“Chance”

Part I, Chapter 4 of *Science and Method*

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I.

“How can we venture to speak of the laws of chance? Is not chance the antithesis of all law?” It is thus that Bertrand expresses himself at the beginning of his “Calculus of Probabilities.” Probability is the opposite of certainty; it is thus what we are ignorant of, and consequently it would seem to be what we cannot calculate. There is here at least an apparent contradiction, and one on which much has already been written.

To begin with, what is chance? The ancients distinguished between the phenomena which seemed to obey harmonious laws, established once for all, and those that they attributed to chance, which were those that could not be predicted because they were not subject to any law. In each domain the precise laws did not decide everything, they only marked the limits within which chance was allowed to move. In this conception, the word chance had a precise, objective meaning; what was chance for one was also chance for the other and even for the gods.

But this conception is not ours. We have become complete determinists, and even those who wish to reserve the right of human free will at least allow determinism to reign undisputed in the inorganic world. Every phenomenon, however trifling it be, has a cause, and a mind infinitely powerful and infinitely well-informed concerning the laws of nature could have foreseen it from the beginning of the ages. If a being with such a mind existed, we could play no game of chance with him; we should always lose.

For him, in fact, the word chance would have no meaning, or rather there would be no such thing as chance. That there is for us is only on account of our frailty and our ignorance. And even without going beyond our frail humanity, what is chance for the ignorant is no longer chance for the learned. Chance is only the measure of our ignorance. Fortuitous phenomena are, by definition, those whose laws we are ignorant of.

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But is this definition very satisfactory? When the first Chaldean shepherds followed with their eyes the movements of the stars, they did not yet know the laws of astronomy, but would they have dreamed of saying that the stars move by chance? If a modern physicist is studying a new phenomenon, and if he discovers its law on Tuesday, would he have said on Monday that the phenomenon was fortuitous? But more than this, do we not often invoke what Bertrand calls the laws of chance in order to predict a phenomenon ? For instance, in the kinetic theory of gases, we find the well-known laws of Mariotte and of Gay-Lussac, thanks to the hypothesis that the velocities of the gaseous molecules vary irregularly, that is to say, by chance. The observable laws would be much less simple, say all the physicists, if the velocities were regulated by some simple elementary law, if the molecules were, as they say, *organized*, if they were subject to some discipline. It is thanks to chance — that is to say, thanks to our ignorance, that we can arrive at conclusions. Then if the word chance is merely synonymous with ignorance, what does this mean? Must we translate as follows? —

"You ask me to predict the phenomena that will be produced. If I had the misfortune to know the laws of these phenomena, I could not succeed except by inextricable calculations, and I should have to give up the attempt to answer you; but since I am fortunate enough to be ignorant of them, I will give you an answer at once. And, what is more extraordinary still, my answer will be right."

Chance, then, must be something more than the name we give to our ignorance. Among the phenomena whose causes we are ignorant of, we must distinguish between fortuitous phenomena, about which the calculation of probabilities will give us provisional information, and those that are not fortuitous, about which we can say nothing, so long as we have not determined the laws that govern them. And as regards the fortuitous phenomena themselves, it is clear that the information that the calculation of probabilities supplies will not cease to be true when the phenomena are better known.

The manager of a life insurance company does not know when each of the assured will die, but he relies upon the calculation of probabilities and on the law of large numbers, and he does not make a mistake, since he is able to pay dividends to his shareholders. These dividends would not vanish if a very far-sighted and very indiscreet doctor came, when once the policies were signed, and gave the manager information on the chances of life of the assured. The doctor would dissipate the ignorance of the manager, but he would have no effect upon the dividends, which are evidently not a result of that ignorance.

II.

In order to find the best definition of chance, we must examine some of the facts which it is agreed to regard as fortuitous, to which the calculation of probabilities seems to apply. We will then try to find their common characteristics.

We will select unstable equilibrium as our first example. If a cone is balanced on its point, we know very well that it will fall, but we do not know to which side; it seems that chance alone will decide. If the cone were perfectly symmetrical, if its axis were perfectly vertical, if it were subject to no other force but gravity, it would not fall at all. But the slightest defect of symmetry will make it lean slightly to one side or other, and as soon as it leans, be it ever so little, it will fall altogether to that side. Even if the symmetry is perfect, a very slight trepidation, or a breath of air, may make it incline a few seconds of arc, and that will be enough to determine its fall and even the direction of its fall, which will be that of the original inclination.

A very small cause which escapes our notice determines a considerable effect that we cannot fail to see, and then we say that that effect is due to chance. If we knew exactly the laws of nature and the situation of the universe at the initial moment, we could predict exactly the situation of that same universe at a succeeding moment. But, even if it were the case that the natural laws had no longer any secret for us, we could still only know the initial situation *approximately*. If that enabled us to predict the succeeding situation *with the same approximation*, that is all we require, and we should say that the phenomenon had been predicted, that it is governed by laws. But it is not always so; it may happen that small differences in the initial conditions produce very great ones in the final phenomena A small error in the former will produce an enormous error in the latter. Prediction becomes impossible, and we have the fortuitous phenomenon.

Our second example will be very much like our first, and we will borrow it from meteorology. Why have meteorologists such difficulty in predicting the weather with any certainty ? Why is it that showers and even storms seem to come by chance, so that many people think it quite natural to pray for rain or fine weather, though they would consider it ridiculous to ask for an eclipse by prayer? We see that great disturbances are generally produced in regions where the atmosphere is in unstable equilibrium. The meteorologists see very well that the equilibrium is unstable, that a cyclone will be formed somewhere, but exactly where they are not in a position to say; a tenth of a degree more or less at any given point, and the cyclone will burst here and not there, and extend its ravages over districts it would otherwise have spared. If they had been aware of this tenth of a degree, they could have known it beforehand, but the observations were neither sufficiently comprehensive nor sufficiently precise, and that is the reason why it all seems due to the intervention of chance. Here, again, we find the same contrast between a very trifling cause that is inappreciable to the observer, and considerable effects, that are sometimes terrible disasters.

Let us pass to another example, the distribution of the minor planets on the Zodiac. Their initial longitudes may have had some definite order, but their mean motions were different and they have been revolving for so long that we may say that practically they are distributed *by chance* throughout the Zodiac. Very small initial differences in their distances from the sun, or, what amounts to the same thing, in their mean motions, have resulted in enormous differences in their actual longitudes. A difference of a thousandth part of a second in the mean daily motion will have the effect of a second in three years, a degree in ten thousand years, a whole circumference in three or four millions of years, and what is that beside the time that has elapsed since the minor planets became detached from Laplace's nebula? Here, again, we have a small cause and a great effect, or better, small differences in the cause and great differences in the effect.

The game of roulette does not take us so far as it might appear from the preceding example. Imagine a needle that can be turned about a pivot on a dial divided into a hundred alternate red and black sections. If the needle stops at a red section we win if not, we lose. Clearly, all depends on the initial impulse we give to the needle. I assume that the needle will make ten or twenty revolutions, but it will stop earlier or later according to the strength of the spin I have given it. Only a variation of a thousandth or a two-thousandth in the impulse is sufficient to determine whether my needle will stop at a black section or at the following section, which is red. These are differences that the muscular sense cannot ;appreciate, which would escape even more delicate instruments. It is, accordingly, impossible for me to predict what the needle I have just spun will do, and that is why my heart beats and I hope for everything from chance. The difference in the cause is imperceptible, and the difference in the effect is for me of the highest importance, since it affects my whole stake.

III.

In this connexion I wish to make a reflection that is somewhat foreign to my subject. Some years ago a certain philosopher said that the future was determined by the past, but not the past by the future; or, in other words, that from the knowledge of the present we could deduce that of the future but not that of the past; because, he said, one cause can produce only one effect, while the same effect can be produced by several different causes. It is obvious that no scientist can accept this conclusion. The laws of nature link the antecedent to the consequent in such a way that the antecedent is determined by the consequent just as much as the consequent is by the antecedent But what can have been the origin of the philosopher's error? We know that, in virtue of Carnot's principle, physical phenomena are irreversible and that the world is tending towards uniformity. When two bodies of different temperatures are in conjunction, the warmer gives up heat to the colder, and accordingly we can predict that the temperatures will become equal. But once the temperatures have become equal, if we are asked about the previous state, what can we answer? We can certainly say that one of the bodies was hot and the other cold, but we cannot guess which of the two was formerly the warmer.

And yet in reality the temperatures never arrive at perfect equality. The difference between the temperatures only tends towards zero asymptotically. Accordingly there comes a moment when our thermometers are powerless to disclose it. But if we had thermometers a thousand or a hundred thousand times more sensitive, we should recognize that there is still a small difference, and that one of the bodies has remained a little warmer than the other, and then we should be able to state that this is the one which was formerly very much hotter than the other.

So we have, then, the reverse of what we found in the preceding examples, great differences in the cause and small differences in the effect Flammarion[1] once imagined an observer moving away from the earth at a velocity greater than that of light. For him time would have its sign changed, history would be reversed, and Waterloo would come before Austerlitz. Well, for this observer effects and causes would be inverted, unstable equilibrium would no longer be the exception; on account of the universal irreversibility, everything would seem to him to come out of a kind of chaos in unstable equilibrium, and the whole of nature would appear to him to be given up to chance.

IV.

We come now to other arguments, in which we shall see somewhat different characteristics appearing, and first let us take the kinetic theory of gases. How are we to picture a receptacle full of gas? Innumerable molecules, animated with great velocities, course through the receptacle in all directions; every moment they collide with the sides or else with one another, and these collisions take place under the most varied conditions. What strikes us most in this case is not the smallness of the causes, but their complexity. And yet the former element is still found here, and plays an important part. If a molecule deviated from its trajectory to left or right in a very small degree as compared with the radius of action of the gaseous molecules, it would avoid a collision, or would suffer it under different conditions, and that would alter the direction of its velocity after the collision perhaps by go or 180 degrees.

That is not all. It is enough, as we have just seen, that the molecule should deviate before the collision in an infinitely small degree, to make it deviate after the collision in a finite degree. Then, if the molecule suffers two successive collisions, it is enough that it should deviate before the first collision in a degree of infinite smallness of the second order, to make it deviate after the first collision in a degree of infinite smallness of the first order, and after the second collision in a finite degree. And the molecule will not suffer two collisions only, but a great number each second.

So that if the first collision multiplied the deviation by a very large number, A, after n collisions it will be multiplied by A^n. It will, therefore, have become very great, not only because A is large — that is to say, because small causes produce great effects — but because the exponent n is large, that is to say, because the collisions are very numerous and the causes very complex.

Let us pass to a second example. Why is it that in a shower the drops of rain appear to us to be distributed by chance? It is again because of the complexity of the causes which determine their formation. Ions have been distributed through the atmosphere; for a long time they have been subjected to constantly

[1]Camille Flammarion, 1842–1925, French astronomer and writer of popular science and science fiction. Perhaps best known for having helped popularize the idea of the heat-death of the universe. See e.g. Wikipedia s.v. "Flammarion".

changing air currents; they have been involved in whirlwinds of very small dimensions, so that their final distribution has no longer any relation to their original distribution. Suddenly the temperature falls, the vapor condenses, and each of these ions becomes the center of a raindrop. In order to know how these drops will be distributed and how many will fall on each stone of the pavement, it is not enough to know the original position of the ions, but we must calculate the effect of a thousand minute and capricious air currents.

It is the same thing again if we take grains of dust in suspension in water. The vessel is permeated by currents whose law we know nothing of except that it is very complicated. After a certain length of time the grains will be distributed by chance, that is to say uniformly, throughout the vessel, and this is entirely due to the complication of the currents If they obeyed some simple law — if, for instance the vessel were revolving and the currents revolved in circles about its axis — the case would be altered, for each grain would retain its original height and its original distance from the axis.

We should arrive at the same result by picturing the mixing of two liquids or of two fine powders. To take a rougher example, it is also what happens when a pack of cards is shuffled. At each shuffle the cards undergo a permutation similar to that studied in the theory of substitutions. What will be the resulting permutation? The probability that it will be any particular permutation (for instance, that which brings the card occupying the position $\phi(n)$ before the permutation into the position n), this probability, I say, depends on the habits of the player. But if the player shuffles the cards long enough, there will be a great number of successive permutations, and the final order which results will no longer be governed by anything but chance; I mean that all the possible orders will be equally probable. This result is due to the great number of successive permutations, that is to say, to the complexity of the phenomenon.

A final word on the theory of errors. It is a case in which the causes have complexity and multiplicity. How numerous are the traps to which the observer is exposed, even with the best instrument. He must take pains to look out for and avoid the most flagrant, those which give birth to systematic errors. But when he has eliminated these, admitting that he succeeds in so doing, there still remain many which, though small, may become dangerous by the accumulation of their effects. It is from these that accidental errors arise, and we attribute them to chance, because their causes are too complicated and too numerous. Here again we have only small causes, but each of them would only produce a small effect; it is by their union and their number that their effects become formidable.

V.

There is yet a third point of view, which is less important than the two former, on which I will not lay so much stress. When we are attempting to predict a fact and making an examination of the antecedents, we endeavor to enquire into the anterior situation. But we cannot do this for every part of the universe, and we are content with knowing what is going on in the neighborhood of the place where the fact will occur, or what appears to have some connexion with the fact. Our enquiry cannot be complete, and we must know how to select. But we may happen to overlook circumstances which, at first sight, seemed completely foreign to the anticipated fact, to which we should never have dreamed of attributing any influence, which nevertheless, contrary to all anticipation, come to play an important part.

A man passes in the street on the way to his business. Some one familiar with his business could say what reason he had for starting at such an hour and why he went by such a street. On the roof a slater is at work. The contractor who employs him could, to a certain extent, predict what he will do. But the man has no thought for the slater, nor the slater for him; they seem to belong to two worlds completely foreign to one another. Nevertheless the slater drops a tile which kills the man, and we should have no hesitation in saying that this was chance.

Our frailty does not permit us to take in the whole universe, but forces us to cut it up in slices. We attempt to make this as little artificial as possible, and yet it happens, from time to time, that two of these slices react upon each other, and then the effects of this mutual action appear to us to be due to chance.

Is this a third way of conceiving of chance? Not always; in fact, in the majority of cases, we come back to the first or second. Each time that two worlds, generally foreign to one another, thus come to act upon each other, the laws of this reaction cannot fail to be very complex, and moreover a very small change in the initial conditions of the two worlds would have been enough to prevent the reaction from taking place. How very little it would have taken to make the man pass a moment later, or the slater drop his tile a moment earlier!

VI.

Nothing that has been said so far explains why chance is obedient to laws. Is the fact that the causes are small, or that they are complex, sufficient to enable us to predict, if not what the effects will be in each case, at least what they will be on the average? In order to answer this question, it will be best to return to some of the examples quoted above.

I will begin with that of roulette. I said that the point where the needle stops will depend on the initial impulse given it. What is the probability that this impulse will be of any particular strength? I do not know, but it is difficult not to admit that this probability is represented by a continuous analytical function. The probability that the impulse will be comprised between a and $a + \epsilon$ will, then, clearly be equal to the probability that it will be comprised between $a + \epsilon$ and $a + 2\epsilon$, provided that ϵ is very small. This is a property common to all analytical functions. Small variations of the function are proportional to small variations of the variable.

But we have assumed that a very small variation in the impulse is sufficient to change the color of the section opposite which the needle finally stops. From

a to $a+\epsilon$ is red, from $a+\epsilon$ to $a+2\epsilon$ is black. The probability of each red section is accordingly the same as that of the succeeding black section, and consequently the total probability of red is equal to the total probability of black.

The datum in the case is the analytical function which represents the probability of a particular initial impulse. But the theorem remains true, whatever this datum may be, because it depends on a property common to all analytical functions. From this it results finally that we have no longer any need of the datum.

What has just been said of the case of roulette applies also to the example of the minor planets. The Zodiac may be regarded as an immense roulette board on which the Creator has thrown a very great number of small balls, to which he has imparted different initial impulses, varying, however, according to some sort of law. Their actual distribution is uniform and independent of that law, for the same reason as in the preceding case. Thus we see why phenomena obey the laws of chance when small differences in the causes are sufficient to produce great differences in the effects. The probabilities of these small differences can then be regarded as proportional to the differences themselves, just be- cause these differences are small, and small increases of a continuous function are proportional to those of the variable.

Let us pass to a totally different example, in which the complexity of the causes is the principal factor. I imagine a card-player shuffling a pack of cards. At each shuffle he changes the order of the cards, and he may change it in various ways. Let us take three cards only in order to simplify the explanation. The cards which, before the shuffle, occupied the positions 123 respectively may, after the shuffle, occupy the positions

$$123, 231, 312, 321, 132, 213$$

Each of these six hypotheses is possible, and their probabilities are respectively

$$p_1, p_2, p_3, p_4, p_5, p_6 \ .$$

The sum of these six numbers is equal to 1, but that is all we know about them. The six probabilities naturally depend upon the player's habits, which we do not know.

At the second shuffle the process is repeated, and under the same conditions. I mean, for instance, that p_4 always represents the probability that the three cards which occupied the positions 123 after the n^{th} shuffle and before the $(n+1)^{\text{th}}$ will occupy the positions 321 after the $(n+1)^{\text{th}}$ shuffle. And this remains true, whatever the number n may be, since the player's habits and his method of shuffling remain the same.

But if the number of shuffles is very large, the cards which occupied the positions 123 before the first shuffle may, after the last shuffle, occupy the positions

$$123, 231, 312, 321, 132, 213 \ ,$$

and the probability of each of these six hypotheses is clearly the same and equal to 1/6; and this is true whatever be the numbers $p_1, \ldots p_6$, which we do not know. The great number of shuffles, that is to say, the complexity of the causes, has produced uniformity.

This would apply without change if there were more than three cards, but even with three the demonstration would be complicated, so I will content myself with giving it for two cards only. We have now only two hypotheses

$$12, 21 ,$$

with the probabilities p_1 and $p_2 = 1 - p_1$. Assume that there are n shuffles, and that I win a shilling if the cards are finally in the initial order, and that I lose one if they are finally reversed. Then my mathematical expectation will be

$$(p_1 - p_2)^n$$

The difference, $p_1 - p_2$, is certainly smaller than 1, so that if n is very large, the value of my expectation will be nothing, and we do not require to know p_1 and p_2 to know that the game is fair.

Nevertheless there would be an exception if one of the numbers p_1, and p_2, was equal to 1 and the other to nothing. *It would then hold good no longer, because our original hypotheses would be too simple.*

What we have just seen applies not only to the mixing of cards, but to all mixing, to that of powders and liquids, and even to that of the gaseous molecules in the kinetic theory of gases. To return to this theory let us imagine for a moment a gas whose molecules cannot collide mutually, but can be deviated by collisions with the sides of the vessel in which the gas is enclosed. If the form of the vessel is sufficiently complicated, it will not be long before the distribution of the molecules and that of their velocities become uniform. This will not happen if the vessel is spherical or if it has the form of a rectangular parallelepiped. And why not? Because in the former case the distance of any particular trajectory from the center remains constant, and in the latter case we have the absolute value of the angle of each trajectory with the sides of the parallelepiped.

Thus we see what we must understand by conditions that are *too simple.* They are conditions which preserve something of the original state as an invariable. Are the differential equations of the problem too simple to enable us to apply the laws of chance? This question appears at first sight devoid of any precise meaning, but we know now what it means. They are too simple if something is preserved, if they admit a uniform integral. If something of the initial conditions remains unchanged, it is clear that the final situation can no longer be independent of the initial situation.

We come, lastly, to the theory of errors. We are ignorant of what accidental errors are due to, and it is just because of this ignorance that we know they will obey Gauss's law. Such is the paradox. It is explained in somewhat the same way as the preceding cases. We only need to know one thing — that the errors are very numerous, that they are very small, and that each of them can be equally well negative Or positive. What is the curve of probability of each of them? We do not know, but only assume that it is symmetrical. We can then show that the resultant error will follow Gauss's law, and this resultant law is independent of the particular laws which we do not know. Here again the simplicity of the result actually owes its existence to the complication of the data.

VII.

But we have not come to the end of paradoxes. I recalled just above Flammarion's fiction of the man who travels faster than light, for whom time has its sign changed. I said that for him all phenomena would seem to be due to chance. This is true from a certain point of view, and yet, at any given moment, all these phenomena would not be distributed in conformity with the laws of chance, since they would be just as they are for us, who, seeing them unfolded harmoniously and not emerging from a primitive chaos, do not look upon them as governed by chance.

What does this mean? For Flammarion's imaginary Lumen[2], small causes seem to produce great effects; why, then, do things not happen as they do for us when we think we see great effects due to small causes? Is not the same reasoning applicable to his case?

Let us return to this reasoning. When small differences in the causes produce great differences in the effects, why are the effects distributed according to the laws of chance ? Suppose a difference of an inch in the cause produces a difference of a mile in the effect. If I am to win in case the effect corresponds with a mile bearing an even number, my probability of winning will be 1/2. Why is this? Because, in order that it should be so, the cause must correspond with an inch bearing an even number. Now, according to all appearance, the probability that the cause will vary between certain limits is proportional to the distance of those limits, provided that distance is very small. If this hypothesis be not admitted, there would no longer be any means of representing the probability by a continuous function.

Now what will happen when great causes produce small effects? This is the case in which we shall not attribute the phenomenon to chance, and in which Lumen, on the contrary, would attribute it to chance. A difference of a mile in the cause corresponds to a difference of an inch in the effect. Will the probability that the cause will be comprised between two limits n miles apart still be proportional to n? We have no reason to suppose it, since this distance of n miles is great. But the probability that the effect will be comprised between two limits n inches apart will be precisely the same, and accordingly it will not be proportional to n, and that notwithstanding the fact that this distance of n inches is small. There is, then, no means of representing the law of probability of the effects by a continuous curve. I do not mean to say that the curve may not remain continuous in the analytical sense of the word. To infinitely small variations of the abscissa there will correspond infinitely small variations of the ordinate. But practically it would not be continuous, since to very small variations of the abscissa there would not correspond very small

[2] Full text online at `http://books.eserver.org/fiction/lumen/`.

variations of the ordinate. It would become impossible to trace the curve with an ordinary pencil: that is what I mean.

What conclusion are we then to draw? Lumen has no right to say that the probability of the cause (that of his cause, which is our effect) must necessarily be represented by a continuous function. But if that be so, why have we the right? It is because that state of unstable equilibrium that I spoke of just now as initial. is itself only the termination of a long anterior history. In the course of this history complex causes have been at work, and they have been at work for a long time. They have contributed to bring about the mixture of the elements, and they have tended to make everything uniform, at least in a small space. They have rounded off the corners, leveled the mountains, and filled up the valleys. However capricious and irregular the original curve they have been given, they have worked so much to regularize it that they will finally give us a continuous curve, and that is why we can quite confidently admit its continuity.

Lumen would not have the same reasons for drawing this conclusion. For him complex causes would not appear as agents of regularity and of leveling; on the contrary, they would only create differentiation and inequality. He would see a more and more varied world emerge from a sort of primitive chaos. The changes he would observe would be for him unforeseen and impossible to foresee. They would seem to him due to some caprice, but that caprice would not be at all the same as our chance, since it would not be amenable to any law, while our chance has its own laws. All these points would require a much longer development, which would help us perhaps to a better comprehension of the irreversibility of the universe.

VIII.

We have attempted to define chance, and it would be well now to ask ourselves a question. Has chance thus defined so far as it can be, an objective character?

We may well ask it I have spoken of very small or very complex causes, but may not what is very small for one be great for another, and may not what seems very complex to one appear simple to another? I have already given a partial answer, since I stated above most precisely the case in which differential equations become too simple for the laws of chance to remain applicable. But it would be well to examine the thing somewhat more closely, for there are still other points of view we may take.

What is the meaning of the word small? To understand it, we have only to refer to what has been said above. A difference is very small, an interval is small, when within the limits of that interval the probability remains appreciably constant Why can that probability be regarded as constant in a small interval? It is because we admit that the law of probability is represented by a continuous curve, not only continuous in the analytical sense of the word, but practically continuous, as I explained above. This means not only that it will present no absolute hiatus, but also that it will have no projections or depressions too acute or too much accentuated.

What gives us the right to make this hypothesis? As I said above, it is because, from the beginning of he ages, there are complex causes that never cease to operate in the same direction, which cause the world to tend constantly towards uniformity without the possibility of ever going back. It is these causes which, little by little, have leveled the projections and filled up the depressions, and it is for this reason that our curves of probability present none but gentle undulations In millions and millions of centuries we shall have progressed another step towards uniformity, and these undulations will be ten times more gentle still. The radius of mean curvature of our curve will have become ten times longer. And then a length that to-day does not seem to us very small, because an arc of such a length cannot be regarded as rectilinear, will at that period be properly qualified as very small, since the curvature will have become ten times less, and an arc of such a length will not differ appreciably from a straight line.

Thus the word very small remains relative, but it is not relative to this man or that, it is relative to the actual state of the world. It will change its meaning when the world becomes more uniform and all things are still more mixed. But then, no doubt, men will no longer be able to live, but will have to make way for other beings, shall I say much smaller or much larger? So that our criterion, remaining true for all men, retains an objective meaning.

And, further, what is the meaning of the word very complex? I have already given one solution, that which I referred to again at the beginning of this section; but there are others. Complex causes, I have said, produce a more and more intimate mixture, but how long will it be before this mixture satisfies us? When shall we have accumulated enough complications? When will the cards be sufficiently shuffled? If we mix two powders, one blue and the other white, there comes a time when the color of the mixture appears uniform. This is on account of the infirmity of our senses; it would be uniform for the longsighted, obliged to look at it from a distance, when it would not yet be so for the short-sighted. Even when it had become uniform for all sights, we could still set back the limit by employing instruments There is no possibility that any man will ever distinguish the infinite variety that is hidden under the uniform appearance of a gas, if the kinetic theory is true. Nevertheless, if we adopt Gouy's ideas on the Brownian movement, does not the microscope seem to be on the point of showing us something analogous?

This new criterion is thus relative like the first, and if it preserves an objective character, it is because all men have about the same senses, the power of their instruments is limited, and, moreover, they only make use of them occasionally.

IX.

It is the same in the moral sciences, and particularly in history. The historian is obliged to make a selection of the events in the period he is studying, and he only recounts those that seem to him the most important. Thus he contents himself with relating the most considerable events of the 16th century, for instance, and similarly the most remarkable facts of the 17th century. If the former are sufficient to explain the latter, we say that these latter conform to the laws of history. But if a great event of the 17th century owes its cause to a small fact of the 16th century that no history reports and that every one has neglected, then we say that this event is due to chance, and so the word has the same sense as in the physical sciences; it means that small causes have produced great effects.

The greatest chance is the birth of a great man. It is only by chance that the meeting occurs of two genital cells of different sex that contain precisely, each on its side, the mysterious elements whose mutual reaction is destined to produce genius. It will be readily admitted that these elements must be rare, and that their meeting is still rarer. How little it would have taken to make the spermatozoid which carried them deviate from its course. It would have been enough to deflect it a hundredth part of a inch, and Napoleon would not have been born and the destinies of a continent would have been changed. No example can give a better comprehension of the true character of chance.

One word more about the paradoxes to which the application of the calculation of probabilities to the moral sciences has given rise. It has been demonstrated that no parliament would ever contain a single member of the opposition, or at least that such an event would be so improbable that it would be quite safe to bet against it, and to bet a million to one. Condorcet attempted to calculate how many jurymen it would require to make a miscarriage of justice practically impossible. If we used the results of this calculation, we should certainly be exposed to the same disillusionment as by betting on the strength of the calculation that the opposition would never have a single representative.

The laws of chance do not apply to these questions If justice does not always decide on good grounds it does not make so much use as is generally supposed of Bridoye's method. This is perhaps unfortunate since, if it did, Condorcet's method would protect us against miscarriages.

What does this mean? We are tempted to attribute facts of this nature to chance because their causes are obscure, but this is not true chance. The causes are unknown to us, it is true, and they are even complex; but they are not sufficiently complex, since they preserve something, and we have seen that this is the distinguishing mark of "too simple" causes When men are brought together, they no longer decide by chance and independently of each other, but react upon one another. Many causes come into action, they trouble the men and draw them this way and that, but there is one thing they cannot destroy, the habits they have of Panurge's sheep. And it is this that is preserved.

X.

The application of the calculation of probabilities to the exact sciences also involves many difficulties. Why are the decimals of a table of logarithms or of the number π distributed in accordance with the laws of chance? I have elsewhere studied the question in regard to logarithms, and there it is easy. It is clear that a small difference in the argument will give a small difference in the logarithm, but a great difference in the sixth decimal of the logarithm. We still find the same criterion.

But as regards the number π the question presents more difficulties, and for the moment I have no satisfactory explanation to give.

There are many other questions that might be raised, if I wished to attack them before answering the one I have more especially set myself. When we arrive at a simple result, when, for instance, we find a round number, we say that such a result cannot be due to chance, and we seek for a non-fortuitous cause to explain it. And in fact there is only a very slight likelihood that, out of 10,000 numbers, chance will give us a round number, the number 10,000 for instance; there is only one chance in 10,000. But neither is there more than one chance in 10,000 that it will give us any other particular number, and yet this result does not astonish us, and we feel no hesitation about attributing it to chance, and that merely because it is less striking.

Is this a simple illusion on our part, or are there cases in which this view is legitimate? We must hope so, for otherwise all science would be impossible. When we wish to check a hypothesis, what do we do? We cannot verify all its consequences, since they are infinite in number. We content ourselves with verifying a few, and, if we succeed, we declare that the hypothesis is confirmed, for so much success could not be due to chance. It is always at bottom the same reasoning

I cannot justify it here completely, it would take me too long, but I can say at least this. We find ourselves faced by two hypotheses, either a simple cause or else that assemblage of complex causes we call chance. We find it natural to admit that the former must produce a simple result, and then, if we arrive at this simple result, the round number for instance, it appears to us more reasonable to attribute it to the simple cause, which was almost certain to give it us, than to chance, which could only give it us once in 10,000 times. It will not be the same if we arrive at a result that is not simple. It is true that chance also will not give it more than once in 10,000 times, but the simple cause has no greater chance of producing it.