Refutation of the Errors of d'Alembert[1]

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1801

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REFUTATION

OF

SOME SINGULAR ERRORS

OF

Mr. D'ALEMBERT

ON THE

PRINCIPLES OF THE CALCULUS
OF PROBABILITIES,

AND

SOLUTION OF A PROBLEM

KNOWN
UNDER THE NAME

OF THE PROBLEM OF PETERSBURG

ON THE GAME

OF HEADS AND TAILS

THAT

NO PERSON HAS RESOLVED TO THE PRESENT

AND THAT

Mr. D'ALEMBERT HAS JUDGED INSOLUBLE.

1801

SINGULAR ERRORS OF Mr. D'ALEMBERT ON THE PRINCIPLES OF THE CALCULUS OF PROBABILITIES

Mr. d'Alembert claims in his melanges of Literature, of History, and of Philosophy Volume 5.

I°. That the principles of the Mathematicians, which serve as foundation to the calculus of probabilities, remain, or at least seem to remain with defect, when one applies them to nature.

Remark.

The principles of the Mathematicians can remain not at all with defect as soon as they are well established. There is only a certitude, a truth; that which is mathematically or metaphysically certain, would not know how to be physically uncertain. When one distinguishes the certitude in metaphysics, morals, physics, history etc. it is only for the facility of the language. In this sense one names certain, physically, morally certain, that which, in rigor, is not at all certain; but is only of a probability which in usage is equivalent to certitude. All the time that the principles of the Mathematicians remain with defect when one applies them to nature, it is a certain proof, either that these principles are ill-posed, or else that they are ill-applied. But Mr. d'Alembert proves not by his writing that these principles remain with defect in this occasion. Mr. d'Alembert claims

IInd. That the Problem proposed, I know not by whom, around fifty years ago,[1] known under the name of Problem of *St. Petersburg*, which no person has been able to resolve, is a proof of it; this Problem being insoluble according to him, which because according to the rules of the Mathematicians, one must by playing at *Heads and* Tails, suppose possible the combination, that *Tails* never arrive.

Remark.

We will see, that this assumption is not an obstacle to the solution of the Problem.

In order to put the reader in a position to judge Mr. d'Alembert,[2] I transcribe here all his writing, such as it is found in this melanges Volume V, and I note in a margin by the letters A, B, C etc. according to the order which agrees best to my reasoning, the different arguments, by which he pushes his opinion, in order that I can refer the reader with so much more facility and without repetition.

[1] I learned that this problem had already been proposed in 1713 to Mr. de la *Montmort*, by Mr. *Nicolas Bernoulli* eldest, editor of the *Ars Conjectandi* of his uncle the celebrated *Jacques Bernoulli*; and I am lead to conjecture, that he has found it proposed, but without solution, in the papers of that one.

[2] I say always Sir in speaking of an Author of our century, because I know few names of authors rather justly celebrated in order to be pronounced without this accompaniment. I find ridiculous, that one reads: *Oeuvres de Mirabeau*, *works of Lessing* etc. It is to Posterity alone to accord this honor after a century at least of well recognized superiority.

DOUBTS AND QUESTIONS OF Mr. D'ALEMBERT ON THE

CALCULUS OF PROBABILITIES

One complains rather commonly that the formulas of the mathematicians, applied to the objects of nature, are found only too much with defect. No person nonetheless has further perceived or believe to perceive this inconvenient in the calculus of Proba-
A bilities. I have dared first to propose some doubts[3] on some principles which serve as base to this calculus. Some great geometers have judged these doubts worthy of attention; other great geometers have found them absurd; because why would I soften the terms to which they avail themselves? The question is to know if they have been wrong to use them, and in this case they could have doubly erred. Their decision, which they have not judged apropos to motivate, has encouraged some mediocre Mathematicians, who are themselves hurried to write on this subject, and to attack me without understanding me. I am going to try to explain myself so clearly, that nearly all my readers will be led to judge me.

I will remark first that it will not be astonishing that some formulas where we ourselves propose to calculate the same *incertitude*, can (in certain regards at least) participate in this incertitude, and allow in mind some clouds on the rigorous truth of the result that they furnish. But I will not at all insist on this reflection, so vague that we can conclude nothing from it. I will not stop myself any longer to show that the theory of Probabilities, such as it is presented in the books which treat it, is towards benefit of the matters neither so enlightening nor so complete as we could believe it; this detail could be understood only by the Mathematicians; and yet one time I am going to try here to be understood by everyone. I adopt therefore, or rather I admit for good in the mathematical rigor, the ordinary theory of the Probabilities, and I am going to examine only if the results of this theory, when they could be outside of the reach of geometrical abstractions, are not susceptible to restriction, when we apply these results to nature.

In order to explain myself in the most precise manner, here is the point of the difficulty that I propose.

The calculus of Probabilities is supported on this proposition, that all the different
B combinations of one same effect are equally possible. For example, if we toss a coin into the air 100 times in sequence, we suppose that it is equally possible that *tails* arrive one hundred times in sequence, or that *tails* and *heads* are *mixed*, by following besides among them such particular succession as we will wish; for example, *tails* on the first trial, *heads* on the following two trials, *tails* on the fourth, *heads* on the fifth, *tails* on the sixth, and on the seventh, etc.

These two cases are without doubt equally possible, mathematically speaking; this is not thence the point of the difficulty, and the mediocre Mathematicians of whom I spoke a little while ago have taken the quite useless effort to write some long dissertations to prove this equal possibility. But the question is to know if these two cases,
B equally possible *mathematically*, are also *physically* and in the order of things; if it is

[3] *Opuscules mathématiques*, T. II. Mémoire X.

physically also possible that the same effect arrive 100 times in sequence, if it is that this same effect is mixed with the others according to that law which we will wish to indicate. Before making our reflections on this subject, we will propose the following question, well known of the Algebraists.

Pierre plays with Paul at *heads* or *tails*, with this condition that if Paul brings forth *tails* at the first trial, he will give an écu to Pierre; if he brings forth *tails* only at the second trial, 2 écus; if he brings it forth only at the third, 4 écus; at the fourth, 8 écus; at the fifth, 16; and thus in sequence until *tails* comes; we demand the expectation of Paul, or that which is the same thing, that which he must give to Pierre before the game begins, in order to play with him at an equal game, or, as we express ourselves ordinarily, for his *stake*.

The known formulas of the calculus of Probabilities show easily, and all the Mathematicians agree with it, that if Pierre and Paul play only to one trial, Paul must give to Pierre a half-écu; if they play only to two trials, two half-écus, or one écu; if they play only to three trials, three half-écus; to four trials, four half-écus, etc. Whence it is evident that if the number of trials is indefinite, as we suppose it here, that is to say if the game must cease only when *tails* will come, that which can (mathematically speaking) never arrive, Paul must give to Pierre an infinity of times a half-écu, that is an infinite sum. No Mathematician contests this consequence; but there is no one who does not sense and does not avow that the result of it is absurd, and that there is no player who wished in a fair game to risk 50 écus alone, and even much less.

Many great Mathematicians have endeavored to solve this singular case. But their solutions, which agree not at all, and which are deduced from circumstances strange to the question, prove only how much this question is embarrassing.[4] One among them believes to have solved it, by saying that Paul must not give an infinite sum to Pierre, because the wealth of Pierre is not infinite, and that he can neither give nor promise more than he has. But in order to see at what point this solution is illusory, it suffices to consider that, whatever enormous riches which we suppose to Pierre, Paul, unless being mad, would not give to him one thousand écus alone, although he must catch up to these thousand écus and beyond if tails will arrive only at the eleventh trial, more than two thousand écus, if tails will arrive only at the twelfth, four thousand écus at the thirteenth, and thus in sequence.

Now if we demand of Paul why he would not give these thousand écus? It is, he will answer, because it is not possible that tails will arrive only at the eleventh trial. But, to him we will say, if tails arrives only after the eleventh trial, that which can be, you will win wealth beyond your thousand écus; I swear, Paul will reply, that in this case I could win considerably; but it is so little probable that tails not arrive before the eleventh trial, that the gross sum that I would win beyond this eleventh trial, is not sufficient to engage me to incur this risk.

When Paul would keep himself to this reasoning, it would be already enough to show that the rules of the Probabilities are with defect when they propose, in order to find the stake, to multiply the expected sum by the probability of the case which must make this sum winning; because, whatever enormity that is the expected sum, the D

[4] We can see these solutions in the fifth volume of the Mémoires de l'Académie de Pétersbourg, in the compilation of M. Fontaine, etc.

probability to win it can be so small, that we would be insane to play a fair game. For example, I suppose that out of 2000 tickets of the lottery, all equal, there must be one of them which bears a lot of twenty million; it would be necessary, according to the ordinary rules, to give ten thousand francs for a ticket; and this is assuredly that which a person would dare not do: if there will be found some men rich enough or foolish enough for that, we put the lot at two thousand millions, each ticket then will be one million, and I believe that for the trial no person would dare to take it.

However it is quite certain that whatever one would win in this lottery, and whatever consequently each of the bettors in particular have expectation to win; instead that in the proposed case, where Paul would be obliged to give to Pierre an infinite sum, Pierre would always be certain to win, however long that the game endured; so that Pierre will be in the right to complain, if having not fixed the number of trials, and tails arriving finally at such trial as we will wish, for example at the twentieth, Paul satisfied himself for his stake to give a sum double or triple, or one hundred times of 524288 écus, a sum which Pierre must on his side give to Paul.

In a word, if the number of trials is not fixed, and if Paul puts into the game, before it begins, such sum as he will wish, put he all the gold or silver which is on the earth, Pierre is right to say to him that he does not put enough, if we deduce it from the received formulas.

Now I demand if it is necessary to go seek very far the reason for this paradox,
and if it does not leap to the eyes that this pretended *infinite sum* due by Paul at the
D beginning of the game, is infinite, in appearance, only because it is supported on a false
assumption; namely on the assumption that *tails* can never arrive, and that the game
can endure eternally?

It is however true, and even evident, that this assumption is possible in mathematical rigor. It is therefore only *physically speaking* that it is false.

It is therefore false, physically speaking, that *tails* can never arrive.

It is therefore impossible, physically speaking, that *heads* arrives an infinity of times in sequence.

Therefore, physically speaking, *heads* can arrive in sequence only a finite number of times.

What is this number? this is that which I at no point undertake to determine. But I am going further, and I demand by what reason *heads* is not known to arrive an infinity of times in sequence, *physically speaking*? We can give for it only the following reason: it is that it is not in nature that an effect is always and constantly the same, as it is not in nature that all men and all trees resemble themselves.

I demand next if is it possible, physically speaking, that the same effect arrive a very great number of times in sequence, ten thousand times, for example, when it is this effect arrive an infinity of times in sequence? For example, is it possible, physically speaking, that if one casts a coin in the air ten thousand times in sequence, there comes in sequence ten thousand times heads or tails? On this I call to all the players. Let Pierre and Paul play together at heads and tails, let it be Pierre who casts, and let heads arrive only ten times in sequence, (this will already be much), Paul exclaims infallibly at the tenth trial, that the thing is not natural, and that surely the coin has been prepared in a manner to bring forth heads always. Paul supposes therefore that it is not in nature that an ordinary coin, fabricated and cast into the air without fraud, falls ten times in sequence on the same side. If we do not find ten times enough, we set it at twenty; there will result always that there is no player at all who makes tacitly this assumption, that one same effect is not known to arrive in sequence a certain number of times.

There is some time that having had occasion to reason on this matter with a wise geometer, the following reflections came to me again, in support of those which I have already exhibited. We know that the mean length of the life of men, to count from the moment of birth, is around 27 years, that is that 100 infants, for example, coming E
at the same time into the world, will live only around 27 years taking one thing with the other; we have recognized likewise that the duration of the successive generations for the community of man is around 32 years, that is that 20 successive generations more or less, must give only around 20 times 32 years; finally we have proved by all the lists of the duration of the reigns in each part of Europe, that the mean duration of each reign is around 20 to 22 years, so that 15, 20, 30, 50 successive Kings and more, reign only around 20 to 22 years taking one thing with the other. We can therefore wager, not only with advantage, but at a sure game, that 100 infants born at the same time will live only around 27 years taking one thing with the other; that 20 generations will endure no longer than 640 years or about; that 20 successive Kings will reign only around 420 years more or less. Therefore a combination which will make the 100 infants live 60 years taking one thing with the other, which will make the 20 generations endure 80 years each, which will make 20 successive Kings reign 70 years taking one thing with the other, will be illusory, and outside the physically possible combinations. However, by being held with it to the mathematical order, this combination will be evidently as possible as any other. Because if two Kings in sequence, for example, have reigned 60 years, there would be no mathematical reason that their successor not reign as much; the one here dies, there would no longer be any mathematical reason that the following was not in the same case, and thus in sequence. Whence there results that there are some combinations which we must exclude, although mathematically possible, when these combinations are contrary to the constant order observed in nature. Now it is contrary to this order that the same effect arrive 100 times, 50 times in sequence. Therefore the combination where we suppose that *tails* or *heads* arrive 100 or 50 times in sequence, is absolutely to reject, although mathematically as possible as those where *heads* and *tails* are mixed.

Another reflection; because the more we think on this matter, the more it furnishes it. There is no Banker at all of Pharaon who does not enrich himself in this occupation; why? It is that the Banker having the advantage in this game, because the number of cases which makes him win is greater than the number of cases which make him lose, F
there arrives at the end of a certain time that there are more times of winning than losing. Therefore at the end of a certain time there has arrived more cases favorable to the Banker than unfavorable cases. Therefore since there are, as the calculus proves it and as we suppose it, more cases favorable to the Banker than cases unfavorable, it is clear that at the end of a certain time, the sequence of events has in effect brought forth more often that which ought more often to arrive. Therefore the combinations which contain more of the unfavorable cases than of the favorable, are (at the end of a certain time) less possible *physically* than the others, and perhaps even must be rejected, although mathematically all the combinations are equally possible. Therefore, in general, the more the number of favorable cases is great in any game, the more at the

G end of a certain time the gain is certain; and we can add even that this time will be so
much less long as the number of favorable cases is greater. Therefore if Pierre and Paul
are supposed to play at *heads* and *tails* during a year, for example, the one who will
wager that *tails* or *heads* will not arrive consecutively during a year, during one month
G even, will be physically, that is, absolutely certain to win and to win much. Therefore it
is necessary to reject all the combinations which would give *heads* and *tails* a too great
number of times in sequence.

Thence, and from that which we have said above, there results again another con-
sequence; it is that if we suppose the time a little long, the combinations of *heads* and
tails will arrive in a manner that at the end of this time, there will be very nearly as
many of the one as of the other; so that if the coin is marked with 1 on the side of *heads*
and with 2 on the side of *tails*, there will arrive at the end of 100 times, or more, that
the sum of the numbers which will come will be very nearly equal to 50 times 2 and 50
H times 1, that is to 150. A new reason in order to reject the number of these physically
possible combinations, those which contain the same case a too great number of times
in sequence.

Here is another question, which is the next of that which just concerns us. If an
I effect has arrived many times in sequence, for example, if *tails* arrives three times in
sequence, is it equally probable that *heads* or *tails* will arrive at the fourth trial? It is
certain that if we admit the preceding reflections, we must wager for *heads*, and it is
NB indeed in this way rightly some players use it. The difficulty is knowing how much the
odds are that *heads* will arrive rather than *tails*; and it is on what the calculus has not
taken enough.

That which we just said is based on the assumption that *tails* has not arrived in sequence a very great number of times: because it will be more probable that this is the effect of some particular cause in the construction of the coin, and for when there will be advantage to wager that *tails* will arrive next. Whatever it be, I imagine that there is no wise player at all who must in this case be embarrassed to know if he will wager *heads* or *tails*, while at the beginning of the game, he will say, without hesitation, *heads* or *tails* indifferently.

I demand therefore in consequence:

1°. If among the different combinations which a game can admit, must we not exclude those where the same effect would arrive a great number of times in sequence, at least when we will wish to apply the calculus to nature?

2°. Suppose that we must exclude the combinations where the same effect will arrive, for example, 20 times in sequence; on what standing will we consider the combinations where the same effect will arrive 19 times, 18 times in sequence, etc.? It seems to me little consequent to regard them as possible also, as those where the effects would be mixed. Because if it is possible also, for example, that *heads* arrive 19 times in sequence, as it is that *tails* arrive on the first trial, *heads* next, next *tails* two times if we wish, and thus of the rest, by mixing *heads* and *tails* together without making the one or the other arrive a long time in sequence; I demand why we would exclude absolutely, as must never arriving in nature, the case where *heads* would come twenty times in sequence? How could it be that *tails* can arrive 19 times in sequence, as well as any other trial, and that *tails* not arrive 20 times in sequence?

For me, I see to this only one reasonable response: it is that the probability of a combination where the same effect is supposed to arrive many times in sequence, is so much smaller, all things equal besides, as this number of times is greater, so that when it is very great, the probability is absolutely null or as null, and that when it is small enough, the probability is only small or point diminished by this consideration.

To assign the law of this diminution, it is this that neither me, nor a person, I believe, can make: but I think to have said enough in order to convince my readers that the principles of the calculus of probabilities could well have need of some restrictions when we will wish to consider them physically.

In order to strengthen the preceding reflections, permit me to add this here.

I suppose that one thousand characters that we found arranged on a table, form a language and a sense; I ask who is the man who will not wager everything that this
arrangement is not the effect of chance? However it is from the last evidence that K
this arrangement of words which gives a sense, is quite possible also, mathematically speaking, as another arrangement of characters, which would form no sense at all. Why does the first appear to us to have incontestably a cause, and not the second? if this is only because we suppose tacitly that it has neither order, nor regularity, in the things where chance alone presides; or at least when we perceive in some thing, order, regularity, a kind of design and project, there is much greater odds that this thing is not the effect of chance, than if we perceived neither design nor regularity.

In order to expand my idea with yet more clearness and precision, I suppose that we find on a table some printed characters arranged in this way:

C o n s t a n t i n o p o l i t a n e n s i b u s,
or a a b c e i i i i l n n n n n o o o p s s s t t t u,
or n b s a e p t o l n o i a u o s t n i s n i c t n,

These three arrangements contain absolutely the same letters: in the first arrangement they form a known word; in the second they form no word at all, but the letters are disposed according to their alphabetical order, and the same letters are found as many times in sequence as they are found in turn in the twenty-five characters which form the word Constantinopolitanensibus; finally, in the third arrangement, the characters are pell-mell, without order, and at random. Now it is first certain that, mathematically speaking, these three arrangements are equally possible. It is not less that all sane men who will cast a glance on the table where these three arrangements are supposed to be found, will not doubt, or at least will wager everything that the first is not the effect of chance, and that he will scarcely be less lead to wager that the second arrangement is not no longer. Therefore this sane man does not regard in some manner the three arrangements as equally possible, physically speaking, although the mathematical possibility is equal and the same for all three.

We are astonished that the moon turns about its axis in a time precisely equal to
the one that it expends to turn about the earth, and we seek what is the cause of it? If M
the ratio of the two times was the one of two numbers taken at random, for example of 21 to 33, we would no longer be surprised, and we would not seek cause; however the ratio of equality is evidently as possible, mathematically speaking, as the one of 21 to 33; why therefore seek a cause in the first and not in the second?

A great geometer, Daniel Bernoulli, has given to us a scholarly memoir where he N seeks by what reason the orbits of the planets are contained in a very small Zone parallel to the Ecliptic, and which is only the seventeenth part of the sphere; he calculates how much are the odds that the five planets, Saturn, Jupiter, Mars, Venus and Mercury, cast at random about the sun, would deviate themselves so little from the plane where the sixth planet turns, which is the Earth; he finds that there are odds more than 1400000 against one that the thing would not arrive so; whence he concludes that this effect is not at all due to chance, and consequently he seeks in it and determines good or harm the cause of it. Now I say, that mathematically speaking, it was equally possible, either that the five planets deviate themselves as little as they do from the plane of the ecliptic, or that they take any other arrangement, which would have much more deviation to them, and dispersed as the comets under all possible angles with the ecliptic; however no person is informed to demand why the comets are not limited in their inclination, and we demand why the planets have them? What can be the reason for it? otherwise again one time because we regard as very likely, and nearly as evident that one combination where it seems from the regularity and a kind of design, is not the effect of chance, although mathematically speaking, it is as possible as any other combination where we would see neither order nor any singularity, and in which by this reason we would not think to seek a cause.

If we will cast five times in sequence a die with seventeen faces, and if all these five times *sonnez*[5] arrives, M. Bernoulli could prove that it had precisely the same odds to make as in the case of the planets, that *sonnez* would not arrive thus. Now, I demand of him if he would seek a cause in this event, or if he would not seek it? If he seeks it not at all, and if he regards it as an effect of chance, why does he seek a cause in the arrangement of the planets, which is precisely in the same case? and if he seeks a cause in the trial of the die, as he must do in order to be consequent, why would he not seek a cause in any other particular combination, where the die with seventeen faces cast five times in sequence, would produce some different numbers, without order and without sequence, for example 3 on the first trial, 7 on the second, 1 on the third, etc.? However there would be odds as great that this combination would not arrive, as there would be odds that *sonnez* would not arrive five times in sequence in a die with seventeen faces. Therefore M. Bernoulli would regard tacitly this last combination of *sonnez* five times in sequence, as being less possible than the other. He would suppose therefore that it is not in nature that the same effect arrive seventeen times in sequence, at least when there are 17 equally possibles at each cast, and that the number of possible cases in five consecutive casts is equal to 17 multiplied four times in sequence by itself?

We go further, always according to the calculation of M. Bernoulli. If the planets were all in the same plane, and if we applied to that case there the reasonings of the Author, we would find that there are odds infinity against one, that this arrangement should not arrive, and we would conclude with him that the odds are infinite to one that this arrangement is produced by a particular cause and not fortune; that is to say, that it is *impossible* that this arrangement is the effect of chance; because to wager the infinite that a thing is not, it is assured that it is impossible. However any other particular and arbitrary arrangement as we will wish to imagine (for example Mercury at 20 degrees inclination, Venus at 15, Mars at 52, Jupiter at 40, Saturn at 83) is unique, as the one

[5] *Translator's note*: "Sonnez" is the event of a double six.

of the arrangement of the planets in the same plane; there are odds likewise of infinity against one that this case will not arrive; why therefore does M. Bernoulli seek a cause in the first case, when he would not at all seek it in the second, if it is not by the reason which we have said?

That which there is of the singular, this is what this great Geometer of whom I speak has found *ridiculous*, at least that which one assures me, my reasoning on the calculus of probabilities. For complete response, I pray only he agree with himself, and to make us understand quite clearly, why he would not seek a cause in certain combinations, while he seeks it in others, which, mathematically speaking, are equally possible?

I would add yet a reflection which seems to me to the advantage of the thesis which I support: it is that it was perhaps more possible, physically speaking, that the planets are found all in the same plane, that it is only one same effect arrives one hundred times in sequence; because it is perhaps more possible that a single cast, a single impulse produces immediately on different bodies an effect which is the same, that it is only a body, launched successively at random one hundred times in sequence, takes the same situation by falling again: thus the reasoning that M. Bernoulli deduces from this calculus could be false, that perhaps ours would yet be correct. This could led me to some other reflections on certain cases which we regard as similar in the calculus of probabilities, and which, physically speaking, could well not be; but I will end here these doubts, by cautioning that if I am quite lengthy in giving them for demonstrations, I will not cease any longer to believe them founded, as much as we will oppose only some purely mathematical considerations, or some responses that I know before that one has made them to me; in a word, as much as we will not resolve in a clear and precise manner the question which I have proposed on the game of *heads* and *tails*, and which we ourselves will believe by right to seek a cause in the symmetric and regular effects.

Perhaps one will say to me, for last resource, that if we seek a cause in the symmetric and regular effects, it is not that absolutely speaking, they could not be the effect of chance, but only because this is not possible. Here is all that which I wish that one agrees with me. I will conclude from it first that if the regular effects due to chance are not absolutely impossible, physically speaking, they are at least much more likely the effect of an intelligent and regular cause, than the non-symmetric and irregular effects; I will conclude from it, in second place, that if there is in rigor, and even physically speaking, any combination which is not possible, the physical possibility of all these combinations, (as much as we will suppose them the pure effect of chance) will not be equal, although their mathematical possibility is absolutely the same. This will suffice to respond to all the difficulties proposed above, and among others to resolve the proposed question on the game of *heads* and *tails*. Because as soon as we will suppose that all the combinations are not equally possible, without even any regard as rigorously impossible in nature, we will find that Paul can not be obliged to give to Pierre an infinite sum. This is that which it would be very easy to prove mathematically; this is likewise of what a mediocre calculator could easily assure himself. But this calculation would be difficult to make understood to the community of our readers. I will suppress it therefore as being able to permit no objection, and I will await that some Geometers, who merit that I read them or that I respond to them, combat or support the new views that I propose on the calculus of probabilities.

P.S. In finishing this writing, I fall by chance on the article *Fatalité* in the Dictionnaire Encyclopédique, an article which we will recognize easily for the work of a man[6] of spirit and of Philosophy; and here is that which I find there,[7] apropos of supposed *good luck* or *bad luck* in the game. "Either it is necessary to have regard to the past trials in order to estimate the next trial, or it is necessary to consider the next trial, independently of the trials already played; *these two opinions have their partisans*. In the first case, the analysis of chances leads me to think, that if the preceding trials have been favorable to me, the next trial will be contrary to me; but if I have won so many trials, the odds are so much that I will lose the one that I come to play, *and vice versa*. I could never say therefore: I am in bad luck, and I will not risk that trial there; because I could say it only after the past trials which have been contrary to me; but these past trials must rather make me hope that the following trial will be favorable to me. In the second case, that is, if we regard the next trial as completely isolated from the preceding trials, we have no reason at all to estimate that the next trial will be favorable rather than contrary, or contrary rather than favorable; thus we cannot regulate its behavior in the game, according to the opinion of destiny, of good luck, or of bad luck."

From this passage I deduce two consequences. The first, that, according to the Author of this excellent article, we can be divided on the question, *if it is equally probable that an effect arrive or not arrive, when it is already arrived many times in sequence*. Now it suffices to me that this is regarded as doubtful, in order to permit me to believe that the object of the preceding writing is not so strange as some clever Mathematicians have imagined it. The second consequence, this is that the analysis of chances, such as the Author of the article imagines it, gives less probability to the combinations which contain the successive repetition of the same effect, than to the combinations where this effect is mixed with others. Now this is only to be said of the analysis of chances considered physically; because to consider it on the mathematical side alone, all the combinations, as we have said, are equally possible. I believe therefore to be able to regard the Author of the article *Fatalité* as partisan of the opinion that I have tried to establish; and a partisan of this merit persuades me anew that this opinion is not an absurdity.

REFLECTIONS ON THIS WRITING.

I divide these Reflections into two §. In the first I will try to resolve the Problem of St. Petersburg; in the second I will expose the reasons which make me think, that of all the arguments of Mr. d'Alembert, there are none which prove that which he has wished to prove in this writing which one just read.

I have known, more than thirty years ago, nearly all the reasonings that I expose in this writing. But I have not at all had them printed. I have not at all dreamed then to resolve the Problem itself. I have resolved it seven years ago and I have printed this work at present among other reasons 1st in order to demonstrate how it is easy to be deceived, even in the science of calculation, when these calculations have some metaphysical basis. 2nd. In order to show, by giving the solution of this Problem

[6] *Translator's note*: André Morellet (1727-1819) is the author of the article *Fatalité* in the Encyclopedia of Diderot.

[7] Tome VI. p. 428. col. 1 to the end.

without having recourse to algebra, that there are some questions in the solution of which this science furnishes only feeble help. Indeed, it gives some rules only in order to resolve the equations and not in order to ask them: now there are some questions, and in this case are nearly all those of morals, of metaphysics, of Legislation etc. which are difficult, only because they are difficult to put into an equation; the application of algebra to these sciences will be therefore never of so great help as one would think it. It could be useful in order to demonstrate, even in these sciences, some truths already discovered; but *the long strides* must be made in advance.

§ 1
Solution of the Problem of Petersburg

This Problem is indeed difficult to resolve; but it is not at all by the reasons that Mr. d'Alembert supposes. He has considered it under a view absolutely false. It is difficult 1st by the extraordinary manner, by which it is proposed. 2nd. because it contains two unknowns, namely: the probability that Paul has to win; and the real value of the sum that he can win. 3rd because the same probability must be considered under two different points of view, first as probability to win and next as determining the real value of the sum, that Paul can win.

I will give three and even in a sense four solutions of this Problem.

First Solution.
Enunciation of the Problem.

"Pierre plays with Paul at *Heads* and *Tails* with this condition that if Paul brings forth *Tails* at the first trial, Pierre will give to him an écu; if *Tails* arrives only at the second trial, two écus; if *Tails* arrives only at the third, four écus; and thus in sequence in the same progression until *Tails* arrives.

"One demands that stake which Paul must give to Pierre before beginning the game."

Mr. d'Alembert believes that the difficulty of the solution of this Problem comes from the false assumption that *Heads* could come an infinity of times in sequence. By supposing possible, says he, as one must suppose it mathematically, that *Tails* comes only after two, three hundred trials, or even never, Paul should give to Pierre an infinity of half-écus for stake; now there is no person who would accept a similar wager etc.

Indeed, if Paul ought to give to Pierre an infinity, or even only 50 half-écus for stake, he would be wrong to accept the wager. But we will see, that this is not there the stake which he must give to him; and supposed that this stake was the one that he would have to give, there would result from it nothing in favor of the opinion of Mr. d'Alembert.

The difficulty of the solution of this Problem comes not from all of this that one supposes the constant appearance of the same face mathematically as possible as the succession of *Heads* and *Tails* mixed in a determined manner.

No combination of *Heads* and *Tails* would render the solution easier.

The conditions of the well understood Problem are:

1°. That Paul, if he does not win an écu by bringing forth *Tails* from the first trial, must first being forth *Heads* and then *Tails* in order to win at the second trial; that, in order to win at the 3rd trial he must bring forth *Heads*, *Heads*, *Tails*; at the 4th *Heads*, *Heads*, *Heads*, *Tails* etc.

2°. That the game must cease only when *Tails* will arrive. One supposes therefore that *Tails* will arrive, or at least, supposing that *Tails* arrived not at all, that Paul in this case would have nothing to pay to Pierre.

3°. That Pierre increases the sum which he offers to Paul, in ratio as *Tails* will arrive later, and increases this offer precisely in the same *ratio* in which the improbability to bring forth *Heads* a long time in sequence increases, and consequently to bring forth *Tails* later.

There results thence, that the more *Tails* arrives late, the larger the sum that Paul will win; but that, the larger this sum is, the smaller is also, before beginning the game, the probability to win it. But Paul would lose nothing, if *Tails* were never able to arrive; but Paul consequently is certain to win; because he wins something, more or less, when *Tails* arrives; and as Pierre would play as a dupe in this manner, that it is in the stake alone that Paul gives to him, that the expectation of Pierre can reside. The expectation of the players (their loss and their gain) can at no Point reside in the *nonappearance of Tails*, by the reason that one is agreed to play until *Tails* arrives; and that one has not at all been able to agree consequently, and it is not at all agreed, that Paul would have to give something to Pierre if *Tails* arrived not at all.

The Expectation of the two players depends therefore uniquely on the period when *Tails* will arrive, and if one could determine in advance at which trial the two players must presume that *Tails* will arrive, he would have not only a long stride in fact toward the solution of the Problem, but, Pierre making his offer in the manner that he makes it, raising the sums that he promises to Paul precisely in the inverse ratio to that, in which the probability to bring forth *Tails* late, diminishes (whence it results, that Paul receives, at some trial that *Tails* arrives, the exact sum that he must receive for this trial) the Problem would be found perfectly resolved; because there would no longer be a question but to say: Paul must give for stake to Pierre precisely the sum that he would receive from him at the trial in which one supposes that *Tails* will arrive. By means of this stake Pierre and Paul would be found to have played always in a fair game at some trial that *Tails* would be able to arrive in fact. The question is therefore to know at which coup one must presume that *Tails* will arrive. Now, in thinking on the nature of the game *Heads* and *Tails*, which has only two combinations, one feels, that the trial in which one must suppose that *Tails* will arrive is incontestably the second trial, by the reason that, having only equal probability that *Tails* arrives or not arrives at the first trial, and having improbability that the arrival of *Tails* delays to the 3rd trial, one must regard the second trial as the true mean proportional of the arrival of *Tails*; and to suppose that it is at the second trial that *Tails* will arrive. Indeed, the probability to bring forth *Tails* at the first trial being $= \frac{1}{2}$, the probability to bring forth *Heads Tails* being $= \frac{1}{4}$, and the probability to bring forth *Heads*, *Heads Tails* being $= \frac{1}{8}$, the probability to bring forth *Heads Tails* $= \frac{1}{4}$ is the mean proportional between the probability to bring forth *Tails* at the first trial $= \frac{1}{2}$ and the probability to bring forth *Heads Heads Tails* at the third trial $= \frac{1}{8}$.

Thus Paul must give to Pierre two écus for stake. Giving to Pierre this sum, their game will be perfectly equal: because *Tails* arriving at the first trial, Pierre, by means of this stake, will win one écu; *Tails* arriving at the second trial, there will be neither loss nor gain on both sides; and *Tails* arriving only at the 3^{rd} trial or after the third trial, Pierre will lose in truth much more than he can win; but (Pierre being able to lose only at the 3^{rd} trial) the probability that *Tails* will arrive before this trial is such, that, if Pierre and Paul play often at this game, the result of their loss and of their reciprocal gain will be = zero: now it is this which one calls playing a fair game.

This solution, such as I have just presented it, contains in it two: one *a priori* and one *a posteriori*; here is why I say that I will give, in counting this last, four solutions: but I must develop yet these two better than I have done it.

I say 1^{st} through the *a priori* reasons, that Paul must give to Pierre two écus for stake. If my reasonings in this regard are incontestable, this first part of my solution is by itself a solution a priori.

I say 2^{nd} that, if Paul gives to Pierre two écus for stake, they will play, at each trial that *Tails* arrives, a fair game; it is this which it is necessary yet better to prove than I have done it: but if I prove it, this second part of my solution will be also by itself and independently of the first part of my solution, a complete solution, but a posteriori, of the Problem of Petersburg.

We begin therefore, in order to attain more quickly our end, by this solution *a posteriori*. We will return to the development of the other later. The expectation of the players and the equality of their game depends on two things: on the sum, which they can win and on the probability that they have to win it. If the sum that they can win is not the same, it is necessary that the *deficit* of the sum of one part be compensated by a probability to win it by so much greater. For example: if Pierre has a probability $= \frac{1}{2}$ to win an écu, and if Paul has a probability $= \frac{1}{4}$ to win two écus, they play a fair game; because the one who has a probability to win less than half only is that of the other, that of the other, has in exchange a sum to expect which is the double of that which the other can win; now it is here precisely the case, in which will be found Pierre and Paul, if Paul gives to Pierre two écus for stake that Pierre puts into his pocket before beginning the game: because then, *Tails* arriving at the first trial, and Pierre, giving in this case to Paul one of the two écus, which he has received from him for stake, wins an *écu*, and his probability to win this écu is precisely $= \frac{1}{2}$. If *Tails* arrives at the second trial, they will be finished, because, in this case, Pierre renders to Paul the two écus that he has received from him; and if *Tails* arrives only at the third trial, or after the third trial, Paul wins at the 3^{rd} trial 4 écus — 2 écus that he has given for stake, consequently *two écus*. Now the probability to win for Paul after the second trial until infinity, being equal to the following series $\frac{1}{8} + \frac{1}{16} + \frac{1}{32} + \frac{1}{64} + \frac{1}{128}$ etc. which when it is supposed infinite, is $= \frac{1}{4}$, Paul has therefore only a probability $= \frac{1}{4}$ to win *two écus* or beyond *two écus*; now according to that which has been said, it is there precisely the position in which he must be found in order to play a fair game with Pierre. It is true on one side, that it is not at the third trial that he has this probability $= \frac{1}{4}$ to win two écus, he has only a probability $= \frac{1}{8}$ to win this sum, this which could make one think that his stake is too high; on another side, it is very true also, that the sums, that he can win after the third trial, go such by increasing, that one could think that his stake is too modest; but, if one reflected 1^{st} that all the trials in which Paul can win to infinity have an equal value for him, since the magnitude of the sum is always perfectly compensated by the smallness of the probability, as Table N.1 demonstrates it for us, whence there results, that each trial has for him the value of the 3^{rd}. 2^{nd} That he has, in truth, only a probability $= \frac{1}{8}$ to win *two écus* at this third trial; but that he has beyond this probability, still a probability $= \frac{1}{8}$ to win after the third trial if he wins not at the one here; that he has therefore $\frac{1}{8} + \frac{1}{8}$ that is to say $\frac{1}{4}$ of probability to win *two écus* or an equivalent value; if one makes, say I, these two reflections, one will sense, that he gives to Pierre exactly the stake that he must give to him.

I must observe here that if, that which is not, one could believe, that Paul by giving *two écus*, gives too much for stake, or else that he gives only some écus too little for stake, the opinion of Mr. d'Alembert, who thought that he must give an infinite sum, could not be less fully refuted, and that the Problem could be resolved in a sense for him also; because its limits at least could be found marked; its limits p. E could be marked, if one had been limited to prove, that Paul is not able to give less than *one écu and a half*, nor more than *four écus* for stake.

After having given this solution *a posteriori*, which makes the second part of my first solution and is by itself a solution of the Problem independent of that one we return to that one, and we show that it is also on its side a solution of the problem independent of this one, although they are given of the day mutually, and can serve as proofs the one to the other. In order to disperse all the clouds that this solution *a priori* could have left in the mind, I must develop two things.

1. That the expectation of Pierre can reside only in the stake that Paul gives to him, and not in the non-arrival of *Tails* as Mr. d'Alembert imagines it. Pierre awaits not at all to this non-arrival; he calculates and the known rules of the calculus of probabilities teach him that he must count that *Tails* will arrive. Because the probability that *Tails* arrives later, diminished in the ratio of the following progression $\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+\frac{1}{16}+\frac{1}{32}+\frac{1}{64}$ etc. or this series, when it is infinite, is $=$ to unity and consequently $=$*to the certainty that Tails will arrive*. But it is not necessary to be mistaken here: this rule does not teach us, that the constant appearance of the same face to infinity is impossible in itself; because in choosing any other combination of *Heads* and *Tails* predetermined in advance, one comes to the same result from it, and that must be, since all these combinations are counted and must be counted, although Mr. d'Alembert says of it, mathematically and physically equally possible among them, as one will see it clearly thereafter in this writing. There are not good reasons, why one of these combinations could be less possible than the other. The reason that Mr. d'Alembert deduces from the variety of nature in its effects, proves nothing here; especially to the man who reflected, that a constant variety, any mixture of two effects always the same to infinity, is not less a real uniformity than the constant appearance of the same effect; however one or the other of these combinations could appear very certainly, if one could play at *Heads* and *Tails* to infinity.

The rule teaches us therefore not that one or the other of these combination to infinity is impossible in itself; *it teaches us that it is impossible for us, to guess in advance what is that which will appear*. It teaches us, that we can form no other conjecture in this regard, since, whatever be that which we will form, there will always be odds of infinity against one that we will deceive ourselves. This is thence the end of the rule and of all the calculation of the probabilities. Pierre awaits therefore not to the nonappearance of *Tails*, and consequently his expectation can reside only in the stake that Paul gives to him.

The second thing that I must develop at present in order that my *a priori* solution be complete, it is II$^{\text{nd}}$ that it is at the second trial that one must presume that *Tails* will arrive. Now here is how I reason: It is incontestable that it is equally probable that *Tails* come, or not come at the first trial; it is therefore equally probable that *Tails* come at the first trial, or else at any trial after the first trial. Table N. 3 confirms this truth, by showing us, that, whatever be the number of combinations of *Heads* and *Tails*, a half of the total number of these combinations brings forth always *Tails* and the other half always *Heads* at the first trial. In the case of the Problem of Petersburg one can not, in order to fix the stake, suppose that it is at the first trial that *Tails* will arrive, by the reason, that in forming this assumption and regulating the stake in consequence, Paul could be certain to win and that he could have consequently no more game; because as soon as there is certitude to win on one part, there is no longer game. The conditions of the Problem not permitting therefore to Paul to suppose that *Tails* will arrive at the first trial, he must presume that it is at the second trial that *Tails* will arrive, and he must presume it not only because the second trial is the one of all the trials after the first, which has the most probability in its favor, and that the reason prescribed to us to presume that an event will take place at the moment which has the most probability for itself; but also because he must sense in advance, that by supposing that *Tails* will arrive at the second trial, he will play with Pierre at a fair game by the reason that the probability of the coming of *Tails* at the second trial being $= \frac{1}{4}$, it is the true mean proportional between the probability of the coming of *Tails* since the first trial $= \frac{1}{2}$, and the probability of bringing forth *Tails* only at the third trial $= \frac{1}{8}$. One could object to me, that the probability of the second trial $= \frac{1}{4}$ is, in truth, this mean proportional when one sets aside all the trials which follow the third; but that it is no longer when one adds to the probability of the third trial $= \frac{1}{8}$ the series of trials which follow this one; but this objection is destroyed by the reflection that one can attain the trials which follow the third only by traversing the third, and that, if it is improbable to attain this one, it is therefore by stronger reason to attain those there, and that one must consequently have no regard, and to establish the mean proportional as I have done it.

If this solution *a priori*, such as I just developed it, dissipates not all the clouds, I flatter myself at least that reunited to the solution a posteriori that I have given, it leaves no doubt in the mind.[8]

[8]When one says, that it is necessary to presume that *Tails* will arrive at the second toss, this one does not wish to say, that the arrival of *Tails* is more probable at the second toss than after; because Table N. 3 demonstrates to us that the probability of the arrival of *Tails* at the second toss, is equal to the probability that *Tails* arrive only after; but likewise, that it is equally probable that *Tails* come since the second toss or come only at one of the following tosses to infinity, it is evident, that it is at the second toss that one must presume its arrival, since this second toss has to it only as much probability as all the other tosses which succeed it have of it in sum. When one says that *Tails* will arrive at the second toss, this one wishes to say that *Tails* will arrive more often at this second toss than at the third, and with stronger reason much more often than at each of those which follow the third and of which the probability becomes consequently always less in geometric progression.

There are some cases, where in order to not be mistaken in these conjectures, it is necessary to presume that an event will take place at an period which is not more probable than each other given period, and to which it is not even probable consequently that this event will arrive in fact. I am going to clarify this paradox by an example:

In the analysis that Mr. d'Alembert makes of the advantages of inoculation, he appears to think (see his mélanges T. V. page 331 § III) that one can not at all determine in justice the ratio which exists between a remote danger and a present danger, and in certain regards (in concreto) he has reason. The circumstances in which one can be found in all the different periods of life, vary so, that one can scarcely be flattered to

Second Point of view
or
second Solution of the Problem,

By considering Table N. I. one sees Ist that Pierre offers to Paul at each toss as many

determine with exactitude the ratio between a present danger and a remote danger; but (in abstracto) setting aside of all the circumstances foreign to the times, one can demonstrate what is the ratio of a danger, or of a good or of a harm certain, remote; to this same danger, or to this same harm, or present advantage; supposing that this danger, this good, this harm, is such by its nature, that it no longer menaces us, or can no longer befall us in a period than at each other period of life. This truth, it seems to me, has not at all been sensed by Mr. d'Alembert and however it appears to me to have importance, because it must serve as base to each other calculation of this kind, that one would wish to attempt.

Theorem

Each danger to lose forever a good, any advantage is, through it alone, that it is farther, setting aside all other circumstance and supposing that we are sure to incur it one time in our life, less than half if it was present. It is likewise of it of all good and of all harm, certain, extended, durable, and of which the period of the coming is absolutely undetermined. Certitude of death alone makes exception.

Demonstration.

Each danger to lose forever any good, life, a leg, sight, a part of his fortune etc., in the half of the lifetime that a man has to travel, is less than half for him than this same danger, if it was present. Much reflection is not necessary in order to sense this truth: now all extended danger, of which we are certain to be attained one time in our life, without having any reason to believe that this will be in one such, rather than in one such other period, must be evaluated by us, as if it ought be attained to us in the just half of our lifetime; because evaluating it in this manner, we have as many chances for us, that we have against us, and we can without risk for us, exchange a situation with the one, which would be certain to be attained from this same danger in the half of his lifetime, supposing that the term of his lifetime was also extended as ours. In order to be convinced that we can exchange a situation with it, one has only to demand oneself, if one could not play a fair game with him under this assumption, that an increase of fortune expected it in the just half of his lifetime, while one same increase of fortune should fall on us in a period absolutely indeterminate in our life, and would consequently fall on us as easily before the latter half of our lifetime. A remote danger is therefore, each thing equal besides, through it alone that it is remote, less than half if it was present; it is likewise of it of every good and of every harm certain and durable. It is not likewise of it of a harm or of a good momentary; because there are of such goods and of such evils, of which the proximity or remoteness changes not at all the value. It is not of it likewise any more of the certitude of death; because death, setting aside the sorrows and the regrets which accompany it, is a harm only because it deprives us of the life which is a good. It must be therefore estimated more or less a harm by reason of the length of life of which it deprives us; this being, the certitude of death in ten years is, all things being equal besides, a harm ten times less than the certitude of death in a year; while the danger of death next year, is in each other ratio with the one of death in ten years; these two dangers are one to the other, if we have yet on thirty years of life to count, as 29 to 20.

I have said that a remote good or harm, of which the period of the coming is indeterminate, must be evaluated as if it should arrive to us at the half of our lifetime; however, and it is to what I wished to come from it, its arrival being supposed equally probable to all the instants of the life, at the beginning, to the half as to the end of our lifetime, it is not only probable, it is even nearly certain, that it will not be precisely in the half of our lifetime that it will arrive; it is necessary therefore sometimes, in order to conjecture well, if not supposing that a thing will arrive indeed at a period in which we have each place to believe that it will not arrive, at least to evaluate it as if it should fall in this period.

If the probability to be attained of a harm, to incur a danger, obtained an advantage, was not the same for us at each age, the period in which one should suppose it to arrive of this harm, of this good etc. would be different. It would be nearer or more remote than the half of our lifetime.

This reasoning has as one sees, much in relation with my first solution; and here is why I am believed well done to place it here.

half-écus as there are combinations in each toss.

2nd. That the probability to bring forth each determined combination (for Example in eight tosses XO, or OX, or XX, or OO,) diminishes in inverse ratio to the sums that Pierre offers to Paul; whence there results that the stake to give by Paul for each determined toss, can never be a half-écu. Thus, if Pierre says to Paul "I wager that you will not bring forth *Tails* at the first toss" or if he says "I wager that you will not bring forth *Tails* eight times in sequence" or else "I wager that you will not bring forth in 50 tosses either *Heads* or *Tails* always alternately, or some other combination, for Example, *Heads* 49 times and *Tails* at the 50th toss, Paul would have to give to him (despite the enormous sums that he could win from him) only a *half-écu* for stake. It would be likewise yet, if Pierre determined not at all the number of tosses, but was content to fix the term of the game: if he would say for example "We will play 50 years in sequence; if during all these times you bring forth any combination (which he would determine: for example always *Heads* and *Tails* alternatively) I will give to you the sum, which (calculation made according to the progression which I have fixed) will return to you at the term of the fiftieth year" Paul would have to give to Pierre only a *half-écu* for stake.

It is therefore, neither because the constant reproduction of the same face is less possible than any other mixed combination of *Heads* and *Tails*; nor because the term of the game is undetermined in the Problem of Petersburg, that this Problem is difficult to resolve. One sees 3° since the sums that Pierre offers to Paul are precisely those that he must offer to him for each trial, that if Pierre wagered with Paul on eight trials, with another person on one trial, and with a third on one hundred thousand trials, all these persons, who would play with Pierre, would have each only one *half-écu* for stake to give to him and could exchange game with them; that is to say: the one take the game of the other, without any loss nor gain of one part or the other, because although one could win enormously more than the other, it would have also so much less probability to win than the other, and by their risks and their gains would be found so balanced, that their positions would be equals. 4° One sees further clearly that there can be no question in the Problem of Petersburg, that Paul gives to Pierre a *half-écu* of stake for each toss, as Mr. d'Alembert believed it; because, although Paul can win at each toss, he can win only one toss: his position is (risk and gain balanced) the same at any toss that he wins, and he had only 50 *half-écus* to give for stake, if he played with fifty different persons wagering with each on any one period whatever determined by the arrival of *Tails*: now in this case he could win fifty times more than by playing with Pierre alone. Paul can not therefore give to Pierre 50 *half-écus* for stake, because he can win only one toss, and that its probability to win increases not at all, by the faculty that Pierre accords to him to pursue his game until *Tails* arrives, by the *Reason* that Mr. d'Alembert imagined; but he must give more than one *half-écu* because he can win at each toss.

It is in this possible arrival of *Tails* at each toss, that the cloud of the difficulty resides; it is necessary therefore to find by what one must evaluate this advantage that Paul has to be able to win at each toss.

By considering the third series of Table N. I, that, which indicates the number of combinations that each toss has for him, and consequently the probability to bring forth *Tails* at the first toss or to bring forth *Tails* only after the first toss, one sees, if one reflects in the least, and one sees it likewise by Table N. 3. without having recourse to calculus, that the probability to bring forth *Tails* at the first toss, being $= \frac{1}{2}$, it is impossible that the probability to bring forth *Tails* only after the first toss (by taking the probability of all the tosses, which follow the first, in sum) is more than $\frac{1}{2}$: for $\frac{1}{2} + \frac{1}{2} = \frac{2}{2} = 1 =$ certainty; the probability to bring forth *Tails* only after the first toss is therefore evidently equal to the probability to bring forth *Tails* at the first toss. This truth seemed to me demonstrated without calculation: but the series of which I just spoke and that I transcribe here, by making it precede from unity, demonstrates it by the calculus:

$$1 + \frac{1}{2} + \frac{1}{4} + \frac{1}{8} + \frac{1}{16} + \frac{1}{32} + \frac{1}{64} \text{ etc.}$$

Because it is demonstrated by Arithmetic 1° that the sum of all the terms of this series, by beginning from the second to infinity is equal to the first term. 2^{nd}. That all this series, by supposing that it is infinite and that the first term is one unit, is $= 2$; by the same reason that, if the first term is a fraction, all the series is equal to the double of this fraction. 3°. That if the series is not infinite, the sum of all the terms, beginning with the second, is equal to the first term only by adding to this series a fraction equal to that of the last term. Now, the series $\frac{1}{2} + \frac{1}{4} + \frac{1}{8} + \frac{1}{16} + \frac{1}{32} + \frac{1}{64}$ etc. expresses the *ratio* by which the probability to bring forth *Tails* only after the first toss, that is to say, to bring forth always *Heads* and finally *Tails*, diminishes; thus, the probability to bring forth *Tails* at some toss that it is to infinity only after the first toss, is equal to the probability to bring forth *Tails* at the first toss.

It is not necessary to confound this general probability, of which I speak, to bring forth *Tails only after the first toss* (a probability that I say *to be equal to the probability to bring forth* Tails *at the first toss*) with the particular probability to bring forth *Tails* at each toss; this here differs from the other as each term of the series differs from the sum of the series. Indeed, at each toss in particular after the first, the probability to win for Paul is, as the series demonstrates it, much less than at the first toss, or, under another point of view, very much more; because, it is much less probable to bring forth *Tails* only at the fourth toss, than to bring it forth at the first; and, by the same reason, the more the arrival of *Tails* delays, the more the probability of its arrival increases; but the probability to win at one or the other of these tosses to infinity, is equal to the probability to win at the first toss. This being, the faculty that Pierre accords to Paul to pursue his game to infinity until *Tails* arrives, makes only to double the probability to win of that one; because we have demonstrated, that all the tosses have for him, however enormous that the difference of the sums be that he can win, only a value always equal; by the reason that he receives at each toss only the exact sum that he must receive; now the faculty that Pierre accords him, to be able to win at each toss, making only to double its probability to win a toss, Paul ought to give to Pierre only the double of the stake that he would have to give to him, if he played with him on a single toss determined in advance; but, as Paul can not in all lose in the case of the Problem of Petersburg; that *the écu*, that Pierre offers to him for the first toss, has for him its entire value, Paul must give for stake to Pierre, as I have proved by my first solution, *two écus* for stake.

Pierre and Paul playing, as they play in the case of the Problem of Petersburg, it is evident, that Paul has at each toss a combination at least against him, than if they play in another manner entirely; because, if *Tails* arrives since the first toss, he wins, and if *Tails* not arrive, he wins yet more; this being, the series which represents the degrees of probabilities favorable to Paul at each toss, $\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+\frac{1}{16}+\frac{1}{32}$ etc. is transformed into the series $\frac{2}{2}+\frac{2}{4}+\frac{2}{8}+\frac{2}{16}+\frac{2}{32}$ etc. Now, this series is equal to the series $1+\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+\frac{1}{16}$ etc.$=2$, therefore etc.

If Pierre and Paul played in the same manner; but on a limited number of tosses, Paul would have to give for stake *two écus less the last term of the series, which would represent in this case the number of tosses, than they would have fixed.*

Third Point of view
or
third Solution of the Problem.

The manner in which this Problem is proposed is extraordinary and makes for confusion; because one could believe that all the advantage of the game is for Pierre: now it is the contrary entirely; all the advantage of the game is for Paul. Indeed, Pierre saying to Paul: if you bring forth *Tails* at the first toss, I will give to you *an écu*; if at the second, I will give to you two of them; if at the third, four and thus in sequence until *Tails* comes; it is as if Pierre said to Paul: I wager that you will never bring forth *Tails*, and at some toss when *Tails* arrives, I will give to you any sum etc.

It is true, that of the manner that Pierre takes in the Problem of Petersburg, raises the price, which he offers to Paul, precisely in the same ratio in which the probability that Paul has to win a quite diminished sum, his generosity is only illusory; he takes back with one hand that which he gives with the other: because it is demonstrated by the rules of probabilities, that one reduces equally a recompense or a benefit, either as one diminishes the promised value; or as one diminishes only the probability to obtain it; and the progression in which the price raises, being just the inverse of that in which the probability to win diminishes, these two series destroy themselves reciprocally; and it is evident, that all the tosses have for Paul only one same real value; but it is not less true, that all the advantage of the game is on his side.

At present in order to find what stake he must give to Pierre in this case, we seek first what stake he ought to give to him, if Pierre, instead of raising the price at each toss, said: I wager that you will never bring forth *Tails*, and at some toss that you bring it forth, I will give to you *an écu*, and before responding even to this, we see what stake Paul ought to give, if Pierre limited himself to say: I wager *one écu*, that you will not bring forth *Tails* eight times in sequence.

Table N. 3 demonstrates to us, that the probability of Paul to win in this case will be $=\frac{255}{256}$; while that of Pierre will be only $\frac{1}{256}$. It would be necessary therefore in order that their game be fair, Pierre offering an écu, that Paul set into the game 255 écus; or else, it would be necessary, that he gave to him for stake $\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+\frac{1}{16}+\frac{1}{32}+\frac{1}{64}+\frac{1}{128}+\frac{1}{256}$ of écus.

This last part would lead to the same result; because, Pierre receiving in advance this stake, would be able to lose only $\frac{1}{256}$, and if he won, he would win $\frac{255}{256}$ écus: now, his probability to win being only $\frac{1}{256}$, since he has only one combination for it; while Paul has 255 of them for himself, it is thence the reciprocal position in which they must be found.

This manner, to give a lesser sum for stake, only the offer is from the one who opens the game, leads not only to the same end than the other, but it is more conformed to the nature of that which one calls the stake, and it leads never to absurdity; while there are some cases where one comes to the absurdity, in proposing to give for *stake* to the one who opens the game, a sum not greater than is his *stake in the game*.

It is more conformed to the nature of the game; because the one, who opens the game, is counted to put the stake into his pocket before beginning the game, so that he is found thence, to play in a fair game with his adversary in some manner that fate turns, either that he gets back to himself *his stake in the game* and that one supposes on the table, or that he loses it.

It never leads to absurdity: indeed, if Pierre said to Paul: I wager an écu, that you will never bring forth *Tails*, and if, in order to equalize the game, Paul thought to give to Pierre in this case a sum greater than this écu, it would be necessary, in this case, that he gave to him for stake an infinite sum; because his probability to win is to that of Pierre as infinity is to unity; now this result would be absurd; while, Paul adopting the manner to give the stake that I have announced, giving consequently to Pierre in the infinite progression $\frac{1}{2} + \frac{1}{4} + \frac{1}{8}$ écus; this *stake*, which maintains the same ratio between the players as the previous, leads not to absurdity; because the result of this manner to procede is, as Paul gives for stake to Pierre in this case precisely the same sum as that one offers to him, that is to say, an écu, since the infinite series $\frac{1}{2} + \frac{1}{4} + \frac{1}{8}$ etc.$= 1$.

Now this result is perfectly wise; because it indicates that he can not at all have the game in this case, for the reason, that Paul is sure to win, and that, since there is certitude to win on one side, he can not have game.

There is also certitude to win for Paul in the case of the Problem of Petersburg; nevertheless the game can take place, because the players are uncertain which sum Paul will win; and here is why their unique end in this case, is to find one such stake, that their game becomes equal *for him*, whatever be the sum that Paul will win: now all the tosses, having among them for Paul an equivalent value, and Paul being sure to win an écu, his stake must be such, that Pierre on his side can also win an écu; now it is that which arrives when the stake is *two écus*.

This is therefore there the stake which Paul must give, and one must sense, I believe, the evidence of this result, without having recourse to the reflection that in this case here all the tosses have for Paul a real value equal and always permanent at each toss; while in the preceding case, where Pierre offers only an écu at each toss, the value of this écu diminishes for Paul at each toss, and that the total value of that which he can win in this case thence, can consequently be represented only by the series $\frac{1}{2} + \frac{1}{4} + \frac{1}{8}$ etc. while in the case of the Problem of Petersburg, where the real value of that which Paul can win is maintained always the same at each toss, and where, by this same reason, he has each toss (as we have said it in the second solution) a lesser combination against it, the total value of that which he can hope, must be represented by the infinite series $1 + \frac{1}{2} + \frac{1}{4} + \frac{1}{8}$ etc. which is the double of the preceding.

Even when one would not regard the three solutions which I just gave, as perfectly distinct from one another, one will not dispute me in the least, that I have not completely resolved the Problem of Petersburg, and consequently destroyed the principle argument of Mr. d'Alembert against the principles of the calculus of probabilities. We pass therefore to his other reasonings.

§ 2.

The calculation of the probabilities teaches only the art of conjecture according to *the rules* founded on observation, *such*, as we arrive *through them* not for us to not error in some particular cases; but for us to not error at length. A man who would always wager that an effect will take place at the most probable period of its coming, would be sure to win. But, in order that the calculus of probabilities not lead to false results, it is necessary, that it be put on some incontestable bases. It is to the observer *historian*, *physician*, *metaphysician* etc. to set the bases; the calculus can be supported only on observation, *an observation of the events*; *observations of the operations of nature*; *observations of the operation of the human Mind*. It can consequently not at all be applied to some objects, which are outside of the range of all our observations; thus, Mr. Daniel Bernoulli, (N) of whom I have not at all read the memoir, could perhaps have had a wrong in the application which he made of this calculus; but it seemed to me not at all in contradiction with him also as Mr. d'Alembert claims it. One is able to wager and one must believe, that a quite improbable combination has not taken place or will not take place while agreeing that it is not at all absolutely impossible. One must believe, that a regular combination and which announces an end, is the effect of a will and not the effect of chance, I° by that which the causes which produce the effects, which one names the effects of chance, can absolutely not at all produce certain effects which the will produces; for Example: I cast against the wall a bottle filled with ink, it can result from it some stains which resemble more or less to anything; but it is impossible, by the nature of the movement that I make, that my cast produce a perfectly nuanced portrait, as a skilled painter would have made it. There are some contours, some forms which can be produced only by movements directed by a premeditated will. 2nd Because there are infinitely more irregular combinations, than there are of regular; thus, even when one would see on a wall the designs such as chance can absolutely produce them, yet one should presume, that it is a will and not chance which has produced them; because it is quite rare that it is chance, and quite common that it is a will, which produces them.

Mr. d'Alembert says (L) that if one found on a table a certain number of printed characters arranged in a manner that there resulted from them a sense, especially, if it was a phrase a little long, or all one period, there would be no person in the world, who would wager, that this arrangement is produced by chance. Now, says he, mathematically speaking, it is also possible, that some printed characters cast at random produce a sense, that it is possible, that there results from it each other combination. This example contains a manifest error; *because there are infinitely fewer combinations of letters, which give a sense, than there are of them which give no sense at all*. Thus, it is quite natural, that in seeing an arrangement of letters which give a sense, one wagers all in the world, and that by following rigorously the known rules of the calculus of probabilities, that such an arrangement is not the effect of chance. If I arranged a great number of letters in a manner that they had no sense, I would wager just as little on the fortuitous reproduction of this combination which has no sense, as I would wager on the fortuitous production of each other combination which would have a sense: and if this combination, to which I suppose a sense, is composed of a fewer number of letters than my arrangement which has no sense, I will wager less on the fortuitous reproduction of my arrangement, than I will wager on the fortuitous production of this arrangement of a fewer number of letters which give a sense. Finally, by supposing the same number of letters, I will wager less on the fortuitous reproduction of the arrangement, than I would have done and which would give no sense, than I would wager on the fortuitous production of an arrangement of the same number of letters which would give any sense whatever; because my arrangement would be a unique combination, while a like number of letters can produce a very great number of combinations which give a sense. Let one be amused by calculating the combinations, which the letters give which form the word *Constantinopolitanensibus*, that Mr. d'Alembert cites, and one will see the difference that there is among the combinations giving a sense, and those which give it not at all.

The end of the calculus of the probabilities is to teach us, what risks we incur in such an enterprise; what opinion is most probable on such subject; how it is necessary to take us by playing in order to play a fair game; but it does not oblige us to obey its results, however infallible that they be. Well understood, it prescribes us on the contrary often to undertake not at all some light affair what would be the risk and some grand advantage to us.

From that which one plays, in a case, evidently at fair game, it does not follow, that it is from prudence to accept the part. There are some cases, where one would be a fool to accept a wager, although one would have all the advantages of the game for himself: for Example: if one offered me two thousand times the value of my wealth, on condition that I agreed to lose all my wealth, if one drew the single black ticket existing in a pouch with 1999 whites, I would not agree, because it matters to me much more to not lose all my wealth, than it matters to me to have two thousand times more than I have. Now, the example that I cite is precisely the wrong way of the one that Mr. d'Alembert cites (I) I would have all the probabilities for me, it would be necessary to be foolish to the highest degree in order to play with me on this condition, and however I would not accept the part; because there are some cases, where it does not suffice in order to play to be sure that one plays a fair game; where it does not suffice even to play with the greatest advantages; but either it is of the wisest to not play at all; or it would be foolish to incur the slightest risk.

Mr. d'Alembert would have wished to prove by this example that when the number of combinations contrary to an effect is very great, the probability of this effect is yet much less than one supposes it according to the rules of the calculus; now it is that which he does not prove.

I am serving myself with the reasoning which I just made in my German work (Betrachtungen. Note B.) in order to prove that it is not always prudent to adopt, among different probable opinions, that which is most probable; that there can be some cases, and that there is much in life, where it is prudent to adopt that, which, although less probably true, is most conformed to our duty, that which gives us most certainty.

It is of the probability of an opinion of it as of the probability to win by playing. However probable that an opinion be, it can be false: however favorable that a game is to us, one can lose.

When my principal end is to enlighten myself, I must without doubt adopt among different opinions that which is most probably true; likewise, determining myself to play, I must not at all play a dupe in it. But there are some questions on which it matters quite little to a reasonable man to enlighten himself. When my principal end is to not lack in my duty; to not trouble my happiness; when my end is to act, and not to instruct myself; I must adopt, not the most probably true opinion; but that which is most conformed to my duty and to my happiness, by the quite simple reason, that an opinion, however probable that it can be can however be false and that I must not incur the risk of it. It appears that Mr. Jacques Bernoulli has very well sensed this difference. (See: *Ars conjectandi*. Edition of Basel 1713 page 213.)

"Ars conjectandi est ars metiendi quam fieri potest exactissime probabilitates rerum: eo fine ut in judiciis et actionibus nostris semper eligere vel sequi possimus id, quod melius, satius, tutius aut consultius fuerit deprehensum, in quo solo omnis philosophi sapientia et politici prudentia versatur."[9]

None of the arguments of Mr. d'Alembert prove that which he has wished to prove. It is without doubt possible in itself, (E) that one hundred infants who we see born, live each 60 years; but this is, neither *physically* nor *metaphysically probable*. It is likewise of a succession of 20 reigns which would each endure 60 years. One would not wager certainly on such a combination; but one would not wager any more that, out of 20 reigns, each will endure any number of years fixed in advance. Besides, a succession of 20 Kings reigning each 60 years, has against itself, beyond the improbability of this successive uniformity, this, that 20 Kings live all a long time, and that their order of succession is arranged however in a manner, that they succeed all 20 youths to the throne, this which, in a hereditary monarchy is nearly incompatible with the longevity of each. It is likewise of it yet of that which he says (F) of the bankers in pharaon.

What difference do these examples given by Mr. d'Alembert indicate between the *physical* probability and the *mathematical* probability? how do they prove, that the rules of the calculus of the probabilities remain with defect when one applies them to nature? presented as he presents them, they have even been able to appear absurd to some persons, who have not at all studied thoroughly his idea; this here however although false, is not at all absurd; it is necessary even, in order to sense the error of it, to show in the metaphysical principles of the rules, much more that the ordinary geometers are in the state to make it.

The basis of his reasoning is reduced in all the arguments, which I have alleged to this.

If all the combinations must be counted mathematically equally possible among them, there is no reason why one of these, which in the physical order are the most improbable, would not appear all as well as each other; now this is not that which happens at all: therefore the mathematical rules of the calculus remain with defect when one applies them to nature.

This reasoning contains in itself three questions which I analyze; but there are first two considerations to make: 1st it is, that if the mathematical rules applied to nature, remained in fact with defect, this would be, as I have already observed, a certain mark, that they are faulty; because there can not be able to be contradiction between mathematical truths and physical truths. Also the doubts of Mr. d'Alembert, although

[9]The art of conjecturing is the art of measuring as exactly as it is possible the probabilities of things: with this end that in our decisions or actions we may be able always to choose or to follow what has been perceived as being *better*, *more preferable*, *safer*, or *more prudent*; in this alone lies all the wisdom of the philosopher and all the discretion of the statesman.

he does not say it formally, bear on the rules themselves.

This is not doubtful, since he proposes (H.K.) to establish other rules in this regard, by which the combinations, which bring forth constantly, or too often the same effect, would be excluded as impossibles.

IInd That the calculus of the probabilities remains not at all in default when one applies it to nature.

The solutions which I have given of the Problem of *Petersburg*, on the insolubility of which are founded principally the reasonings of Mr. d'Alembert, and all that which I have said in this writing, proves it, it seems to me, sufficiently. Because Ist the improbability of the successive repetition of the same effect increases so according to the rules such as they are, that it ends by being equivalent to an absolute impossibility; now, I do not see how physically one could claim another thing. 2nd Tables No. 2 and 3 demonstrate, that in the combinations of *Heads* and *Tails*, those, which bring forth the same effect often in sequence, are of them such small number, in comparison to those, which bring forth some successions more mixed with the two faces, that the rules, such as they are, give precisely the results, that Mr. d'Alembert would wish to obtain through the other rules. 3rd The rules teach us, that all the combinations of *Heads* and *Tails*, considered individually, are equally possible among them; but they teach us at the same time, that the greatest or least probability of an event, depends, and can depend only on the greatest or least combinations which are contraries to it; now, experience does not teach us, and can not teach us another thing. It does not teach us that the repetition of *Tails* 50 times in sequence is absolutely impossible; it does not teach us further, that one hundred infants, who we see born, could absolutely not attain each 60, or 80, or even 100 years; that a banker in pharaon could not absolutely lose constantly during ten years in sequence; but it teaches us, that it would be necessary to be foolish, in order to expect these events. Now the rules such as they are, and although they admit the equal possibility of all the combinations among them, lead us to these same results, demonstrating to us from them, that the number of combinations contraries to that which *Tails* comes 50 times in sequence; to that which one hundred infants attain, all one hundred, a most advanced age; to that which a banker at pharaon loses ten years in sequence, is so prodigious, that it would be necessary to be foolish to await the one or the other of these events which have so many combinations against them.

The reasoning to which all the arguments of Mr. d'Alembert are reduced, contains in itself the following three questions. I. why, if all the combinations are mathematically equally possible, those, which in the physical order are the most improbable, do they never appear? but this question here Mr. d'Alembert can not be counted to have made at all; because he knows very well, that the most extraordinary combinations and consequently the most improbable take place sometimes. 2. why, if all the combinations are equally possible, those, which physically are the most improbable, do not always appear; for example: why, in playing at *Heads* and *Tails*, does one of the faces not appear constantly? why, all the infants who are born do they not attain the age of one hundred years? etc. this question has a certain profundity; but it leads to the absurd. Thus, I suppose not at all, that Mr. d'Alembert has it in view. It leads to the absurd: because it is as if one demanded, why is that which, *is*? The calculus of probabilities cannot render reason, and its end is not to render reason, why the things exist, or why they exist in such manner rather than in any other. Its end is to give us some rules, based on experience, in order to conjecture, (according to that which is, and has been), that which will be, and to what we can, or we must await.

One cannot reject at all as mathematically impossible the constant appearance of the same effect; for example: that all the infants to be born to date of today will attain in the future all to the age of one hundred years, or else, that by playing at *Heads and Tails*, it will be always *Tails* which will arrive to infinity. There is not, as one would think it, and as I have been carried myself an instant to think it, a contradiction *in the terms*; to depart from the assumption, based on experience, that nature is varied in its effects, and to admit at the same time as absolutely possible, that it is not it, or ceases to be it in one case. There is not thence contradiction in the terms; because we know *only that which is*, we cannot know that which will be. We see, that nature is varied in its effects; but we cannot know, if it will not cease to be it. Would it be reasonable *to admit as more possible* only those always uniform combinations are the ones that Mr. d'Alembert would wish to exclude, *the appearance of Heads and Tails alternating constantly during all of one hundred thousand trials?* or else, *that one will guess correctly, what other combination will take place during these one hundred thousand trials? what different ages all the infants to be born will attain in the future by name in particular?* None of these things can be regarded by us as absolutely impossible in itself; because it is not absolutely impossible, that the world is destroyed tomorrow. The constant appearance of *Tails* can be so much less to be reputed absolutely impossible, that it is not impossible, that a man be skillful enough to make the same face fall each time on his hand without that the medal have more tendency to fall on one side than on the other. But all these things are of an improbability which is equivalent to us to an absolute impossibility. Now it is thence this that the rules of the calculus teach us. One is mislead in these sorts of metaphysical doubts, only because one does not consider attentively enough what is the end and what are the bases of the calculus.

Anything beyond us, beyond the limited intelligence of beings as us is neither possible, nor probable; all in nature is, or is not, has, or has not been; will be, or will not be; all is certain, and this certitude, this necessity, which it is necessary well to distinguish from fatality, is not in contradiction with the most perfect liberty of the human will of which one can make oneself an idea.

The more an intelligence is limited, the less there is of real certitude for it. It is true that the least illuminated men are ordinarily those who believe themselves the most sure in their fact; an ignorant, presumptuous half-wise names impossible all that which they cannot imagine, but their certitude is only an apparent certitude; the doubt is son of meditation.

It is not less necessary to be sure of his fact in order to affirm that one thing is rigorously impossible, that in order to affirm that it is, that it has been, or that it will be. It is necessary, in the two cases, to see with evidence that its existence, or its nonexistence implies contradiction with another thing, of which the existence is evident to us. If one said to me that there exists some part a *green* horse, or in the color *of fire*, I would not believe it; but I would guard myself well to say, that it is impossible; because I do not see these colors imply contradiction with the nature of the horse.

The term *impossible* expresses therefore something more than our way to see; we wander so to speak beyond us, in saying, that a thing is impossible, likewise that by affirming that it is, will be, or has been. But the terms: *possible*, *doubt*, *probable*, express nothing of that which is beyond us; they express only a state of our understanding, namely: our way to perceive the objects.

Thus, however improbable that a thing be, it can be true, however probable that it be, it can be false.

Despite this one can say, that there is a probability and a real possibility; and a possibility and a probability, which are only relatives.

When the most perfectly enlightened human reason can perceive in a fact, an opinion etc., only the possibility or the probability, then this possibility or this probability are real; when on the contrary the enlightened reason knows positively in what is held by it on a subject, in which a less enlightened reason can perceive only more or less possibility or probability; then this possibility and this probability are only relatives.

One can judge on the probability of a thing *a priori and a posteriori*. Mr. Jacques Bernoulli says page 2 of his letter on the game of tennis. (see Jacob Bernoulli *ars conjectandi*) "We pose that there are in a sack a quantity of tickets in part white and in part black, and that I know neither the number of the ones nor of the others; what would I do in order to discover it? I would draw them one after the other (by returning each time into the sack the ticket I had drawn from it, before taking the following, so that the number of tickets in the sack diminish not at all) and if I observed one hundred times, that I drew from it a black; and two hundred times that I drew from it a white, I would not hesitate to conclude, that the number of whites was around the double the one of the blacks; because it is very sure, that the more I would make of these observations by drawing, the more I would be able to expect to approach the true ratio which is found between the numbers of these kinds of tickets, being even a demonstrated thing, which one can so much make from it, that it will be in the end probable of each given probability, and consequently that it will be morally certain, that the ratio between the numbers that one will have thus found by experience, differs from the truth by as little as one will wish, which is all that which one can desire. It is also in this manner that in the games of art and of skill one can understand by how much a player is stronger than another player."

One could make on this occasion an objection; one could say: therefore this method to judge *a posteriori* of the number of white and black tickets, which are found in the sack, is as sure as Mr. Bernoulli thinks it; therefore this can be, as he says it, *a demonstrated thing*, which one will arrive through this means to the discovery of the truth, it is necessary, not only that it be improbable that a single or a small number of black tickets, mixed with a very great number of white tickets, reappears often; it is necessary, but it is impossible, that in an infinite number of drawings this small number of black tickets appear constantly without that there ever appear a white of them. Because, if between two tickets alone of which one white and the other black, it is possible that the black ticket exits from the sack an infinity of times in sequence, it appears, that it must be impossible to judge *a posteriori* the number of white and black tickets which are found within a sack.

This objection that I have made myself in my thought on the rules of the calculus, is resolved, by reflecting that these rules consider themselves the constant coming of the same ticket, as being of an improbability equivalent to an absolute impossibility; now, it is all that which it is necessary in order to justify fully the reasoning of Mr. Jacques Bernoulli and the principles of the calculus.

However it is necessary to note here that which I myself just said on it: in order that the probability of a thing be real, it does not suffice that this thing *appear* probable; it is necessary to be certain of its probability; it is necessary therefore to know the number of the combinations, or of the modifications, or of the qualities which are lacking to it in order to be certain; and it is necessary consequently to have the certitude that there not exists, and can not exist combinations etc., which are contraries to it beyond those there.

A probability that we know only *a posteriori* is therefore rarely as real, as a probability, that we know *a priori*.

Three persons can judge an event in three different ways, and each of them can have reason; because the possibility and the probability of an event has no relation with its reality. When there is question to draw a ball from a sack in which there is only one black and one white, and if I put may hand into the sack in order to draw one, it is certain in advance which of the two will exit from it. This drawing is a necessary consequence of the position of the sack; of that of the balls; of the movement of my fingers; of the ideas which direct my choice etc.; but, as the causes which make one such ball exit, can not be calculated by us, and as the effects, that they produce, are such, that, of any manner that we ourselves took, we can never predict what ball will exit from the sack, we say, that it is equally possible, or equally probable, that it is the one or the other; this wants to say: that we have no reason to believe that it will be one rather than the other. This disposition of our mind is the same, even when one of the two balls is actually drawn from the sack by a third, who knows consequently which of the two is exited from it. Relative to us, who are ignorant of it, the possibility and the probability of it is no less the same: we can wager in this case on the one or the other with as much reason, as if they were again in the sack.

This being, we suppose that I hold in hand a purse in which there is one white ball and one black ball; that I have drawn in the presence of Paul four times in sequence this last; that I draw a fifth time, that I regard the ball and demand to Paul, without showing it to him, what ball he supposes to be exited from the purse, and that he says to me that he would wager on the white by reason that the black has already appeared four times in sequence consecutively. Let there occur a third, who knows not consequently that the black ball has already appeared four times consecutively; that I demand of him on what ball he would wager, and that he says to me (as reason) that he would not wager more on the one than on the other, or else on the one as gladly as on the other, by the reason that he does not see more probability to the exiting of the one than to the exiting of the other.

Here are three different judgments on the same event. Mine, because, having seen the ball I am sure of my fact and can not wager at all. *The judgment of Paul*, who regards the exiting of the white ball as more probable than the exiting of the black ball; and *the judgment of the third*, who knowing not at all, that the black ball was already exited four times in sequence, does not see more probability to the exiting of the one than to the exit of the other.

The question is to know, if Paul judges sanely, or else if it is prejudice which guides him?

Mr. d'Alembert appears to be of the opinion of Paul, and regards this opinion of Paul as a proof, that the rules of the calculus applied to nature remain with defect (G).

I am of the opinion of Paul, but I am not at all of the opinion of Mr. d'Alembert.

I am not at all of the opinion of that one, because the opinion of Paul could be, only a prejudice, as it is one of the part of the players to believe in some fortunate and unfortunate days; (P.) a prejudice, which can not at all invalidate the geometrical calculations.

I am not at all of the opinion of Mr. d'Alembert 2° because this question, of any manner that one decides it, has no relation with the manner how it is necessary, before beginning the game, to evaluate the probability of the arrival of *Tails* in any period whatever. 3° because the rules of the calculus are rather against, than for the opinion of the players, who wish not at all to wager on a trial which is already repeated many times in sequence; and that the reasoning of Mr. d'Alembert carries consequently absolutely to false.

But I am of the opinion of Paul; because I do not believe, that in this occasion, the opinion of the players who regard as improbable the repetition of an event, which has already taken place in the same game many times in sequence, is a prejudice. It is incontestable, I agree with it, that at each trial, in playing at *Heads* and *Tails*, it was the 50^{th}, after *Tails* had already appeared consecutively 49 times in sequence, there is never, to consider this trial in itself, setting aside from those which have preceded it, that odds one against one on the arrival of *Tails*; it is incontestable that, however great that the difference of the sum would be, that one would receive for example: if, after having wagered to bring forth *Tails* 50 times in sequence, one brought it forth; and that which one would receive after 49 trials, this last sum would however never be but precisely the half of the first, and that consequently, to win the 50^{th} trial, after having won the 49^{th}, is never another thing than to win a trial, the sum that one received after the 50^{th} trial being just the double of that which one received after the 49^{th}. it is very true also, that it must appear little philosophic, to admit a sort of influence of the past events on the future events when one perceived no relation between these events. However I am of the opinion of Paul, I would wager always on the face contrary to that, which would have already appeared a certain number of times in sequence, at least that I was not able to presume a tendency of the medal to fall rather on one side than on the other; and the reason which determines me to be of the opinion of Paul is precisely this, *that it is always equally possible, at each trial, that it is Heads or Tails which appear*. Because, I myself say, the possibility that this is *Heads* or *Tails* which arrives, being, setting aside the preceding events, incontestably perfectly equal, my judgment is perfectly in equilibrium; now, when a balance is in perfect equilibrium; now when a balance is in perfect equilibrium, the lightest weight added on a side decides its tendency; this being, the reflection: that one of the faces has already appeared consecutively a given number of times in sequence, must therefore likewise determines me to wager on the coming of the face contrary to this, although I have no reason to presume and not presume at all, that the past events had a direct influence on the future events; and I think that my determination, far from being in contradiction with the mathematical rules of the calculus, is perfectly conformed to them; I think even, that one could easily find a series of fractions, which would indicate exactly by how much diminished at each trial, in this case, the probability to bring forth the face which has already appeared more or less times in sequence.

There remains for me yet to respond to the third question contained in the reasoning, to which I have said that all the arguments of Mr. d'Alembert are reduced.

Here is this question of which I have not at all yet made mention. *Why, if all the combinations of Heads and Tails must be counted mathematically equally possible, do those, which physically are the most improbable not appear as often as the others?*

Response. There is no combination of *Heads* and *Tails* which is physically more probable than each other, and Mr. d'Alembert is deceived in believing, that one of these combinations appears more often than the other. Without doubt *Heads* and *Tails* appear nearly always mixed; it is quite rare that *Tails* or *Heads* are present a long time in sequence without mixing; but why? we consider tables No. 2 and 3. They will say to us that it is because in eight trials, there are 254 combinations *more or less mixed*, and that there are only *two* of non-mixed. The probability of an event depends on the most or least number of combinations which it has for, or against itself.

An event can therefore be more probable than another *event*; but a *combination*, supposing that it is not composed itself of many other combinations, can not be more probable than another combination.

One sees by table No. 2, that there are 256 combinations of *Heads* and *Tails* for eight trials, and that these 256 combinations can be contained in five kinds 1° all *Heads* or all *Tails*, this which makes two combinations, 2° one time the one of the two, and seven times the other, this which gives sixteen of them, 3° two times the one of two, and six times the other, this which gives fifty-six of them, 4° three times the one of the two, and five times the other, this which furnishes 112, finally 5° as many of the one as of the other, this which gives 70 of them.

One sees by this table what is the relation of probability of these five kinds of combinations among them; that the most probable of these five kinds of combinations in itself, is the fourth; and that the one who would wager consequently, to bring forth rather any combination of this fourth kind, than any one of the four other kinds that he would determine, would have for him a probability to win more or less great according to the kind of combination against which he would wager.

There results thence, in truth, by reflecting, that, if one repeated to play at *Heads* and *Tails* in eight trials, 256 times (this which would give a number of trials $= 256 \times 8 = 2048$) it would not be at all probable that the 256 combinations written here, would all appear; it would be on the contrary very probable that they would not be presented totally, but that there would appear some ones of them repeated, not only of those of the fourth kind, which by its nature, being the most numerous in combinations, must furnish the most repetitions; but also of those of the 5, of the 3, and perhaps of the 2 kind; but it is not necessary to conclude with Mr. d'Alembert, (see his opuscules mathématiques Volume VII page 39) that the one or the other of the 256 combinations that the game of *Heads* and *Tails* gives in eight trials, is, considered in itself, less probable than each of the 255 others. Each combination considered individually is physically as probable as each other; the difference of probability exists only for *the kinds*, and not for the combinations considered individually. There are no reasons, why in 256 combinations one could bring forth less easily xxxxxxxx than xxoxoxox, although this last combination is of the fourth kind which furnishes the most combinations; I am quite right that one can be carried to think it; but in order to convince ourselves that this is not, we suppose a sack in which there are one hundred balls, of which 99 white and one black, and that it is a question to draw a single ball from this sack; it is evident that there are in this case odds of 99 against 1, that the ball, which one will draw, will be one of the 99 whites, and not the unique black which is found in the sack; but does it follow thence that each of the 99 white balls which are found there, has a greater probability for us to be drawn, than has the black ball from it? certainly not; and in order to be convinced with evidence, one has only to suppose at present, that instead of being all whites, these 99 balls are all different among them in a manner that each is unique in its kind like the black ball; that there is one yellow, another color of rose etc. will one say under this last hypothesis, that there is more probability to draw one of these 99 balls in particular, for example: the unique ball the color of rose, than there is to draw the black from them? however it will not be less true, that there are odds 99 against one, that one will not draw the black. Being this, there is therefore not, in playing at *Heads* and *Tails* in eight trials, reason *to suppose* the appearance of the combination xxoxoxox physically *more probable*, than the appearance of the combination xxxxxxxx.

There is no difference between the game of a man against whom one would wager, that he will not bring forth, in playing at *Heads* and *Tails*, *Heads* eight times in sequence; and another, against whom one would wager, that he will not draw from a sack, in which there are 256 balls, the unique black ball, which is found there; that which one of the two must give for stake, the other must give it. Another question is, if (as I have already said) the assumption that one could make is based on truth, that in a number of drawings or of repetitions of the same game = to the number of the combinations, is it probable that each combination will appear one time? an assumption, on which one could believe that is based this that one does, that in 256 trials, a unique black ball which is found mixed in a sack with 255 others must probably appear one time, although after each trial one recasts the ball drawn into the sack. This assumption (as I have remarked above) would not be based at all certainly on truth; it is to the contrary quite probable, and the more the number of the combinations is great, the more it is probable, that by repeating the game as many times as there are combinations, all these combinations will not appear at all; but there will be of them some more or less repeated, while some others will not exit at all from the wheel of fortune; but, as each combination is found in the same case, as each can not at all appear at all, or reappear more than one time, this observation alters nothing from it the relation of the combinations among them and one can draw no induction in favor of the opinion of Mr. d'Alembert.

One sees finally by this table that by playing at *Heads* and *Tails* a long time in sequence, one must bring forth, according to all the rules of probabilities, one of the faces as often as the other, and that the principles of the calculus lead consequently (as I have already observed) precisely to the result to which Mr. d'Alembert would have wished to arrive (G and H) by other rules; and I can not at all prevent myself to observe, that, if this great geometer has been able to error as greatly and as obstinately (because he returns to this matter three or four times in his opuscules mathématiques) in a science of which he made profession; it is not necessary to be surprised, that he has made false steps in the sciences, which he had not at all equally studied thoroughly. I say not at all, that all the principles of the calculus of probabilities are well established: less still that some analysts have not often quite badly applied them; because with the exception of some bits of the *ars conjectandi* of Jacques Bernoulli, and some parts of the writings of Mr. d'Alembert, of which there is question here, I have read nothing in this genre; and if one demands of me why, I will say without detour that it is 1° because I find much more easily in my head the speculative truths which it matters for me to know, than in the works of the others. 2° because I am nothing less than a Geometer, and that it is necessary that I give myself much pain, in order to comprehend the works Algebraically treated.

At present we consider again a moment table No. 3.

I have arranged this table in the manner which has appeared to me most proper in order to demonstrate perfectly the offer that Pierre makes to Paul in the Problem of Petersburg, and what is consequently I. the probability of the one to win at each trial. 2. at what trial he is able to expect to win, and 3. how much he must he give for stake to Pierre?

Indeed, one sees by this table, that in the number of 256 combinations which can take place, the one just as well as the other, by playing at *Heads* and *Tails* out on eight trials, there are 128 of them which bring forth *Tails* at the first trial, 64 which bring it forth at the second; 32 at the third; 16 at the fourth; 8 at the fifth; 4 at the sixth; 2 at the seventh; and one alone at the eighth, likewise that there is only one of them which does not bring it forth at all. One sees therefore that Paul has a probability $= \frac{1}{2}$ to win at the first trial; $\frac{1}{4}$ at the second etc. etc. One sees that this progression would be the same, if, instead of playing out of eight trials, one played, as Pierre and Paul on an indefinite number of trials, that we will name a; the probability to win at the first trial would be $= \frac{1}{2}a$; at the second $\frac{1}{4}$ of a; and that consequently the general probability to win of Paul, being as I have said and repeated in my solutions, equal to the infinite series $\frac{1}{2} + \frac{1}{4} + \frac{1}{8} + \frac{1}{16}$, is equal to certitude.

One sees that the probability to win a sum more or less considerable is also in this same progression; but as Paul is sure to win at least one écu, and as there is doubt only concerning the most that he will win perhaps beyond this écu, this probability that one seeks, of the sum that he can expect to win, can not at all be represented by the series $\frac{1}{2} + \frac{1}{4} + \frac{1}{8} + \frac{1}{16}$ etc. but must be represented by the series $1 + \frac{1}{2} + \frac{1}{4} + \frac{1}{8} + \frac{1}{16}$ etc. infinity $= 2$.

There remains for me to develop my ideas on the stake, on that which one must understand as equalizer, either the game, or different positions among them; on that which one must understand by the expectation of the players, and before all, to give a clearer idea of the certitude, than have been those of Mr. d'Alembert in this regard.

He says in his opuscules mathématiques Volume II, Volume IV, and Volume VII in his different articles on this subject, and especially Volume IV, that certitude and probability are incommensurables; that metaphysically and rigorously speaking, certitude is to the simple probability (besides he says to the greatest probability) as that infinity is to unity. If this proposition were true, one would be therefore, some strong reasons that one would have to believe a thing, so far from the truth, as those who have no idea of it; the better founded conjectures would lead to nothing, and I do not see how one could ever arrive to make some discoveries; it would be necessary to suppose, that all those which are made, are of pure effects of chance; now it is that which Mr. d'Alembert was quite removed to believe. (see his mélanges)

However probable that a proposition be, it can be false; I have said and repeated; but it follows not at all thence, that the probability is not at all a routing to the truth.

Mr. Jacques Bernoulli had a much clearer idea of certitude, by saying: (see ars conjectandi) that the probability is *a partial certitude*.

I have said the same thing before having read his work. (see: Betrachtungen from § 41 to § 47 and § 71)

We suppose that the word *certitude* is written on a table, and that I cover it entirely with my hand; no person could guess, what word it is; because I suppose that there is nothing written before or after of which the sense could be made to guess it. But in measure as I will uncover the letters c. e. r. t. i. one will approach the discovery of the truth. It is likewise of it of all truths; each truth which is not of first intuition, can be recognized only by degrees; because each proposition which is not at all *an axiom* is composed of other propositions, which must be each true in order that it be. The more we have before us of these elementary propositions, of which we recognize the truth with evidence, the more we ourselves approach the discovery of the truth of the composite proposition; but as the same elementary propositions joined to the others, can form different composite propositions, it is necessary, that we know all those, of which a proposition is composed, and that we are certain consequently that there enters neither more nor less into its composition than those that our analysis has made us perceive, in order to be sure of our fact.

Thus, the more we know of elements of a thing and the more the thing is probable in our eyes; but as long as we do not know it all, it can be false. Being this, in order to be certain that a thing is probable, or to what point it is, we must have, as I have already said, this same conviction, otherwise we can not say, that it is probable; we can only say, that it appears to us so; now it is remote from one of these assertions to the other. The more I discover of the letters of the word that I have covered with my hand, and the more there is of facility to guess it: but, if one calculated the degrees of probability to recognize it, according to the number of letters discovered, one would be mislead however greatly. In order to guess what is the word covered by my hand, of which I have discovered the five letters c, e, r, t, i, it is necessary, supposing that this is a word which has a sense, and a French word, to know how many words there exist in this language which begin with these five letters; now in the Dictionary of the academy I find, besides the word certitude, the following words: *certificat*, *certificateur*, *certification*, *certifier*, *certifié*, *ée* (participle); thus in counting the different tenses and terminations of the verb *certifier*, there exists a great number of combinations contrary to the word *certitude*. But, if I discovered a single letter more, that the visible letters were the following six: c, e, r, t, i, t, one would be certain, without seeing the entire word, that it is the word *certitude* which is covered by my hand; because there exist no others of them, at least in the Dictionary of the academy, which begins with these six letters.

If certitude and the probability were incommensurables, no game would be possible, and it would follow to renounce to all the speculations of commerce, of finance, of agriculture which oblige to risk small *certain* sums in the expectation to make great *probable* profits, or even doubtful.

I know not if one has ever said it more clearly, but it is not less true, that all the theory of games is supported on this principle, *that the state of certitude must be estimated to have the double of the value of the state of perfect doubt*. Indeed, when one plays at *Heads* and *Tails* at one trial, it is perfectly doubtful which of the two faces will appear; because there is no more probability in favor of one than of the other. However in playing thus, everyone agrees; and Mr. d'Alembert contests the point not at all, that one plays in a fair game by setting into the game the same sum on both sides; now what does one make in setting the same sum into the game in each case? one risks the certain possession of that which one sets into the game, in favor of the perfectly doubtful expectation of doubling it: one risks there the certain $= 1$, in the hope of having 2. one estimates therefore the certitude $= 2$, and the perfect doubt $= 1$. This is a reflection that Mr. d'Alembert has not made at all; but this evaluation itself, could one say, is it not faulty? *I respond*: no, it is metaphysically and rigorously exact; because, if the state of certitude can be acquired by us only gradually, by considering the objects under all their faces, by analyzing all their elementary parts, each degree, which leads us to the recognition of the thing, is a step that one makes toward the truth, a step, which one must name a degree of probability; and *perfect doubt* is the state, in which we find ourselves, when we have no more probability in favor of the existence of the thing, than in favor of its nonexistence. Being this, representing *certitude*, as one must, by a fraction equal to unity: e.g. $\frac{100}{100}$, the state of perfect doubt must be represented by the fraction $\frac{50}{100}$, which is the half of the preceding; because the degrees below lead to the truth contrary of that which one seeks. The first degrees of probability in favor of the object, toward which we tend, is $\frac{51}{100}$, and consequently it is only when we have $\frac{75}{100}$, that we are at mid-path on the discovery of the truth, and it is this state, and not $\frac{50}{100}$, which one calls a half-proof. A half-proof has therefore a value $= \frac{3}{4}$ and perfect doubt a value $= \frac{1}{2}$. (see my Betrachtungen printed at Nuremberg in 1787 § 41, and § 71)

It is no less true, that the possibility and the probability have no relation with the reality of the things existing outside of us; these terms express only the different states of our understanding; but *these states* are real *in us*, and their relation among them is such as I just enunciated it. It is certain that in many cases one would have great wrong to exchange *his state of certitude* against *a state of perfect doubt*; that one would have often great wrong also, if one was certain (see note 3.) to be attained of a harm in the appropriate half of his lifetime, to exchange a position with another who would be equally certain to be attained of this same harm, but would not know the period of his life in which he will attain it; however these two positions are, setting aside the particular relations in which one is found, and the fears or the particular sentiments of which one is affected, perfectly equal, and it is not necessary to conclude from that which I just said, that the value of one of these positions is the double of the other; because in this case the doubt travels only on the period of the coming of the harm, the certitude of it being attained is supposed the same in the two positions.

Mr. d'Alembert claims in Volume IV of his opuscules mathématiques 17th memoir, that one has not at all yet attached some clear ideas to that which one understands by *stake*, by the expectation of the players; less yet by *total expectation* and *partial expectation*; that can be; the clear ideas in general are quite rare, even among the philosophers; but here are my ideas, and I dare to believe that they are clear enough.

When one compares different positions, be it physics, or morals among them, I defy that one is guided by some other principal ideas, than by those *of compensation and by the mean proportional*. These two metaphysical ideas (or, if you like it better, physical; because they are themselves daughter of observation) are the two mother ideas of the principles of mechanics, likewise of those of the calculus. In mechanics the *forces* and the *weights* are compensated by the *speed* and the *distances*; in the applied calculus either in the game, or in commerce, or in all other things moral and intellectual, it is *the good* or *the bad*, *the advantage* and *the disadvantage*, which are compensated, increased or diminished, either by their *probability or improbability*, or also by their *remoteness* or their *proximity*; and in order to find this compensation, it is always in the idea of the mean proportional that it is necessary to have recourse. In mechanics this mean proportional is the *supporting point*; when one wishes to compare or set in equilibrium some weights or some equal forces, the supporting point must be placed in the middle of the distance which separates these weights or these forces; it is thence that which produces *the ordinary balance* etc. when on the contrary one wishes to make a considerable weight hold in equilibrium by a light weight, or to move it with a slight force, the supporting point must no longer be in the middle; it must be placed in a manner that these weights or these forces are compensated among them by their more or less distance from this supporting point; it is thence that which produces the *lever*, the *spring balance* or the *Roman balance*. (Die Schnellwage) It is likewise of it when the question is to calculate any ratios whatever. When the advantages and the probabilities are the same, the mean proportional is in the middle; it is $= \frac{1}{2}$; when these advantages and these probabilities differ, the mean proportional must, in order to compensate them, fall, as in the case of the Problem of Petersburg, on another point entirely. All is similar in nature, and all is varied; a small number of principles, I have said it in more than one of my works, are the basis of all human knowledge; but it is given to few men to perceive them; to less yet to know them, and to some of them to attain all the results.

I see no further relation *a priori* between the distance and weight; than between the probability of an event and the advantage that this event can procure to me; I see even *a priori* much less relation; however experience teaches me and no person doubts, that the distance from the supporting point increases the motive force, and that, if a machine is made in a manner that I can with a force $= 1$ to raise a weight $= 10$, I can likewise with a force $= 10$ raise a weight $= 100$. Being this, why would one doubt that the probability increases really the value of an event? that the probability or the improbability to win a sum, changes really the value of this sum, and that the one who has $\frac{1}{10}$ of probability to win ten écus, and the one who has $\frac{1}{1000}$ of probability to win 1000 écus, play the same game, and must give for stake the same sum? It is true, that the one who has only $\frac{1}{1000}$ of probability to win; risks really one hundred times more to lose his écu, than the one, who has $\frac{1}{10}$ of probability; but, beyond that he can also win one hundred times more than that one, and that his ris is found compensated thence, there is yet another reflection to make, it is that the games are equalized only by the length; because to play in a fair game wishes to say: *to arrange the game in a manner, that, if the players repeat the same game as often as they have of chances for and against them, the result of the game will be, that he will have neither loss nor gain on both sides*.

In mechanics the more the weights which there is question to raise is enormous, the more the machine is difficult to make; a machine, which with a force $= 1000$ must raise up a mass = one million, is much more difficult to construct, than a machine, which with a force $= 1$, raises a weight $= 1000$; however the ratio is the same; from it has one ever concluded that the Theory mechanics remains with defect when one applies it to nature?

The abstract principles of the sciences must be the base of our determinations; but in the practical there are yet other objects to consider. The end of the calculus is to make us know the ratio which exists among the different positions; but in the practical it is necessary often to compare this ratio, with the particular ratios in which we find ourselves. There are some cases, where it would be unreasonable to exchange position, precisely because the positions are perfectly equal. Indeed, why change when one wins nothing in changing? why make an acquisition in which one finds no profit? in order to change position, when the positions are perfectly equal, it is necessary, that those who change, have each of the particular reasons to prefer that which they have not, to that which they have.

We come finally to that which one must understand by *the Expectation* of the players and by *the stake*.

Simple Good-sense, destitute of all science, dictates to us, that when we make an enterprise, which can turn in different manners for us, we must not ourselves expect that it will turn, either in the happiest manner, or in the unhappiest manner. This reasoning, that the laborer makes behind his plow, just as well and often better than our scholars in their offices, consolidate and determine with precision by the calculus, form that which one calls *the expectation* of the players, or of the actors in all the cases where there are some risks to incur.

If I have $\frac{1}{1000}$ of probability to win 1000 écus, the real value that I can expect, or *my expectation*, is an écu. No reasonable person would give to me more in this case for my position, and it is also *the stake* that I must give to the one, who plays against me, in order to play with him in a fair game. Thus the sum, which makes the expectation of each player is precisely that which he must give for stake.

Simple Good-sense saying to us, that the more an event is probable, the more it has of value, it is evident that the rule which says, that in order to find the stake, it is necessary to multiply the sum that one can win, by the probability that one has to win it, is quite exact in itself; but in the simple cases it suffices in order to find *the expectation of the player* and consequently *the stake* which he must give; to divide the number of combinations that he has for it, by the total number of combinations for and against.

When a player can, among different sums all equally probable, win from it only one, these different sums must be added together and next divided by the total number of combinations which he has for and against him; the fraction which will result from it, will express *his stake*.

When a player can win different sums and when the probability to win one or the other of these different sums is not the same for him, then it is necessary to know first the expectation which he has to win each of these sums in particular; this expectation is found by multiplying each of these sums with the probability which he has to win it. These different *products* will form his *partial expectations*, and in order to have his total expectation, it is necessary to add all these different *partial products* together, and to divide the sum which results from it by the total number of combinations which he has for and against him; this fraction will express his *total expectation* and consequently *the stake* that he must give.

When there are different combinations, and that the player can lose or win more or less, it is not necessary to believe that the sum that he gives of stake and which forms his *total expectation*, represents the sum which he will win indeed; he will lose or he will win so much more and so much less; his total expectation is a *mean proportional* among all the different events which can take place, *such*, that if the game is repeated often, the players will be found all taken account, losses and reciprocal gains deduced, having neither gains nor losses; now it is that which one calls to play in a fair game.

I know not if I have developed my ideas with clarity; but, if I am arrived to make myself understood by each man capable of an average attention, one will conclude from it, that the synthesis and the metaphysical analysis illuminate much more the mind than the algebraic analysis, and one will be less surprised to encounter often grand algebraists quite embarrassed by their science, when one proposes to them some questions on which one can not at all operate with some X or some Y.

Table I

This table shows

A. How many écus Pierre offers to Paul for each trial.
B. How many combinations there are of *Heads* and *Tails* at each trial.
C. What is the probability to bring forth *Tails* at each trial.

A.

	1st	2nd	3rd	4th	5th	6th	7th	8th	Trial
Écus	1	2	4	8	16	32	64	128	etc.

B.

	1st	2nd	3rd	4th	5th	6th	7th	8th	Trial
Combinations	2	4	8	16	32	64	128	256	etc.

C.

	1st	2nd	3rd	4th	5th	6th	7th	8th	Trial
Probabilities	$\frac{1}{2}$	$\frac{1}{4}$	$\frac{1}{8}$	$\frac{1}{16}$	$\frac{1}{32}$	$\frac{1}{64}$	$\frac{1}{128}$	$\frac{1}{256}$	etc.

Table II

This Table represents all the possible combinations of x and o in eight trials. There are 256 of them. These 256 combinations are classed into five kinds of combinations, of which the first contains only two of them. The second 16, the third 56, the fourth 112, and the fifth 70.

First Kind of *two* Combinations

1.	2.
oooooooo	xxxxxxxx

Second Kind of *sixteen* Combinations

3.	4.	5.	6.	7.
oxxxxxxx	xoxxxxxx	xxoxxxxx	xxxoxxxx	xxxxoxxx
8.	9.	10.	11.	12.
xxxxxoxx	xxxxxxox	xxxxxxxo	ooooooox	oxoooooo
13.	14.	15.	16.	17.
ooxooooo	oooxoooo	ooooxooo	oooooxoo	ooooooxo
		18.		
		xooooooo		

Third Kind of *fifty-six*

19.	20.	21.	22.	23.
ooxxxxx	oxxxxxxo	oxxxxxox	oxxxxoxx	oxxxoxxx
24.	25.	26.	27.	28.
oxxoxxxx	oxoxxxxx	xooxxxxx	xoxxxxox	xoxxxoxx
29.	30.	31.	32.	33.
xoxoxxxx	xoxxoxxx	xoxxxxxo	xxooxxxx	xxoxxoxx
34.	35.	36.	37.	38.
xxoxxxo	xxoxxxox	xxoxoxxx	xxxooxxx	xxxoxxxo
39.	40.	41.	42.	43.
xxxoxoxx	xxxoxxox	xxxxooxx	xxxxoxxo	xxxxoxox
44.	45.	46.	47.	48.
xxxxxoox	xxxxxoxo	xxxxxxoo	oxxooooo	ooxxoooo
49.	50.	51.	52.	53.
oooxxooo	ooooxxoo	oooooxxo	ooooooxx	oxooooxo
54.	55.	56.	57.	58.
oxooxooo	oxoooxoo	ooxoooxo	ooxooxoo	oxooooox
59.	60.	61.	62.	63.
ooxoooox	oooxooox	ooooxoox	oooooxox	oxoxoooo
64.	65.	66.	67.	68.
ooxoxooo	oooxoxoo	ooooxoxo	oooxooxo	xoooooox
69.	70.	71.	72.	73.
xooooxo	xooooxoo	xoooxooo	xooxoooo	xoxooooo
		74.		
		xxoooooo		

Fourth Kind of *one hundred twelve*

75.	76.	77.	78.	79.
oooxxxxx	oxoxoxxx	ooxoxxxx	oxxoxxox	oxxxxoo
80.	81.	82.	83.	84.
oxoxxxxo	oxxoxxxo	oxxoxoxx	oxxooxxx	oxooxxxx
85.	86.	87.	88.	89.
oxxxoxox	ooxxxxo	oxxxooxx	ooxxxoxx	oxxxxoxo
90.	91.	92.	93.	94.
oxxxoxxo	oxoxxxox	ooxxxxox	ooxxoxxx	oxxxxoox
95.	96.	97.	98.	99.
oxoxxoxx	xoooxxxx	xoxoxoxx	xoxxooxx	xooxxoxx
100.	101.	102.	103.	104.
xoxxxoox	xoxooxxx	xoxoxxxo	xooxxxox	xoxxxoxo
105.	106.	107.	108.	109.
xoxxxxoo	xooxxxxx	xooxoxxx	xoxoxxox	xoxxoxox
110.	111.	112.	113.	114.
xoxxoxxo	xxoooxxx	xxoxoxox	xxoxxoox	xxooxxox
115.	116.	117.	118.	119.
xxoxxoxo	xxoxxxoo	xxoxoxxo	xxooxoxx	xxoxooxx

120.	121.	122.	123.	124.
xxooxxxo	xxxoooxx	xxxoxxoo	xxxoxoxo	xxxoxoox
125.	126.	127.	128.	129.
xxxooxxo	xxxooxox	xxxxooox	xxxxoxoo	xxxxooxo
130.	131.	132.	133.	134.
xxxxooo	oooooxxx	ooooxxxo	oooxxxoo	ooxxxooo
135.	136.	137.	138.	139.
oxxxoooo	ooxoxoxo	oxoxoxoo	ooxxooxo	oxooxxoo
140.	141.	142.	143.	144.
oxxooxoo	oxoooxxo	oxooooxx	oxoxxooo	oxoxooox
145.	146.	147.	148.	149.
oxxoooxo	oxoooxox	ooxooxxo]	ooxooxox	ooxoooxx
150.	151.	152.	153.	154.
ooxoxoox	oooxooxx	oooxoxox	oooxoxxo	oxxoooox
155.	156.	157.	158.	159.
ooooxxox	ooxxooox	ooxxoxoo	ooxoxxoo	oooxxoox
160.	161.	162.	163.	164.
oxxoxooo	ooooxoxx	oxoxooxo	oxooxoxo	oooxxoxo
165.	166.	167.	168.	169.
oxooxoox	xoxoxooo	xooxooxo	xooooox x	xooooxxo
170.	171.	172.	173.	174.
xoxoooox	xooxooox	xooxoxoo	xooxxooo	xoooxoxo
175.	176.	177.	178.	179.
xoxxoooo	xoooxxoo	xooooxox	xoooxoox	xoxooxoo
180.	181.	182.	183.	184.
xoxoooxo	xxoxoooo	xxooooox	xxoooxoo	xxooooxo
	185.		186.	
	xxooxooo		xxxooooo	

Fifth Kind of *seventy* Combinations

187.	188.	189.	190.	191.
ooooxxxx	oooxxxxo	ooxxxxoo	oxxxxooo	oxoxoxox
192.	193.	194.	195.	196.
oxxoxoxo	oxoxoxxo	ooxoxoxx	oxxoxxoo	oxxooxxo
197.	198.	199.	200.	201.
oxxooxox	ooxxooxx	oooxxoxx	ooxooxxx	ooxxoxxo
202.	203.	204.	205.	206.
ooxxoxox	oooxoxxx	oxxoxoox	oxoxxoxo	oxoxxxoo
207.	208.	209.	210.	211.
oxxxooox	oooxxxox	oxoooxxx	ooxxxoox	ooxxxoxo
212.	213.	214.	215.	216.
ooxoxxox	ooxoxxxo	oxxxoxoo	oxoxxoox	oxooxxox
217.	218.	219.	220.	221.
oxxxooxo	oxooxoxx	oxoxooxx	oxooxxxo	oxxoooxx

222.	223.	224.	225.	226.
xooooxxx	xoxoxoxo	xooxooxx	xooxxoox	xooxoxox
227.	228.	229.	230.	231.
xoxoooxx	xoxxooxo	xoxooxox	xoxoxoox	xooxxoxo
232.	233.	234.	235.	236.
xoxxxooo	xooxxxoo	xoooxxxo	xoooxxox	xoooxoxx
237.	238.	239.	240.	241.
xoxooxxo	xooxoxxo	xoxoxxoo	xoxxoxoo	xoxxooox
242.	243.	244.	245.	246.
xxooooxx	xxooxoxo	xxoxooxo	xxoxoxoo	xxooxxoo
247.	248.	249.	250.	251.
xxxoxxooo	xxiixiix	xxoooxxo	xxoooxox	xxoxooox
252.	253.	254.	255.	256.
xxxoooox	xxxooxoo	xxxoxooo	xxxoooxo	xxxxoooo

Table III

Represent also the 256 possible combinations in eight trials by X and O, but differently arranged. One sees by this table that there are 128 combinations which bring forth *Tails* at the first trial. 64 which bring it forth at the second. 32 at the third. 16 at the fourth. 8 at the fifth. 4 at the sixth. 2 at the seventh one alone at the eighth; likewise that there is only one alone which does not bring it forth at all.

128

Combinations which bring forth *Tails* at the first trial.

1.	2.	3.	4.	5.
oooooooo	oxxxxxxx	ooooooox	oxoooooo	ooxooooo
6.	7.	8.	9.	10.
oooxoooo	ooooxooo	oooooxoo	ooooooxo	ooxxxxxx
11.	12.	13.	14.	15.
oxxxxxxo	oxxxxxox	oxxxxoxx	oxxxoxxx	oxxoxxxx
16.	17.	18.	19.	20.
oxoxxxxx	oxxooooo	ooxxoooo	oooxxooo	ooooxxoo
21.	22.	23.	24.	25.
oooooxxo	ooooooxx	oxooooxo	oxooxooo	oxoooxoo
26.	27.	28.	29.	30.
ooxoooxo	ooxooxoo	oxooooox	ooxoooox	oooxooox
31.	32.	33.	34.	35.
ooooxoox	oooooxox	oxoxoooo	ooxoxooo	oooxoxoo
36.	37.	38.	39.	40.
ooooxoxo	oooxooxo	oooxxxxx	oxoxoxxx	ooxoxxxx
41.	42.	43.	44.	45.
oxxoxxox	oxxxxxoo	oxoxxxxo	oxxoxxxo	oxxoxoxx
46.	47.	48.	49.	50.
oxxooxxx	oxooxxxx	oxxxoxox	ooxxxxxo	oxxxooxx

51.	52.	53.	54.	55.
ooxxxoxx	oxxxxoxo	oxxxoxxo	oxoxxxox	ooxxxxox
56.	57.	58.	59.	60.
ooxxoxxx	oxxxxoox	oxoxxoxx	oooooxxx	ooooxxxo
61.	62.	63.	64.	65.
oooxxxoo	ooxxxooo	oxxxoooo	ooxoxoxo	oxoxoxoo
66.	67.	68.	69.	70.
ooxxooxo	oxooxxoo	oxxooxoo	oxoooxxo	oxooooxx
71.	72.	73.	74.	75.
oxoxxooo	oxoxooox	oxxoooxo	oxoooxox	ooxooxxo
76.	77.	78.	79.	80.
ooxooxox	ooxoooxx	ooxoxoox	oooxooxx	oooxoxox
81.	82.	83.	84.	85.
oooxoxxo	oxxoooox	ooooxxox	ooxxooox	ooxxoxoo
86.	87.	88.	89.	90.
ooxoxxoo	oooxxoox	oxxoxooo	ooooxoxx	oxoxooxo
91.	92.	93.	94.	95.
oxooxoxo	oooxxoxo	oxooxoox	ooooxxxx	oooxxxxo
96.	97.	98.	99.	100.
ooxxxxoo	oxxxxooo	oxxoooxx	oxoxoxox	oxxoxoxo
101.	102.	103.	104.	105.
oxoxoxxo	ooxoxoxx	oxxoxxoo	oxxooxxo	oxxooxox
106.	107.	108.	109.	110.
ooxxooxx	oooxxoxx	ooxooxxx	ooxxoxxo	ooxxoxox
111.	112.	113.	114.	115.
oooxoxxx	oxxoxoox	oxoxxoxo	oxoxxxoo	oxxxooox
116.	117.	118.	199.	120.
oooxxxox	oxoooxxx	ooxxxoox	ooxxxoxo	ooxoxxox
121.	122.	123.	124.	125.
ooxoxxxo	oxxxoxoo	oxoxxoox	oxooxxox	oxxxooxo
126.	127.	128.		
oxooxoxx	oxoxooxx	oxooxxxo		

64

Which bring forth *Tails* at the second trial.

1.	2.	3.	4.	5.
xoxxxxxx	xooooooo	xooxxxxx	xoxxxxox	xoxxxoxx
6.	7.	8.	9.	10.
xoxoxxxx	xoxxoxxx	xoxxxxxo	xoooooox	xoooooxo
11.	12.	13.	14.	15.
xooooxoo	xoooxooo	xooxoooo	xoxooooo	xoooxxxx
16.	17.	18.	19.	20.
xoxoxoxx	xoxxooxx	xooxxoxx	xoxxxoox	xoxooxxx
21.	22.	23.	24.	25.
xoxoxxxo	xooxxxox	xoxxxoxo	xoxxxxoo	xooxxxxo

26.	27.	28.	29.	30.
xooxoxxx	xoxoxxox	xoxxoxox	xoxxoxxo	xoxoxooo
31.	32.	33.	34.	35.
xooxooxo	xooooxx	xooooxxo	xoxoooox	xooxooox
36.	37.	38.	39.	40.
xooxoxoo	xooxxooo	xoooxoxo	xoxxoooo	xoooxxoo
41.	42.	43.	44.	45.
xooooxox	xoooxoox	xoxooxoo	xoxoooxo	xooooxxx
46.	47.	48.	49.	50.
xoxoxoxo	xooxooxx	xooxxoox	xooxoxox	xoxoooxx
51.	52.	53.	54.	55.
xoxxooxo	xoxooxox	xoxoxoox	xooxxoxo	xoxxxooo
56.	57.	58.	59.	60.
xooxxxoo	xoooxxox	xoooxxxo	xoooxoxx	xoxooxxo
61.	62.	63.	64.	
xooxoxxo	xoxxoxoo	xoxoxxoo	xoxxooox	

32

Which bring forth *Tails* at the third trial.

1.	2.	3.	4.	5.
xxoxxxxx	xxooxxxx	xxoxxoxx	xxoxxxxo	xxoxxxox
6.	7.	8.	9.	10.
xxoxoxxx	xxooooo	xxoooxxx	xxoxoxox	xxoxxoox
11.	12.	13.	14.	15.
xxooxxox	xxoxxoxo	xxoxxxoo	xxoxoxxo	xxooxoxxx
16.	17.	18.	19.	20.
xxoxooxx	xxooxxxo	xxoxoooo	xxoooox	xxoooxoo
21.	22.	23.	24.	25.
xxooooxo	xxooxooo	xxooooxx	xxooxoxo	xxoxooxo
26.	27.	28.	29.	30.
xxoxoxoo	xxooxxoo	xxoxxooo	xxooxoox	xxoooxxo
31.				32.
xxoooxox				xxoxooox

16

Which bring forth *Tails* at the fourth trial.

1.	2.	3.	4.	5.
xxxoxxxx	xxxooxxx	xxxoxxxo	xxxxxxxx	xxxoxxox
6.	7.	8.	9.	10.
xxxoooxx	xxxoxxoo	xxxoxoxo	xxxoxoox	xxxooxxo
11.	12.	13.	14.	15.
xxxooxox	xxxooooo	xxxoooox	xxxooxoo	xxxoxooo
		16.		
		xxxoooxo		

8

Which bring forth *Tails* at the fifth trial.

1.	2.	3.	4.	5.
xxxxoxxx	xxxxooxx	xxxxoxxo	xxxxoxox	xxxxooox
	6.	7.	8.	
	xxxxooxo	xxxxoxoo	xxxxoooo	

4

Which bring forth *Tails* at the sixth trial.

1.	2.	3.
xxxxxoxx	xxxxxoox	xxxxxoxo
	4.	
	xxxxxooo	

2

Which bring it forth only at the seventh trial.

1.	2.
xxxxxxox	xxxxxxxo

Finally one which is brought forth only at the eighth trial,
and one which is not brought forth at all.

1.	2.
xxxxxxxo	xxxxxxxx

NEW OBSERVATIONS ON THESE TABLES

In considering these tables one could believe, that, if Pierre, instead of playing with Paul at *Heads* and *Tails*, would make a lottery of any number of tickets all winning except one alone; tickets, of which the half would have the value of one écu; the quarter that of two écus, the eighth that of four écus; the sixteenth that of eight écus etc. according however to Paul only the faculty of drawing a single one of them; that which would be the same as the one of the Problem of Petersburg; but it would be an error. These two cases are quite different. In the case of the Lottery, the stake to give by Paul varies in ratio with the number of tickets; while in the Problem of Petersburg it remains always the same less the fraction expressing the probability of the last trial; and the reason of this difference comes, from the difference of probability that exists between the events subject to a certain order, and those which are not at all subject to such an order; a difference, of which one can be convinced easily by the following example.

Suppose that I have to draw a ticket from a sack, in which there are 999 whites and one black, my probability to draw the black is $\frac{1}{1000}$. We suppose at present, that one substitutes to the one of the 999 white tickets which are in this sack, a red ticket; my general probability to draw the black will be such as it was before$= \frac{1}{1000}$. This probability would not vary more, if I put into a similar sack 499 white tickets, 500 red, and a single black; my probability to not draw at all a white ticket would be in truth quite different than in the preceding case, since my probability to draw a red ticket would be $\frac{1}{2}$; but my probability to draw the black ticket would not be altered, it would be always= $\frac{1}{1000}$; while, if, instead of leaving all these tickets together, I put a single red ticket with a single white ticket in a box, and 999 white tickets with a single black ticket in another box, and that it was permitted to me to draw a ticket from this last box, only in the case, that I had begun by drawing the white ticket that I have placed in the first box with a single red ticket, my probability to draw that one would be $\frac{1}{2}$; but my probability to draw the black ticket existing in the second box would be found less than half in the preceding case, and would be consequently= $\frac{1}{2000}$ and not= $\frac{1}{1000}$. If the red ticket was worth an écu, and the black ticket one thousand écus, I would have in this case to give for stake for the red ticket $\frac{1}{2}$ écu as in the case where there would be 500 red tickets in a similar sack with 499 whites and one black; but I would have to give for stake likewise for the black ticket, being worth one thousand écus, only one *half-écu*; while I would have to give for this ticket *one écu* in the other case where the tickets would be found all reunited in the same sack. The more an event, that it is necessary to cross, is probable, the less it is probable, that one will attain another from it, in which the success of this one give the exclusion.

It is thence that which distinguishes the case of the Problem of Petersburg, from the case of the lottery, and it is a new proof of the exactitude of my solutions.

One Combinations of successive effects is neither more nor less probable than a Combination of simultaneous events; but the effects, which are subject to a certain order, either of coexistence, or of succession contain in them more of different combinations than other effects less subject to a certain order.

If there were in the same sack an indefinite number of white tickets with as many black tickets as there are of whites, my probability to draw from it a white would be= $\frac{1}{2}$; but, if one said to me: you can draw from this sack as many tickets one after the other as you wish until you draw a black from it, by recasting each time into the sack the ticket that you will have drawn from it; but, as soon as you will have drawn a black, were this at the first trial, the game must be finished; and if this black ticket arrives at the first drawing, one will give to you one écu; if it arrives only at the second drawing, after having drawn first and recast into the sack a white ticket, one will give to you *two écus*; if this black ticket arrives only at the third drawing, you will have *four écus* and thus in sequence until the first black ticket presents itself; this case would be the same as the one of the Problem of Petersburg.

If Pierre and Paul played at *Heads and Tails* on eight trials, it would be also perfectly indifferent if they cast eight pieces of money into the air at the same time, or if they cast into the air the same piece eight times consecutively. Eight trials give always 256 combinations; there are always odds of 255 against the coming of each combination in particular, whether the casts are successive or that they are simultaneous.

Each of these 256 combinations is as probable as the other; but an event, which has two combinations for itself, being again one time more probable than an event which has for itself only one unique combination, it is evident, that the *repetition* of an event which has two combinations, *is as probable as it is probable* that an event which has only a single combination *took place one single time*. Being this, if Pierre played at *Heads and Tails* on eight trials with 256 persons at the same time, of which each would cast his piece of money into the air eight times consecutively, there would not be reason why the combination xxxxxxxo would not appear as well as each other in particular; but the event that *Tails* arrive on the first trial, having 128 combinations for itself, the probability that, *sometimes the one, sometimes the other of these combinations which bring forth Tails at the first trial, will be repeated*, is to the probability *that an event which has for itself only one unique combination will arrive a single time*, as 64 to 1. Being this, it is therefore probable, that it would be necessary to repeat the same game 64 times, that is to say to play 64 times with these 256 persons on eight trials, in order to be able to reckon that all the 256 combinations will appear *each one time*; now, this being it is manifest that, if Pierre played on eight trials with 256 persons at the same time in which each would cast his piece of money into the air eight times in sequence, and in which each would give to him, according to my solutions, *two écus less* $\frac{1}{256}$ for stake, he would play with them a fair game; Because, although he would receive from them much less, than there would be in the case to disburse if all the 256 possible combinations in eight trials were presented in the first game, this appearance of all the combinations is so improbable, and the frequent repetition of the events advantageous for him is of so great a probability, that at length, by reiterating the same game often, he would have neither loss nor gain on both sides. It suffices to consider the probability of the first three trials of the game of *Heads* and *Tails*, in order to be convinced with evidence that of this truth; because the probability of the arrival of *Tails* at the first trial being= $\frac{1}{2}$, while the probability of its arrival at the third trial is only= $\frac{1}{8}$, it is evident, that on *one time* that Pierre will disburse 4 écus, he will win *four times* one écu. There will be therefore neither loss nor gain on both sides at length. Because that which is true of the first three trials, it is equally of those of the following. (see my first solution.)

REFLECTIONS
AT
THE OCCASION OF MY SOLUTIONS.

I have sensed that Paul must give *two écus* for stake to Pierre a long time before having been able to demonstrate to me this truth perfectly to myself; and it is thus of nearly all speculative truths; their discovery is much less difficult and less painful than their perfect development. Whence does it come? *this comes* (see my treatise on the animal organization; in particular on the generation of the understanding and the notes O and P) *from this that* our organs operate, compare, and furnish the masses of ideas and sentiments without our intervention; or at least without a sufficient attention on our part, in order that we can perceive the elements, of which they make usage in order to form these results, these masses, which they give evidence in our memory. Now, the great difficulty it is to recover these elements; it is thence the art of the demonstration. One arrives there not at all without a profound meditation, and however, as much as we are not arrived at all; so much we ourselves have not at all perfectly developed in ourselves these results that we perceive in us; we can, neither communicate to the others the truths that we sense; nor to have ourselves a true certitude of it.

These masses of ideas and of sentiments, produce without the concurrence of our attention, that we perceive in us *non-developed*, are, that which one calls, according to the diversity of the cases, *sentiment or the instinct of the true*; *of the just*; *of the* *beautiful* etc. *taste*; *tact*; etc. sometimes *of the presentiments*. These are of the gifts of nature and of the circumstances; because we are not at all indebted for them to our will.

La Bruyere[10] has said true in saying, that a man of spirit has in himself the seed of all the truths; but it is far from the seed, to the development; to maturity and to harvest. These gifts are dangerous gifts as soon as one does not cultivate them at all with care; they mislead us with a great facility, and it is not astonishing since they are not at all formed in us *by reason*, but by circumstances. One understands nothing; one knows nothing well without meditation, without decomposition. And these men of the world, *who*, since they have perceived feebly all (perceived often purely borrowed and unformed in themselves) *believe* to be able to judge all and to decide on all; these people of letters, who, because they know how to write agreeably, *believe* to be scholarly; these supposed thinkers, who one encounter in our century in all the classes of society, are of men so detrimental to the progress of the sciences, that their existence is fatal to the happiness of the human race.

I will demonstrate with evidence in the first Memoir that I will publish: *That the great multiplication of men, who occupy themselves in the non-exact sciences, is one of the greatest obstacles to the progress of these sciences, and at the same time one of the principal causes of the perversity and of the extravagance of our century.*

End.

[10] *Translator's note*: Jean de La Bruyère (1645-1696) is known primarily for his popular work "Les Caractères de Théophraste, traduits du Grec, avec les caractères et les moeurs de ce siècle." in which he caricatured prominent Parisians and members of the French court.