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PROBABILISM*

A Critical Essay on the Theory of Probability and on the Value of Science

> Therefore his concern was to teach the prudence and tricks by means of which one can succeed in formulating propositions that have a meaning
>
> Giovanni Papini[1]

1.

"Truth no longer lies in an imaginary equation of the spirit with what is outside it, and which, being outside it, could not possibly touch it and be apprehended; truth is in the very act of the thinking thought. The absolute is not outside our knowledge, to be sought in a realm of darkness and mystery; it is in our knowledge itself. Thought is not a mirror in which a reality external to us is faithfully reflected; it is simply a biological function, a means of orientation in life, of preserving and enriching it, of enabling and facilitating action, of taking account of reality and dominating it."

For those who share this point of view, which is also mine, but which I could not have framed better than with these incisive sentences of Tilgher's,[2] what value can science have? In what spirit can we approach it?

Certainly, we cannot accept determinism; we cannot accept the "*existence*", in that famous alleged realm of darkness and mystery, of immutable and necessary "*laws*" which rule the universe, and we cannot accept it as true simply because, in the light of our logic, it lacks all meaning. Naturally, then, science, understood as the discoverer of absolute truths, remains *idle* for lack of absolute truths. But this doesn't lead to the destruction of science; it only leads to a different conception of science. Nor does it lead to a "devaluation of science": there is no common unit of measurement for such disparate conceptions. Once the cold marble idol has fallen in pieces, the idol of perfect, eternal and universal science that we can only keep trying to know better, we see in its place, beside us, a living creature, the science which our thought freely creates. A living creature: flesh of

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our flesh, fruit of our torment, companion in our struggle and guide to the conquest.

Nature will not appear to it as a monstrous and incorrigibly exact clockwork mechanism where everything that happens is what must happen because it could not but happen, and where all is foreseeable if one knows how the mechanism works. To a living science nature will not be dead, but alive; and it will be like a friend about whom one can learn in sweet intimacy how to penetrate the soul and spirit, to know the tastes and inclinations, and to understand the character, impulses and abandonments.

So no science will permit us say: this fact will come about, it will be thus and so because it follows from a certain law, and that law is an absolute truth. Still less will it lead us to conclude skeptically: the absolute truth does not exist, and so this fact might or might not come about, it may go like this or in a totally different way, I know nothing about it.

What we can say is this: *I foresee* that such a fact will come about, and that it will happen in such a way, because past experience and its scientific elaboration by human thought make this forecast seem reasonable to me.

Here the essential difference lies in what the "why" applies to: I do not look for *why* THE FACT that I foresee will come about, but *why* I DO *foresee* that the fact will come about. It is no longer the facts that need causes; it is our thought that finds it convenient to imagine causal relations to explain, connect and foresee the facts. Only thus can science legitimate itself in the face of the obvious objection that our spirit can only think its thoughts, can only conceive its conceptions, can only reason its reasoning, and cannot encompass anything outside itself.

2.

What is the import of such a total reversal? We must be precise. Too many would be tempted to conclude hastily that this view logically precludes the very possibility of science, and too many others would conclude as hastily that, on the contrary, the whole thing reduces to a subtle philosophical distinction which is interesting for the critique of science, but has no importance for its development.

We can reassure the former quite categorically: our new conception might enlarge, but can never restrict the practical bearing of science. All practical consequences of what, at a certain stage of our knowledge, the old view would consider as "natural laws" are obviously, and a fortiori, events whose happening we expect with practical certainty. All such "natural laws" then keep their value as *laws of thought* for the forecasting of natural phenomena.

Thus they keep the very same practical value for us that they would have if, unaware of uttering a meaningless sentence, we were to say that "they are true".

One could say that my point of view is analogous to Mach's positivism, where by "positive fact" each of us means only his own subjective impressions. A proposition can be said to be "true" if one who asserts it intends to state that the impression he wants to express through that proposition really is an impression of his. To think that it has a value and a meaning of its own, before he gives it the value and the meaning which express that impression of his, is a logical antinomy, like saying "the smallest whole number not definable in less than 1000 words" when, in saying so, one defines it in 10 words, and the other well-known analogous antinomies.[3] "All the objects, men, and things of which I speak are, in the last analysis, only the content of my present act of thought: the very statement that they exist outside and independently of me is an act of my thought: I CAN ONLY THINK THEM AS INDEPENDENT OF ME BY THINKING THEM, *I.E.*, MAKING THEM DEPENDENT ON ME."[4]

But it would be a mistake to believe that this clarification of the subjective and relative value of the concept of "truth" is all that matters. The practical value of the new conception is immense. If we clear the road of all the inexplicable prejudices that have so long obscured and encumbered our freedom of reasoning, boundless horizons open themselves to our spirit. Already in pure mathematics the exact consciousness has been reached that a proposition is meaningful only if and because and from the moment at which we attribute to it, by an arbitrary convention (nominal definition) what we want to attribute to it, and only in this way was it possible for mathematics – as it did in the last century – to reach the most impeccable rigor and to open vast new possibilities of development.

Mathematics, logic, and geometry are now immune to the pseudo-hypothesis (so to speak) of the existence of the world, the existence of an external reality, the existence of a metaphysical reality. I cannot doubt that if all our thought were to rid itself of that embarrassing and mysterious pseudo-hypothesis, it would have everything to gain – in clarity, depth and rigor – in every field, not least in that of the formal sciences, where, by their very nature, the danger of metaphysical deviations is by far the least.

3.

In the world of rationalism, Science had Logic as its basis; by launching an overwhelming attack on Rationalism, relativistic thought[5] cannot, I think, escape between the horns of an iron dilemma: either destroy science, or deny to logic the pretension to shape science. In talking about foresight I have already mentioned my own point of view on the subject: it consists not in giving up science, but in taking a living, elastic, and psychological logic as the fundamental instrument of scientific thought, instead of the ordinary, categorical, rigid and cold logic.

The logical instrument that we need is the subjective theory of probability, and that is what I would like to talk about. Substantively, it is nothing but the purely subjectivistic interpretation of the classical theory of probability, and what we shall say can thus be considered, and be of interest, under two different aspects: as an example of the application of the relativistic mentality to such an increasingly important branch of modern mathematics as the probability calculus, and as an essential part of the new vision of science which we want to give in an irrationalist, and, as we shall say, probabilist form.[6]

There is also a third aspect, that I care a lot about, and, as a mathematician, in a special way, but here we need do no more than mention it. It is the critique of the principles of the probability calculus, which is still far from the formal rigor that all other mathematical fields have already reached.[7]

4.

"Thanks to generalization, each observed fact enables us to foresee a great many; but we should not forget that only the first is certain, that all the others are only probable. No matter how firmly based a prediction may seem, we can never be *absolutely* sure that experience will not refute it when we try to verify it. But the probability is often so high that in practice we can be satisfied with it. It is better to predict without certainty than not to predict at all.

"So in many circumstances the physicist is in the same position as a gambler weighing his chances. Whenever he reasons by induction he makes more or less conscious use of the probability calculus."

So says Poincaré,[8] showing that he has clearly understood that only an accomplished fact is certain, that science cannot limit itself to theorizing about accomplished facts but must foresee, that science is not certain, and that what really makes it go is not logic but the probability calculus.

"On this account all the sciences would only be unconscious applications of the calculus of probabilities; to condemn this calculus would be to condemn science entirely."[9]

That's all very well, and I would not need to change a single syllable in order to express my own opinion in Poincaré's words, so far. But we must go further; why does he stop? Because his point of view, perhaps like any living, intelligent and subtle point of view leads – thought through to the end – to relativism and absolute subjectivism. Horrified by such a conclusion, many stop halfway. Poincaré is one of these. Nor can we say that he failed to understand the subjective value of probability, "that obscure instinct", as he says at one point, that "we cannot ignore". But he doesn't seem content to consider it for what it is; he seems to want to give it an objective value, not only to meet certain objections that we shall consider below, but because otherwise subjectivism would flood through the probability calculus into every field of science.

If we don't fear that conclusion – if, on the contrary, our conception is based essentially upon it – we shall be able to develop "Probabilism" without preconceptions, to the most extreme consequences: Probabilism, which surfaces and declares itself without yet having reached full consciousness of itself in the passages cited from the great French thinker.

5.

One cannot admit that the concept of probability has an objective value. But let us first clarify the point of asserting that. Having declared that everything is subjective it might seem incoherent to waste words showing that one particular concept is subjective.

From now on we shall use the words "objective" and "subjective" in the sense they have in the empirical conception, which still has a well-defined meaning, even for us. An empiricist would count *facts* as objective; would count as objective propositions that are true or false depending on whether or not a given event happens; and also propositions whose truth and falsity reduce to a pure and simple proof that is imposed upon us. Now while from a formal point of view it makes little difference whether I actually think such facts constitute an external reality or consider them only as my sensations, one thing is indisputable: I always know in what circumstances I must call such a proposition true, and in what others false. My calling it true or false implies nothing about my state of mind, signifies no judgment, has no conceptual value.

To give the name "empirical reality" to those of our impressions that depend exclusively and immediately on our sensations is a linguistic convention which we are free to adopt, and hereafter we shall consider it as adopted. We can call propositions that concern empirical reality "objective". And in practice this definition will have the same value (or at least the same extension) that it would have in the mouth of an empiricist who believed in the reality of this "reality"; the remaining difference is the same as what there is between two English speakers, both of whom know and use the word "moon", but one of whom says that the Germans are mistaken in believing that satellite to be called "Mond", but that, rather, "it is the moon"!

By denying any objective value to probability I mean to say that, however an individual evaluates the probability of a particular event, no experience can prove him right, or wrong; nor, in general, could any conceivable criterion give any objective sense to the distinction one would like to draw, here, between right and wrong.

One might then ask, has probability no value? On the contrary. I have already said that for me it is the primary instrument of our thinking. The contradiction is only apparent, and I will clarify this by stating my ideas more precisely: I will explain in what sense and to what extent the concept of probability is valid even while having no objectivity, and I will explain why it has no objectivity in spite of many authoritative opinions to the contrary.

6.

What do we mean when we say, in ordinary language, that an event is more or less probable? We mean that we would be more or less surprised to learn that it has not happened. We mean that we would feel more or less confidence that it will happen. Probability, in this as yet vague and obscure sense, is constituted by a degree of doubt, of uncertainty, of conviction, which our instinct makes us feel in thinking of a future event, or, anyway, of an event whose outcome we don't know.

Does this instinct obey laws? Why should it? This is a chapter in the logico-psychological critique of the principles of probability theory that we cannot deal with here.[10] I will only mention that in measuring probability numerically and showing that it combines according to the well-known classical theorems, at least three paths can be followed, two of which are inspired by ordinary methods, the third being entirely original. Personally, I find that only the last satisfies me.

No matter how they are demonstrated or accepted, these laws represent the relations that must hold among the values that my instinct (not my caprice) is a priori free to fix for the probabilities of various events, so that there will be no internal contradiction among them.

A numerologist who thinks that 7 and 39 are very likely to come up in a given draw of a lottery and so concludes that the pair 7–39 is also very likely would be reasoning no less legitimately according to the logic of probability than someone who judges all the combinations to be equally probable. For example, someone who evaluates the probabilities of 7 and 39 respectively as 9/10 and 8/10 should necessarily, in order to be consistent, evaluate the probability of the combination 7–39 at no less than 7/10. In this example we have considered a state of mind – that of the numerologist – which seems ludicrous. We do not hesitate to call him crazy, and to say that anyone who assigns different values to the probabilities of different numbers lacks common sense.

I fully agree, but I do not require that this, my state of mind, however deeply rooted in my instinct, and however universally shared, has any hidden meaning or positive value. If someone tells me that the most delicious drink in the world is castor oil I certainly shudder with horror at his bad taste, but what would I mean by saying that he is in error? If someone draws a house in perfect accordance with the laws of perspective, but choosing the most unnatural point of view, can I say that he is wrong? Well, this second example can be translated into an actual geometrical interpretation of the problem at hand. If we imagine an appropriate geometrical representation of the various logically possible cases, the various internally consistent opinions about their probabilities can analogously be considered as all the perspectives obtainable by varying our point of view.

Every judgment of the probabilities of different possible events depends on the logical relations connecting them, but varies infinitely with the variations of the points of view that instinct can determine. Similarly, the look of an object is constrained by the fact that it has a shape of its own, but varying according to the point of view, and every point of view is, a priori, equivalent to every other. Anybody's taste will guide him to his best choice, and there might be cases where the aesthetic taste of most or even all people will agree more or less exactly on that choice. And in fact we see thousands and thousands of identical photographs, all showing some monument in the same perspective. But would it be legitimate – or, better, convenient – to interpret a free coincidence of tastes and opinions as the expression of an arcane metaphysical truth?

7.

But – one might object – isn't probability by definition the ratio of favorable to possible cases? What's subjective in that?

It is well known, though, that the cases must be *equally probable*, and if I am supposed to know what it means to say that two cases are equally probable, I have already overcome the conceptual difficulty in the definition of probability. I don't mean that the classical definition contains the vicious circle that many have seen in it. I only say that it cannot define or even clarify the concept of probability, but can at best introduce, after the concept of probability has already been acquired, a useful but conventional criterion for the numerical representation of probabilities. Its task and value are purely mathematical and formal, and there is no reason for us to deal with it here, just as it is of no importance in the analysis of the sensations of heat and cold whether they are measured in centigrade or fahrenheit. What it is interesting and also necessary to analyze in the classical definition of probability are the reasons, at first glance quite convincing, why the identification of "equally probable cases", and hence probability, should have, at least in certain contexts, an objective value. So the 90 numbers of lotteries, and 6 sides of a die, the 40 cards, would really be equally probable cases, because they are produced in circumstances which are really equal, so that we can rule out the possibility of causes which make such probabilities different.

Is such a statement meaningful? We might remark that it is meaningful for those who already know what "probable" means. One who, not yet knowing, considered it as a postulate (which in any case would be difficult to make sufficiently precise) would be able to build on it a sterile algorithm, of which he himself would not understand the rationale. For one who would give the word "probable" an objective value our statement would not be a principle but only a theorem or an experimental truth.[11] Would someone who knows what "weight" means accept as a principle the statement, "Two iron balls of equal volume have equal weights"?[12]

Only if it showed that the subjective concept of probability "corresponds to something objective" once it has already been acquired would the statement have an essential role.[13] This is one of the commonest ways of proceeding and is expressed with admirable clarity by Lévy,[14] who, perhaps more than anyone else, appears to be concerned with the questions that inspire my critiques.

I assign an equal probability to any of the 90 numbers of a lottery, i.e., on subjective grounds, I do not feel a more confident anticipation of the drawing of certain numbers rather than others. Am I right? Am I wrong? Would I be wrong if I were more confident about the numbers a numerologist suggested to me?

It will be said: it is absurd to attribute different probabilities to equal cases. But in what sense are these cases *equal*? It's certainly not logical identity that's in question, the only thing that would permit the a priori deduction "If A and B are identical cases, A's probability is the same as B's".[15] There are differences among the cases, e.g., for the lottery, two different balls are different at least because they have different numbers and because, at the moment of the drawing, they occupy two different positions in the urn. Why don't we consider these circumstances?

The question will seem naive, and it will be thought sufficient to answer that the difference of the numbers is not a *cause* which influences the drawing, and that the position of the ball, even if one admits as reasonable that it might have some influence, is not known, and we cannot take it into account. "*Equal*" cases are then only cases that differ in respects that are either unknown or causally unrelated to their happening.

8.

So it seems that the concept of probability is relative: the fact that two cases appear equally probable depends on the circumstances, known or unknown.

"One can bet, in heads or tails, after the coin, already tossed, is in the air, so that its movement is determined. One can also bet after the coin has landed, on the sole condition that one does not see on what side it has landed. Probability does not lie in the fact that the event is undetermined (in the more or less philosophical sense of the term) but only in our inability to *predict* what possibility will take place, or to *know* what possibility has taken place."[16]

This is what gives probability its essentially relative character, destroying the myth of a *true* probability, existing in the "realm of darkness and mystery" of ultrasensible reality, overthrowing a kind of semi-determinism that considers two equally probable cases as two cases in which nature is still free to choose, and which, having no feature that would make one preferable to the other, puts nature in the terribly embarrassing situation of Buridan's ass.

It seems impossible, but this is what someone thinks: probability depends on the fact that an event is not yet "decided".

"What is the probability that it will rain tomorrow?" asks Bertrand[17] – "It doesn't exist. Not because it changes from one day to the next with the state of the sky and the direction of the wind; but because in no circumstances has it an *objective* value, the same for all who evaluate it without making mistakes.

"It will or will not rain: one of the two events is certain *as of now*, and the other impossible. The physical forces the rain depends on are as well determined, and obey laws as precise, as those that rule the planets."

But what is an objective probability for Bertrand? It is a probability that has "an objective value independent of known information and the good judgment of those who use it"![18]

This concept is intrinsically nonsense. "If we weren't ignorant there would be no probabilities; there would only be room for certainty."[19] Probability exists for me only as a function of the degree of ignorance in which I find myself at the time; it would be absurd, even if it were not meaningless, to consider probability as a mysterious and unreachable metaphysical entity, existing in abstraction, on which the occurrence of an event somehow or other depends.

9.

The probability of an event is then relative to our degree of ignorance; but one can still think that it has an objective value in a certain sense. It is possible to think that someone who knows a certain well determined group of circumstances and does not know the rest logically ought to evaluate the probabilities, at least of certain events, in a well determined way.[20]

If the distinction between known and unknown circumstances is clearly relative – relative to our degree of ignorance – one can still attribute objective meaning to the distinction between circumstances that can and cannot stand in a causal relation to the occurrence of a certain event. This will let us say, if not that the different cases we enumerate are equally probable in an absolute sense, at least that they are equally probable in relation to known circumstances. Then we would say that two cases which do not differ in any known circumstances that would influence their occurrence are equally probable, and everything will be all right – if we manage to explain which are the circumstances that can have an influence.

But let us examine our conscience, and see when it is that we admit that a circumstance can influence a certain event. Isn't it precisely when knowledge of it influences our probability judgment? Do we mean anything more than that? No matter what we say or think, in the end we come to this: the concept of cause is only subjective, and it depends essentially on the concept of probability.

I observe a conjunction of events and I wonder whether it is casual or causal. What do I mean? If I am talking about the past, I only mean: is it suggestive to note this fact, or not? Does it serve to clarify my ideas, or not? Does it touch my imagination or not? But in this case the essence of the idea of cause escapes me entirely: it only shows itself when I pass from what is already known to predicting the unknown, when the factual data affect our state of mind, when from the easy science of hindsight we want to get a rule of action for the future.[21]

Suppose it to have been observed that many times, after an eclipse there is a war. Why don't I say that the eclipse is a cause of war, and why do superstitious people believe it? And why do we call them superstitious? In saying that the eclipse is not a cause of war I mean that, if tomorrow I see an eclipse, the outbreak of war will not therefore seem more likely to me than if an eclipse had not happened.

One who says that the eclipse is a cause of war would mean that for him, on the contrary, after an eclipse he would see the threat of war as imminent. I call him superstitious because his state of mind is different from mine and from that of the society to which I belong, because it clashes with the conception of the world which is the innermost part of my imagination and of the imagination of my century. But if I want to strip away the part of my thought that is my own creation, if I want to distill from my opinions the objective part, i.e., the part that is purely logical or purely empirical, I will have to recognize that I have no reason to prefer my state of mind to that of a superstitious person except that I actually feel the state of mind which is mine, while that of a superstitious person repels me.

The example I gave is an extreme case, and it might seem paradoxical. But there are infinitely many others where it would not surprise a contemporary if I said that I do not know how to tell whether or not a causal relation exists; there are infinitely many cases that daily give rise to such discussions. I expressed my opinion: the concept of cause is subjective. Whoever wonders whether or not to accept a causal nexus, and wants to find truth through physical experiments and logical deductions, reaches his aim as if he were throwing darts in the dark. We should not look for truth, but should only become conscious of our own opinions. We should not question nature but only examine our consciences. At most I can question nature so that it will give me data as elements of my judgments, but the answer is not in the facts; it lies in my state of mind, which the facts cannot compel but which nevertheless can spontaneously feel itself compelled by them.

If from the idea of cause I deduce a criterion to judge when cases are equally probable, the concept of probability will thereby receive a value which is not only relative, but subjective.

10.

For a long time my ears have had no peace: it seems that a thousand voices advise me and they shout that it is not possible to speak of the probability – at least in an objective sense – of a single event. We must imagine that it is repeated very many times, or even infinitely many times. Then probability acquires an objective sense, because frequency has an objective meaning. The time has come to discuss this opinion.

What do we mean by saying that two events are *trials* of the same *phenomenon*?[22] Even on first analysis it will turn out that this has only a conventional meaning, case by case. Two events are "trials of the same phenomenon" if they belong to a class of events which is characterized by a special name, or if you prefer, to a class of events which it might be interesting to consider as collected in one class. Are the death of Tizio and the death of Caio trials of the same phenomenon? If I consider the phenomenon "death of a person", yes. But Tizio is not only a person; he will also be, for example, a 40 year old lawyer, of Italian nationality, and then if I consider the phenomena "death of a lawyer", "death of a 40 year old", "death of an Italian", "death of a 40 year old Italian", "death of an Italian lawyer", Tizio's death will have all of these denominations and all the others, more or less interesting, that one might want to introduce. And among them there will be many that apply to Caio, too, and also many others that will not apply to him.

The concept of "trials of the same phenomenon" is arbitrary, as is in logic that of "elements of the same class". Any two objects can be put into one class or distinct classes, but among the innumerable classes one can form there are some which are of practical interest and for which it is generally thought useful to introduce a special denomination. The viper and the horse both belong to the classes "vertebrate", "animal with two eyes", "animal with trisyllabic Italian name", but only the viper belongs to the class "reptile", "poisonous animal", "animal with an Italian name beginning with v". In relation to their practical importance, the first of the three mentioned classes is always the most notable, and the last the most useless. But it is only a question of utility and degree; it would be vain to look for a philosophical substratum, and it would be vain to think of the concept of "trials of the same phenomenon" as something meaningful in itself.

The only importance of this is to clear the problem of a metaphysical fog, which is always and everywhere noxious. In speaking of "trials of the same phenomenon" we should not feel authorized, by the simple fact of using this term, to protect some delicate question, taboo, from the probe of logic. That's all.

11.

If I play heads and tails, and I say that the probability of getting a head in a particular trial is 1/2, experience cannot tell me that I was right or wrong, but if on repeating the experiment, e.g., 1000 times, I obtain a sequence of "head" (H) and "tail" (T) in almost equal numbers, I can conclude that the probability is *actually* 1/2. The probability calculus leads to the law of large numbers, and the law of large numbers is confirmed by experience. In this sense the calculus of probability is confirmed by experience and it has an objective value.

This argument is really surprising. What do I get by 1000 repetitions? I get one of the 2^{1000} sequences of 1000 letters H or T. Whether I get the sequence in 1000 trials, letter by letter, or draw it all at once, is a side-issue; the real point is that all 2^{1000} sequences are equally probable. But, no matter what sequence is obtained, how do I conclude that actually all possible sequences were equally probable? Does it make any logical difference if an experiment is made up of many successive trials? Isn't that an entirely external, inessential, superficial circumstance?

Still, I can foresee frequencies almost with certainty. This is an objective fact. And this fact is not trivialized by the paradoxical remark just made, which has a form far from the spirit in which the probability calculus should properly be used so as to show how it fits reality.

But when can I say that observing a frequency *proves* an evaluation of probability? Isn't it only when I accept that I can evaluate the probability on the basis of frequency? What is actually our position?

Let us remember that we don't yet know what probability is, at least in the objective sense. We have shown that to believe in the objective meaning of "a priori" criteria is illusory: they give only subjective probabilities. If one is not happy with this subjective value, but wants to make it objective, one can only think of getting it from "a posteriori" criteria, such as the observation of frequencies. But only if the observed frequency allows us to calculate the probability does this idea have a basis, for only then would one be able to compare the two.

This way of reasoning will immediately make us believe that whoever has the courage to maintain it does not understand probability at all.

But it is inevitable that the negative and destructive phase of criticism, no matter how exhaustive, always leaves one perplexed, until the second phase, the reconstruction, comes to dissolve fears and clarify ideas. There is no error but that contains some truth, which, when the error is attacked, we fear to see buried under the ruins. A humanly understandable fear! But from the ruins, what do we see arising, lofty and shining? The very truth that one has glimpsed distorted by the error, and which now appears in a new form.

So I ask for a bit of patience to let me say one thing at a time, and I hope that at least in this I will not seem unreasonable.

12.

"Generally in a large number of trials the probability and the frequency differ by little." What can we deduce from this statement? Rigorously, given that we don't yet know what probability is, we can only see in it some kind of definition. Furthermore there is the condition that the trials be independent and made under equal conditions, and we return to the earlier critique of "equally possible cases" and the concept of "trials of the same phenomenon". But let us ignore all the difficulties that would make us lose sight of our aim. Let us suppose that the evaluation of probability and the conditions under which the trials were made are such as to satisfy my hypothetical opponent, and let us not stop to note that the very feeling of satisfaction is subjective. What can he conclude? That frequency in a large number of trials is close to probability?

To make this statement susceptible of confirmation or refutation, we should say, e.g., "the frequency in n trials differs from the probability by less than ϵ (in absolute value)". Do we want to say this? No. It is known that we cannot choose an n and an ϵ in such a way as to be able to make such a statement. Nevertheless we can choose n and ϵ in such a way that the condition is satisfied *with practical certainty*. But isn't practical certainty only a very high degree of probability? And then isn't it as subjective as the concept of probability? No, one will say. If a phenomenon is practically certain, it happens always or almost always. If a phenomenon is practically impossible, it happens never or almost never.

But even now, if we want to open such a statement to confirmation or refutation, we must formulate it less enigmatically, in a way that commits us. It is irresponsible to say that a sentence has experimental value and at the same time to make reservations that completely devalue the outcome of the experiment a priori. And not only is it irresponsible: it is nonsense. It is as if, having decided to call the "weight" of a body the force with which it is attracted toward the earth, one were to accept that there might be "weighty" bodies which are not attracted toward the earth.

In the case in question it is obvious that we cannot attribute an objective value to the concept of practical certainty, "because of the contradiction which rules it out". If we were to say that a phenomenon is practically impossible when its probability is such as not to allow it to happen more than once in a thousand trials, or a million, or a billion trials, we would fall into the preceding difficulty: the connection between probability and frequency cannot be expressed with an effective limitation. The trick of considering, instead of a large sequence of trials, a great many such sequences, is just an idle complication of ideas. If, in a large number of sequences of trials, those that give rise to a frequency which is almost the same as the probability are in the great majority, we can gather all the sequences into one, and the resulting frequency will be almost the same as the probability. Instead of changing the formulation it is sufficient to increase the number of trials: all we do is eliminate an excessively restrictive condition, and we have the advantage that we immediately understand, for a reductio ad absurdum, that the trick doesn't give us anything new, doesn't allow any conclusion. Instead, wanting to use the vagueness and the complication that it brings, people have thought of doubling and tripling them, introducing sequences of sequences, sequences of sequences of sequences, and on! To the infinite! A strange pretense of wanting to obtain an objective condition by piling up an infinity of conditions that lack any objective value! Furthermore, independently of the foregoing generic observation, there remains the established fact that introducing a sequence of sequences, or even greater complications, is of less use than taking account of longer sequences.

13.

What objection can there be?

I can think of two opponents. One is a pale, abstract theorist with metaphysical tendencies who would like to transform practical certainty into absolute certainty by making the number of trials grow until we consider a denumerable infinity,[23] and I only need a few words to dismiss him. The other is the practical sort who, in the name of practicality, will deny my right to apply logical reasoning to things that can't stand it. And he has a point, but he is confused, and it is a pity, because otherwise we might agree.

Let's increase the number of trials to infinity. The probability that the frequency and the probability will coincide – to a given degree of approximation – will increase indefinitely towards 1, and 1 is the probability of a certainty. What can we conclude? Nothing. In the first place, the converse is not true: "If an event has probability = 1, it is logically certain." In the second place, if we consider the conclusion, "In every denumerable sequence of trials the limit of the frequency is the probability", we immediately see that it is absurd. For a particular sequence to satisfy it (for values between 0 and 1, including the extremes) it has to contain the infinite subsequences of trials which are all favorable or all unfavorable, and these partial subsequences do not satisfy the condition. The objections to this reasoning are easy to imagine, but even easier to overcome by those who have followed so far. I will just add, in a different vein, that even if one were to grant that the tendency of the frequency to approach a limit were a theorem in the mathematical sense it would be practically useless unless we knew the rate of convergence,[24] which, alone, justifies an approximate evaluation based on a finite number of data, and this is a further absurd supposition.

14.

But let's see what an empiricist would say. He will say that these observations are correct, but they only specify the difference between practical and absolute certainty. So they just carry coals to Newcastle.

On the other hand we cannot help relying on the concept of practical certainty, because all the experimental sciences are based on it. "This slightly unorthodox notion of certainty might offend the pure mathematician who only knows his own science. But anyone who is open minded knows that, outside mathematics, this is the only certainty we can talk about. In the physical world, in everything that matters to life, an event is *certain* when it is immensely probable."[25] The relationship between probability and frequency is an empirical postulate that thousands of years of experience force us to accept, and if there were no postulate and no confirming experience, the probability calculus would have no practical importance.

The probability calculus "cannot foresee the result of an actual experiment until it is made concrete through an empirical observation". The terms in which it is legitimate to formulate a prediction are imprecise and they leave doubts, but "imprecision is inevitable whenever we compare an abstract notion with empirical data".

Thus the probability calculus would be a sort of idealized schema that is suggested by experience and is sufficiently validated by experience for us to think it approximately true in practice. All experimental sciences that use mathematics proceed in this way. They construct a purely logical schema, a world of abstract symbols that does not and cannot have any physical meaning, any positive meaning, until it is explained, in a necessarily imprecise way, what relationship we want to establish between the explicit but empty concepts of the theory and the practical but vague notions of experience. A mathematical theory has no experimental value, it is only a certain concrete interpretation of the symbols occurring in it that can have such value, and can be more or less correct, or, following Poincaré, more or less useful. Such a link between theory and application is always and necessarily imprecise, for in fact empirical notions are essentially imprecise; we can't expect anything else.

Then why be surprised that the probability calculus, like any other positive science, is connected to applications by a rather imprecise empirical postulate? This analogy is perfect: we must "consider probability as a purely experimental notion, the notion of a *physical constant* whose measure we can approach by making actual observation of the frequency of a fact", we must "use the general theorems as rules of numerical calculation to predict future events", finally, we must treat "the theory of probability as a positive science where, indeed, mathematics enters at every point, but which must start from a certain number of factual notions, taken from experience, which are only valid to the extent that they correspond to reality".

Thus Fréchet,[26] who, among those who share these ideas, is most outstanding for the consistency with which he applies them in his own original work.

15.

Can we accept such ideas?

I say no.

The analogy between the probability calculus and the experimental sciences is only apparent, and it is easy to see after even a little analysis that it fails at the most essential point.

What do we do when we introduce an empirical postulate? We state that a certain category of natural facts actually conforms to a certain theory, or at least that the differences are practically negligible, being very small, of little importance, and of minimal probability. Having built a world to our taste by means of a mathematical theory, we assert that the world of our sensations behaves in the same way, to a sufficient approximation.

The function of empirical postulates is nicely clarified by Poincaré in the section of *La valeur de la science* that concerns the critique of nominalism,[27] where he explains briefly and exhaustively the ideas I have just mentioned. For example, what is the value of Galileo's law? According to a nominalist, the value is purely nominal. "When I say that heavy bodies in free fall traverse distances proportional to the squares of the times, I only give the definition of free fall. Whenever this condition is not met I will say that the fall is not free, so that the law will never fail." But, Poincaré rightly observes, "It would have been useless to have given the name free fall to falls that conform to Galileo's law if I did not otherwise know that in such circumstances the fall will be *probably* free or *approximately* free. Then that is a law that can be true or false, but which does not amount to a convention."

If I fire a bullet and I want to predict its motion, what is the bearing of theory? Simply that of noticing, among all the consequences of free fall, i.e., of motion with constant acceleration, those that interest me. It doesn't give anything that I hadn't implicitly admitted at the moment when I assumed that the fall was free. But why didn't I make a different assumption? The reason is empirical. From experience I know that in the circumstances in which I fire the bullet, the fall will be free, or better, it will be *probably almost free*. Doubt and imprecision cannot be eliminated, by the very nature of any empirical postulate. First of all, in practice there are factors we overlook, such as the resistance of the air, that will falsify the result; if we knew of none, and even if we assume that there could be none, we could not state that the law is exact, for it is an experimental law, and no experience lets us reach exactness; finally, even if we had reached exactness, it would still be doubtful that the law that was verified before still holds.

The grounds for doubt are many, but where are they? In the empirical postulate, in the act of linking theory and facts. A mathematician who wants to develop the theory can omit it entirely, saying how facts ought to go whenever certain conditions hold. This was Torricelli's attitude: "It is not important that the principles of the doctrine of motion are true or false, let people imagine that they are true as we suppose, and then let people take all the other speculations which are derived from those principles, not as mixed (i.e., as applied), but as purely geometrical. If the lead, iron, stone balls do not obey this supposed proportion, so much the worse for them, we shall say we are not talking about them."

Then we have a theory that lets us say exactly how the facts ought to go,[28] and an empirical postulate to say that in fact they go almost like that. We only need the empirical postulate for applications; if I only want to know how facts *ought* to go, it is useless.

Does this analogy hold for the probability calculus?

There would be an analogy if, for example, the probability calculus said that, playing head or tail, in 1000 trials 500 give head and 500 give tail, and practice showed that this is almost what happens. Or if, in any other sense, the probability calculus said that a fact ought theoretically to go in a certain way, and we would explain the imperfect obedience of the empirical world to its laws by the fact that sometimes practice belies it.

But it is not so. If the occurrence of a fact, although extremely improbable, extraordinary, and unlikely, were thought to belie the probability calculus, we could not blame practice. Can I say: if nature had studied the probability calculus better she would not have played this trick on me? No! If I were to *know* that nature never produces exceptional, extremely improbable events, only in that case would the probability calculus be revealed as indisputably false.

The analogy is only apparent, then, and very superficial. On the other hand it is sufficient to observe that none of the foregoing critiques of the meaning of probability relate to practical difficulties, to the possibility of actually performing an experiment, to its probative value, to its more or less favorable outcome. They have a purely conceptual content, and this was enough to show a priori, even if I hadn't made it clear, how absurd is every attempt to admit the aforementioned viewpoints into the realm of scientific thought under the false label of "approximate empirical truths", which are exempt from logical rigor.

16.

It may seem that, arguing in this way, I must necessarily arrive at the point diametrically opposite the one I aimed at: instead of giving the probability calculus the great importance I claimed for it, it will seem that I want to deny any value to it. It seems that until now no sophism has been more misleading than this: either experience confirms the probability calculus, and it has experimental-empirical value, that is, objective value, or it does not confirm it, and then it has no value. If, as I claim, it is meaningless to speak of experimental confirmation of the probability calculus, that calculus, it will be concluded, does not say anything, hence it is useless.

This is precisely what I say: that a rationalist or positivist thinker, to be coherent, must come to such a conclusion. So Auguste Comte thought, and, from his point of view, he was right. Probability theory is "the kind of purely speculative and rather vain investigation that satisfies the Byzantinism of certain scholars."[29] But I am neither a rationalist nor a positivist, and thus I can attach value to the probability calculus.

To understand and appreciate the probability calculus, and to use it, is it then necessary, useful and sufficient to adopt the relativistic point of view?

I hope I have shown that it is necessary; and it proves little that famous scientists have dealt with this issue in an admirable way without sharing this opinion. We do not diminish their merit by saying that they have constructed a great theory without properly analyzing its fundamental concepts. This has often happened, and perhaps it is necessary that it should happen. The concept of limit was correctly defined only after centuries in which mathematics was founded on it, and was using derivatives, integrals, infinite algorithms. Does this diminish Euler's greatness?

The doubt will remain that our conception is not enough to explain the value, the power and the successes of the probability calculus. I have already said that the negative and destructive phase of criticism always and inevitably leaves one perplexed, until the second phase, the reconstruction, dissipates fears and clarifies ideas, and I emphasized the extreme importance of this reconstructive phase. We shall develop this more amply and, I hope, exhaustively.

Third point: utility. What are the advantages of my point of view?

In trying to demonstrate that the usual conceptions are untenable, and that the one I want to put in their place is perfectly logical, I already indicate the first and most outstanding reason for usefulness, but it would be a useless repetition to talk about it here. What I want to notice here is the importance of the knowledge and exact awareness of the relative and subjective value of probability, even for those who do not care about critical, logical and conceptual issues, but are only worried about practical issues. Could they really claim to be indifferent to this disagreement?

Well, as far as dice and lotteries are concerned, I can admit that it is of little practical interest whether the notion of equally probable cases has an objective meaning or whether its meaning is just subjective but corresponds to a state of mind so natural as to seem universal. But, in the same way, a mathematician may have no interest in the definition of integral if he has to find the area of a figure for which the intuitive notion of area serves us, or in the definition of limit if it is intuitively clear how the sum of a series can be calculated. Would this be a reason to think it of no practical interest whether the concept of limit only has meaning if a nominal definition is given, and only in virtue of this definition, or, instead, has a metaphysical meaning in itself, and the usual definitions of limit are only methods for recognizing its value in each case? But what is the difference between (1) tranquilly saying, e.g., that the series $1-1+1-1+\cdots$ has no sum in the ordinary sense, because it doesn't converge, but has the sum 1/2 in Cesaro's sense, knowing very well what we mean when we say that, and (2) the vain wandering in the dark that we would be condemned to if we were to ask ourselves, looking at the question as a metaphysical one, whether the sum *exists* or *does not exist*, and, if it exists, whether it *will* be the one obtained by Cesaro's method, or whether such a *hypothesis is wrong*!

In the probability calculus – if we depart, even a little, from the artificial and schematic examples that conceal every conceptual difficulty – it is very often asked whether the probability of an event *exists* or *does not exist*, and the *hypothesis* is made that such a probability has a certain value p, a hypothesis that is regarded as possibly *true* or *false*. All this is meaningless, and this clot of senseless prejudices burdens the probability calculus like a lead weight.

No mistake could be more serious, for as Bacon said, "*Truth comes out of error more readily than out of confusion*". Then even practical people must pay attention to Goethe, who warns us: "Usually man believes, when he only hears words, That this must also give him something to think".

This is a danger from which one is never completely safe: a danger not only to the logical purity of our arguments, which one might not care about, but also and especially to the practical capacity for getting results, which would be paralyzed by it.

We must insist on forestalling a possible attempt at devaluation. In fact, I have no difficulty in admitting that everyone is more or less convinced that the probability calculus has a very special character, in that logic and experience are not enough to give it meaning: common sense plays the most essential role. In the end it may be that I am not really saying much more than that, but I say it with full consciousness of all the consequences that such an admission inevitably entails. And some of these consequences, as we have already indicated, have real practical importance.

17.

Why are we not content to consider probability a subjective notion, as it can only be?

This is no isolated case of logical perversion: it is only a necessary and unavoidable manifestation of a unique disease that I hope relativistic thought will cure humanity of. I say that something is beautiful, just, important. What do I mean? I mean that I like it, that I approve of it, that it interests me. Or else that the majority of mankind like it, that it fits a certain social setting, that it interests the public. And yet how many would be astounded at hearing this stated so plainly! How many prefer to invent grandiose abstractions, Beauty, Justice, as immutable, absolute, universal terms! Small wonder if these same people, aware in practical life of always feeling the sensation of waiting for a certain fact with more or less confidence, have thought it well to invent Probability! Small wonder if in destroying the myth of objective Probability it seems to them that I empty the probability calculus of all content! It is always the same misunderstanding: the one for which Enriques reproaches the mathematical logician for whom "there is no middle way: everything loses value for him as soon as he realizes that it has no absolute meaning. He is like Manzoni's crowd: when it is persuaded that someone does not deserve hanging, it is easily persuaded that he should be hailed in triumph!".[30]

So in the case of probabilities: either they have an absolute, objective value, or they have only a *conventional* value! Either it is meaningful to ask whether the evaluation of a probability is right or wrong, or nothing can constrain our caprice. But we must distinguish between the arbitrary and the subjective, caprice and instinct, convention and opinion! Most of our mental activity, especially in practice, operates on opinions: do we want to say that they are chosen at random and have no value, not even for those who believe them? Or is it necessary, when one is convinced of the value of an opinion, to assume the existence of a posited "absolute truth" conforming to it in order to justify it?

From the beginning and repeatedly I have insisted that one must say only what one knows that he wants and means to say. This premise is enough to prevent us from appealing to mysterious, transcendent, metaphysical entities. To those sufficiently penetrated by the spirit that animates and informs my thought, no more need be said. But it is better, here as well, to go to the heart of the matter, to analyze the points of difficulty for adoption of the new view, and to show their inconsistency.

18.

"A gambler wants to make a bet; he asks my advice. If I gave it to him I would rely on the probability calculus, but I could not guarantee success. That is what I would call *subjective probability*. But I suppose that an observer is there, who notes the outcomes over a long period; when he reviews the record he will see that the outcomes fall out in conformity with the probability calculus. That is what I would call *objective probability*, and it is this phenomenon that we must explicate."[31]

That is a difficulty that leads many into error: how can one not be persuaded – one would ask – that the value of probability is not simply subjective?

In all these cases, in all similar arguments, what is impressive is only one fact: that a practically certain event actually comes about, or it is foreseeable that it will come about. But should we be impressed? When I say that an event is practically certain I mean that I should be amazed if it didn't occur: am I then entitled to be amazed at having guessed, to be amazed that an extraordinary fact whose occurrence would have amazed me did not in fact occur?

Poincaré says that those who are present at the game "will see that the outcomes fall out in accordance with the probability calculus". First of all they would be able to see that *some remarkable and practically certain circumstances*[32] occurred, relative, e.g., to the frequencies, while it is impossible that *all* the practically certain facts have occurred. It suffices to think that it was practically impossible that the particular sequence of outcomes that has taken place would have taken place. Then we must limit attention to just one or a few remarkable and practically certain circumstances. Poincaré says that they will happen. But why does he say it? Because he is certain of it, not absolutely, but practically. And didn't we already have to assume that he was practically certain of it? When I evaluate a probability as very close to 1, I express this sensation: that, almost without doubt, the event will occur. Do I add anything more when I repeat that, almost without doubt, it will occur? Do I have the right to think: first I evaluate a probability, and then I ask myself if I can actually anticipate the event with the corresponding state of mind? This is what many do, and, when they can answer affirmatively, they say that probability has an objective value.

But, when I evaluate a probability, I only express my state of mind, and what does it mean to ask whether I can or cannot have a state of mind which corresponds with the state of mind which is actually mine? If such a doubt corresponds to something which is not meaningless and is actually mine, it was already a part of my state of mind, and I will already have used it in my evaluation of the probability. But once I have evaluated it (and as long as I suppose that my state of mind will not change: if it changes, then certainly I can modify my earlier evaluation!) it is meaningless to think that my evaluation is wrong, because it is meaningless apart from me, it has no other function than to express my state of mind.

Why, when an event appears to me as practically certain (i.e., when I evaluate its probability as close to 1) have I the right to be practically certain that it will occur? Because when I say that an event is practically certain (when I evaluate its probability as close to 1) I do not say nor can I want to say more or less than this: that I am practically certain it will occur.

If it is true that "*opium facit dormire*", can we think it true that "*opium habet virtutem dormitivam*"? This is no less difficult and no less deep a philosophical problem! I leave it to the reader's acumen to see whether the comparison is apt.

19.

It seems strange that from a subjective concept there follow rules of action that fit practice. And Poincaré keeps explaining why the subjective explanation seems insufficient to him, mentioning practical applications in the field of insurance. "There are many insurance companies that apply the rules of the probability calculus, and they distribute to their shareholders dividends whose objective reality is incontestable."

Basically, this is only the preceding case, simplified by the fact that here it is very clear what the "remarkable circumstance" is that one must consider, and it has a very concrete importance: the dividend. We make a budget in such a way that it is practically certain that the gains will be such-and-such. Naturally, it is meaningless to say "practically certain" if I don't say *for whom* they are so; in this case it will be the managers, the actuaries, the shareholders. When an enterprise is sound and has little risk, it is easy to reach a universal or almost universal consensus on this opinion, and there is nothing to be surprised about, since it is exactly because of this that the enterprise is said to be sound and have little risk.

But this is not sufficient: it is not just a feeling of the managers, actuaries, and shareholders, someone will say. You will see that the dividend will prove them right. What does this mean? I mean that this someone shares the feeling, the persuasion, the faith, which the managers, the actuaries, and the shareholders already have. At the end of the year the dividend is regularly distributed. See, that one will say, that certainty was not just my feeling, it must have had an objective ground. But why? If he – even on the basis of a totally groundless conviction – thought it unlikely that there would be no dividend, he would have to find it very natural that there is the dividend, and it would be pointless, unnecessary, and useless to look for an explanation. Least of all for a purely verbal and abstract explanation, like the one that consists of inventing "chance" and "the laws of chance".

But let us look into the function of the probability calculus in the field of insurance.

Whatever enterprise one wants to undertake, whatever firm one wants to manage, one always proceeds by consciously or unconsciously making a budget, in which we equalize the hope of profits and the fear of losses, the hope and the fear that the profits and the losses will be more or less great. We can love risk more or less, we can be prudent or speculative, and our preferences will be different. We could be guided by the hope of a risky gain and risk everything, or we might prefer the modest tranquility of those who feel safe from the tricks of fortune. We are perfectly free with regard to this choice; everyone can do as he wishes. The probability calculus cannot say we are right or wrong: it is true, the concept of mathematical expectation is known, and it is very important, but its task is not (as some seem to think) that of constraining our freedom of choice in this case. The notion of moral expectation has also been introduced, which, besides not solving the problem, is also an artificial and unimportant notion. In any case, we must consider all the alternatives together with their probabilities and their consequences, and then act as we see fit.

In the case of an enterprise that must be secure and have little risk we must act so that, as in the case of insurance, our profits may not be fantastic, but they should be sufficient and practically safe. That's all that non-speculative firms do, without using the probability calculus, and nevertheless this certainty is not too often belied by the facts. And there is nothing strange in this, for an obvious reason: if these forecasts were always belied, we would not make them, and we would act in some other way, and it would be this other way that would inspire us to have greater or less confidence in the various alternatives.

That a fact *is* or *is not* practically certain is an *opinion*, not a *fact*; that *I judge it practically certain* is a *fact*, not an *opinion*. That I should act according to this opinion is only apparently a corollary, because this opinion only exists in that I think I must govern my action in accordance with it.

20.

This very general explanation also holds in the case of insurance. Then why should success in that case support the probability calculus? Why does the probability calculus come in?

Only because the set of circumstances on which the total gain depends is of such a nature that, to evaluate its probability law, it is better to start from an evaluation of the probability of elementary gains. The well known laws of the calculus allow us to combine the individual probabilities and to conclude that, to be coherent, we must attribute a certain probability – e.g., as we may suppose, practical certainty – to the fact of having a satisfactory gain overall.

The only new fact is the intervention of the arithmetical operations of the probability calculus. Is it they that must be shown in practice to be right? No. Because they are demonstrated a priori they are rigorously exact, and have a purely formal value. It would be as if I wanted to show that 1 and 1 make 2 by measuring the length of the sum of two 1 meter segments. But it is only because we agreed to measure the length of a segment so that the additive property holds, and because we know that 1 and 1 make 2, that we can conclude that the segments together measure 2 meters! I have already said that the proof of the formal laws of the probability calculus is independent of any contingency and pretense of objective significance, and we cannot expect anything in this regard to come from experience.

However, if the evaluation of elementary probabilities were made differently, the results yielded by the rules of the calculus would also be different, and it is obvious that it is always the evaluation of the probability, not the calculus of probability, that guides us in a forecast. And then, what remains? It remains that in certain enterprises it can be better to evaluate the probability of favorable outcomes all in a block, to see at a glance whether the investment is secure or insecure, and in others it is better to reach this conclusion starting from an analysis of the individual factors that are in play. There is no conceptual difference between the two cases. Someone who wants to estimate the area of a rectangular field can with the same right estimate the area directly in hectares, or estimate the lengths of the sides and multiply them: reasons of convenience, practicality and custom will make one method preferable to the other, the one that seems to us more trustworthy in relation to our capacity to judge, or we can follow both methods, or try other ones, and consider all of them in fixing our opinion. Are the areas estimated by different methods different sorts of magnitudes? If, once measured, I find that the indirect evaluation is right, can I conclude that this "proves" the "rule" which says: the area of the rectangle is the product of the lengths of the sides?

So practical certainty has the value it has because it means that *I am* practically certain, however I reached this conclusion. That I can reach it by combining elementary probabilities is a fact that can be explained a priori, and needs no confirmation: it is contradictory to evaluate probabilities in a way in which they are not combined according to the fundamental theorems. The success of the concept of practical certainty that guides all of our forecasts, and in particular in economic, commercial and industrial life, is proved as much in the case where the appeal to the notion of probability is unconscious, and directly prompts a sense of full confidence, as in the contrary case in which it is useful to do arithmetical operations on elementary probabilities. The success does not depend on the use of the probability calculus, which is needed only as a formal instrument for the indirect evaluation of a magnitude – probability – which is essentially subjective, as a method to reach a practical certainty which, being a consequence of certain premises corresponding to our state of mind, we cannot refuse to accept as fitting our state of mind, and which has value only in that it actually expresses our state of mind.

Just this is the task of logic. It cannot tell me whether my opinions are *right* or *wrong*, because this has no meaning, but only if they are coherent or contradictory. And the probability calculus is only the logic of our practical convictions, which are subject to a greater or lesser degree of doubt.

21.

We stated that no experience can confirm or contradict the probability calculus, insisting in particular that such confirmation or contradiction cannot be expected from the determination of frequencies. This can seem paradoxical. It will seem paradoxical because it might seem at first glance that then it is not legitimate to evaluate probabilities using experience, experience which ordinarily, as is known, generally consists in observation of frequencies. In statistics, e.g., one proceeds only in this way, and one cannot give up this conviction and this method.

Certainly the mode of reasoning by which it is ordinarily justified is meaningless for us, but this doesn't prevent the conclusions from being right. Usually people argue in the following way. There are various hypotheses about the probability of a certain event, experience shows me which is the most reliable, or, more generally, it shows me the degree of reliability of each of them. In the particular case in which I must evaluate the probability on the basis of the observed frequencies, the case for which, by way of example, we shall make precise the meaning of this sort of reasoning, we argue as follows. Suppose that a certain class of events, which we shall call "trials of the same phenomenon", are independent and equally probable; we can make different hypotheses about the common value of their probability, and, in general, we can suppose that it is any number in the interval (0, 1). Every hypothesis will have a priori a certain probability which we shall think of as known; after having observed the frequency f over a great number of trials, the probabilities of the hypotheses vary (conformably with Bayes' theorem) because, a posteriori, the hypotheses that give p a value close to f are the most reliable. And thus p will be close to f.

It must be noted that many authors have scruples about the explicit use of Bayes' theorem. This is true, but it does not change anything, because conceptually such people always think along the beaten track as we have already explained. They think it inadvisable to put their reasoning in formulas, taking it to be sufficiently justified by good sense. But what is essential in their method remains: the attribution to p of one or another value is a *hypothesis* which experience conjoined with good sense will call more or less reliable. An event has occurred in about half of a large number of trials; why can I foresee that it will occur in about half of the subsequent trials? Because if it occurred in almost half of the trials, the hypothesis that the probability $= 1/2$ is very likely.

But the value of the probability is no factual datum, it has no objective meaning. The sentence "the probability is equal to a given number p" expresses no fact, is neither true nor false, can be no hypothesis, cannot be called more or less reliable. Then there is the condition that the events be independent and equally probable, which in the case under consideration becomes something much trickier than usual, even though the widespread superficiality has not noticed it.

How shall we proceed?

Very simply. In the example at hand we shall turn the argument upside down, as follows. An event has occurred in almost half of a large number of trials; why do I give the value 1/2 (or almost 1/2) to the probability that it will occur in a new trial? Because if in the past it occurred in about half of a large number of trials, then it seems to me that I must expect it to occur in about half of the subsequent trials.

And why do I think that the frequency is nearly stable? There is really almost no sense in this question. Or better, if one asks in this way for an objective reason, a philosophical explanation, an external connection of cause and effect, one asks for something meaningless. Instead, it is meaningful to look for the "because" in a subjective, introspective, psychological sense. Here as in all questions of probability I can try to justify an opinion of mine by showing that it follows from other opinions of mine that seem to me simpler and more immediate. In the case of probabilities that are evaluated on the basis of frequencies there exists a "because" in this sense, and I shall explain it in the following section. But no matter how such an opinion is justified or immediately acquired, it is only because it is actually our opinion that it enters the mechanism of the arguments that lead to the evaluation of a probability on the basis of experience. This is a very simple mechanism, which reduces to the theorems on compound probabilities. What is the probability of the event E after experience has made me know the complex of circumstances A? If, before knowing the existence of the complex A, p is the probability that E and A both occur, and q is the probability that the circumstance A occurs, the probability of E subordinately to occurrence of the circumstance A is p/q.

This extremely elementary theorem, despite appearances, contains all that is correct in the usual arguments. The absurd complications that people love to introduce only serve to make the arguments incomprehensible, wrong and meaningless, without bringing in the shadow of a new idea.

22.

Let us study what is in fact the most practically interesting and also the most carefully analyzed problem: that of evaluating probability on the basis of frequency in a series of "independent and equally probable" events (or of trials of a phenomenon, as one might want to say).

It is easy to strip the problem of all the metaphysical apparatus of "constant but *unknown* probabilities", of "*independent* trials", of "*hypothetical* values of probabilities", and take it in a perfectly meaningful sense. That is what I did in my note "Funzione caratteristica di un fenomeno aleatorio,"[33] and here I shall briefly explain the leading ideas.

From the use ordinarily made of the conceptually meaningless notion of "independent events with constant but unknown probability" it is clear that one is supposed to be able to deduce from it that, if we make n trials and m have favorable results, all possible ways in which the favorable and unfavorable trials can alternate among themselves appear equally probable. For example: we have a coin, and to say it in the old language, "we don't know if it is perfect or favors *head* or *tail*". Whatever the "reliability" of the various "hypotheses" I must think it equally probable, e.g., that the next 6 trials will give any one of the following 15 permutations in which H appears twice and T appears 4 times:

HHTTTT	THHTTT	TTHHTT	TTTHHT	TTTTHH
HTHTTT	THTHTT	TTHTHT	TTTHTH	
HTTHTT	THTTHT	TTHTTH		
HTTTHT	THTTTH			
HTTTTH				

And analogously all of the 6 permutations that contain H once and T 5 times, all of the 20 permutations with 3 H and 3 T, all the 15 with 4 H and 2 T, all the 6 with 5 H and 1 T, are equally probable. Two different sequences of 6 letters H or T can only have different probabilities if they contain different numbers of H's and T's. And, in general, all the permutations containing H m times and T $(n-m)$ times have the same probability of occurring on n predetermined trials; only sequences of n letters in which the proportion of H's and T's is different can have different probabilities.

Now this is a perfectly sensible condition; it might very well be that my opinion consists in thinking these sequences equally probable. Moreover it is also a meaningful and practically interesting condition. This is shown by the fact that often, classes of events that are "independent and with constant but unknown probability" are considered, which, corresponding to a grounded intuition, obscurely and badly expressed, are nothing but classes of events for which our probability judgments satisfy such conditions.

And in fact that suffices for the deduction of the whole theory of a posteriori probabilities in an impeccably rigorous way.

We also find, through advanced processes of analysis (based on the inversion of the Fourier integral), the *formal* justification of the ordinary method, which nevertheless remains meaningless, That is, it is shown that by increasing the number n of trials, the probability that the frequency is in an assigned interval (ξ_1, ξ_2) tends to a well determined limit. On the ordinary view, this limit is the probability of the hypothesis that the constant but unknown probability lies in the assigned interval (ξ_1, ξ_2). Then in this case we have a limit law which is the one that in the old jargon would be called "the law of the probability of the probability".

But this does not alter the fact that, first, the ordinary method is conceptually meaningless; second, that the existence of the limit law on which it is based is an extremely difficult question, and it is not advisable to take it as a starting point, even though we were able to change the statement of it into a meaningful expression.

The hypothesis, or, better, the condition which constitutes our starting point, is instead very clear and simple. From it follow all the conclusions of the ordinary theory of a posteriori probabilities, and particularly those that allow the probability to be evaluated on the basis of frequency. It will not be out of place to repeat that its task is this: to show that our mental disposition to expect the future frequency not to differ much from that of the past – unless the fact of having obtained that frequency appeared to us a priori as unlikely and exceptional – is *justified* as much as it is meaningful to ask for a justification, and is *explained* as much as it is meaningful to ask for an explanation, if we feel that we are in the following state of mind: of judging two sequences of trials which differ only in their order as equally probable.

23.

A fact must be noted that was rightly pointed out by Poincaré and many after him. In the case we have just studied, the conclusion is almost independent of the initial opinion. Whatever the initial opinion (a priori probability) may be – except for a generic restriction – after many trials our state of mind is completely determined by the frequency. Of the two factors, initial opinion and experience, the second has increasingly more influence, and ends up becoming decisive as the number of trials increases.

There are many other important cases of this kind, where the conclusion, i.e., the evaluation of a probability that interests us practically, and which depends on an initial opinion, after due consideration turns out to be almost independent of our initial opinion. Thus Poincaré, in order to conclude that the distribution of the asteroids is presumably almost uniform, must calculate starting from an opinion (he says: hypothesis) about the initial distribution. But to reach that conclusion it was almost useless to make the hypothesis precise. "I don't have much idea what hypothesis to make on the subject of the initial distribution; but, whatever hypothesis I make, the result will be the same, and that is what saves me from embarrassment."[34] Better known, and by now classical, are the problems of roulette and card games.

Naturally, some have seen in this fact a method for giving an objective value to probability. But it will immediately be understood, after what I have repeatedly said, that this criterion, too, can have no function different from the others: to deduce the evaluation of a probability in a complex case from evaluations relative to simpler cases, and arrive at a quantitatively rather precise conclusion starting from merely qualitative evaluations.

This is the character of all useful methods in the empirical sciences: to avoid direct measurement of a magnitude that can be gotten more easily indirectly, and to determine it with possibly greater precision, or, at least, no less precision, than that characterizing the initial data.

This discusssion has brought us to the verge of a new field. So far we have only analyzed opinions that are to be controverted, examined critiques which our conception saw as a priori meaningless, and it was enough to show *why* they were untenable. But the critiques that we now consider have value for us, constituting for us the only point of view according to which a critical examination makes sense, is useful, is inevitable.

24.

What can and must be asked is whether, probability being only our purely psychological sensation, it is legitimate to measure it and subject it to mathematical treatment. I have already said that, entirely within the subjectivistic conception, we can establish criteria suitable for measuring probabilities by means of numbers and can show that they combine according to the well known classical theorems, but it is obvious that, whatever property we use for this purpose, it is presupposed that our state of mind is determined with a precision that will allow us to use it profitably. No matter how we demonstrate the theorem of total probability it will emerge that if the scheme of equally probable cases can be applied (of course, "equally probable" in the subjective sense), the probability of an event is the ratio of the favorable to the possible cases. It is easy to imagine a scale of comparison for the different degrees of likelihood containing all rational values of probability, and, assuming that sensations of probability were precise enough always to allow us to say whether one of two probabilities is greater than, equal to, or less than the other, there would be no difficulty in numerical evaluation. Then the transition from qualitative to quantitative properties doesn't warrant excessive diffidence, but it is in the field of qualitative properties that uncertainty reigns.

Is it more probable that tomorrow it will rain or that Rome will win the soccer championship next year? Is it more probable that when I leave this evening I will miss the train, or that the population of Italy will go over 50 million in the 1951 census? Remember – if there is still any need to be reminded – that probability has only subjective value, and we only ask which of these events can be expected with more confidence. It is not a philosophical problem, we are not asked to justify an opinion: we are asked to recognize whether we have an opinion, whatever it is. But even from this point of view, in which all pseudo-metaphysical difficulties are automatically eliminated, it is hard to answer: hard because our own opinion is generally determined with a very rough degree of approximation, so that a few graduations, such as *most*, *much*, *enough*, *little*, *least likely* seem sufficient to express it in a way that can't be made more precise.

Indisputably, these objections are perfectly well founded, and I don't report them in order to answer them but to put the right light on their value. It is necessary to understand them and take them into account in order not to claim with ingenuous optimism to evaluate a state of mind with mathematical exactness. It is no less necessary to establish the limits of their theoretical and practical value, in order not to hastily condemn the probability calculus.

In the first place we observe that the same difficulty is encountered in all practical cases where a magnitude has to be measured. The approximation can be greater or less, but there is always a certain limit, which is itself not well determined, within which the difference of two measurements is negligible. Hefting them, you can easily feel the difference between two weights of 13 and 11 grams, but a 12 gram weight can't be distinguished from the one or the other. With a balance this will happen on a smaller scale, e.g., for weights of 10.1, 10.2, 10.3; with a more exact balance it will happen for weights of 10.01, 10.02, 10.03 grams, but basically it's the same thing. The fundamental property of the physical continuum would then be[35]

$$A = B, \quad B = C, \quad A < C,$$

but it is obvious that you cannot base a useful algorithm on it, and, if you want to apply mathematics, you must act as though the measured magnitudes have precise values. This fiction is very fruitful, as everyone knows; the fact that it is only a fiction does not diminish its value as long as we bear in mind that the precision of the result will be what it will be. If we suppose, e.g., that the error on each measurement does not exceed a certain limit, we can calculate the limits between which the error of the result will surely lie. But even if we accept that the "exact measure" is meaningless, that not only can it not be determined in practice, but it cannot even be conceived as something with a physical meaning, I will never be able to renounce *inventing*, i.e., nominally defining, the real numbers as logical entities and making use of them in empirical calculations. To go, with the valid help of mathematics, from approximate premises to approximate conclusions, I must go by way of an exact algorithm, even though I consider it an artifice. Since it allows me to take account of the influence that the imprecision or the indeterminacy of the premises can have, it suffices that I consider it, and there is no reason for diffidence.

25.

From this point of view, the probability calculus is actually analogous to an experimental science. The analogy is not illusory as in the case of frequencies, but full and correct.

In the experimental sciences a fictitious world in which quantities have an exactly determinable value is substituted for the world of sensations; in the probability calculus I substitute for my own vague and elusive state of mind that of a fictitious individual who knows no uncertainty in judging his degrees of confidence.

Here it is useful to return to the geometrical interpretation that has already been used (Section 6). The different states of mind would be the different perspectives from which we can see the logical framework of future possibilities; our psychological position would simply be the position from which we view it. Every point represents a different psychological position, and gives rise to a different perspective, to a different opinion. But, both for the geometrical and the psychological problem, it has no meaning to exactly evaluate our position by representing it mathematically by a point. From an empirical point of view the "space", the "continuum" within which our position can vary is constituted, so to speak, by indiscernible partially overlapping regions: it is only because of the exigencies of mathematical method that I resort to the abstract conception of ideally resolving space into an infinity of extensionless elements called points, and consequently interpreting the regions of the space as sets of points.

My position, empirically speaking, is never represented by a point, but by a region of the space; even if I were to say "the position of the center of my pupil" it is clear that I would not define such a position precisely enough so that it would make sense to think of it as determined up to a thousandth of a millimeter. I never determine more than a region of the space, and not even a sharply distinct region, but only a kind of blurred nebula. But while I remain in this order of ideas I will never be able to do geometry: Hjelmslev's ingenious attempts at an intuitive geometry (a circle and its tangent have a segment in common, etc.) just confirm this judgment. I must imagine the nebula ideally resolved into points, i.e., think, in place of my own approximately defined position, of all the mathematically precise positions with which it is satisfactorily assimilable. If I know that I am "near the point *A*", I can in other words examine the perspective which presents itself to me as if "at the point *A*".

This happens under two conditions. That the region of my doubt be not very large, and that a small change in my position does have no serious or interesting consequences for my perspective. We shall next analyze these two conditions with reference to our case.

But in summary let us notice what the task of the probability calculus is. It is that of studying the constraints, the relations, the interdependencies that must subsist among the probabilities attributed by a single coherent individual to various events: all these conditions determine the set of possible perspectives, among which instinct is free to choose. This choice is not forced into mathematical exactness. We do not choose *a* point, but the *neighborhood* of a point. We do not choose *a* perspective but a *certain idea* of a perspective. In drawing a conclusion we have to see whether it would not become defective if the premises were modified within limits that seem acceptable to us. As happens in all the experimental sciences.

Theory gives us an infallible weapon for treating an idealized case. To make any application we must idealize a practical case. Such an idealization can have a certain degree of arbitrariness, and it must be noted whether or not the conclusion depends on what is arbitrary in it.

26.

To these general considerations we must add, as we have already said, some other considerations which more properly refer to the probability calculus. Not because in this case there is any substantially new circumstance, but because the reasons that allow greater precision or impose greater diffidence can vary from one field to another.

In the probability calculus we have a distinctly unfavorable circumstance and a distinctly favorable one. To directly measure a psychological and subjective sensation is certainly a very vaguely determined problem, much more vague than that of measuring any physical magnitude. I do not deny that a few, uncertain graduations would suffice in many cases: the use we make of them in ordinary talk clearly shows this.

But luckily, there is a favorable circumstance not found in any other experimental science: the algorithm of the probability calculus allows one to improve the precision of measurement in a surprising way, by deducing practically precise consequences from qualitative or roughly approximate premises.

It is the opposite of what usually happens: generally the mistakes add, multiply, become gigantic, and we must start from very exact measurements to reach reliable conclusions. If this were to happen in the probability calculus, it would lose nothing as a theory, but in no practical case would a numerical evaluation or an arithmetical calculation make sense, except as a curiosity or as an example.

A mere curiosity and example, just to point out, is the following application of Bertrand's. "If, after having called a doctor, one evaluates at 9/10 the probability that he will come and at 1/3 the probability that he will cure the illness if he comes; without questioning these figures we can note what they imply: the probability that the sufferer will be visited and cured by the doctor is, *for me*, $9/10 \times 1/3 = 3/10$."[36] Theoretically, the calculation is exact; certainly if I evaluate the two probabilities as 9/10 and 1/3, the fact that the evaluation of a probability always has an approximate sense does not allow any doubt that the composite probability differs from 3/10 even by a thousandth. But the difficulty is that the same initial evaluations claim to represent a state of mind with a precision greater than that with which it is determined: instead of saying that I evaluate the two probabilities as 9/10 and 1/3, I will have to say that they are of that order of magnitude, e.g., in order to indicate the degree of approximation I attribute to them I might say that they lie between 0.80 and 0.95 and between 0.25 and 0.40, respectively. By this I would mean that my state of mind is indistinguishable from, is practically identifiable with, is very close to, the one I would be in when anticipating the drawing of a white ball from an urn containing 80 to 95 white balls out of 100 (or 25 to 40 out of 100), when I anticipate the drawing of any one of the 100 balls with the same degree of confidence. Or that, if I were to call a doctor many times in circumstances that put me in the same state of mind that I now feel, I would expect that he would come, presumably, from 80 to 95 times out of 100, and that, in 100 times that he came, from 25 to 40 he would cure the sufferer. And then I could conclude that the probability that the sufferer would be visited and cured is contained between

$$0.80 \times 0.25 = 0.20 \qquad \text{and} \qquad 0.95 \times 0.40 = 0.38,$$

i.e., my degree of confidence would be greater than what I would have in anticipating the extraction of a white ball from an urn with 20% white balls, and smaller than I would have if it were 38% (and I anticipated all of them with the same degree of confidence). Or that, in a great number of repetitions in which I have the same subjective degree of confidence in which I now find myself, it would seem to me likely to find it occurring in 20–38% of the trials.

If all problems were like this, the numerical evaluation of a probability and the application of arithmetic would be practically ridiculous and sterile, because it would be useful to justify only the following intuitive argument: if the coming of the doctor is almost certain, but it is uncertain that he would cure the sufferer, it is really uncertain, or, better, even more uncertain than before, that the sufferer will be visited and cured by the doctor. So it would not be worth writing volumes and volumes on the calculus of probabilities.

27.

But it is not like that.

On the one hand, it is well known in how many problems an exact numerical determination derives from simple qualitative opinions, i.e., from judgments of *equality* of two probabilities. All questions concerning lotteries, card games, drawings, fall in this easiest case.

The relation between a judgment of probability and a forecast of frequency makes it easy, in a great number of problems, to make our judgments numerically precise. And this is the case into which almost all the statistical applications fall.

On the other hand, the algorithms that meet the most essential needs of the probability calculus allow us, in general, to reach remarkably precise conclusions from approximate premises, or from premises that satisfy only easy generic restrictions. This is what we mentioned in Section 23, and the preceding discussion exhibits the importance of such a fact in its true light.

For the rest, if it is true that books and books have been written on the probability calculus and that many problems have been mathematically treated, this means that mathematical treatment has been found useful in many cases. Were those applications inspired by a different point of view? It matters not at all. Those who did them intended probability to have an objective value, which we deny. But those people *felt* that opinion, and we must think that it was quite spontaneous and rooted in their minds if they attached an objective value to it. And this suffices to show that one can feel opinions – naturally, like all opinions, subjective – to which it appears useful to apply the probability calculus in all of its rigor as an impeccable mathematical construction.

We need not reject anything from what has been done in the past. The adoption of the new point of view only conduces to making conceptually precise the meaning of the method and the successive deductions, recognizing their essentially and purely subjective value. Also the usual definitions preserve their practical value, the one based on the computation of cases and the one inspired by the empirical determination of frequencies, along with any other that has been or could be imagined. But they are no longer definitions: they are only criteria that help us in the empirical evaluation of certain probabilities. They will be usable or not, depending on the case, and in some cases more advantageous and powerful than in others. They cannot be taken as definitions because one cannot understand them *before* knowing what probability is, because they do not clarify the psychological value of it, and because, independently of any conceptual reason, they always have a very restricted field of application.

28.

I would now be finished if it didn't seem better to me to append to the general discussion of the meaning and value of the probability calculus an analysis of two points about which there might still be doubt.

An idea which – according to the usual conceptions – springs from the view that probability has an objective meaning, is that a valuation of probability can have a *greater* value in certain cases than in others. For example, drawing a white ball from an urn containing half white and half black balls has (on the usual hypothesis, which we presuppose, of equal subjective probability for the different balls) probability = 1/2; if the urn contains balls of the two colors in the proportion 1 to 2, but I don't know if the white or the black balls are twice the other, and the two hypotheses are equally likely, the probability is still 1/2, and the same happens in general if the probable value of the percentage of the white balls is 1/2, whatever the possible combinations and the respective probabilities are. In all these cases the probability is always 1/2. But in the first case, when we *know* that half of the balls are white, the statement that the probability = 1/2 seems to have a greater value. Naturally it will not be a *more objective* value, given that objectivity can never be; the subjective value, since it expresses that I am equally uncertain between the white and the black balls, does not express anything more in this than in the other cases; but we do feel a difference between the cases, and we have too strong an intuitive feeling to think it a senseless and artificial metaphysical prejudice. How shall we resolve the apparent contradiction?

We can easily and unhesitatingly indicate the procedure: it suffices to analyze how our state of mind differs in the two cases. In what does it differ? It differs in what we might call its *stability*. The *actual* state of mind, considered *in itself*, is really the same, and, analyze it as we may, we shall find no difference. But our state of mind differs if we enlarge the analysis to include our state of mind in relation to other events, from which it might or might not be *independent* and by which, consequently, it will or will not be modified if they occur, or if we learn of their occurrence.

So, if I don't know the composition of an urn, the probability of drawing a white ball is the probable value p of the percentage of white balls, and this statement has the same intrinsic value and the same meaning that it would have if we knew the composition of the urn and we knew that the percentage was equal to p. But there are many circumstances of which our probability judgment is independent in this case, while it was dependent on them in the other case. In case the composition is unknown, my state of mind can be influenced by learning the outcome of the preceding draws, by more or less reliable news and rumors that I might pick up about the way the urn was filled, by the visual impression I might have by glancing inside it, and by other circumstances which could give me information or hints that might increase or diminish my doubts about the to me unknown composition. Whereas if the composition is known, no such circumstances can make me know it better.

But we must not say that in this case the judgment is *stable*, and has an objective value. Stability is *always* relative, because there are always circumstances that I don't know, knowledge of which would modify my state of mind. We have already discussed this in Sections 7 and 8, and it would be useless to repeat it. But it is obvious that in order to exclude the existence of unknown circumstances I would have to know *everything*, and then I would also know whether the event in question occurred or not, or, respectively, if it will or will not occur. And then I could not talk of probability: I could not measure the degrees of a doubt that does not exist.

To feel a sensation of uncertainty, and so to talk about probability, it is necessary that I be *ignorant* of something. Here we can distinguish, relative to my state of mind, *known* and *unknown* circumstances; this we already knew. But among the unknown circumstances it is necessary to make a new distinction – and this is the new conclusion that we shall come to. To fully analyze our state of mind regarding an event, we must distinguish among the unknown circumstances those of which my state of mind is not *independent*. This is what *stability* consists in for that opinion, for that confidence: in the greater or lesser extent, importance and accessibility of the circumstances, knowledge of which could modify that opinion.

It will not be completely useless to notice the purely subjective meaning of this distinction. That two events are judged by me as *independent* means no more and no less than this: that the probability which my state of mind attributes to their co-occurrence (logical product) is the product of the probabilities which my state of mind attributes to the two events.

29.

The other point that must be analyzed is a much-used method which, rigorously, is incompatible with what I say. But this contradiction, even if it is theoretically irresolvable, does not diminish the practical value of this method as a way of approximation, if we keep this approximative character in mind.

In most applications it happens – to say it in the usual language – that one *provisionally* accepts a *schema* (e.g., of drawings from urns) which one takes to be a good *representation* of the *conditions* in which the phenomenon occurs, subject to abandonment if experience *belies* it. That such reasoning has no meaning we have already shown, and repeated several times, but here we are talking about something else. We are talking about the practical and approximative value that a method can have which is developed "as if" one were to reason so.

To fix ideas, take the simplest kind of example, which can nevertheless represent all of the others. I play heads and tails. I make the *hypothesis* that, in each trial, and independently of the other trials, the two alternatives are equally probable. But if the frequency excessively favors "head", I will sooner or later admit that the coin is imperfect, that the probability of "head" is higher, that the "hypothesis" is "wrong".

This evidently has no meaning at all, nor can it become meaningful if we state it in a subjective way. Elsewhere it was enough to say this;

but here, besides the nonsense, there is a contradiction. That in every single trial, and independently of the outcomes of the others, the two alternatives are always equally probable, is a judgment that reflects a state of mind that I can feel or not, but not a fact that I can call *true* or *false*, and it cannot be considered as a *hypothesis* that undergoes experimental verification. If I accept it, i.e., if I feel it, if I find that it conforms to my state of mind, I cannot also accept that I will modify it if "head" appears with excessively high frequency: it is exactly the state of mind that would suggest this modification to me which I deny and exclude myself from having when I say that, for me, for my state of mind, the different trials are *independent*. If the outcome of the preceding trials can modify my opinion, it is for me *dependent* and *not* independent. Obviously. It follows immediately (if a formula can further clarify this already obvious fact) that, in the case of independent trials, even if the frequency of "head" in the first n trials is exceptionally high or low, the probability in the $(n+1)$st trial will still be 1/2, because whatever the number m of repetitions of "head" may be so far, and whatever their order of occurrence, it always happens that

$$\frac{p^{m+1}(1-p)^{n-m}}{p^m(1-p)^{n-m}} = p.$$

If I admit the possibility of modifying my probability judgment in response to observation of frequencies, it means that – by definition – my judgment of the probability of one trial is not independent of the outcomes of the others, and so I really should use for the representation of my state of mind the general theory of aleatory phenomena that I talked about in Section 22. The probability of getting "head" on the $(n+1)$st trial will then depend on the frequency of "head" in the preceding n trials, in the manner given by Bayes's theorem.

In practice one uses, not this theory, but the intrinsically contradictory scheme cited above. Why?

Because an exact method requires an exact analysis of our state of mind, and in many cases that is not worth the trouble. In many cases: but we must notice *when*, *why*, and *to what extent* we can ignore it. In the game of head or tail, if the coin does not seem abnormal, I am almost sure that on many trials the two sides will appear with practically equal frequencies. If this doesn't happen, I feel that I will modify my judgment, and this shows that my state of mind is not the one that judges all trials as independent and equally probable. It means, as would follow from the theory of aleatory phenomena, that I am *a little less* sure of the equality of frequencies.

Let us suppose that the frequencies of "head" and "tail" on the first n trials are almost the same. Under that hypothesis it follows that for every single value of the frequency the posterior probability for the $(n+1)$st trial is always near 1/2, i.e., near the value one obtains from the schema of independent and equiprobable trials. It means that the values provided by that schema correspond with a sufficient approximation to my state of mind any time the frequency lies between certain limits, and that the possibility that the frequency goes outside these limits, beyond which the approximation becomes insufficient, does not seem something to worry about. In the coexistence of these two circumstances is the whole empirical value of the two preceding schemata. It is mathematically absurd to treat probability as independent of the outcomes of trials, and so to suppose that one always judges it as constant, up to the point where, no longer able to "attribute to chance" an excessive difference in the frequency, one is obliged to abruptly modify one's judgment. Such a modification cannot happen instantaneously, but will be the sum of many insensible modifications which will happen in response to each trial. So one neglects small modifications up to the point where they are no longer negligible. And the practical importance of the method consists in this: that it allows evaluation of our states of mind to a good approximation – abstracting from those subordinated to some eventuality that we're not worried about – starting from simple qualitative evaluations and using an even simpler algorithm. We can neglect the most delicate part of judgment, which is the one the probabilities depend on in the exceptional cases, and which is not interesting enough to go into deeply, until such exceptional cases arise.

30.

Render unto Caesar that which is Caesar's: this is what inspired me in the discussion I have just finished. Render unto logic that which belongs to logic, and recognize the subjective character of what is subjective.

What is my position? I am – indeed! – a logician-mathematician who declines to argue if not impeccably, a cruel diminisher who eliminates everything that does not withstand the most refined critiques. Is it absurd, mad, to apply such pretensions to practical questions?

It would be if I wanted to explain everything through logic, if I wanted the practical concepts to have precise meanings like those I create with nominal definitions, if I wanted to reject as lacking logical value all that is not objective. If I did so I could only speak about mathematical truths, which are pure tautologies, and all the reasonings, judgments, sensations I find in life would be without any value for me. But it is not like that. I do not want to diminish the importance of what is extralogical: I assert only the necessity of realizing, in every argument, what is logical, what has an empirical value, what has a purely subjective value.

What is logical is exact, but it says nothing. Formal logic only teaches us to avoid an intrinsic contradiction among our opinions, in that it allows us to recognize the identity of the same opinion when it is expressed in various forms. Outside logic there exist no *truths* but only *opinions* whose value is just that of being actually felt as opinions. To imagine that they correspond to an "external reality" we must first invent the "external reality", imagining a physico-mathematical model (space, time, matter, energy) with which to represent and externalize our impressions. This is useful in many cases, and that is why we used such an artifice in Section 5, giving an opportune meaning, as a free convention, to the term "empirical reality". With that definition we settled on tacitly understanding the subjective value of, and hence abstracting from, all those of our impressions which are usually interpreted as "sensations of brute facts".

That is, we find it useful to be able to say elliptically: "The pencil *exists*, is *red*, is *wooden*", tacitly understanding the subject "I", where the complete sentence would be "I feel this particular sensation of seeing, touching, . . . which I characterize with the word 'pencil', and furthermore those that correspond to the words 'red', 'wooden'." This is actually very convenient, so that no logico-practical difficulty derives from the fact that, through long habituation to these elliptical forms, one forgets or even denies their character of elliptical abbreviations, seeing in them a kind of "truth", which is independent of us, of our sensations, of our thought.[37] The one obstacle to acceptance of these ideas is the logical paradox which I reported in capitals in Section 2, with the words of Adriano Tilgher.

Externalizing one of our impressions means petrifying it. To leave tacit the subject "I" is to renounce examination of the function of my thought. This is all very well with regard to "brute facts", because in those of my impressions that I call "sensations of brute fact" my consciousness only intervenes passively. If, on the contrary, it intervenes actively, externalizing an impression means mutilating it.

It is so for the idea of *cause*. We spoke about it (Sections 1 and 9), we examined its value, origin, and scope. It is a fruit of my state of mind, from which I cannot separate it without its withering and losing all meaning. I can observe relations of succession that are more or less constant, perhaps surprisingly constant, but while I remain a passive spectator of my sensations I have no reason to give importance to these relations of succession, or even less to think that they will be repeated in the future. The idea of cause presupposes the active intervention of my mind, and I must not think of attributing it to those images of my sensations which I invented by introducing a physico-mathematical model and calling it "the external world". It would be as if at the cinema I were to think that what happens on the screen happens because of ideas, sentiments and passions that move the human shadows who move on the screen.

If it is useful in practice to forget the subjective value of our sensations and to project them onto a screen external to ourselves,[38] because it can be useful to understand the subject "I" tacitly when it only has a passive part, it would be harmful to forget the subjective value of our opinions, and to project them onto an external screen, the screen of "absolute truth". Because our opinions are us, consciously us, actively us, who give them life, meaning, existence. If I interpret my opinions as "external truths" I give up control of them, I build an absurd barrier between my thinking and my thoughts, making their meaning incomprehensible to me, their creator.

There is, indeed, a second step. There are relations of succession which are so constant, fixed, universally considered noteworthy, that they are universally felt as causal relations, and in the most rigid sense, i.e., of relations which give us practical certainty, of "laws" which appear to us as almost "necessary".[39] In practice we can almost always forget the subjective meaning of such causal relations. Instead of saying "I am certain" that a particular fact will occur, I can say without bad consequences that it "must" occur. But only if we do not become victims of Narcissus's illusion, taking the artificial image of our ideas as something pre-existing and more important than our ideas, only if we do not go blind in adoration before the idols we have made with our own hands.

31.

But let us suppose that someone wants to "believe" in the "existence" of these laws, refusing to analyze the meaning, which does not exist, of the word "existence" that occurs in their formulation. Let us suppose that he wants to remain a determinist, and adhere to the ideas here maintained only in the field of probability, where their necessary character is most evident. He would "believe" in the "objective existence" of the world and of the "laws" governing it, and would make subjective judgments of the probability of those events regarding which he "ignores" the laws or the factual data that would permit him to foresee their outcome with certainty.

What would be left?

Little. Perhaps nothing.

In the struggle against determinism, modern physics is in fact our ally.

From the moment at which the kinetic theory of gasses opened the breach, the expansion of probabilistic theories in physics could not be stopped, and it submerges the most presumptuous bulwarks which seemed to challenge eternity. It is not that the modern physical views confirm mine, nor that they bring any essential element to the opinion one might form of my views; they can, though, prompt a favorable disposition of mind. Above all, they make more uncertain and indefinite the position of those who might want, as I have supposed, to abandon the objective value of probability but not the objective value of causality. Between necessity and probability, between causality and randomness, where shall the boundary be drawn?

A century ago one would not have hesitated to assign to the domain of determinism all inorganic[40] phenomena, with all the laws then known, especially the simple ones. Among them, e.g., the laws of Boyle and Mariotte: at equal temperatures the volume of a gas is inversely proportional to the pressure. Now this law has for us only a statistical value: it is the disordered, fantastically rapid motion of the molecules which incessantly jostle and rebound and bombard every obstacle in their path, which from the complexity of their disorder, compensating and evening out the mass of opposing inequalities, gives rise to the appearance of the most perfect order.

This is not to say that those who uphold the kinetical theory deny determinism. "Far from it, they are the most intransigent mechanists. Their molecules follow rigid trajectories, from which they only deviate under the influence of forces that vary with the distance according to a perfectly determinate law. Their system leaves not even the smallest place for freedom."[41]

But *the* law, the *true* law, the *law* in the deterministic sense, is not the law of Boyle and Mariotte, which is only immensely probable: it still exists, but it is another, more hidden. It is like this wherever modern physics substitutes for an indefeasible law a statistical law; one can always imagine that there still is an indefeasible law, but relative to the elementary phenomena from the immense numbers of which the statistical law takes its immense probability.

Absolutely true. But let us pin down three facts:

(1) That in the current state of scientific thought more and more natural laws are passing over into the category of statistical laws.

(2) That we have no criterion by which to rule out the possibility that the same metamorphosis will happen to any law which today seems certain.

(3) That it is difficult to reconcile the hope of saving the absolute value of anything in the field of natural laws with the application of statistical mechanics to the phenomena of atoms and electrons, which should constitute the essence and give the explanation of all other phenomena. Nor can we say that in such questions the probabilistic applications are made only to a great number of elementary facts, in which case the objection would have no value. The function of probabilistic concepts is deeper, so much so that one speaks of the "animality" of atoms, as I have heard said in an interesting lecture of Prof. Voghera's.[42]

Another current of ideas in modern physics: relativity.

Literally, there is not much wrong with the view of those who see in Albert Einstein a renewer and enlivener of determinism. In their peremptory character as inviolable laws, the new gravitational equations are perfectly equivalent to the classical equations that they supplant. But Copernicus too, in his time, only meant to transfer from the earth to the sun the privilege of *absolute rest*, and his revolution is celebrated and is important because it destroyed the concept of *absolute rest*. While relativity can quite legitimately appear as an innovation that does not go beyond the limits of determinism, I cannot see any outcome for it but the relativistic conception which denies determinism. The moving spirit is relativistic,[43] even if unconscious, even if hidden, even if denied. And this current, too, flows into the irresistible tide of relativistic thought.

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And it is not only physics that has taken this path. The attack is launched along the whole front. The ideal concomitance of seemingly disparate phenomena cannot be missed by any acute observer, who wants to penetrate beneath words, symbols and facts, and strip bare the spirit. Activism, relativism, fascism, futurism, bolshevism: different aspects of a single reality, of which we are all children: the twentieth century.[44] "In the immense variety of their manifestations, all these spiritual phenomena grow from the same root, translate the same intuition of the world and of life into various domains, where the spirit rebels against accepting one truth, one justice, one goodness, in a word, a theoretical or practical order of values that exist in themselves, independently of its activity, and before which the spirit can only bow down and submit." Thus Tilgher,[45] with his usual acute clarity, rightly insisting on the need to liberate oneself from overly restricted views, and to survey in a single glance the "unique and undivided movement" that characterizes a civilization.

"The poet, the philosopher, the scientist can justify their discoveries to themselves and to the public by a critique of their predecessors, but this critique comes after their initial intuitions, it presupposes them and is conditioned by them. Their voices and inner visions, their truly original intuitions they take from the spiritual environment in which they are immersed, considered as a unique and living totality, which everything has contributed to transform."

It is because of this fact, which I fully believe, that the discussion would not have satisfied me if it had developed within the narrower confines of an argument. And this is in spite of the fact that, for their immediate aims, the critique and the arguments that refer to it seem to me impeccable, complete, and I would say definitive.

As a boy I began to comprehend that the concept of "truth" is incomprehensible. That is the essential fact.

And so I have tried to analyze – case by case, more or less unconsciously – what we really mean to say when we say, in the common locution, that something "is true".

Only now does my thirst to understand this problem seem slaked. To mathematical logic[46] (in particular: the theory of nominal definition) and to the positivistic critique of the empirical world[47] – in which I found many things conforming to my ideas, and which therefore contributed greatly to their development – there has recently been added a third and definitive base of my point of view: probabilism. It corrects and integrates the other two in the points that I could not accept: those in which anything seemed to be considered as having an absolute value, transcending the psychological value it has for me, and independent of it. And in these points I am close to Poincaré, who, although of a different mentality, has the merit of having used psychological analysis to put life back into some formally arid questions which it is not enough to consider only from the formal point of view.

But where my spirit rebelled most ferociously and clashed against the concept of "absolute truth" was in the political field, and I could not say what part, surely very great, this sense of impatient revolt must have had in the development of my ideas. To be confronted by papier-mâché idols and a miserable political class that would have preferred Italy in ruins rather than failing (sacrilege!) to render due homage! Those delicious absolute truths that stuffed the demo-liberal brains! That impeccable rational mechanics of the perfect civilian regime of the peoples, conforming to the rights of man and various other immortal principles!

October of '22! It seemed to me I could see them, these Immortal Principles, as filthy corpses in the dust. And with what conscious and ferocious voluptuousness I felt myself trampling them, marching to hymns of triumph, obscure but faithful Blackshirt![48]

NOTES

* Bruno de Finetti, 'Probabilismo', Napoli, *Logos* **14** (1931), 163–219. Translated by Maria Concetta Di Maio, Maria Carla Galavotti, and Richard C. Jeffrey.

[1] G. Papini, *Stroncature*. Quattordici: 'Mario Calderoni', p. 248.

[2] A. Tilgher: 1923, *Relativisti contemporanei*, IV ed., pp. 49, 46, 23–24.

[3] Of Burali-Forti, Russell, Richard, etc. Cf. D. Hilbert and Ackermann: 1928, *Grundzüge der theoretischen Logik*, Berlin, pp. 92ff. An account in elementary form: F. Severi: 1928, 'Moderni indirizzi nelle matematiche', *Atti della Società Italiana per il Progresso delle Scienze*, p. 112.

[4] A. Tilgher, *op. cit.*, pp. 73–74.

[5] Of Vaihinger, Rougier, Spengler, (concerning which see Tilgher, *op. cit.*), of Aliotta, etc.

Concerning Aliotta, I think it necessary to report the following passage, to avoid what might be an easy misunderstanding.

"It is necessary to distinguish relativism from relativism. There is one of its forms (the one commonly pointed to when relativism is accused [of skepticism]) that relegates our knowledge to the realm of relativity, opposing to it an absolute reality that will always elude knowledge. In this form relativism has a skeptical and agnostic flavor and often goes together with mysticism. In the blinding light of the absolute our relative world devaluates, degenerating into a vain apparent shadow. We are the dream, the absolute is reality. And life becomes the painful chase of those shadows, vainly trying to become light.

"But there is another form of relativism (and this is mine), in which what is relative is itself the reality and leaves nothing outside itself. What we know is not the shadow, but the light, not a copy, but the true and concrete original" (*Relativismo e Idealismo*, Naples, 1922, p. 92).

This is exactly my opinion, and I wish to note, for more complete rigor, that the sentence "what is relative *leaves nothing outside itself*" must not be understood as saying that the sentence "there exists something outside what is relative" is FALSE, but that it is meaningless, so that it is impossible even to pose the question as to its truth and falsity. This is, after all, the interpretation that conforms to Aliotta's thought, as appears clearly further along in the text, where "*the being in itself and outside any relation of things*" is seen as "one of the many verbal statements to which there correspond no ideas, and which have become true and proper puzzles of philosophy" (ibid.).

[6] The term "*probabilism*" is usually employed to indicate an important aspect of the philosophy of the New Academy, which has some points of contact with the views supported here. For example, E. Morselli says (*Principi di Logica*, p. 150): "The philosophers of the New Academy, above all Arcesilaus and Carneades, being acute observers of life, maintain that in no domain of knowledge can we reach truth, and, consequently, absolute certainty, but that in every case we must content ourselves with simple probability." For ampler information see A. Aliotta, *Il problema delle scienze nella storia* (p. 33 ff.) and F. Enriques, *Per la storia della logica*, Bologna, 1922, p. 44; from this last reference it is especially apparent how much the sense of such critiques was, however, essentially, far away from ours (and I remark this for the problem set forth in the previous note).

[7] See 'Sul significato soggettivo della probabilità', in press, in *Fundamenta Mathemati-*

cae (Rendiconti del Seminario Matematico di Varsavia); "Fondamenti logici del ragionamento probabilistico", *Bollettino dell'Unione Matematica Italiana*, 1930; 'Problemi determinati e indeterminati nel calcolo della probabilità', *Rendiconti della Reale Accademia Nazionale dei Lincei*, 1930, II sem.; 'Sui fondamenti logici del ragionamento probabilistico', *Atti della Società Italiana per il Progresso delle Scienze*, Congresso Bolzano-Trento, 1930, Vol. II.

Already in April 1928 I had prepared a complete exposition of the foundations of probability theory according to my point of view; but, having encountered difficulties which I was far from imagining not only in having my thesis accepted but even in having it understood exactly, I had to try to unfold the essential points more fully. I have done that in the present essay in regard to the philosophical critiques, and in the above-mentioned works in regard to the definition of probability (except for conditional probability) and to the critiques of the principles and fundamental theorems. The analogous treatment of conditional probabilities will be developed as soon as possible. A particular argument which, though directly connected with my critiques (as seen in Section 22) could also be considered within the classical theory of probabilities, is that treated in the article mentioned there.

[8] H. Poincaré: 1906, *La science et l'Hypothèse*, Paris, pp. 171, 214.

[9] Op. cit., p. 217.

[10] Cf. my works mentioned in Note 7.

[11] M. Fréchet does this, consistently with his point of view, to be examined later.

[12] One cannot at the same time state that a proposition is "a priori" true and established by experience: it is important to insist on this point, "which not everyone has fully realized", as Poincaré rightly says.

"We cannot admit at the same time that it is impossible to imagine a 4-dimensional space and that experience shows us that space has 3 dimensions. The experimenter asks nature a question, "Is it this way or that?", and he cannot ask this without imagining the two alternatives. If it were impossible to imagine one of them then it would be useless and anyway impossible to resort to experience. We do not need observation to know that the hand of a watch doesn't point to 15, since we know in advance that there are only 12 figures, and it would be impossible to look at the 15 to see whether the hand is there since it doesn't exist." (*La Valeur de la Science*, Paris, 1905, p. 67).

[13] This would be possible only in cases where one would admit that the subjective judgment of all individuals who can be considered normal must coincide exactly.

[14] P. Lévy: 1928, *Calcul des probabilités*, Paris.

[15] "With things which are identical, they must be such that whatever is predicated of one must be predicated also of the other." St. Thomas Aquinas, *Summa Theologica*, Part 1, Question XL, Art. I, 3.

[16] E. Borel, *Traité de calcul des probabilités*, 1939, Vol. II, Fasc. I, *Applications a l'arithmetique, etc.*, Chap. 1.

[17] J. Bertrand, *Calcul des probabilités*, Paris, 1889, p. 90.

[18] J. Bertrand, op. cit., p. 91.

[19] H. Poincaré, *La science et l'hypothése*, cit., p. 220.

[20] This seems to me to be Keynes' point of view; but I cannot judge well, since I have only been able to skim his essay quickly.

[21] For my purposes here I could even concede – though this is not my opinion – that the concept of cause has an objective value when it is expressed through a necessary and

unchangeable relation. Then the critique would refer only to the concept of "cause" in the sense of a circumstance which "has a certain influence, but not a decisive one".

[22] Many authors say "trials of the same event". I prefer to say "phenomenon" here, reserving the term "event" for a single trial, or, in general, an isolated fact. This seems to me more opportune and convenient.

[23] Under this heading see, e.g., von Mises: 1928, *Mathematische Zeitschrift*, Vol. 5, and du Pasquier, *Comunicazione al Congresso Internationale dei Matematici*, Bologna, Section IV–A.

[24] It should be superfluous to note that here I do not speak of the rate of convergence in a stochastic sense, to which, e.g., Khinchine's theorem and the researches of Kolmogorov, Levy etc., refer; what we would need, and is impossible, is a bound which is *mathematically certain*.

[25] G. Castelnuovo, *Calcolo delle Probabilità*, Bologna, 1925, Vol. I, p. XXV and (for successive citations) pp. 4, 5.

[26] M. Fréchet-Halbwachs, *Le calcul des probabilités a la portée de tous*, pp. IX–X, 2. A largely acute critique of this conception (and also of the definition based on the computation of equally probable cases) is that of C. E. Bonferroni: 1926, *Intorno al concetto di probabilità*, Bari, but his conclusions do not avoid the critique developed here.

[27] pp. 235ff.

[28] Far be it from me to use this phrase in some metaphysical sense! It is a locution that can be useful for coming to terms, but does not mean anything. When I say "as the facts ought to go" I imply "in case they go according to what a certain theory assumes". Taken by itself, the idea would be better expressed by saying "the facts might go".

[29] Cited in M. Fréchet, op. cit., preface.

[30] F. Enriques, *Per la storia della Logica*, cit., p. 204.

[31] H. Poincaré, *La science et l'hypothèse*, cit., p. 218.

[32] Cf. Lèvy, *Calcul des probabilités*, *passim*, and in particular in Note 35.

[33] *Memorie della Reale Accademia Nazionale dei Lincei*, S. 6ª, Vol. IV, Fasc. V, 1930.

[34] H. Poincaré, *La science et l'hypothèse*, cit., p. 231.

[35] H. Poincaré, *La science et l'hypothèse*, cit., Chap. 11, 'La grandeur mathématique et l'expérience'.

[36] Bertrand, *Calcul des probabilités*, cit., p. 27.

[37] Consequently, it is not at all necessary *also* to share my ideas on the subjective meaning of "empirical reality" in order to accept the part that relates to probability, which is what matters here. But of course the integral conception allows a fuller eurythmy.

[38] In the only sense in which this can be called "external", that is, "of which we pretend to forget that it is internal".

[39] Cf. Note 21.

[40] We might add, by the way, that even this distinction between organic and inorganic had very soon lost its absolute value.

[41] Poincaré, *La valeur de la science*, cit., p. 253.

[42] The following simple example may clarify this point.

"Radium atoms decay at successive times, with a half-life of about two thousand years. So, in a piece of radium there are atoms that will decay in the next minute, and others that will decay only in thousands of centuries. The new theoreticians claim that one cannot find, and indeed that there does not exist, any difference among these kinds of atoms, that have such different lifetimes, or any difference in the ambient conditions surrounding their decay. The decay of one or the other is purely random."

This example has been quoted, among others, by Corbino, during the discussion of the principle of causality that took place in Florence during the "Mathesis" Congress in 1929 following the presentations of Fermi and Persico, and is summarized in *Periodico di Matematiche* 1930, No. 2; in the same issue, and in the preceding one, also appeared the above mentioned papers of Fermi and Persico.

A deeper and more detailed exposition of my ideas on this topic is to be found in the paper "Le leggi differenziali e la rinunzia al determinismo" that I gave at the Mathematical Seminar in Rome on 5 April 1930 (VIII).

43 See for example, A. Aliotta, *La teoria di Einstein e le mutevoli prospettive del mondo*, the essay on A. Einstein in Tilgher, op cit., and the papers by the same authors that appeared in the Italian edition of Kopff's treatise.

44 As concerns the more properly philosophical aspect, into which I do not intend to go deeper, nor could I, see the works of A. Aliotta, and in particular his *La reazione idealistica contro la scienza*, and *Le origini dell'irrationalismo contemporaneo*, Napoli, 1950, which is an updated partial rewriting of the former.

45 Op. cit., p. 49, and for the following citations, pp. 54, 53.

46 C. Burali Forti: 1919, *Logica Matematica*, 2nd ed., Hoepli, Milano.

47 E. Mach: 1883, *Die Mechanik in ihrer Entwickelung*, Leipzig.

48 That fascism represents the relativistic attitude in politics as against the staticity of empty doctrinaire ideologies has been explicitly stated by Mussolini himself, in an article ("Relativismo e Fascismo", *Popolo d'Italia*, 22 November 1921) occasioned by the publication of the repeatedly cited work of Tilgher and reprinted in its 4th ed. 1923, pp. 77–78.