EXTRACTS OF LETTERS ON THE CALCULUS OF PROBABILITIES, AND ON THE CALCULUS RELATED TO INOCULATION

JEAN D'ALEMBERT
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§ I. *On the calculus of probabilities*

1. You say, Sir, that the formula $\frac{px-qy}{p}$ of which I have spoken to you in a preceding letter (V. above p. 82) would not give the lot of the Player. I am persuaded, & I am likewise warned that I did not give it correctly; it is in fact easy to see that it is not it, because in making $y = 0$, one would find $z = x$, when likewise q would not be $= 0$, which would evidently be too much: because the wager z of the Player must not be equal to the sum x which he can win, if there are some tosses which must not procure any gain to him. Thus I have spoken of this formula only to show how it is easy to be mislead in this matter: because would it not be natural to think that after the game, the one who has already staked in the game the sum z, must give no more, in case that he loses, than the sum $y - z$; is this not true?

2. You say to me perhaps that I must, for the same reason distrust my principles on this matter; also I have proposed them not only as some doubts that I submit to the judgments of the Mathematicians, but to the truth of some able Mathematicians, who will be simultaneously Philosophers, & who will not think to have me refuted by repeating to me wrongly that which one finds in all the books on the analysis of games.

3. I will believe at any rate to be right regarding my principles also as good as the received principles, as much as one will not give, after these last principles, a clean & satisfactory solution of the very clear & very simple problem proposed in Book V of the Memoirs of Petersburg. I know up to the present five to six solutions as least of this problem, of which not one accords itself with the others, & of which none appear satisfactory to me; & I ask if this slight accord not mark the insufficiency & incorrectness of the principles of the analysis of the games?

4. I would desire also that one adhere to giving some neater ideas on that which one calls the *expectation* of the Players; to make well understand how one can give to the *incertitude* a precise & determined value by the calculus, a value which is a fraction of certitude, although *metaphysically* & rigorously speaking, certitude is, with respect to the simple probability, that which infinity is in relation to unity.

5. There are nearly thirty years since I have formed these doubts in reading the excellent book of M. Bernoulli *de Arte conjectandi*; it seems to me that this matter needs to be treated in a more clear manner; I well saw that the expectation was so much greater, 1° as the expected sum was greater; 2° as the probability of winning was also. But I did not see with the same evidence, & I do not see yet, 1° that the probability is estimated correctly by the usual methods; 2° that when it was, the expectation must be proportional to this

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Translated by Richard J. Pulskamp, Department of Mathematics and Computer Science, Xavier University, Cincinnati, OH .

simple probability, rather than to a power or even a function of this probability; 3° that when there are many combinations which give different advantages or different risks, (that one regards like the negative advantages) it fails be satisfied by simply *adding* together all the *expectations* in order to have the total *expectation*. Here is, Sir, what I would desire to see well clarified.

6. If anything was capable of strengthening my doubts, it would be the letters which I have received on this subject from many capable Mathematicians. One agrees that I have been right, in article XVII of my tenth memoir, to count only three possible tosses (instead of four as one ordinarily counts) in the game of *heads & tails* of which there is question in this place; he claims only that these three tosses are not equally possible, & that in the ratio of $\frac{1}{2}$ to $\frac{1}{4}$ & $\frac{1}{4}$. Another assures that one yields to me absolutely, since one confesses that there are only three possible tosses; & that in the terms where the question is found reduced, the question is no more than to count the combinations in order to see if one has all of them, & next to make the sum of those where *heads* faces in order to form the lot of the Player. This mathematician claims therefore that there are really here four cases, & not just three; & the reason that he brings to it, is that this would be the same thing according to him of playing to heads or tails on two tosses with one single piece; or of playing at a single toss with two pieces; here is that which I deny, because in this last case, the combination which would bring about *heads & heads*, must evidently enter into the line of count, because there are four combinations in all for two pieces cast at once; instead that in the first case, as soon as the piece is cast & as it brings about *heads*, it is likewise useless as ridiculous to cast it a second time; because that which must result from it, adds absolutely nothing to the lot of the Players, & it is also foreign to the game that if one of the Players, instead of casting the piece a second time, vanishes to Rome. It would be childish to say that one must count the second toss when heads has happened in the first, for the reason that one is agreed to play to *two* tosses is not to agree to play *two tosses*; for to agree to play *to two tosses*, whatever thing happens, because it would be illusory & ridiculous to play the second toss, if *heads* happens first. What astonishes me, is that of great Geometers, who one does not name, having been able to confound these two cases.

7. Also I am very remote to believe with the common Analysts, that it is the same thing to cast a piece into the air m times in sequence, or to cast m pieces altogether a single time. The examination of the two cases of which we just spoke, proves evidently the difference which can result as for the lot of the Players; & besides that does one know? It is perhaps more possible, physically speaking, to bring about at one time the same repeated event, than to bring it about successively; to bring about *heads* all at one time with ten pieces in a single cast, than to bring about successively with a single piece cast ten times; as it is perhaps more possible to cast at one time by a single toss ten pieces at the same height, than to cast successively ten times the same piece; in the first case, it is a single & same cause which acts at one time in order to produce m outcomes; in the second, it is a repeated cause which acts successively in order to produce m successive outcomes. Now it is perhaps more possible, all the rest remaining equal besides, that the outcomes would be similar in the first case as in the second; by reason that in the first case it is a unique cause which produces them, & that in the second it is a repeated cause, which by this circumstance even is able to vary further. I know well that in *Mathematics* one sets aside, & with reason, all these physically possible differences; & it is also for this that the two cases are considered as being the same *mathematically*; but in the calculus of combinations applied to the *physical* events, the concern is to distinguish what is physically possible from what is not, perhaps even what is more from what is less; & this is an attention that one

has not paid enough to the present in the analysis of games. Another consideration which is able to serve to show that the two cases in question, are not physically similar, it is that it is very possible & even easy to produce the same event in one single toss as many times as one would wish; & that on the contrary it is very difficult to produce it many times successively, & perhaps impossible, if the number of tosses is very great. If I have 200 pieces in the hand, & if I cast them into the air at one time, it is certain that one of the two tosses *heads* or *tails* will be found at least one hundred times or more in the cast pieces; instead if one cast a piece successively into the air one hundred times, one could play perhaps all eternity before producing *heads* or *tails* one hundred times in sequence. In this here is, I believe, more than it is necessary to indicate that one has had very great harm to regard the two cases of which there is question, as being perfectly & physically the same.

8. Here is another reflection which could show how it is easy to be mistaken in the assumption that one has that all the cases given by the combinations are equally possible. In the game of *heads & tails* in two tosses of which we just spoke in article 6 above, (according to § XVIII of the tenth Memoir) it is certain & on the greatest evidence that one must count only three tosses, *heads, tails & heads, tails & tails;* because in fact there are only these three tosses which decide the event of the game. Now if one says that the three cases are not equally possible (by whatever reason that this can be) therefore, I will conclude, from this that the cases *heads, tails & heads, tails & tails* are here the cases (& the only three) which *are able to happen*, it does not follow that *they are all able to happen equally*. Now if one takes care, the implied reasoning which one makes according to the combinations in the calculus of probabilities returns to this: "Here are all the combinations mathematically possible; each of these combinations mark a case which *can happen*; therefore each of these cases are able to happen as the other; therefore all the cases *are able to happen equally*." Moreover (& I have already said in the tenth memoir, § XXVI) if the three cases *heads, tails & heads, tails & tails*, they alone which are able to happen in the proposed game, are not equally possible, it is not, it seems to me, by the reason that one brings to it commonly, that the probability of the first is $\frac{1}{2}$, & that of the two others $\frac{1}{2} \times \frac{1}{2}$ or $\frac{1}{4}$. The more I think, the more it appears to me that *mathematically* speaking, these three tosses are equally possible, by the reason that *heads* or *tails* happening on the second toss, suppose that tails has necessarily happened on the first, in a manner that the second case *tails & heads*, and that the third case *tails & tails* form each only a single individual case & as a single toss, as unique, as indivisible as the first case *heads*, & consequently also possible; in a way that one could likewise no longer speak of *tails* arriving on the first toss, because this case entails necessarily a second, & say: "There will happen one of these three things, either *heads* on the first toss, or *heads* on the second toss,or *tails* on the second toss. Now there is no reason in order that one of these three cases happen rather than the other; therefore they are equally possible, mathematically speaking." If they are not equally possible, physically speaking, it is perhaps as I have said, because *tails* arriving twice in sequence, is perhaps a little less possible than *tails* & *heads* arriving successively. I know not if I mislead myself; but you saw a little while ago that some able Mathematicians are not at all in accord on the manner of counting the cases in the game in question, & that in uniting in them that which they both accord to me, I could gain the day.

9. There are some times that a Player asked of me in how many consecutive tosses one could wager with advantage to bring about a given face on a die, which I will call a, the others being supposed b, c, d, e, f. My response to this question, very simple & very easy to resolve, is that, according to the rules of probabilities, if one played to n tosses, the

probability was of $6^n - 5^n$ against 5^n; so that that one could wager with advantage when $6^n > 2 \times 5^n$, that is to say, when $n = 4$. This Player responded to me that experience had appeared to him contrary to this result, & that in playing four tosses in sequence in order to bring a given face a, there happened to him much more often to win than to lose. Suppose it is true, perhaps one could conclude from it that the little accord of experience with the calculus, comes from this that the calculus is founded on the false assumption of which we have already spoken, articles 6 & 7. For example, if one plays in two tosses, there are, according to this calculus, eleven cases to bring about the face a; instead of which there are really only six, namely a on the first toss, after which the game ceases; or else on the first toss there follows necessarily to a second, ba, ca, da, ea, fa. According to this principle, the number of cases which will bring about the face a will be $1 + 5 + 5^2 + \cdots + 5^{n-1}$, if one plays to n tosses, & this number $= \frac{5^n - 1}{4}$. It is true that this number is always smaller (moreover by the half) that the half $\frac{6^n}{2}$ of the total number of cases, & that thus it would appear to ensue from this calculus that one could never wager with advantage on bringing about the face a in any number of tosses as one will wish, which is certainly false. But my response to this objection is that I have already made to a similar objection in my tenth memoir, art. XXVI, pages 23 & 24 of Volume II of my Opuscules; namely that it is not necessary to regard as also possible *physically* as the others, the cases where the same event will be found repeated a certain number of times in sequence. It is true that the proportion of the probabilities will be in this case very difficult, & perhaps impossible to fix; but perhaps this is the nature of the question which is supposed; & perhaps this is a thing as difficult as desirable, that a theory of probabilities which would be founded on some simple & bright principles, & which would be at the same time perfectly conformed to experience.

10. The objection what I have made to M. Bernoulli (page 89) after his theory of the inclination of the orbits of the Planets, is rather an argument, as one says, *ad hominem*, than a direct proof that I have wanted to bring in favor of my opinion; because I agree moreover that the reasoning of M. Bernoulli is little solid; the odds are certainly an infinity against one that the Planets must not be found in the same plane; this is not a reason to conclude from it that this arrangement, if it held, could have necessarily from another cause than chance; because there could be like odds of infinity against one that the Planets could not have a certain arrangement determined at will; however one could not conclude from it that this arrangement could not be the work of chance. This is why if there is a place to believe the arrangement of the Planets would have physical cause, it is uniquely because in the order of nature, (such at least as it is known to us) every uniformity announces a physical cause. It is likewise for this reason that I have claimed that the same event cannot happen, physically speaking, a great number of times in sequence, so much that one will suppose the things abandoned to chance. In fact, if it is physically also possible that the same event happen a great number of times in sequence, as it is that different events succeed themselves; why, since the world exists, has the first of these cases, as possible as any other taken in particular, never happened?

11. But when I would have erred on this point, (that which seems difficult to me to prove) it would not be less certain that M. Bernoulli must (in his principles) be absolutely of my counsel; in fact, I desire that one show me the difference of these two reasoning here:

There are odds nearly 1500000 against one, that the Planets, if they had been cast at random, could not be found in a zone as narrow as that which they occupy.

Therefore this arrangement of the Planets is not the result of chance. This is the reasoning of M. Bernoulli.

Here is that which I make according to him:

There are odds nearly 1500000 against one, that heads will not happen in such number of times in sequence;

Therefore if heads happens in fact such number of times in sequence, this will not be the result of chance.

Therefore if one holds to pure chance, one must not introduce into the line of count the combination which would make heads happen this number of times in sequence.

The parity of the following two reasonings will be perhaps yet more striking.

There are odds an infinity against one that the Planets being cast at random, would not be moving in a same plane.

Therefore if the Planets moved in a same plane, it would be impossible (because it would be infinity against one) that this arrangement be the result of chance; this is the reasoning of M. Bernoulli.

Here is now the parallel reasoning.

There are odds an infinity against one that the same toss *heads* or *tails* will not happen an infinity of times in sequence, if one abandons the tosses to chance.

Therefore if one holds to pure chance, it is impossible that *heads* or *tails* happen an infinity of times in sequence.

Therefore one must suppose in the analysis of the games that *heads* will happen in the end after *tails*, or *tails* after *heads*.

12. These reasonings are absolutely the same; I avow again one time that they are not concluded, & that the reason for which one must exclude *heads* arriving an infinity or likewise a great number of times in sequence, this is not for the sake of little mathematical probability (because each of the other tosses in particular is not more probable mathematically, & however it is quite necessary that one of these cases happens); it is because the same event never happens in nature a very great number of times in sequence.

13. We would have, I believe, more light on this subject, if we had more understanding of nature, or even just more of observed facts. In order to make sense by a striking example, it is certain that each man, taken in particular, can live sixty years & beyond; therefore, mathematically speaking, one can suppose that one hundred persons born at one time will each live sixty years & beyond; because there is no reason why each of these persons, taken in particular, will die before that age; thus this conclusion would appear to us evident, if experience had taught us only it is not true, & that, physically speaking, one hundred persons born together would not live sixty years each. Thus in the things where experience enlightens us, we well exclude some combinations which without the light which it gives us, would appear perfectly correct to us. Who will assure us that it would not be the same if we were more enlightened on the possible?

14. In order to render this more sensible, I wish to make again two parallel reasonings as in article 11 above.

Let as many men as one will wish a, b, c, d, &c. who one supposes born at one time; it is certain that a can live one hundred years, that b can live also one hundred years, & likewise c, &c. Therefore one will conclude, a, b, c, d, &c taken together, could live each one hundred years.

This conclusion is denied by experience; it is perfectly sensible to that here:

If one casts a piece in the air a thousand times in sequence, heads can happen on the first toss, it can happen on the second, on the third; &c. therefore it can happen successively on the first, on the second, on the third, on the fourth, &c. Now I say that this second conclusion, perfectly sensible in the preceding, could well be all too false; & that the experience which denies formally the first, renders at least the second very suspect.

15. Also a very profound & very able Analyst, who has preference to examine my reasoning on these matters, as to judge them lightly, wrote to me in his own words: "I think as you that the calculus of probabilities, of which one has made so many applications, has need to be taken back into its principles; because if on the one hand it supposes, for example, that a very long sequence of *heads* is as possible as a mixed sequence of heads & of tails according to a given law & of the same length, it supposes tacitly on the other hand that these different sequences happen in the end the one after the other; now it seems to me that these two principles are not absolutely concordant; the second seems to be sufficiently in accord with that which occurs in nature; but this is perhaps a reason in order that the first does not conform." This reasoning, which seems to me as solid as ingenious, could, being deepened & developed, furnish new proofs in favor of my sentiment. Another Mathematician of the greatest reputation & the highest merit, after having said to me that my reflections on inoculation, imprinted in the fifth volume of my Mêlanges de Philosophie, "are full of very fine & very correct views & reflections which were overlooked by all those who had already treated this matter, & which renders it entirely new & interesting", adding, "in regard to your difficulties on the calculus of probabilities, I agree that there are some things quite specious which merit the attention of the Philosophers." Another very enlightened Writer, who has cultivated Mathematics with success, & who is known by an excellent Work on Philosophy, wrote to me on the subject of Book V of my *Mélanges*: "that which you say on probability is excellent & very evident; the old calculus of probabilities is ruined[1] & evidently faulty; & one can only restore it anew if when one will have discovered some laws in these variations of nature; but is this possible?" These authorities, who value well, I believe, the decisions which one raises as an objection to me, or which one could raise as an objection to me, were joined to the reasons which I have brought in favor of my opinions, prove, it seems to me, that it is at least worthy to be examined by some profound Mathematicians & Philosophers, but not by those who will believe it only to be. I have found nothing, no more than you, in the little known brochure of which you teach me the existence, I have only seen that the Author does not understand how $(1+a)^p$ becomes $\sqrt{1+a}$ when $p = \frac{1}{2}$, & how $\sqrt{1+a}$ is $< 1 + \frac{a}{2}$. One can judge from there the rest. The same Author informs me further (in order to prove to me that I know this only because it is the mean life) that if one hundred persons taken together & born at the same time, live 27 years taking one thing with the other, there will be about 44 of them who will live taking one thing with the other 60 years, because $\frac{2700}{60} = 44$ about; whence there results this curious consequence, that if one hundred persons live 27 years taking one thing with the other, 200 persons will live only the half of 27 years; because $\frac{2700}{200} = \frac{27}{2}$; or this which is yet more marvelous, that *of the one hundred* persons who live 27 years, taking the one with the other, *there will be* 200 who will live (taking the one with the other) only the half.

16. We leave this, & we return again a moment to the similitude of a great number of successive events. One knows that the duration of three successive generations is about one hundred years, and that each is of very nearly 32 years. If one supposed one hundred persons from father to sons, who taken together must live 3200 years, & who one supposed in some calculus of combination that each of the one hundred persons lived exactly 32

[1] I do not demand so much, by a great deal; I do not claim to *ruin* the calculus of probabilities, I desire only that it be clarified & modified.

years, I ask if all those to whom one would present this calculation would not reject, as contrary to experience, the assumption on which it is based, although in this assumption the one hundred persons taken together did not live longer than the law of nature permits it. Now here is precisely the case of heads or tails arriving one hundred times in sequence or further. In a word, all show us that in the order of things, the same events never happen a very great number of times in sequence, & very rarely even a small number of times. I refer you on this subject to that which I have said in my tenth Memoir, p. 10, on the case of 2^{100} Players who cast each one hundred times in sequence a similar piece into the air; & I say that one can wager without any risk that any of these Players will bring forth neither heads nor tails one hundred times in sequence.

17. Again a word on the problem of Petersburg. You say, Sir, that the reason for which one finds the wager infinite, is the tacit assumption that one makes that the game is able to have an infinite duration, this which is not admissible, note that the life of men endure only a time. But what of responses to make to this objection? 1° We suppose two men, or if you wish two beings who must live eternally, the objection will no longer take place, & the solution will not be worth more than before. 2° When one finds the wager infinite, this signifies only that some sum that one of the Players gave in advance to the other, in order to offset the risk that this latter courts, he will never give enough; now this is here that which I claim absurd, since in supposing that the game could endure only twenty years, & that one plays a toss per second, one of the Players must give an exorbitant sum. 3° We suppose that the number of tosses be fixed, for example, to one hundred thousand; the wager will be 50000 écus following the admitted rule, & however those who have proposed this problem, agree that one would be insane to give just 20 écus. This is therefore not the infinity (supposed possible) of the duration of the game, which renders here the result absurd, but the assumption alone that one of two tosses happen constantly a very great number of times in sequence. 4° Instead of supposing that one of the Players must give to the other an écu on the first toss, two on the second, four on the third, &c. all the other conditions being besides absolutely the same, we suppose that he must give only an écu on each toss; one will find that then the wager must be $\frac{1}{2}+\frac{1}{4}$, &c. to infinity $= 1$ écu; although one supposes, as in the first case, that the duration of the game could be infinite. It is not therefore the duration of the game supposed infinite which renders the wager infinite; since in the case of which we just spoke, the wager is only finite & even less considerable.

18. But this last case can furnish against me one objection which a person has made to me, & which can nevertheless appear very strong. One could say: "In the preceding case the calculation gives 1 for the sum that one of the Players must give to the other before the game; & this result is in fact conformed to reason: because since the Player who must give this wager, will receive infallibly from the other an écu, neither more nor less, something which happens, it is clear that in order to render the condition of the two Players equal, he must give an écu to the other. Now this is that which must not be according to your manner of evaluating the probabilities: because the probability that one of the two tosses will happen a very great number of times in sequence, being null, according to you, the series $\frac{1}{2}+\frac{1}{4}+\frac{1}{8}$, &c. will lead, after a certain finite number of terms, to absolute zero, & consequently the product of 1 by this series will be < 1." I respond that this objection, without me making change of sentiment on the physical impossibility of a great number of similar events among them, an impossibility that I believe incontestable by experience, renders another rule of the calculus of probabilities alone very suspect to me, of which I have already described (art. 5, n°. 3) & which consists in adding the *partial expectations* in order to have the *total expectation*. In fact, supposing that the game, instead of being of

an infinite duration, is fixed at 1000 tosses, one finds, according to the rule in question, that the wager must be $1 \times (\frac{1}{2} + \frac{1}{4} \ldots + \frac{1}{2^{1000}}) : 1 \times \left(\frac{1}{4} - \frac{1}{2^{1002}}\right)$, this which is a little less than an écu; now I say that this wager is too feeble, & that as it is impossible according to me, physically speaking, that one of the two tosses happen constantly 1000 times in sequence, it is impossible, physically speaking, that the Player who must give the wager, not win an écu; that consequently it is an écu which he must give; that consequently if one supposes, as I make it, the sequence of probabilities $\frac{1}{2}$, $\frac{1}{4+\alpha}$, $\frac{1}{8+\zeta}$, &c. the wager must be greater than the sum of the partial expectations $1 \times \frac{1}{2} + 1 \times \frac{1}{4+\alpha} + 1 \times \frac{1}{8+\zeta}$, &c. If two Players would play thus during all their life, in recommencing the game after one thousand tosses, & if to each new game, the one who must give the wager, gives a little less than one écu according to the ordinary rule, I am quite sure that he would win at this game here, & I do not doubt that all men more reasonable than argumentative are not in this of my advice. Instead if two Players play one écu at heads & tails in a single toss & in many matches in sequence, & if for each toss one of the two Players gives, as he must, a half-écu to the other, one can assure with certitude that none of the two Players will enrich himself in this game here. In this last case (of heads or tails in a single toss) there is no person, who when the wager is given, does not bet indifferently on one of the two Players; in the case of *heads* or *tails* in a thousand tosses, there is no person who does not bet for the one who has given the *wager* of a little less than one écu. However it would be necessary, in order that the rules of the analysis of the games were good without exception, that in all cases, when the game is fixed & given between two Players, a third Player occurring, can bet indifferently for one or for the other.

19. Besides when one supposes (in the objection of article 17) that the game can never finish, & that one finds by the ordinary rules an écu for the wager in this case here, it is easy for all those who were formed of the clear ideas of the sum of the infinite series, to see that this sum (an écu) which one finds by the calculus, is not truly the wager, but the *limit* of the wager, that is to say, that the wager will be too great if it is an écu, & too little if it is less; too great, because it can happen, mathematically speaking, that the game never ends, & that also the Player never gives the écu that he has promised; too little, because the number of the tosses being indefinite, the wager must be greater than if the game were fixed to a number of tosses as great as one would wish. Now I say that it is absurd to claim that the wager can never be too great in this game here, & of it to claim by the metaphysical & ideal reason, that the game can never end; there is no Player who wavers, in the case in question, to give his écu; & who will not be quite sure to regain it in very short time.

20. You will ask of me perhaps how it can be that the total expectation be greater than the sum of the partial expectations. I will respond to you that this will appear to you less a paradox, when you will reflect that one has not yet attached of a clear idea to that which one understands by *expectation*, & to greater reason by *partial expectation* & by *total expectation* (See above, art. 5.) I see well that a Player has accordingly more expectation to win as he has more of cases for him, & the expectation to win in *total* a sum accordingly greater than each of these cases must be to win further; but I do not see clearly how this expectation is evaluated with precision by the ordinary method, & I will never agree that the *expectation* to win an écu with the probability $\frac{1}{2}$, the *certitude* to win $\frac{1}{2}$ écu with the probability 1 (probability which is equivalent to absolute certitude) & the *expectation* to win one thousand écus, with probability $\frac{1}{200000}$, are equals among themselves, in a way that a Player to whom one of these *lots* will be overdue can give to him for one of the two others indifferently; because it is again a thing that one supposes in the analysis of the game, & according to me a very false thing, that two lots that one finds equal by the

calculus, are able to be changed one for the other; the falsity of this assumption, falsity that I believe evident, is perhaps, as well as the reflection which is found at the end of article 17, one of the strongest objections that one is able to make against the received rules of the games.

21. It seems to me that in this analysis one falls into two errors; 1° one combines together some entirely foreign things the one to the other; one confounds the *expectation* which depends uniquely on the probability of the number of the favorable tosses, with the *expected sum* which is totally independent of this probability, & which surrenders indeed the greater gain, but not the greater *expectation*. If there is probability $\frac{1}{2}$ that I will win an écu, & probability $\frac{1}{200000}$ that I will win one hundred thousand; the *expectation* is the greater in the first case, & the *expected sum* in the second; & there is confounded, it seems to me, all the ideas, so to say, as there result from the rules of the analysis of the games, as in these two cases the lot is equal. 2° One compares moreover in this analysis some disparate & incommensurate things, the certitude with the probability. Let z be the sum which must be put into the game the one of the Players who has the advantage, because he has probability $\frac{1}{m}$ of winning the sum x; one makes $z = \frac{x}{m}$; this equation supposes tacitly, or rather there results from it, that for one of the Players, for the one who has set the wager z, the *certitude* of losing the sum z is equal *to the probability* of winning the sum x, & that for the other the certitude of winning this sum z is equal to the *probability* of losing the sum x; whence one concludes that the lot of the two Players has, by this means, become equal. Now I deny that the certitude of losing an écu is equal to the probability $\frac{1}{1000}$ of winning 1000 écus; I deny also that the two Players, of whom the one will have probability $\frac{1}{100}$ of winning 1000 écus, & the other the probability $\frac{99}{100}$ of losing $\frac{1000}{99}$, have an equal lot. However that one takes care, there is here this which one supposes tacitly in the result of the calculus of the games of chance; because here is implicitly the reasoning that one makes: "Let p be the number of tosses which make win the sum x, $p+q$ the total number of tosses, & z the wager; the certitude for the first Player to lose his wager z, is equal to the probability $\frac{p}{p+q}$ of winning the sum x; & the lot of this Player, which is the probability p of winning the sum $z - x$, is equal to the lot of the other Player who has a probability q of winning the sum z." Now here are two assertions that I deny for all the reasons reported above. And it is again for these reasons that I have said besides, that it was not surprising that there can remain uncertainty in the principles of a calculus where one is proposed to appreciate the same uncertainty, in the comparing to the certainty what is incommensurable to it, & by claiming to modify this uncertainty by the value of the expected sum.

22. I must not forget, Sir, of making to you an observation on the subject of the word *Constantinopolitanensibus*, which one will find written on a table with the Imperial characters, & on the reasoning that I have made on this subject in Book V of my *Mélanges de Philosophie*, page 293 & following. It is certain that every person who would find this word written in this way, would hold as certain that this would not be the result of chance, that he must be certain of the existence of the City of Rome; & however he would not form this judgment, that because there is found by chance a language in which these twenty-five characters so arranged form a sense; if there is no language in the world in which *Constantinopolitanensibus* is a word, one would not hesitate a moment to attribute this arrangement to chance. There is more: if the word written had very few letters, as *amor*, one would be much less assured that this arrangement is not due to chance; & if it had only two, as *et*, one would be assured nothing more absolutely. Therefore since we would be assured so steadily that the word *Constantinopolitanensibus* written on a table is the work

of an intelligent cause, although this word forms a sense only for an arbitrary & accidental institution; how much must we be further carried to be assured that one event which happens one hundred thousand times in sequence is not the result of chance? Experience proves to us, as much as is possible, the variety & not resemblance in the successive events of Nature: & when one would wish not to surrender himself to this proof, one will agree at least that all which one sees, must lead us to believe that this variety in Nature a type of law, & to doubt if the similitude of the successive events is not contrary to the general & unknown combinations which result from the constitution of the universe; consequently, & in remaining even in the abstract & the mathematical possibility, there is surely more to wager, according to experience, for the possibility of variety, than for that of similitude; & it will result at least, that in drawing from the same experience some purely mathematical conclusions, the variety of successive events is more possible than their similitude; or, in order to express with greater precision, that the odds are greater for the first than for the second.

23. Here is enough of it, Sir, for you to engage to think on this question. You suppose easily, in bringing together the one from the other all my reasons of doubt, to the order that I could have put there, & which would have again augmented the force of them. I am not surprised that the vulgar of the Mathematicians, accustomed to reject all this which comes out the common ideas, are little disposed to adopt that which I propose; I imagine even that they may seem foreign to some very great Geometers, especially to those who are held to the ordinary principles, they have given to us, according to me, only erroneous solutions to the problem of Petersburg. But I hope therefore that my doubts will enlist some clever folk without prejudice to delve into this prickly matter, & to give to it the degree of evidence of which it is able to be susceptible.

24. In order to summarize in a word all my doubts on the calculus of probabilities, & to put them before the eyes of the true Judges; here is that which I grant & that which I deny in the explicit or implicit reasonings on which this calculus appears to me founded.

First reasoning. The number of the combinations which bring about such a case, is to the number of the combinations which bring about such other case, as p is to q. I agree to this truth which is purely mathematics; therefore, one concludes, the probability of the first case is to that of the second as p is to q. Here is that which I deny, or at least of which I strongly doubt; & I believe that if, for example, $p = q$, & if in the second case the same event is found a very great number of times in sequence, it will be less probable *physically* than the first, although the mathematical probabilities be equal.

Second reasoning. The probability $\frac{1}{m}$ is to the probability $\frac{1}{n}$ as np écus is to mp écus. I agree to it; therefore $\frac{1}{m} \times mp$ écus$=$ $\frac{1}{n} \times np$ écus; I agree to it again; therefore the *expectation*, or that which is the same thing, the *lot* of a Player who will have the probability $\frac{1}{m}$ to win mp écus, will be equal to the expectation, to the lot of a Player who will have the probability $\frac{1}{n}$ to win np écus. Here is that which I deny; I say that the *expectation* is greater for the one who has the greatest probability, although the expected sum be less, & that one must not balance to prefer the lot of a Player who has the probability $\frac{1}{2}$ to win 1000 écus, to the lot of a Player who has the probability $\frac{1}{2000}$ to win 1000000.

Third reasoning which is only implicit. Let $p + q$ be the total number of cases, p the probability of a certain number of cases, q the probability of the others; the probability of each will be to the total certitude, as p & q are to $p + q$. Here is that which I deny again; I agree, or rather I grant, that the probabilities of each case are as p & q; I agree that there will happen certainly & infallibly one of the cases of which the number is $p + q$; but I deny that of the ratio of the probabilities between them, one can conclude from it their ratio to the absolute certitude, because the absolute certitude is infinite in ratio to the greatest probability.

Do you ask of me perhaps what are the principles which are necessary, according to me, to substitute for those of which I revoke by doubting the correctness? My response will be that which I have already made; I make nothing of it, & I am likewise quite led to believe that the matter in question, is not able to be subject, at least in many regards, to a correct & precise calculus, equally clean in its principles & in its results.