SCIENCE
AND HYPOTHESIS

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CHAPTER XI.

No doubt the reader will be astonished to find reflections on the calculus of probabilities in such a volume as this. What has that calculus to do with physical science? The questions I shall raise —without, however, giving them a solution—are naturally raised by the philosopher who is examining the problems of physics. So far is this the case, that in the two preceding chapters I have several times used the words "probability" and "chance." "Predicted facts," as I said above, "can only be probable." However solidly founded a prediction may appear to be, we are never absolutely certain that experiment will not prove it false; but the probability is often so great that practically it may be accepted. And a little farther on I added:—"See what a part the belief in simplicity plays in our generalisations. We have verified a simple law in a large number of particular cases, and we refuse to admit that this so-often-repeated coincidence is a mere effect of chance." Thus, in a multitude of circumstances the physicist is often in the same position as the gambler who reckons up his chances. Every time that he reasons by

induction, he more or less consciously requires the calculus of probabilities, and that is why I am obliged to open this chapter parenthetically, and to interrupt our discussion of method in the physical sciences in order to examine a little closer what this calculus is worth, and what dependence we may place upon it. The very name of the calculus of probabilities is a paradox. Probability as opposed to certainty is what one does not know, and how can we calculate the unknown? Yet many eminent scientists have devoted themselves to this calculus, and it cannot be denied that science has drawn therefrom no small advantage. How can we explain this apparent contradiction? Has probability been defined? Can it even be defined? And if it cannot, how can we venture to reason upon it? The definition, it will be said, is very simple. The probability of an event is the ratio of the number of cases favourable to the event to the total number of possible cases. A simple example will show how incomplete this definition is:—I throw two dice. What is the probability that one of the two at least turns up a 6? Each can turn up in six different ways; the number of possible cases is $6 \times 6 = 36$. The number of favourable cases is 11; the probability is $\frac{11}{36}$. That is the correct solution. But why cannot we just as well proceed as follows? —The points which turn up on the two dice form $\frac{6 \times 7}{2} = 21$ different combinations. Among these combinations, six are favourable; the probability

is $\frac{6}{21}$.   Now why is the first method of calculating
the number of possible cases more legitimate than
the second ?   In any case it is not the definition
that tells us.   We are therefore bound to complete
the definition by saying, ". . . to the total number
of possible cases, provided the cases are equally
probable."   So we are compelled to define the
probable by the probable.   How can we know
that two possible cases are equally probable ?
Will it be by a convention ?   If we insert at the
beginning of every problem an explicit convention,
well and good !   We then have nothing to do but to
apply the rules of arithmetic and algebra, and we
complete our calculation, when our result cannot
be called in question.   But if we wish to make the
slightest application of this result, we must prove
that our convention is legitimate, and we shall find
ourselves in the presence of the very difficulty we
thought we had avoided.   It may be said that
common-sense is enough to show us the convention
that should be adopted.   Alas ! M. Bertrand has
amused himself by discussing the following simple
problem :—" What is the probability that a chord
of a circle may be greater than the side of the
inscribed equilateral triangle ?"   The illustrious
geometer successively adopted two conventions
which seemed to be equally imperative in the eyes
of common-sense, and with one convention he finds
$\frac{1}{2}$, and with the other $\frac{1}{3}$.   The conclusion which
seems to follow from this is that the calculus of
probabilities is a useless science, that the obscure

instinct which we call common-sense, and to which
we appeal for the legitimisation of our conventions,
must be distrusted.   But to this conclusion we can
no longer subscribe.   We cannot do without that
obscure instinct.   Without it, science would be
impossible, and without it we could neither discover
nor apply a law.   Have we any right, for instance,
to enunciate Newton's law?   No doubt numerous
observations are in agreement with it, but is not
that a simple fact of chance? and how do we know,
besides, that this law which has been true for so
many generations will not be untrue in the next?
To this objection the only answer you can give is:
It is very improbable.   But grant the law.   By
means of it I can calculate the position of Jupiter
in a year from now.   Yet have I any right to say
this? Who can tell if a gigantic mass of enormous
velocity is not going to pass near the solar system
and produce unforeseen perturbations?   Here
again the only answer is: It is very improbable.
From this point of view all the sciences would only
be unconscious applications of the calculus of prob-
abilities.   And if this calculus be condemned, then
the whole of the sciences must also be condemned.
I shall not dwell at length on scientific problems
in which the intervention of the calculus of prob-
abilities is more evident.   In the forefront of these
is the problem of interpolation, in which, knowing
a certain number of values of a function, we try
to discover the intermediary values.   I may also
mention the celebrated theory of errors of observa-

tion, to which I shall return later; the kinetic theory of gases, a well-known hypothesis wherein each gaseous molecule is supposed to describe an extremely complicated path, but in which, through the effect of great numbers, the mean phenomena which are all we observe obey the simple laws of Mariotte and Gay-Lussac.   All these theories are based upon the laws of great numbers, and the calculus of probabilities would evidently involve them in its ruin.   It is true that they have only a particular interest, and that, save as far as inter-polation is concerned, they are sacrifices to which we might readily be resigned.   But I have said above, it would not be these partial sacrifices that would be in question; it would be the legitimacy of the whole of science that would be challenged. I quite see that it might be said: We do not know, and yet we must act.   As for action, we have not time to devote ourselves to an inquiry that will suffice to dispel our ignorance.   Besides, such an inquiry would demand unlimited time.   We must therefore make up our minds without knowing. This must be often done whatever may happen, and we must follow the rules although we may have but little confidence in them.   What I know is, not that such a thing is true, but that the best course for me is to act as if it were true.   The calculus of probabilities, and therefore science itself, would be no longer of any practical value.

Unfortunately the difficulty does not thus dis-appear.   A gambler wants to try a *coup*, and he

asks my advice. If I give it him, I use the calculus of probabilities; but I shall not guarantee success. That is what I shall call *subjective probability*. In this case we might be content with the explanation of which I have just given a sketch. But assume that an observer is present at the play, that he knows of the *coup*, and that play goes on for a long time, and that he makes a summary of his notes. He will find that events have taken place in conformity with the laws of the calculus of probabilities. That is what I shall call *objective probability*, and it is this phenomenon which has to be explained. There are numerous Insurance Societies which apply the rules of the calculus of probabilities, and they distribute to their shareholders dividends, the objective reality of which cannot be contested. In order to explain them, we must do more than invoke our ignorance and the necessity of action. Thus, absolute scepticism is not admissible. We may distrust, but we cannot condemn *en bloc*. Discussion is necessary.

I. *Classification of the Problems of Probability.*—In order to classify the problems which are presented to us with reference to probabilities, we must look at them from different points of view, and first of all, from that of *generality*. I said above that probability is the ratio of the number of favourable to the number of possible cases. What for want of a better term I call generality will increase with the number of possible cases. This number may be finite, as, for instance, if we take a throw of the

dice in which the number of possible cases is 36. That is the first degree of generality.   But if we ask, for instance, what is the probability that a point within a circle is within the inscribed square, there are as many possible cases as there are points in the circle—that is to say, an infinite number. This is the second degree of generality.   Generality can be pushed further still.   We may ask the probability that a function will satisfy a given condition.   There are then as many possible cases as one can imagine different functions.   This is the third degree of generality, which we reach, for instance, when we try to find the most probable law after a finite number of observations.   Yet we may place ourselves at a quite different point of view.   If we were not ignorant there would be no probability, there could only be certainty.   But our ignorance cannot be absolute, for then there would be no longer any probability at all.   Thus the problems of probability may be classed according to the greater or less depth of this ignorance.   In mathematics we may set ourselves problems in probability.   What is the probability that the fifth decimal of a logarithm taken at random from a table is a 9.   There is no hesitation in answering that this probability is 1-10th.   Here we possess all the data of the problem.   We can calculate our logarithm without having recourse to the table, but we need not give ourselves the trouble. This is the first degree of ignorance.   In the physical sciences our ignorance is already greater.

The state of a system at a given moment depends on two things—its initial state, and the law according to which that state varies. If we know both this law and this initial state, we have a simple mathematical problem to solve, and we fall back upon our first degree of ignorance. Then it often happens that we know the law and do not know the initial state. It may be asked, for instance, what is the present distribution of the minor planets? We know that from all time they have obeyed the laws of Kepler, but we do not know what was their initial distribution. In the kinetic theory of gases we assume that the gaseous molecules follow rectilinear paths and obey the laws of impact and elastic bodies; yet as we know nothing of their initial velocities, we know nothing of their present velocities. The calculus of probabilities alone enables us to predict the mean phenomena which will result from a combination of these velocities. This is the second degree of ignorance. Finally it is possible, that not only the initial conditions but the laws themselves are unknown. We then reach the third degree of ignorance, and in general we can no longer affirm anything at all as to the probability of a phenomenon. It often happens that instead of trying to discover an event by means of a more or less imperfect knowledge of the law, the events may be known, and we want to find the law; or that, instead of deducing effects from causes, we wish to deduce the causes

from the effects.   Now, these problems are classified
as *probability of causes*, and are the most interesting
of all from their scientific applications.   I play at
écarté with a gentleman whom I know to be per-
fectly honest.   What is the chance that he turns
up the king ?   It is $\frac{1}{8}$.   This is a problem of the
probability of effects.   I play with a gentleman
whom I do not know.   He has dealt ten times,
and he has turned the king up six times.   What
is the chance that he is a sharper ?   This is a
problem in the probability of causes.   It may be
said that it is the essential problem of the experi-
mental method.   I have observed n values of x
and the corresponding values of y.   I have found
that the ratio of the latter to the former is prac-
tically constant.   There is the event ; what is
the cause ?   Is it probable that there is a general
law according to which y would be proportional
to x, and that small divergencies are due to errors
of observation ?   This is the type of question that
we are ever asking, and which we unconsciously
solve whenever we are engaged in scientific work.
I am now going to pass in review these different
categories of problems by discussing in succession
what I have called subjective and objective prob-
ability.

II. *Probability in Mathematics.* — The impossi-
bility of squaring the circle was shown in 1885, but
before that date all geometers considered this im-
possibility as so "probable" that the Académie des
Sciences rejected without examination the, alas !

too numerous memoirs on this subject that a
few unhappy madmen sent in every year. Was
the Académie wrong? Evidently not, and it
knew perfectly well that by acting in this
manner it did not run the least risk of stifling
a discovery of moment. The Académie could
not have proved that it was right, but it knew
quite well that its instinct did not deceive it.
If you had asked the Academicians, they would
have answered: "We have compared the prob-
ability that an unknown scientist should have
found out what has been vainly sought for so
long, with the probability that there is one mad-
man the more on the earth, and the latter has
appeared to us the greater." These are very
good reasons, but there is nothing mathematical
about them; they are purely psychological. If
you had pressed them further, they would have
added: "Why do you expect a particular value of
a transcendental function to be an algebraical
number; if π be the root of an algebraical equa-
tion, why do you expect this root to be a period of
the function $sin\ 2x$, and why is it not the same
with the other roots of the same equation?" To
sum up, they would have invoked the principle of
sufficient reason in its vaguest form. Yet what
information could they draw from it? At most a
rule of conduct for the employment of their time,
which would be more usefully spent at their
ordinary work than in reading a lucubration
that inspired in them a legitimate distrust. But

what I called above objective probability has
nothing in common with this first problem.   It is
otherwise with the second.   Let us consider the
first 10,000 logarithms that we find in a table.
Among these 10,000 logarithms I take one at
random.   What is the probability that its third
decimal is an even number?   You will say with-
out any hesitation that the probability is $\frac{1}{2}$, and in
fact if you pick out in a table the third decimals
in these 10,000 numbers you will find nearly as
many even digits as odd.   Or, if you prefer it, let
us write 10,000 numbers corresponding to our
10,000 logarithms, writing down for each of these
numbers + 1 if the third decimal of the correspond-
ing logarithm is even, and − 1 if odd; and then
let us take the mean of these 10,000 numbers.   I
do not hesitate to say that the mean of these
10,000 units is probably zero, and if I were to
calculate it practically, I would verify that it is
extremely small.   But this verification is needless.
I might have rigorously proved that this mean is
smaller than 0.003.   To prove this result I should
have had to make a rather long calculation for
which there is no room here, and for which I
may refer the reader to an article that I pub-
lished in the *Revue générale des Sciences*, April
15th, 1899.   The only point to which I wish to
draw attention is the following.   In this calcula-
tion I had occasion to rest my case on only two
facts—namely, that the first and second derivatives
of the logarithm remain, in the interval considered,

between certain limits. Hence our first conclusion is that the property is not only true of the logarithm but of any continuous function what-ever, since the derivatives of every continuous function are limited. If I was certain beforehand of the result, it is because I have often observed analogous facts for other continuous functions; and next, it is because I went through in my mind in a more or less unconscious and imperfect manner the reasoning which led me to the preceding in-equalities, just as a skilled calculator before finish-ing his multiplication takes into account what it ought to come to approximately. And besides, since what I call my intuition was only an incom-plete summary of a piece of true reasoning, it is clear that observation has confirmed my predic-tions, and that the objective and subjective proba-bilities are in agreement. As a third example I shall choose the following:—The number u is taken at random and n is a given very large integer. What is the mean value of sin nu? This problem has no meaning by itself. To give it one, a convention is required—namely, we agree that the probability for the number u to lie between a and $a + da$ is $\phi(a)da$; that it is therefore proportional to the infinitely small interval da, and is equal to this multiplied by a function $\phi(a)$, only depending on a. As for this function I choose it arbitrarily, but I must assume it to be continuous. The value of sin nu remaining the same when u increases by 2π, I may without loss of generality assume that

u lies between 0 and 2π, and I shall thus be
led to suppose that $\phi(a)$ is a periodic function
whose period is 2π. The mean value that we
seek is readily expressed by a simple integral,
and it is easy to show that this integral is smaller
than $\frac{2\pi M_\kappa}{n^\kappa}$, M_κ being the maximum value of the
κth derivative of $\phi(u)$. We see then that if the
κth derivative is finite, our mean value will
tend towards zero when n increases indefinitely,
and that more rapidly than $\frac{1}{n^{\kappa-1}}$. The mean
value of $\sin nu$ when n is very large is therefore
zero. To define this value I required a conven-
tion, but the result remains the same *whatever
that convention may be.* I have imposed upon
myself but slight restrictions when I assumed that
the function $\phi(a)$ is continuous and periodic, and
these hypotheses are so natural that we may ask
ourselves how they can be escaped. Examination
of the three preceding examples, so different in all
respects, has already given us a glimpse on the
one hand of the rôle of what philosophers call the
principle of sufficient reason, and on the other hand
of the importance of the fact that certain pro-
perties are common to all continuous functions.
The study of probability in the physical sciences
will lead us to the same result.

III. *Probability in the Physical Sciences.*—We
now come to the problems which are connected
with what I have called the second degree of

ignorance—namely, those in which we know the
law but do not know the initial state of the
system. I could multiply examples, but I shall
take only one. What is the probable present
distribution of the minor planets on the zodiac?
We know they obey the laws of Kepler. We may
even, without changing the nature of the problem,
suppose that their orbits are circular and situated
in the same plane, a plane which we are given.
On the other hand, we know absolutely nothing
about their initial distribution. However, we do
not hesitate to affirm that this distribution is now
nearly uniform. Why? Let b be the longitude
of a minor planet in the initial epoch—that is to
say, the epoch zero. Let a be its mean motion.
Its longitude at the present time—*i.e.*, at the time
t will be $at + b$. To say that the present distribu-
tion is uniform is to say that the mean value of
the sines and cosines of multiples of $at + b$ is zero.
Why do we assert this? Let us represent our
minor planet by a point in a plane—namely, the
point whose co-ordinates are a and b. All these
representative points will be contained in a certain
region of the plane, but as they are very numerous
this region will appear dotted with points. We
know nothing else about the distribution of the
points. Now what do we do when we apply the
calculus of probabilities to such a question as
this? What is the probability that one or more
representative points may be found in a certain
portion of the plane? In our ignorance we are

compelled to make an arbitrary hypothesis. To
explain the nature of this hypothesis I may be
allowed to use, instead of a mathematical formula,
a crude but concrete image.   Let us suppose
that over the surface of our plane has been
spread imaginary matter, the density of which is
variable, but varies continuously.   We shall then
agree to say that the probable number of repre-
sentative points to be found on a certain portion
of the plane is proportional to the quantity of
this imaginary matter which is found there.   If
there are, then, two regions of the plane of the
same extent, the probabilities that a representative
point of one of our minor planets is in one or
other of these regions will be as the mean densities
of the imaginary matter in one or other of the
regions.   Here then are two distributions, one
real, in which the representative points are very
numerous, very close together, but discrete like the
molecules of matter in the atomic hypothesis; the
other remote from reality, in which our representa-
tive points are replaced by imaginary continuous
matter.   We know that the latter cannot be real,
but we are forced to adopt it through our ignorance.
If, again, we had some idea of the real distribution
of the representative points, we could arrange it so
that in a region of some extent the density of this
imaginary continuous matter may be nearly pro-
portional to the number of representative points,
or, if it is preferred, to the number of atoms which
are contained in that region.   Even that is im-

possible, and our ignorance is so great that we are forced to choose arbitrarily the function which defines the density of our imaginary matter. We shall be compelled to adopt a hypothesis from which we can hardly get away; we shall sup- pose that this function is continuous. That is sufficient, as we shall see, to enable us to reach our conclusion.

What is at the instant t the probable distribu- tion of the minor planets—or rather, what is the mean value of the sine of the longitude at the moment t—i.e., of sin $(at+b)$? We made at the outset an arbitrary convention, but if we adopt it, this probable value is entirely defined. Let us decompose the plane into elements of surface. Consider the value of sin $(at+b)$ at the centre of each of these elements. Multiply this value by the surface of the element and by the corresponding density of the imaginary matter. Let us then take the sum for all the elements of the plane. This sum, by definition, will be the probable mean value we seek, which will thus be expressed by a double integral. It may be thought at first that this mean value depends on the choice of the function ϕ which defines the density of the imagin- ary matter, and as this function ϕ is arbitrary, we can, according to the arbitrary choice which we make, obtain a certain mean value. But this is not the case. A simple calculation shows us that our double integral decreases very rapidly as t increases. Thus, I cannot tell what hypothesis to

make as to the probability of this or that initial distribution, but when once the hypothesis is made the result will be the same, and this gets me out of my difficulty. Whatever the function ϕ may be, the mean value tends towards zero as t increases, and as the minor planets have certainly accomplished a very large number of revolutions, I may assert that this mean value is very small. I may give to ϕ any value I choose, with one restriction: this function must be continuous; and, in fact, from the point of view of subjective probability, the choice of a discontinuous function would have been unreasonable. What reason could I have, for instance, for supposing that the initial longitude might be exactly $o°$, but that it could not lie between $o°$ and $1°$?

The difficulty reappears if we look at it from the point of view of objective probability; if we pass from our imaginary distribution in which the supposititious matter was assumed to be continuous, to the real distribution in which our representative points are formed as discrete atoms. The mean value of $\sin (at + b)$ will be represented quite simply by

$$\frac{1}{n} \ \Sigma \ \sin (at + b),$$

n being the number of minor planets. Instead of a double integral referring to a continuous function, we shall have a sum of discrete terms. However, no one will seriously doubt that this mean value is practically very small. Our repre-

sentative points being very close together, our
discrete sum will in general differ very little from
an integral.   An integral is the limit towards
which a sum of terms tends when the number of
these terms is indefinitely increased.   If the terms
are very numerous, the sum will differ very little
from its limit—that is to say, from the integral,
and what I said of the latter will still be true of
the sum itself.   But there are exceptions.   If, for
instance, for all the minor planets $b = \dfrac{\pi}{2} - at$, the
longitude of all the planets at the time t would be
$\dfrac{\pi}{2}$, and the mean value in question would be
evidently unity.   For this to be the case at the
time o, the minor planets must have all been
lying on a kind of spiral of peculiar form, with
its spires very close together.   All will admit that
such an initial distribution is extremely im-
probable (and even if it were realised, the distribu-
tion would not be uniform at the present time—for
example, on the 1st January 1900 ; but it would
become so a few years later).   Why, then, do we
think this initial distribution improbable ?   This
must be explained, for if we are wrong in rejecting
as improbable this absurd hypothesis, our inquiry
breaks down, and we can no longer affirm any-
thing on the subject of the probability of this or
that present distribution.   Once more we shall
invoke the principle of sufficient reason, to which
we must always recur.   We might admit that at
the beginning the planets were distributed almost

in a straight line. We might admit that they were irregularly distributed. But it seems to us that there is no sufficient reason for the unknown cause that gave them birth to have acted along a curve so regular and yet so complicated, which would appear to have been expressly chosen so that the distribution at the present day would not be uniform.

IV. *Rouge et Noir.*—The questions raised by games of chance, such as roulette, are, fundamentally, quite analogous to those we have just treated. For example, a wheel is divided into thirty-seven equal compartments, alternately red and black. A ball is spun round the wheel, and after having moved round a number of times, it stops in front of one of these sub-divisions. The probability that the division is red is obviously $\frac{1}{2}$. The needle describes an angle θ, including several complete revolutions. I do not know what is the probability that the ball is spun with such a force that this angle should lie between θ and $\theta + d\theta$, but I can make a convention. I can suppose that this probability is $\phi(\theta)d\theta$. As for the function $\phi(\theta)$, I can choose it in an entirely arbitrary manner. I have nothing to guide me in my choice, but I am naturally induced to suppose the function to be continuous. Let ϵ be a length (measured on the circumference of the circle of radius unity) of each red and black compartment. We have to calculate the integral of $\phi(\theta)d\theta$, extending it on the one hand to all the red, and on the other hand to all

the black compartments, and to compare the
results. Consider an interval 2ϵ comprising two
consecutive red and black compartments. Let
M and m be the maximum and minimum values of
the function $\phi(\theta)$ in this interval. The integral
extended to the red compartments will be smaller
than Σ Mϵ; extended to the black it will be greater
than Σ $m\epsilon$. The difference will therefore be
smaller than Σ (M – m) ϵ. But if the function ϕ is
supposed continuous, and if on the other hand the
interval ϵ is very small with respect to the total
angle described by the needle, the difference M – m
will be very small. The difference of the two
integrals will be therefore very small, and the
probability will be very nearly $\frac{1}{2}$. We see that
without knowing anything of the function ϕ we
must act as if the probability were $\frac{1}{2}$. And on
the other hand it explains why, from the
objective point of view, if I watch a certain
number of *coups*, observation will give me almost
as many black *coups* as red. All the players
know this objective law; but it leads them into a
remarkable error, which has often been exposed,
but into which they are always falling. When
the red has won, for example, six times running,
they bet on black, thinking that they are playing
an absolutely safe game, because they say it is
a very rare thing for the red to win seven times
running. In reality their probability of winning
is still $\frac{1}{2}$. Observation shows, it is true, that
the series of seven consecutive reds is very rare,

but series of six reds followed by a black are also very rare. They have noticed the rarity of the series of seven reds; if they have not remarked the rarity of six reds and a black, it is only because such series strike the attention less.

V. *The Probability of Causes.*—We now come to the problems of the probability of causes, the most important from the point of view of scientific applications. Two stars, for instance, are very close together on the celestial sphere. Is this apparent contiguity a mere effect of chance ? Are these stars, although almost on the same visual ray, situated at very different distances from the earth, and therefore very far indeed from one another ? or does the apparent correspond to a real contiguity ? This is a problem on the probability of causes.

First of all, I recall that at the outset of all problems of probability of effects that have occupied our attention up to now, we have had to use a convention which was more or less justified; and if in most cases the result was to a certain extent independent of this convention, it was only the condition of certain hypotheses which enabled us *à priori* to reject discontinuous functions, for example, or certain absurd con- ventions. We shall again find something analogous to this when we deal with the prob- ability of causes. An effect may be produced by the cause *a* or by the cause *b*. The effect has just been observed. We ask the probability

that it is due to the cause a. This is an *à
posteriori* probability of cause. But I could not
calculate it, if a convention more or less justified
did not tell me in advance what is the *à priori*
probability for the cause a to come into play—
I mean the probability of this event to some one
who had not observed the effect. To make my
meaning clearer, I go back to the game of *écarté*
mentioned before. My adversary deals for the
first time and turns up a king. What is the
probability that he is a sharper? The formulæ
ordinarily taught give $\frac{8}{9}$, a result which is
obviously rather surprising. If we look at it
closer, we see that the conclusion is arrived at
as if, before sitting down at the table, I had
considered that there was one chance in two
that my adversary was not honest. An absurd
hypothesis, because in that case I should certainly
not have played with him; and this explains the
absurdity of the conclusion. The function on
the *à priori* probability was unjustified, and that
is why the conclusion of the *à posteriori* probability
led me into an inadmissible result. The import-
ance of this preliminary convention is obvious.
I shall even add that if none were made, the
problem of the *à posteriori* probability would have
no meaning. It must be always made either
explicitly or tacitly.

Let us pass on to an example of a more
scientific character. I require to determine an
experimental law; this law, when discovered, can

be represented by a curve. I make a certain number of isolated observations, each of which may be represented by a point. When I have obtained these different points, I draw a curve between them as carefully as possible, giving my curve a regular form, avoiding sharp angles, accentuated inflexions, and any sudden variation of the radius of curvature. This curve will represent to me the probable law, and not only will it give me the values of the functions intermediary to those which have been observed, but it also gives me the observed values more accurately than direct observation does; that is why I make the curve pass near the points and not through the points themselves.

Here, then, is a problem in the probability of causes. The effects are the measurements I have recorded; they depend on the combination of two causes—the true law of the phenomenon and errors of observation. Knowing the effects, we have to find the probability that the phenomenon shall obey this law or that, and that the observations have been accompanied by this or that error. The most probable law, therefore, corresponds to the curve we have traced, and the most probable error is represented by the distance of the corresponding point from that curve. But the problem has no meaning if before the observations I had an *à priori* idea of the probability of this law or that, or of the chances of error to which I am exposed. If my instruments are

good (and I knew whether this is so or not before
beginning the observations), I shall not draw the
curve far from the points which represent the
rough measurements.   If they are inferior, I may
draw it a little farther from the points, so that I
may get a less sinuous curve; much will be sacri-
ficed to regularity.

Why, then, do I draw a curve without sinu-
osities?   Because I consider *à priori* a law
represented by a continuous function (or function
the derivatives of which to a high order are small),
as more probable than a law not satisfying those
conditions.   But for this conviction the problem
would have no meaning; interpolation would be
impossible; no law could be deduced from a
finite number of observations; science would
cease to exist.

Fifty years ago physicists considered, other
things being equal, a simple law as more probable
than a complicated law.   This principle was even
invoked in favour of Mariotte's law as against
that of Regnault.   But this belief is now
repudiated; and yet, how many times are we
compelled to act as though we still held it!
However that may be, what remains of this
tendency is the belief in continuity, and as we
have just seen, if the belief in continuity were
to disappear, experimental science would become
impossible.

VI. *The Theory of Errors.*—We are thus brought
to consider the theory of errors which is directly

connected with the problem of the probability of causes. Here again we find *effects*—to wit, a certain number of irreconcilable observations, and we try to find the *causes* which are, on the one hand, the true value of the quantity to be measured, and, on the other, the error made in each isolated observation. We must calculate the probable *à posteriori* value of each error, and therefore the probable value of the quantity to be measured. But, as I have just explained, we cannot undertake this calculation unless we admit *à priori*—*i.e.*, before any observations are made— that there is a law of the probability of errors. Is there a law of errors? The law to which all calculators assent is Gauss's law, that is represented by a certain transcendental curve known as the "bell."

But it is first of all necessary to recall the classic distinction between systematic and accidental errors. If the metre with which we measure a length is too long, the number we get will be too small, and it will be no use to measure several times—that is a systematic error. If we measure with an accurate metre, we may make a mistake, and find the length sometimes too large and sometimes too small, and when we take the mean of a large number of measurements, the error will tend to grow small. These are accidental errors.

It is clear that systematic errors do not satisfy Gauss's law, but do accidental errors satisfy it?

Numerous proofs have been attempted, almost all of them crude paralogisms. But starting from the following hypotheses we may prove Gauss's law: the error is the result of a very large number of partial and independent errors; each partial error is very small and obeys any law of probability whatever, provided the probability of a positive error is the same as that of an equal negative error. It is clear that these conditions will be often, but not always, fulfilled, and we may reserve the name of accidental for errors which satisfy them.

We see that the method of least squares is not legitimate in every case; in general, physicists are more distrustful of it than astronomers. This is no doubt because the latter, apart from the systematic errors to which they and the physicists are subject alike, have to contend with an extremely important source of error which is entirely accidental—I mean atmospheric undulations. So it is very curious to hear a discussion between a physicist and an astronomer about a method of observation. The physicist, persuaded that one good measurement is worth more than many bad ones, is pre-eminently concerned with the elimination by means of every precaution of the final systematic errors; the astronomer retorts: "But you can only observe a small number of stars, and accidental errors will not disappear."

What conclusion must we draw? Must we continue to use the method of least squares?

We must distinguish. We have eliminated all
the systematic errors of which we have any
suspicion ; we are quite certain that there are
others still, but we cannot detect them ; and yet
we must make up our minds and adopt a definitive
value which will be regarded as the probable
value ; and for that purpose it is clear that the
best thing we can do is to apply Gauss's law.
We have only applied a practical rule referring
to subjective probability. And there is no more
to be said.

Yet we want to go farther and say that not
only the probable value is so much, but that the
probable error in the result is so much. *This
is absolutely invalid :* it would be true only if
we were sure that all the systematic errors
were eliminated, and of that we know absolutely
nothing. We have two series of observations; by
applying the law of least squares we find that the
probable error in the first series is twice as small
as in the second. The second series may, how-
ever, be more accurate than the first, because the
first is perhaps affected by a large systematic
error. All that we can say is, that the first series
is *probably* better than the second because its
accidental error is smaller, and that we have no
reason for affirming that the systematic error is
greater for one of the series than for the other,
our ignorance on this point being absolute.

VII. *Conclusions.*—In the preceding lines I have
set several problems, and have given no solution.

I do not regret this, for perhaps they will invite the reader to reflect on these delicate questions.

However that may be, there are certain points which seem to be well established. To undertake the calculation of any probability, and even for that calculation to have any meaning at all, we must admit, as a point of departure, an hypothesis or convention which has always something arbitrary about it. In the choice of this con- vention we can be guided only by the principle of sufficient reason. Unfortunately, this principle is very vague and very elastic, and in the cursory examination we have just made we have seen it assume different forms. The form under which we meet it most often is the belief in continuity, a belief which it would be difficult to justify by apodeictic reasoning, but without which all science would be impossible. Finally, the problems to which the calculus of probabilities may be applied with profit are those in which the result is inde- pendent of the hypothesis made at the outset, provided only that this hypothesis satisfies the condition of continuity.