CHAPTER IV

CHANCE

I

"How dare we speak of the laws of chance? Is not chance the antithesis of all law?" So says Bertrand at the beginning of his *Calcul des probabilités*. Probability is opposed to certitude; so it is what we do not know and consequently it seems what we could not calculate. Here is at least apparently a contradiction, and about it much has already been written.

And first, what is chance? The ancients distinguished between phenomena seemingly obeying harmonious laws, established once for all, and those which they attributed to chance; these were the ones unpredictable because rebellious to all law. In each domain the precise laws did not decide everything, they only drew limits between which chance might act. In this conception the word chance had a precise and objective meaning: what was chance for one was also chance for another and even for the gods.

But this conception is not ours to-day. We have become absolute determinists, and even those who want to reserve the rights of human free will let determinism reign undividedly in the inorganic world at least. Every phenomenon, however minute, has a cause; and a mind infinitely powerful, infinitely well-informed about the laws of nature, could have foreseen it from the beginning of the centuries. If such a mind existed, we could not play with it at any game of chance; we should always lose.

In fact for it the word chance would not have any meaning, or rather there would be no chance. It is because of our weakness and our ignorance that the word has a meaning for us. And, even without going beyond our feeble humanity, what is chance for the ignorant is not chance for the scientist. Chance is only the measure of our ignorance. Fortuitous phenomena are, by definition, those whose laws we do not know. N.B

But is this definition altogether satisfactory? When the first

Chaldean shepherds followed with their eyes the movements of the stars, they knew not as yet the laws of astronomy; would they have dreamed of saying that the stars move at random? If a modern physicist studies a new phenomenon, and if he discovers its law Tuesday, would he have said Monday that this phenomenon was fortuitous? Moreover, do we not often invoke what Bertrand calls the laws of chance, to predict a phenomenon? For example, in the kinetic theory of gases we obtain the known laws of Mariotte and of Gay-Lussac by means of the hypothesis that the velocities of the molecules of gas vary irregularly, that is to say at random. All physicists will agree that the observable laws would be much less simple if the velocities were ruled by any simple elementary law whatsoever, if the molecules were, as we say, *organized*, if they were subject to some discipline. It is due to chance, that is to say, to our ignorance, that we can draw our conclusions; and then if the word chance is simply synonymous with ignorance what does that mean? Must we therefore translate as follows?

"You ask me to predict for you the phenomena about to happen. If, unluckily, I knew the laws of these phenomena I could make the prediction only by inextricable calculations and would have to renounce attempting to answer you; but as I have the good fortune not to know them, I will answer you at once. And what is most surprising, my answer will be right."

So it must well be that chance is something other than the name we give our ignorance, that among phenomena whose causes are unknown to us we must distinguish fortuitous phenomena about which the calculus of probabilities will provisionally give information, from those which are not fortuitous and of which we can say nothing so long as we shall not have determined the laws governing them. For the fortuitous phenomena themselves, it is clear that the information given us by the calculus of probabilities will not cease to be true upon the day when these phenomena shall be better known.

The director of a life insurance company does not know when each of the insured will die, but he relies upon the calculus of probabilities and on the law of great numbers, and he is not deceived, since he distributes dividends to his stockholders. These

dividends would not vanish if a very penetrating and very indiscrete physician should, after the policies were signed, reveal to the director the life chances of the insured. This doctor would dissipate the ignorance of the director, but he would have no influence on the dividends, which evidently are not an outcome of this ignorance.

II

To find a better definition of chance we must examine some of the facts which we agree to regard as fortuitous, and to which the calculus of probabilities seems to apply; we then shall investigate what are their common characteristics.

The first example we select is that of unstable equilibrium; if a cone rests upon its apex, we know well that it will fall, but we do not know toward what side; it seems to us chance alone will decide. If the cone were perfectly symmetric, if its axis were perfectly vertical, if it were acted upon by no force other than gravity, it would not fall at all. But the least defect in symmetry will make it lean slightly toward one side or the other, and if it leans, however little, it will fall altogether toward that side. Even if the symmetry were perfect, a very slight tremor, a breath of air could make it incline some seconds of arc; this will be enough to determine its fall and even the sense of its fall which will be that of the initial inclination.

A very slight cause, which escapes us, determines a considerable effect which we can not help seeing, and then we say this effect is due to chance. If we could know exactly the laws of nature and the situation of the universe at the initial instant, we should be able to predict exactly the situation of this same universe at a subsequent instant. But even when the natural laws should have no further secret for us, we could know the initial situation only *approximately*. If that permits us to foresee the subsequent situation *with the same degree of approximation*, this is all we require, we say the phenomenon has been predicted, that it is ruled by laws. But this is not always the case; it may happen that slight differences in the initial conditions produce very great differences in the final phenomena; a slight error in the former would make an enormous error in the

latter. Prediction becomes impossible and we have the fortuitous phenomenon.

Our second example will be very analogous to the first and we shall take it from meteorology. Why have the meteorologists such difficulty in predicting the weather with any certainty? Why do the rains, the tempests themselves seem to us to come by chance, so that many persons find it quite natural to pray for rain or shine, when they would think it ridiculous to pray for an eclipse? We see that great perturbations generally happen in regions where the atmosphere is in unstable equilibrium. The meteorologists are aware that this equilibrium is unstable, that a cyclone is arising somewhere; but where they can not tell; one-tenth of a degree more or less at any point, and the cyclone bursts here and not there, and spreads its ravages over countries it would have spared. This we could have foreseen if we had known that tenth of a degree, but the observations were neither sufficiently close nor sufficiently precise, and for this reason all seems due to the agency of chance. Here again we find the same contrast between a very slight cause, unappreciable to the observer, and important effects, which are sometimes tremendous disasters.

Let us pass to another example, the distribution of the minor planets on the zodiac. Their initial longitudes may have been any longitudes whatever; but their mean motions were different and they have revolved for so long a time that we may say they are now distributed *at random* along the zodiac. Very slight initial differences between their distances from the sun, or, what comes to the same thing, between their mean motions, have ended by giving enormous differences between their present longitudes. An excess of the thousandth of a second in the daily mean motion will give in fact a second in three years, a degree in ten thousand years, an entire circumference in three or four million years, and what is that to the time which has passed since the minor planets detached themselves from the nebula of Laplace? Again therefore we see a slight cause and a great effect; or better, slight differences in the cause and great differences in the effect.

The game of roulette does not take us as far as might seem

from the preceding example. Assume a needle to be turned on a pivot over a dial divided into a hundred sectors alternately red and black. If it stops on a red sector I win; if not, I lose. Evidently all depends upon the initial impulse I give the needle. The needle will make, suppose, ten or twenty turns, but it will stop sooner or not so soon, according as I shall have pushed it more or less strongly. It suffices that the impulse vary only by a thousandth or a two thousandth to make the needle stop over a black sector or over the following red one. These are differences the muscular sense can not distinguish and which elude even the most delicate instruments. So it is impossible for me to foresee what the needle I have started will do, and this is why my heart throbs and I hope everything from luck. The difference in the cause is imperceptible, and the difference in the effect is for me of the highest importance, since it means my whole stake.

III

Permit me, in this connection, a thought somewhat foreign to my subject. Some years ago a philosopher said that the future is determined by the past, but not the past by the future; or, in other words, from knowledge of the present we could deduce the future, but not the past; because, said he, a cause can have only one effect, while the same effect might be produced by several different causes. It is clear no scientist can subscribe to this conclusion. The laws of nature bind the antecedent to the consequent in such a way that the antecedent is as well determined by the consequent as the consequent by the antecedent. But whence came the error of this philosopher? We know that in virtue of Carnot's principle physical phenomena are irreversible and the world tends toward uniformity. When two bodies of different temperature come in contact, the warmer gives up heat to the colder; so we may foresee that the temperature will equalize. But once equal, if asked about the anterior state, what can we answer? We might say that one was warm and the other cold, but not be able to divine which formerly was the warmer.

And yet in reality the temperatures will never reach perfect equality. The difference of the temperatures only tends asymptotically toward zero. There comes a moment when our ther-

mometers are powerless to make it known. But if we had thermometers a thousand times, a hundred thousand times as sensitive, we should recognize that there still is a slight difference, and that one of the bodies remains a little warmer than the other, and so we could say this it is which formerly was much the warmer.

So then there are, contrary to what we found in the former examples, great differences in cause and slight differences in effect. Flammarion once imagined an observer going away from the earth with a velocity greater than that of light; for him time would have changed sign. History would be turned about, and Waterloo would precede Austerlitz. Well, for this observer, effects and causes would be inverted; unstable equilibrium would no longer be the exception. Because of the universal irreversibility, all would seem to him to come out of a sort of chaos in unstable equilibrium. All nature would appear to him delivered over to chance.

IV

Now for other examples where we shall see somewhat different characteristics. Take first the kinetic theory of gases. How should we picture a receptacle filled with gas? Innumerable molecules, moving at high speeds, flash through this receptacle in every direction. At every instant they strike against its walls or each other, and these collisions happen under the most diverse conditions. What above all impresses us here is not the littleness of the causes, but their complexity, and yet the former element is still found here and plays an important rôle. If a molecule deviated right or left from its trajectory, by a very small quantity, comparable to the radius of action of the gaseous molecules, it would avoid a collision or sustain it under different conditions, and that would vary the direction of its velocity after the impact, perhaps by ninety degrees or by a hundred and eighty degrees.

And this is not all; we have just seen that it is necessary to deflect the molecule before the clash by only an infinitesimal, to produce its deviation after the collision by a finite quantity. If then the molecule undergoes two successive shocks, it will suffice to deflect it before the first by an infinitesimal of the second order, for it to deviate after the first encounter by an infinites-

imal of the first order, and after the second hit, by a finite quantity. And the molecule will not undergo merely two shocks; it will undergo a very great number per second. So that if the first shock has multiplied the deviation by a very large number A, after n shocks it will be multiplied by A^n. It will therefore become very great not merely because A is large, that is to say because little causes produce big effects, but because the exponent n is large, that is to say because the shocks are very numerous and the causes very complex.

Take a second example. Why do the drops of rain in a shower seem to be distributed at random? This is again because of the complexity of the causes which determine their formation. Ions are distributed in the atmosphere. For a long while they have been subjected to air-currents constantly changing, they have been caught in very small whirlwinds, so that their final distribution has no longer any relation to their initial distribution. Suddenly the temperature falls, vapor condenses, and each of these ions becomes the center of a drop of rain. To know what will be the distribution of these drops and how many will fall on each paving-stone, it would not be sufficient to know the initial situation of the ions, it would be necessary to compute the effect of a thousand little capricious air-currents.

And again it is the same if we put grains of powder in suspension in water. The vase is ploughed by currents whose law we know not, we only know it is very complicated. At the end of a certain time the grains will be distributed at random, that is to say uniformly, in the vase; and this is due precisely to the complexity of these currents. If they obeyed some simple law, if, for example the vase revolved and the currents circulated around the axis of the vase, describing circles, it would no longer be the same, since each grain would retain its initial altitude and its initial distance from the axis.

We should reach the same result in considering the mixing of two liquids or of two fine-grained powders. And to take a grosser example, this is also what happens when we shuffle playing-cards. At each stroke the cards undergo a permutation (analogous to that studied in the theory of substitutions). What will happen? The probability of a particular permutation (for

example, that bringing to the nth place the card occupying the $\phi(n)$th place before the permutation) depends upon the player's habits. But if this player shuffles the cards long enough, there will be a great number of successive permutations, and the resulting final order will no longer be governed by aught but chance; I mean to say that all possible orders will be equally probable. It is to the great number of successive permutations, that is to say to the complexity of the phenomenon, that this result is due.

A final word about the theory of errors. Here it is that the causes are complex and multiple. To how many snares is not the observer exposed, even with the best instrument! He should apply himself to finding out the largest and avoiding them. These are the ones giving birth to systematic errors. But when he has eliminated those, admitting that he succeeds, there remain many small ones which, their effects accumulating, may become dangerous. Thence come the accidental errors; and we attribute them to chance because their causes are too complicated and too numerous. Here again we have only little causes, but each of them would produce only a slight effect; it is by their union and their number that their effects become formidable.

V

We may take still a third point of view, less important than the first two and upon which I shall lay less stress. When we seek to foresee an event and examine its antecedents, we strive to search into the anterior situation. This could not be done for all parts of the universe and we are content to know what is passing in the neighborhood of the point where the event should occur, or what would appear to have some relation to it. An examination can not be complete and we must know how to choose. But it may happen that we have passed by circumstances which at first sight seemed completely foreign to the foreseen happening, to which one would never have dreamed of attributing any influence and which nevertheless, contrary to all anticipation, come to play an important rôle.

A man passes in the street going to his business; some one knowing the business could have told why he started at such a

time and went by such a street. On the roof works a tiler. The contractor employing him could in a certain measure foresee what he would do. But the passer-by scarcely thinks of the tiler, nor the tiler of him; they seem to belong to two worlds completely foreign to one another. And yet the tiler drops a tile which kills the man, and we do not hesitate to say this is chance.

Our weakness forbids our considering the entire universe and makes us cut it up into slices. We try to do this as little artificially as possible. And yet it happens from time to time that two of these slices react upon each other. The effects of this mutual action then seem to us to be due to chance.

Is this a third way of conceiving chance? Not always; in fact most often we are carried back to the first or the second. Whenever two worlds usually foreign to one another come thus to react upon each other, the laws of this reaction must be very complex. On the other hand, a very slight change in the initial conditions of these two worlds would have been sufficient for the reaction not to have happened. How little was needed for the man to pass a second later or the tiler to drop his tile a second sooner.

VI

All we have said still does not explain why chance obeys laws. Does the fact that the causes are slight or complex suffice for our foreseeing, if not their effects *in each case*, at least what their effects will be, *on the average*? To answer this question we had better take up again some of the examples already cited.

I shall begin with that of the roulette. I have said that the point where the needle will stop depends upon the initial push given it. What is the probability of this push having this or that value? I know nothing about it, but it is difficult for me not to suppose that this probability is represented by a continuous analytic function. The probability that the push is comprised between α and $\alpha + \epsilon$ will then be sensibly equal to the probability of its being comprised between $\alpha + \epsilon$ and $\alpha + 2\epsilon$, *provided* ϵ *be very small.* This is a property common to all analytic functions. Minute variations of the function are proportional to minute variations of the variable.

But we have assumed that an exceedingly slight variation of the push suffices to change the color of the sector over which the needle finally stops. From α to $\alpha + \epsilon$ it is red, from $\alpha + \epsilon$ to $\alpha + 2\epsilon$ it is black; the probability of each red sector is therefore the same as of the following black, and consequently the total probability of red equals the total probability of black.

The datum of the question is the analytic function representing the probability of a particular initial push. But the theorem remains true whatever be this datum, since it depends upon a property common to all analytic functions. From this it follows finally that we no longer need the datum.

What we have just said for the case of the roulette applies also to the example of the minor planets. The zodiac may be regarded as an immense roulette on which have been tossed many little balls with different initial impulses varying according to some law. Their present distribution is uniform and independent of this law, for the same reason as in the preceding case. Thus we see why phenomena obey the laws of chance when slight differences in the causes suffice to bring on great differences in the effects. The probabilities of these slight differences may then be regarded as proportional to these differences themselves, just because these differences are minute, and the infinitesimal increments of a continuous function are proportional to those of the variable.

Take an entirely different example, where intervenes especially the complexity of the causes. Suppose a player shuffles a pack of cards. At each shuffle he changes the order of the cards, and he may change them in many ways. To simplify the exposition, consider only three cards. The cards which before the shuffle occupied respectively the places 123, may after the shuffle occupy the places

123, 231, 312, 321, 132, 213.

Each of these six hypotheses is possible and they have respectively for probabilities:

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

The sum of these six numbers equals 1; but this is all we know of them; these six probabilities depend naturally upon the habits of the player which we do not know.

At the second shuffle and the following, this will recommence, and under the same conditions; I mean that p_4 for example represents always the probability that the three cards which occupied after the nth shuffle and before the $n+1$th the places 123, occupy the places 321 after the $n+1$th shuffle. And this remains true whatever be the number n, since the habits of the player and his way of shuffling remain the same.

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

123, 231, 312, 321, 132, 213

and the probability of these six hypotheses will be sensibly the same and equal to 1/6; and this will be 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 cards the demonstration would be complicated; let it suffice to give it for only two cards. Then we have only two possibilities 12, 21 with the probabilities p_1 and $p_2 = 1 - p_1$.

Suppose n shuffles and suppose I win one franc if the cards are finally in the initial order and lose one if they are finally inverted. Then, my mathematical expectation will be $(p_1 - p_2)^n$.

The difference $p_1 - p_2$ is certainly less than 1; so that if n is very great my expectation will be zero; we need not learn p_1 and p_2 to be aware that the game is equitable.

There would always be an exception if one of the numbers p_1 and p_2 was equal to 1 and the other naught. *Then it would not apply because our initial hypotheses would be too simple.*

What we have just seen applies not only to the mixing of cards, but to all mixings, to those of powders and of liquids; and even to those of the molecules of gases in the kinetic theory of gases.

To return to this theory, suppose for a moment a gas whose molecules can not mutually clash, but may be deviated by hitting the insides of the vase wherein the gas is confined. If the form

of the vase is sufficiently complex the distribution of the molecules and that of the velocities will not be long in becoming uniform. But this will not be so if the vase is spherical or if it has the shape of a cuboid. Why? Because in the first case the distance from the center to any trajectory will remain constant; in the second case this will be the absolute value of the angle of each trajectory with the faces of the cuboid.

So we see what should be understood by conditions *too simple;* they are those which conserve something, which leave an invariant remaining. Are the differential equations of the problem too simple for us to apply the laws of chance? This question would seem at first view to lack precise meaning; now we know what it means. They are too simple if they conserve something, if they admit a uniform integral. If something in the initial conditions remains unchanged, it is clear the final situation can no longer be independent of the initial situation.

We come finally to the theory of errors. We know not to what are due the accidental errors, and precisely because we do not know, we are aware they obey the law of Gauss. Such is the paradox. The explanation is nearly the same as in the preceding cases. We need know only one thing: that the errors are very numerous, that they are very slight, that each may be as well negative as positive. What is the curve of probability of each of them? We do not know; we only suppose it is symmetric. We prove then that the resultant error will follow Gauss's law, and this resulting law is independent of the particular laws which we do not know. Here again the simplicity of the result is born of the very complexity of the data.

VII

But we are not through with paradoxes. I have just recalled the figment of Flammarion, that of the man going quicker than light, for whom time changes sign. I said that for him all phenomena would seem due to chance. That is true from a certain point of view, and yet all these phenomena at a given moment would not be distributed in conformity with the laws of chance, since the distribution would be the same as for us, who, seeing them unfold harmoniously and without coming out of a primal chaos, do not regard them as ruled by chance.

What does that mean? For Lumen, Flammarion's man, slight causes seem to produce great effects; why do not things go on as for us when we think we see grand effects due to little causes? Would not the same reasoning be applicable in his case?

Let us return to the argument. When slight differences in the causes produce vast differences in the effects, why are these effects distributed according to the laws of chance? Suppose a difference of a millimeter in the cause produces a difference of a kilometer in the effect. If I win in case the effect corresponds to a kilometer bearing an even number, my probability of winning will be 1/2. Why? Because to make that, the cause must correspond to a millimeter with an even number. Now, according to all appearance, the probability of the cause varying between certain limits will be proportional to the distance apart of these limits, provided this distance be very small. If this hypothesis were not admitted there would no longer be any way of representing the probability by a continuous function.

What now will happen when great causes produce small effects? This is the case where we should not attribute the phenomenon to chance and where on the contrary Lumen would attribute it to chance. To a difference of a kilometer in the cause would correspond a difference of a millimeter in the effect. Would the probability of the cause being comprised between two limits n kilometers apart still be proportional to n? We have no reason to suppose so, since this distance, n kilometers, is great. But the probability that the effect lies between two limits n millimeters apart will be precisely the same, so it will not be proportional to n, even though this distance, n millimeters, be small. There is no way therefore of representing the law of probability of effects by a continuous curve. This curve, understand, may remain continuous in the *analytic* sense of the word; to *infinitesimal* variations of the abscissa will correspond infinitesimal variations of the ordinate. But *practically* it will not be continuous, since *very small* variations of the ordinate would not correspond to very small variations of the abscissa. It would become impossible to trace the curve with an ordinary pencil; that is what I mean.

So what must we conclude? Lumen has no right to say that

the probability of the cause (*his* cause, our effect) should be represented necessarily by a continuous function. But then why have we this right? It is because this state of unstable equilibrium which we have been calling initial is itself only the final outcome of a long previous history. In the course of this history complex causes have worked a great while: they have contributed to produce the mixture of elements and they have tended to make everything uniform at least within a small region; they have rounded off the corners, smoothed down the hills and filled up the valleys. However capricious and irregular may have been the primitive curve given over to them, they have worked so much toward making it regular that finally they deliver over to us a continuous curve. And this is why we may in all confidence assume its continuity.

Lumen would not have the same reasons for such a conclusion. For him complex causes would not seem agents of equalization and regularity, but on the contrary would create only inequality and differentiation. He would see a world more and more varied come forth from a sort of primitive chaos. The changes he could observe would be for him unforeseen and impossible to foresee. They would seem to him due to some caprice or another; but this caprice would be quite different from our chance, since it would be opposed to all law, while our chance still has its laws. All these points call for lengthy explications, which perhaps would aid in the better comprehension of the irreversibility of the universe.

VIII

We have sought to define chance, and now it is proper to put a question. Has chance thus defined, in so far as this is possible, objectivity?

It may be questioned. I have spoken of very slight or very complex causes. But what is very little for one may be very big for another, and what seems very complex to one may seem simple to another. In part I have already answered by saying precisely in what cases differential equations become too simple for the laws of chance to remain applicable. But it is fitting to examine the matter a little more closely, because we may take still other points of view.

What means the phrase 'very slight'? To understand it we need only go back to what has already been said. A difference is very slight, an interval is very small, when within the limits of this interval the probability remains sensibly constant. And why may this probability be regarded as constant within a small interval? It is because we assume that the law of probability is represented by a continuous curve, continuous not only in the analytic sense, but *practically* continuous, as already explained. This means that it not only presents no absolute hiatus, but that it has neither salients nor reentrants too acute or too accentuated.

And what gives us the right to make this hypothesis? We have already said it is because, since the beginning of the ages, there have always been complex causes ceaselessly acting in the same way and making the world tend toward uniformity without ever being able to turn back. These are the causes which little by little have flattened the salients and filled up the reentrants, and this is why our probability curves now show only gentle undulations. In milliards of milliards of ages another step will have been made toward uniformity, and these undulations will be ten times as gentle; the radius of mean curvature of our curve will have become ten times as great. And then such a length as seems to us to-day not very small, since on our curve an arc of this length can not be regarded as rectilineal, should on the contrary at that epoch be called very little, since the curvature will have become ten times less and an arc of this length may be sensibly identified with a sect.

Thus the phrase 'very slight' remains relative; but it is not relative to such or such a man, it is relative to the actual state of the world. It will change its meaning when the world shall have become more uniform, when all things shall have blended still more. But then doubtless men can no longer live and must give place to other beings—should I say far smaller or far larger? So that our criterion, remaining true for all men, retains an objective sense.

And on the other hand what means the phrase 'very complex'? I have already given one solution, but there are others. Complex causes we have said produce a blend more and more inti-

mate, but after how long a time will this blend satisfy us? When will it have accumulated sufficient complexity? When shall we have sufficiently shuffled the cards? If we mix two powders, one blue, the other white, there comes a moment when the tint of the mixture seems to us uniform because of the feebleness of our senses; it will be uniform for the presbyte, forced to gaze from afar, before it will be so for the myope. And when it has become uniform for all eyes, we still could push back the limit by the use of instruments. There is no chance for any man ever to discern the infinite variety which, if the kinetic theory is true, hides under the uniform appearance of a gas. And yet if we accept Gouy's ideas on the Brownian movement, does not the microscope seem on the point of showing us something analogous?

This new criterion is therefore relative like the first; and if it retains an objective character, it is because all men have approximately the same senses, the power of their instruments is limited, and besides they use them only exceptionally.

IX

It is just the same in the moral sciences and particularly in history. The historian is obliged to make a choice among the events of the epoch he studies; he recounts only those which seem to him the most important. He therefore contents himself with relating the most momentous events of the sixteenth century, for example, as likewise the most remarkable facts of the seventeenth century. If the first suffice to explain the second, we say these conform to the laws of history. But if a great event of the seventeenth century should have for cause a small fact of the sixteenth century which no history reports, which all the world has neglected, then we say this event is due to chance. This word has therefore the same sense as in the physical sciences; it means that slight causes have produced great effects.

The greatest bit of chance is the birth of a great man. It is only by chance that meeting of two germinal cells, of different sex, containing precisely, each on its side, the mysterious elements whose mutual reaction must produce the genius. One will agree that these elements must be rare and that their meeting is still more rare. How slight a thing it would have required to deflect from its route the carrying spermatozoon. It would have

sufficed to deflect it a tenth of a millimeter and Napoleon would not have been born and the destinies of a continent would have been changed. No example can better make us understand the veritable characteristics of chancé.

One more word about the paradoxes brought out by the application of the calculus of probabilities to the moral sciences. It has been proved that no Chamber of Deputies will ever fail to contain a member of the opposition, or at least such an event would be so improbable that we might without fear wager the contrary, and bet a million against a sou.

Condorcet has striven to calculate how many jurors it would require to make a judicial error practically impossible. If we had used the results of this calculation, we should certainly have been exposed to the same disappointments as in betting, on the faith of the calculus, that the opposition would never be without a representative.

The laws of chance do not apply to these questions. If justice be not always meted out to accord with the best reasons, it uses less than we think the method of Bridoye. This is perhaps to be regretted, for then the system of Condorcet would shield us from judicial errors.

What is the meaning of this? 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 so, since they conserve something. We have seen that this it is which distinguishes causes 'too simple.' When men are brought together they no longer decide at random and independently one of another; they influence one another. Multiplex causes come into action. They worry men, dragging them to right or left, but one thing there is they can not destroy, this is their Panurge flock-of-sheep habits. And this is an invariant.

X

Difficulties are indeed involved in the application of the calculus of probabilities to the exact sciences. Why are the decimals of a table of logarithms, why are those of the number π distributed in accordance with the laws of chance? Elsewhere I have already studied the question in so far as it concerns log-

arithms, and there it is easy. It is clear that a slight difference of argument will give a slight difference of logarithm, but a great difference in the sixth decimal of the logarithm. Always we find again the same criterion.

But as for the number π, that presents more difficulties, and I have at the moment nothing worth while to say.

There would be many other questions to resolve, had I wished to attack them before solving that which I more specially set myself. When we reach a simple result, when we find for example a round number, we say that such a result can not be due to chance, and we seek, for its explanation, a non-fortuitous cause. And in fact there is only a very slight probability that among 10,000 numbers chance will give a round number; for example, the number 10,000. This has only one chance in 10,000. But there is only one chance in 10,000 for the occurrence of any other one number; and yet this result will not astonish us, nor will it be hard for us to attribute it to chance; and that simply because it will be less striking.

Is this a simple illusion of ours, or are there cases where this way of thinking is legitimate? We must hope so, else were all science impossible. When we wish to check a hypothesis, what do we do? We can not verify all its consequences, since they would be infinite in number; we content ourselves with verifying certain ones and if we succeed we declare the hypothesis confirmed, because so much success could not be due to chance. And this is always at bottom the same reasoning.

I can not completely justify it here, since it would take too much time; but I may at least say that we find ourselves confronted by two hypotheses, either a simple cause or that aggregate of complex causes we call chance. We find it natural to suppose that the first should produce a simple result, and then, if we find that simple result, the round number for example, it seems more likely to us to be attributable to the simple cause which must give it almost certainly, than to chance which could only give it once in 10,000 times. It will not be the same if we find a result which is not simple; chance, it is true, will not give this more than once in 10,000 times; but neither has the simple cause any more chance of producing it.