsurmountable difficulties. First of all, a geometric figure is made up of points, which are not any more sub-divisible, which would make one think (as in fact one is let to believe by many treatises) that elementary cases exist which cannot be divided any further; while in reality each event  $E$  can always be further divided (for instance, in " $E$  and  $A$ " and " $E$  and  $\bar{A}$ ", if  $A$  is an event that can occur or cannot occur along with  $E$ ). Moreover, even by limiting oneself to the elementary cases (not in the meaningless absolute sense, but in the relative sense of the most detailed partition which is of interest for a particular problem), they could be in a higher number than the points in the plane (for instance, with aleatory functions possible cases have a power higher than the one of the continuum) and then the representation is not possible, while for other simple cases the representation, although theoretically possible, would be so complicated that it would become totally useless (for instance, when the possible cases are the points of a three or more-dimensional space<sup>4</sup>, which, as it is known, is representable on a plane (Cantor), but only in a discontinuous way).

Finally, the notion of area is intuitive for many figures and to others it can be extended through Lebesgue's concept of measure; we can't give a sense to the remaining ones. What will probability be in the second and in the third case? Cantelli admits that in the second case the probability is represented by measure, and he also generally admits that, in analogy with measure theory, the theorem of total probability for denumerable classes holds (an event formed by a denumerable infinity of incompatible events has a probability equal to the sum of the series of probabilities); by this, one prevents the possibility of attributing any probability to the events of the third case (by Vitali's theorem and Kuratowski-Banach's theorem)<sup>5</sup>. Moreover, the possibility of extending the total probability theorem to denumerable classes is under discussion<sup>6</sup> and whichever will be the chosen opinion, it is obvious that the problem cannot simply be eliminated by introducing a geometric representation or by identifying the probability with the measure<sup>7</sup>. On this account it is in fact important to observe that von Mises-

<sup>4</sup> A. DENJOY, *Sur les variables pondérées multipliables de M. Cantelli*, "C.R. Ac. Se.", vol. 196, pp. 1712–1714, 1933. It is sufficient to say that in order to demonstrate the possibility that three or more aleatory numbers are independent, it is necessary to use the properties of Peano's curve! This simply because such an evident fact has its natural representation in a multi-dimensional space, and we want to forcibly transform it into a plane representation.

<sup>5</sup> See P. LÉVY, *Sur quelques questions de calcul des probabilités (IV. Le problème de la mesure)*, "Prac. Matematyczno-Fizycznych", Warszawa, T. XXXIX, 1931.

<sup>6</sup> See, for instances, M. FRÉCHET and the Author in "Rend. R. Ist. Lombardo", vol. LXIII, 1930.

<sup>7</sup> Really, Cantelli tries to give a demonstration of the property under discussion, but this is based on an unmentioned hypothesis equivalent to the theorem itself. Such an hypothesis is explicitly announced by Kolmogoroff (textitGrundbegriffe der Wahrscheinlichkeitsrechnung "Ergebnisse der Mathematik", Vol. II, fasc. 3, 1933), and defined as axiom of continuity; its equivalence with the theorem of complete ad-

Kamke's theory leads to the opposite result and that is to deny the validity of the total probability theorem for denumerable classes<sup>8</sup>, which is in accordance with my own personal opinion. A very simple example will be sufficient: given whatever integer  $n$ , let us denote with  $f(n)$  the difference between  $n$  and the maximum square not larger than  $n$  (hence  $f(n) = 0$  means that  $n$  is a square number,  $f(n) = 1$  that  $n$  is of the form  $m^2 + 1$ , etc.). It is known and it is easy to see that the limit frequency of square numbers is zero, and hence is also zero the limit frequency of numbers with  $f(n) = 1$ , of numbers with  $f(n) = 2$  and generally, of numbers with  $f(n) = k$ , where  $k$  is any specific integer.

The possible cases are  $K=0, 1, 2, \dots$ , each of them has a limit frequency of zero, but the sum of all the possible cases has naturally a limit frequency equal to 1. While, by admitting the additive property for denumerable classes, the limit frequency of the sum of a denumerable infinity of events with limit frequency of zero should equally result zero ( $0 + 0 + 0 + \dots + 0 + \dots = 0$ ).

5. But what would be the aim of a systematic formulation according to a purely formal representation? Cantelli himself says this very clearly: the only aim is of allowing purist mathematicians to apply themselves to the problems important for probability theory, without encountering those suspicions that probability definitions, with related discussions, arise in them<sup>9</sup>. In other words, the aim is that one of teaching probability theory without teaching its meaning, of teaching which are the relationships existing among probabilities without saying what they are and how the probabilities are evaluated; hence the aim is the same one of the axiomatic method, and hence it will be reached better by a definition of probability based on formal postulates. In fact, it is not necessary to eliminate the notion of event which has a precise logical meaning (categorical proposition); it is sufficient to eliminate the empirical notion of probability for the consideration of any function having the necessary properties. Let  $E_1, E_2, \dots, E_n$  be events; generally we could study functions  $f(E)$ , which to any event associate a real number, and particularly the additive ones, such that  $f(E_1 + E_2) = f(E_1) + f(E_2)$  when  $E_1$  and  $E_2$  are incompatible. Formally, the study of the additive functions

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ditivity results from previous researches by O. Nikodym. According to my viewpoint, the so defined continuity is a very interesting property which a probability law may use, but not necessarily has to.

<sup>8</sup> See KAMKE. I incidentally observe that, apart from the followers of von Mises-Kamke's viewpoint, complete additivity is (implicitly or explicitly) denied by the many Authors who consider successions of cases as "equally probable"; this appears explicitly and systematically in Lomnicki's paper (*Nouveaux fondements de la théorie des probabilités*, "Fund. Math.", T. IV., Warszawa, 1923), while for instance, in appendix to his paper, Lévy, after having at first outlined an axiomatic theory of probability including complete additivity, abandons such a restriction in order to discuss examples with a denumerable infinity of equally probable cases.

<sup>9</sup> I quote from Cantelli: "I am interested in dealing with this subject so that, with a lot of tranquillity, one might consider the theoretical aspect of probability theory as a part of pure mathematical analysis not likely to arise any suspicion". "And I feel there is no doubt that this very orientation is the most suitable to satisfy the intellectual requirements by those purist mathematicians who want to avoid any application".



of events represents<sup>10</sup> the probability calculus and this can be understood and applied without suspicion by any purist; empirical problems and discussions are relegated to the time of choosing amongst all the additive functions, the one we consider respondent to the probability problem under consideration. Formally, two events are independent in relation to function  $f$  if  $f(E_1E_2) = f(E_1)f(E_2)$  and similarly all the concepts which are necessary for probability theory can be introduced and all the theory can be developed. The theory takes almost the form identical to Cantelli's, by substituting, as for done in that one, for probability an additive function, whose meaning is not specified (and which could for example be called "weight" or "value" or "measure", *etc.*) but without substituting for events entities of different nature, which is not necessary nor advisable. By doing so, automatically, all the objections are eliminated which are caused by the imperfections in the natural field representation of probability theory, made up of events, by figures, sequences, classes of possible cases or other artificial schemes. One fully responds to the aim of rigorously establishing everything which in probability theory constitutes the formal mathematician theory<sup>11</sup> independent from any discussion on probability.

6. Having followed a natural road, without abandoning the very field, made of events of probability theory, we have the advantage of being able to naturally move into the concrete field, instead of having to start again from the beginning on totally different bases and through a completely different road, as we would have been forced to do if the established formal theory would only be an isolated and artificial build-up which was empty of actual relationships with the probability field.

By keeping within the appropriate field, nothing will have to be modified in order to move on to the actual study of probability; it needs only to name func-

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<sup>10</sup> Under the non-essential restriction that  $f(E) = 1$  when  $E$  is certain (choice of the measurement unit for the probabilities).

<sup>11</sup> The merit of having based probability theory on logical calculus dates back to Boole (See for instance, P. MEDOLAGHI, *La logica matematica e il calcolo delle probabilità*, "Boll. Ass. It. Attuari", n. 18, 1907, and its references), and the merit of having tried for the first time a system of postulates in order to make the foundation of probability theory, to Bohlmann (*Die Grundbegriffe der Wahrscheinlichkeitsrechnung*, "Atti IV Congr. Mat.", vol. III, Roma 1908). But they did not intend the aim of their researches in the direction developed above, as to separate the characterisation of all the possible probability functions (additive functions) from researching what one wants to pre-choose, instead they thought of always announcing some properties of probability (as an intuitive notion or anyway as one already considered definite). Similar is Kolmogoroff's position in the quoted book which undoubtedly represents the best summary report of probability theory according to modern views. Nearer to my viewpoint are the Authors who show to conceive probability theory as a special multi-valued logic; such an orientation, the idea of which was already present in the logical works by Lukasiewicz (for instance *Philosophische Bemerkungen zur mehrwertigen Systemen des Aussagenkalküls*, "C.R. Soc. Se. Varsovie", XXIII, 1930), and which is now widely spread, has found a beginning of concrete development in a recent short note by Mazurkiewicz (*Zur Axiomatik der Wahrscheinlichkeitsrechnung*, "C.R. Soc. Se. Varsovie", XXV, 1933).

tion  $f(E)$  as “probability of  $E$ ”, naturally after having introduced - whichever way one prefers - the notion of the probability of an event, and after demonstrating that probability (or better its numeric measurement) satisfies the hypotheses formulated for  $f$ . Now, two different ways can be thought of in order to understand and carry out such a transition to the probabilistic interpretation: the first, which could be called rigid, consists in defining (by whichever way and whichever concept) the probability of an event, thus univocally determining a function  $P(E)$  which will be a particular one among functions  $f$ , on the basis of theorems to be established later on; the second, which on the contrary could be called elastic, consists in demonstrating that all functions  $f$  have the necessary and sufficient properties to represent not-intrinsically contradictory probability evaluations (here also defined according to whichever concept and by whichever road), leaving then to a second (extra-mathematical) phase the discussion and the analysis of motives and criteria for the choice of a particular one amongst all these possible evaluations.

Although nothing prevents *a priori* to follow one or the other of these two roads, whichever is the probability concept one wishes to embrace, still there is no doubt that they introduce themselves on a different light, depending if the probability is considered as an objective notion or a subjective one; if one follows the subjective concept, the second way of proceeding is not only the most natural one but, with its differentiation between characterizing the allowed evaluations and choosing a particular one of them, it responds to a precise essential need. If the subjective concept is followed instead, the first will be the most natural procedure, as on first judgement, and only the requirements of a deeper analysis will show the advantage of the splitting caused in the second. To the follower of this case, the splitting will have the same methodological meaning as the geometric distinction between projective and metric: to develop a theory, at first independently of one of the intervening notions, thus to obtain a more general build-up, pointing out what really depends from the neglected notion, what is independent of it and how and which level of arbitrarily might be introduced afterwards.

In our case, it is a matter of separating those which are the formal abstract laws of probability theory from what, instead, has a relationship with the single real cases, and that is the probability evaluation case by case. Such a separation is quite natural, as in logic it is natural to separate the study of formal laws from the analysis of the meaning which a single phrase has and the statement that it is true or false. For instance, formal logic has not got the task of saying if it is true or false that Socrates is a man or a tree, and that men are mortal and trees are snakes; but only of dispassionately leading us to conclude that if Socrates is a man and men are mortal, then Socrates is mortal, while if Socrates is a tree and trees are snakes, then Socrates is a snake. Logic cannot, and should not, judge or discuss about the greater or lesser validity and likelihood of one or the other opinion: it teaches each individual how to draw from his own opinions, of which he alone is responsible, those consequences that, in order to keep coherent with himself, he must recognize as true. Woe to logic, if before starting some laws, it would land itself in a fix of philosophical discussions to find out if and when it makes sense to say that an opinion is right or wrong! Also in probability theory it is certainly equally worth to develop the study of formal laws independently of difficult and complex discussions about the meaning of probability.

7. At this point what will appear more obscure is by which way it will be possible to establish the formal laws of probability theory before giving a criterion for evaluating probabilities; naturally, nothing can be obtained without admitting anything, but there are some properties, which can be expressed and which are well comprehensible and also *a priori* justifiable, which allow to distinguish formally acceptable probability evaluations and intrinsically contradictory probability evaluations and which allow the identification of the first with the previously considered additive functions.

Naturally, one can proceed in many different ways: the most elementary and intuitive one undoubtedly consists in considering the coherence problem for the determination of betting ratios. Let us assume that an individual estimates at 20, 30, 70, generally  $p$ 100 liras, the cost of one bet giving a gain of 100 liras if a specific circumstance or event  $E$  occurs: we will say that 0.20, 0.30, 0.70 and generally  $p$ , is the betting ratio decided by the given individual for event  $E$ . On the basis of such betting ratio let us assume that he is willing to bet, either for or against, whatever ratio (for the purpose of the discussion the practically necessary limitation of not surpassing a maximum amount is indifferent, so it is better to leave it out): having set the ratio in 0.30, in case event  $E$  occurs, he will indifferently accept to promise 10, 100, 1000 liras to the person who pays him 3, 30, 300 or inversely to pay 3, 30, 300 liras to the person who agrees to pay him 10, 100, 1000, always in the considered case.

Regarding these betting ratios we can consider two problems: a mainly mathematical one, consisting in researching if they can be arbitrarily set, or if conditions exist, and which ones, which have to be satisfied in order to set them avoiding inconsistencies; the other one of an empirical-psychological nature, consisting in examining the motives to which we resort while setting them. Evidently, one finds an inconsistency if the betting ratios are set in such a way that a competitor, by combining together in an opportune way some bets, can ensure himself of winning: if, for instance, we have three chances (let us say: winning, even score, losing, in a specific competition of any type), and if the betting ratios are set as 0.40, 0.20, 0.30, a competitor can make 100 liras for each of the three cases, that is receive for sure 100 liras, having paid out  $40 + 20 + 30 = 90$ ; if instead they are set as 0.50, 0.20, 0.40, he might make  $50 + 20 + 40 = 110$  liras towards a commitment of paying 100 in each of the three cases and that is, finally, receive 110 for paying 100; only if the three amounts add up to unity, such an inconsistency will disappear. This reasoning is general and from this the theorem of addition of betting ratios can be deduced: that is it can be demonstrated that if an event is the sum of two or more other incompatible events, the betting ratios of that one must be put equal to the sum of the betting ratios of these.

But what do these betting ratios represent? Undoubtedly, they somehow measure the confidence level felt by the individual who set them on corresponding events taking place: the greater is such degree of belief, the greater will be the betting ratio that will be particularly set close to zero for events judged as practically impossible, close to 1 for events judged as practically certain. For those who follow the subjective viewpoint, the (subjective) probability is only such a degree of belief, and the theory, which has been outlined for the betting

ratios, is immediately interpreted as probability theory<sup>12</sup>. For those who resort to an objective viewpoint, it will anyway appear as plausible, whichever way he wished to conceive or define probability, that an individual might resort to such a probability for the determination of betting ratios, without lending himself into some incoherence, that is to say, without lending himself into some incoherence, he might associate to an event a higher betting ratio the higher its probability is: if this were not true, it would mean that it is impossible to follow as a general line the criterion of expecting an event with the more confidence the more it is probable, and surely such a conclusion would be unacceptable by everyone. But, if such a conclusion is excluded, it must be admitted that the betting ratios can be defined as function of probability and indeed they supply the most practical and convenient way for the measurement of probability<sup>13</sup>. And thus it follows that the addition theorem, demonstrated for the betting ratios, must likewise be valid for probabilities, in which area it is known as the total probability theorem.

In such a way, independently of any possible definition of probability, the fundamental theorems of probability theory can be established, and the more general class of the formally allowed "probability evaluations" can be characterized: all and only those who have the additive property.

8. How can probability be evaluated? This is the only question open to discussion, for those who so far accept the previous reasoning. The fact that such a question is open to all the answers inspired by all different kinds of viewpoints shows that what previously said can actually be accepted by everybody without him being forced to abandon his preferred opinions in advance.

But, the reached results have the healthy effect of devaluating and making of secondary importance those discussions which, until they constituted prejudicial cases for the very discussion of probability theory, represented rather serious difficulties and arose incurable disagreements. Now, instead, all possible and imaginable definitions of probability are only different practical criteria for the actual evaluation of probabilities and, indeed, one might as well adopt an eclectic viewpoint resorting for each particular case to the concept which is better suited to each of them. Thus, for the urn and game problems, one can rely on the consideration of equally probable cases, in statistical problems on the observation and forecasting of frequencies, and so on.

Now one will already be substantially in agreement with all my ideas<sup>14</sup>, if he makes the simple observation that the whole complex of reasoning rules regarding the probable and constituting probability theory is applied with equal right and equal rigor, either when probabilities are defined according to any objective criteria or when they only have the subjective value of a betting ratio. He will

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<sup>12</sup> B. DE FINETTI, *Fondamenti logici del ragionamento probabilistico*, "Boll. U.M.I.", anno IX, n. 5, 1930; *Problemi determinati e indeterminati nel calcolo delle probabilità*, "Atti Soc. It. Progr. Se.", vol. II, Congr. Bolzano-Trento, 1930.

<sup>13</sup> B. DE FINETTI, *Sul significato soggettivo della probabilità*, "Fund. Math.", T. XVII, Warszawa, 1931.

<sup>14</sup> B. DE FINETTI, *Probabilismo*, "Bibli. Di filosofia diretta da A. Aliotta" Perrella, Napoli, 1931.

also be in agreement if he will conclude that it is an unnatural and arbitrary restriction defining as probability calculus the probabilistic reasoning for cases where that certain preferred evaluation criterion is applicable and excluding the same type of reasoning when it is applied to all the cases of life which refuse to submit to the difficulty of a too narrow evaluation. Again, he will be in agreement also if he will abandon all the aprioristic prejudices against the subjective probability concept. In fact, only the discussion would remain if probability may sometimes have an objective meaning or if it has always and only a subjective meaning (as I maintain), even if in some cases there are reasons which are so spontaneous that they make all common-sense people agree with the evaluation. But this would not be any more than a philosophical question, one devoid of interest for concrete applications.

The important matter is to free oneself from those special and unilateral views which force probability theory to whither in a narrow field without breathing space or life, as if it were unable to give us its help for all the cases where we have to base ourselves on forecast and conjectures, as if it were an artificial creature alien to our instinctive faculties, as if it did not have the necessary strength to dare into the open sea of reality.

#### SUMMARY

As it has been already done for the Corrado Gini's works, STATISTICA has accepted the invitation of some researchers who asked to republish the English translation of some fundamental De Finetti's papers. These works were translated into English and published in the volume: Bruno De Finetti, *Induction and Probability*, Biblioteca di Statistica, eds. P. Monari, D. Cocchi, Clueb, Bologna, 1993. On the Probability Concept is one of the first fundamental philosophical papers in which we can find the essential basis of De Finetti's subjective approach to the Theory of probability.

*Keywords:* Prior probability; posterior probability; exchangeability; statistical inference.