REFLECTIONS ON THE CALCULUS OF PROBABILITIES

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I.

The ordinary rule of *the analysis of games of chance*, is this one: *multiply the gain or the loss that each event must produce, by the probability that that event must happen; add together all these products, by regarding the losses as negative gains; & you will have the expectation of the player, or, that which amounts to the same, the sum that this player ought to give before the game, in order to begin to play from start to finish.* No analysis, that I know, has hitherto called this rule into question, & all are conformed to it in the calculations that they have made of different probabilities. There are found nevertheless some cases where it appears to be at fault, & which make the material of some reflections.

II.

The first case is the one of which there is made mention in Book V of the Memoirs of the Academy of Petersburg. Pierre plays with Jacques at *heads* or *tails*, on this condition, that if Pierre brings about *heads* at the first toss, Jacques will give to him an écu; if he brings about *heads* only on the second toss, two écus; if on the third toss, four écus; if on the fourth, eight écus, & thus in sequence by geometric progression; one asks the expectation of Pierre, or that which he must give to Jacques in order to play with him in an equal game.

According to the ordinary rules, the probability that *heads* will happen on the first toss, is $\frac{1}{2}$, on the second $\frac{1}{4}$, on the third $\frac{1}{8}$, &c. & thus in sequence; therefore conforming to the rule above, the expectation or the stake of Pierre will be

$$1 \times \frac{1}{2} + 2 \times \frac{1}{4} + 4 \times \frac{1}{8} + \text{\&c.} = \frac{1}{2} + \frac{1}{2} + \frac{1}{2} \text{ \&c. to infinity} = \infty;$$

that is to say, that Pierre must give to Jacques before beginning the game, an infinite sum, in order to play with him in an equal game. Now, independently of this that an *infinite sum* is a chimera, there is no person who would wish to give it in order to play in this game, I say no infinite sum, nor even a sufficiently modest sum. The rule appears therefore to be in error, at least for this case.

III.

The first idea which presents itself in order to justify it; is to say, that if the expectation where the stake of Pierre is found infinite, it is because one supposes tacitly that the game must or is able to endure an infinite time; that is to say, that *heads* in able to happen only after an infinite number of casts. Now, one will say, this assumption is absurd; because it will be very necessary that *heads* happen in the end after a *finite* number of casts, as great as one will wish. The proposed game therefore neither must endure always, nor could it even be able, & consequently the expectation of Pierre is only finite.

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IV.

To this I respond first that one supposes gratuitously that *heads* must happen *necessarily* after a finite number of tosses; because it is in the order of possible things (such as the ordinary analysis of games of chance consider them) that *tails* happen in all the tosses, & that consequently *heads* never happens. The analysis of the games of chance (once such again as all the Mathematicians have followed up to the present) suppose that all the combinations are equally possible, each in particular. If one plays, for example, to 60 tosses, instead of playing to an indefinite number of tosses; the number of possible combinations is 2^{60}, & on these combinations there is one which will never bring about *heads*; but this combination is regarded by the Analysts, as being as possible, as any of the other combinations taken in particular. It is therefore possible (at least in following the principles adopted until the present by the Analysts) that *heads* never happen; & consequently one must not object to the preceding calculation, neither this assumption, nor the necessary consequence which results from it, namely an infinite sum for the expectation or stake of Pierre; or else, if one attacks this assumption, it will be necessary to reform, in many other regards, the analysis of probabilities; this is that which we will discuss below.

V.

In second place, I well wish to suppose that *heads* will happen in the end *necessarily* after a finite number of tosses; it is at least evident that one would not be able to fix this number of tosses, that it is *indeterminate* or *indefinite*; whence I conclude two things; 1° that any finite sum which one assigns to the expectation or the stake of Pierre, this sum may be below the one which one must really give to Jacques. Suppose, for example, that one assigns thirty écus for the expectation of Pierre; one will have supposed therefore that *heads* must happen *necessarily* in sixty tosses; this is absurd. Because it is evident (§ preceding) that by being limited to consider that which is rigorously possible, *tails* can happen sixty times in sequence; & besides why would heads happen *necessarily* in sixty tosses, rather than in fifty-nine or in sixty-one? It will be the same in every other assumption that one may make. 2° If one says that the sum which indicates the expectation of Pierre, is *finite & indeterminate*, one serves only to elude the question; because it is evident that one may suppose two players who play together under the proposed conditions; it is evident moreover that Pierre must have in his game a great advantage, & it is the question to know how to estimate this unknown advantage; because it is evident further that this advantage is not infinite, although the calculus seems to give greater than any finite advantage. Here is therefore a case, very possible in the games of chance, where the rule is at fault; therefore this rule is not general.

VI.

In third place, I suppose that one plays to a finite number of tosses, for example, to one hundred tosses; one will find that Pierre must give fifty écus to Jacques. Now there is no player at all who wished to give this sum in similar cases; because it would be necessary, in order to make up again this sum by playing, that *heads* come only in the seventh toss; & assuredly Pierre would believe too risky to wait that this case happen.

VII.

A celebrated Geometer of the Academy of Sciences, full of knowledge & sagacity, with whom I reasoned one day on this question, gave me a solution of it which seems first satisfactory, & which is very simple, although very ingenious. "One must not suppose, he says to me, that the number of tosses be infinite, nor even indeterminate; because Jacques, as rich as one supposes him, has no infinite sum of money to give to Pierre; he has, &

is able to have only a certain finite quantity of money. Suppose him rich to 2^{99} écus, an exorbitant sum, & which passes the credible; it is evident that he may play beyond one hundred tosses; & that thus the expectation or stake of Pierre is fifty écus. Here is what Pierre must give to Jacques in order to play with Jacques in a fair game: & in general if the wealth of Jacques is 2^x, or between 2^x & 2^{x+1}, he is never able to have more than $x+1$ possible tosses, & the expectation or stake of Pierre will be $\frac{1+x}{2}$ écus." Such is the solution imagined by this wise geometer.

VIII.

But the remark made in § VI, indicates, it seems to me, the insufficiency of this solution, so ingenious & so simple as it is. Because in the proposed case, where the wealth of Jacques is supposed 2^{99} écus, & where one plays to one hundred tosses, it is very certain that Pierre would believe to risk much beyond that which he must, by giving fifty écus to Jacques. Why this? It is, as we have said, that it would be necessary, in order that Pierre make up with his wager & beyond, that *heads* happen only on the seventh toss; which, according to the ordinary rules of the calculus of combinations, there are odds of 127 against one that *heads* will rather happen, in which case Pierre will lose his wager in part or in total; & that a probability of 127 against one is so small, that one must not risk a sum of money (even sufficiently average) relative to this probability, when likewise the gain which could result from it would be immense. Here is the proof of it. Let one propose to some man that he may win ten million in a lottery of 128 tickets, where there is only this single lot of ten million; his expectation & his stake consequently, that which he must give in order to play at par (according to the ordinary rules of probability) must be $\frac{10000000}{128} = 78125$. However what would make the man insane enough to risk this sum?

IX.

Does one say that this sum is not able to be risked for this single reason, that being so great, it would be a very considerable breach in the wealth of the Player? But 1°, it follows at least there, that however great the expected sum be (which is here ten million) the wager must not be always proportional to it, all the rest besides being equal; & that also it would have at least in this regard some modifications to give to the rule, until the present admitted by all the Analysts, that the wager must be proportional to the sum that one expects. 2° We suppose that instead of six million, the lot or the expected sum be only 128 écus, it will be necessary that the player give an écu for his wager; & although one of 128 tickets must be brought out from the wheel, & that this ticket may be absolutely the one which carries the lot, there is no person who in this case would not regard his wager as lost money, by the great risk which it incurs. It is true, that if the player in not too poor, this loss will inconvenience him little; but in the end this is always a loss; & in the analysis of the games of chance, one considers the loss or the game in an absolute manner, & independently of the fortune of the Players.

X.

What concludes from these reflections? It is that *when the probability of an event is very small, it must be regarded & treated as null; & that it is not necessary to multiply* (as one prescribes until the present) *this probability by the expected gain, in order to have the stake or the expectation.* For example, let Pierre play with Jacques to 100 tosses, on this condition that if Pierre brings heads on the one hundredth toss, & not before, he will receive from Jacques 2^{100} écus: one finds (by following the ordinary rule) that Pierre must give an écu to Jacques before the game. Now I say that Pierre must not give this écu; because he will *certainly* lose it, & that *heads* will happen *certainly* before the one hundredth toss, although it must not happen *necessarily*.

XI.

In order to confirm what I just said, I suppose that one casts a piece in the air one hundred times in sequence: it is certain; 1° that the number of the possible combinations is 2^{100}, that is to say, that there are 2^{100} different possible combinations of the wau by which *heads & tails* are able to happen, when one tosses the piece into the air one hundred times in sequence; that which makes altogether $2^{100} \times 100$ tosses. 2° That if consequently one tosses the piece into the air $2^{100} \times 100$ times in sequence, that is to say, if one recommences the game 2^{100} times, there will happen 2^{100} combinations of *heads & tails* taken in one hundred consecutive tosses. 3° That consequently each of the 2^{100} events will be found one time, or some many times, among the 2^{100} combinations that *heads* or *tails* must produce in this case. Now I say that one is able to wager without any fear, that of these 2^{100} combinations, those which will bring about *heads* one hundred times in sequence, or *tails* one hundred times in sequence, will not happen one single time in the 2^{100} that one has (*hyp.*) restarted the game, by casting in each game the piece into the air one hundred times in sequence; consequently some one or several of the combinations, where *heads & tails* are found mixed, will happen necessarily many times in these 2^{100} times. I add that the combinations which will happen most frequently, will be those where *heads & tails* will be found most mixed, that is to say, where *heads & tails* will not be found a great number of times in sequence; whence it follows, it seems to me, that one must regard the combinations where *heads & tails* are found mixed, as the most probable & the most *possible* of all. In order to render this yet more sensible, I suppose that 2^{100} Players cast at the same time an écu into the air, one hundred times in sequence; I say that in any of these casts, one will have one hundred times in sequence neither *heads* nor *tails*, & that consequently there will be many casts which will give the same thing; & that the casts where *heads & tails* are intermingled, without finding themselves a great number of times in sequence, will be those which will be repeated.

XII.

What it is necessary to distinguish between is that which is *metaphysically* possible, & that which is *physically* possible. In the first class are all the things of which the existence has nothing of absurdity; in the second are all those of which the existence not only has nothing of absurdity, but similarly nothing too extraordinary, & which are not in the daily course of events. It is *metaphysically* possible, that one brings a rafle[1] of six with two dice, one hundred times in sequence; but this is impossible *physically*, because this has never happened, & will never happen. In the ordinary course of nature, the same event (whatever it be) happens rarely enough twice in sequence, more rarely three & four times, & never one hundred times consecutively; & there is no person who with all certainty is not able to wager all his wealth, as great as it be, that a rafle of six will never happen one hundred times in sequence.

XIII.

One is able therefore, it seems to me, to put for the rule, that when the probability is very small, one must in the ordinary usage of life, regard it as zero, & treat it as such. Now for this one must ask the following questions.

[1]To throw a rafle of six with two dice is to throw a pair of sixes. The probability of doing this is $1/36$.

1. What is the term where the probability begins to be able to be regarded as null? What is the fraction which expresses the first term of this sequence of probabilities equivalent to zero?

2. Suppose that one can fix this term, & let this be, for example, when the probability is $\frac{1}{1000}$, how will it be necessary to estimate the probabilities which differ very little from this, although a little greater, for example, the probabilities $\frac{1}{999}$, $\frac{1}{998}$, &c? If it is not necessary to regard these probabilities as smaller than they are in fact, I ask how the probability $\frac{1}{999}$ becomes all in one stroke$= 0$ in the case where it is $\frac{1}{1000}$? Is the expression of the probability able to be passed thus brusquely & without gradation, from a finite expression to a null value? And if it is necessary to regard these probabilities as smaller than they be, I ask according to what law it is necessary to diminish them? If the Analyst responds that he ignores it, in this case it must be acceptable that the general rule of probabilities is faulty and imperfect; this which we wished to prove.

3. If it is necessary to diminish these probabilities $\frac{1}{999}$, $\frac{1}{998}$, $\frac{1}{997}$, $\frac{1}{996}$ &c. which form a kind of series, up to what term will it be necessary to diminish them? If it is necessary to diminish them only up to a certain term, why is it necessary to stop at that term there? If it is necessary to diminish all the terms, even those which contain some fractions great enough, as $\frac{1}{4}$, $\frac{1}{3}$, &c. for then the rule of the probabilities will be found faulty & imperfect, even in the case where the probability will not be very small.

XIV.

Here is more that is necessary, is seems to me, in order to indicate to the Mathematicians that the general rule of the calculus of probabilities is defective in certain regards. I try to show again by other examples. But before I will propose an idea which has come to me, in order to estimate in the preceding cases the ratio of the probabilities.

I suppose, for example, that one casts a piece into the air four times in sequence; one will have 2^4 or 16 different combinations of *heads & tails* taken four by four. If therefore one restarts this game a number of times which is a multiple of 16, or, that which amounts to the same, if 32 or 64 &c different Players play simultaneously this game, each in particular, each of them casting an écu into the air four times in sequence, it is evident that some one or some ones of the 16 combinations will be found repeated. Now I believe that the combinations which are repeated most rarely, & which perhaps will not happen at all in a great number of tosses, will be those in which *heads* is found four times in sequence, or *tails* four times in sequence. After this experience, repeated a great number of times in sequence, one could perhaps estimate the ratio of the probabilities, by the number of the events. It is true that the result may leave some doubts; & that moreover the experiment could be impractical, if the number of casts, instead of being four, as so one has supposed, was very much greater, as one hundred; but here is, it seems to me, the only way to arrive in this case to a result which is at least approaching truth.

XV.

We come to other examples which I have promised in the preceding article, of little correctness of the ordinary calculus of probabilities.

In this calculation, by combining all the possible events, one makes two assumptions which are able, it seems to me, to be contested.

The first of these assumptions is, that if a like event has already happened several times in sequence, for example, if in the game of *heads & tails*, *heads* has happened three times in sequence, is it equally probable that *heads* or *tails* will happen in the fourth toss? Now I demand if this assumption is quite true, & if the number of times that *heads* has already happened in sequence by hypothesis, does it not render more probable the arrival of *tails* in the next toss? Because in the end it is not probable, it is even *physically* impossible that *tails* never happen, therefore the more heads will happen in consecutive times, the more it is probable that tails must happen the following toss. If it is this, as it appears to me that one would not recover by disowning, the rule of the combinations of the possible events is therefore yet defective in this regard.

XVI.

One other assumption which the Analysts ordinarily make, & which has relation to the preceding, is that in the number of possible combinations, that which will bring about the same event many times in sequence, is as possible as each of the others in particular. For example, in a game where one must play *heads* or *tails* in one hundred tosses, one regards the combination which will bring about *heads* one hundred times in sequence, as also possible as each of those where *heads & tails* will be mixed. Now I demand if this assumption is quite correct; since it is *physically* certain (§ X & XI) that *heads* will never happen one hundred times in sequence, & that it is not that a combination where *heads & tails* would be mixed at will, will not happen. One can reduce this to the following question. If A represents *heads* & B *tails*, the combinations $AAAAAAAA$ &c. must it be regarded as possible as any other particular combination, for example $AABABABB$ &c. where *heads & tails* are mixed without order & without sequence? This is what I do not believe, for the reason that I have already said above; knowing, that the variety of successive events is a constant phenomenon of nature; & that their constant similitude or repeated a great number of times, is to the contrary a phenomenon which does not happen.

XVII.

Now if one does not regard all the combinations as equally possible; if one must reject, or at least subordinate to the others, those which could bring about the same event a great number of times in sequence, what rule must one make for this subject? Must one extend this restriction to the combinations which could bring about the same event a small number of times in sequence, for example, three or four times? And if one must not extend up to these combinations, which is it where it will be necessary to begin? Here are, it seems to me, some very worthy questions to exercise the Mathematicians, supposing nonetheless that it is possible to solve them.

XVIII.

Another inconvenience where one falls in the calculus of probabilities. I have already remarked in the *Encyclopedia*, under the word *CROIX* ou *PILE*, that in this calculus one makes often a faulty enumeration of the possible events. For example, one asks how much one can wager to bring about heads in two tosses? "All the possible combinations, one responds, are these here:

First toss	Second toss
Heads	*Heads*
Heads	*Tails*
Tails	*Heads*
Tails	*Tails*

"Now of these four combinations the last alone makes a loss, & the three others make a win; the probability is therefore three against one."

It is easy to see that this enumeration is faulty. Because as soon as *heads* will happen on the first toss, the game is ended, one will not play a second; & thus the first two combinations *heads heads, heads tails*, are reduced to *heads* alone. There are only therefore three possible tosses;

First toss	Second toss
Heads	
Tails	*Heads*
Tails	*Tails*

Whence I have concluded at the place cited, that the probability was only two against one, & not of three against one. I will examine more below if I have been right to reduce the probability to the ratio of two to one; but it is at least very certain that the manner by which one proves that it is three to one, is a paralogism.

XIX.

The paralogism is yet greater, if one wagers to bring about *heads*, not in two tosses, but in one hundred tosses in sequence. Because in this case, by following the ordinary reasoning, one supposes that the combination which would bring about *heads* one hundred times in sequence, is as possible as any of the others in particular. Now this assumption (§ XVI) is at least very susceptible to dispute. It is therefore at least demonstrated, that this manner to resolve the Problem is uncertain, & perhaps faulty.

XX.

I know that one may envisage the choice in another manner, & make the following reasoning. "The probability that *heads* will happen on the first toss is $\frac{1}{2}$, the probability that *tails* will happen on the first toss, is likewise $\frac{1}{2}$; or in this second case, the probability that *heads* will happen in the second toss is $\frac{1}{2} \times \frac{1}{2}$, & the one that *tails* will happen in the second is $\frac{1}{2} \times \frac{1}{2}$; thus the sum of the favorable probabilities, is to that of the unfavorable probabilities, as $\frac{1}{2} + \frac{1}{2 \cdot 2}$ is to $\frac{1}{2 \cdot 2}$, or as 3 to 1. Therefore the probability is always as three to one, even by considering only the three really possible tosses; namely, *heads* on the first toss; *tails & heads* on the first & on the second toss; or else *tails & tails* on the first & on the second toss."

XXI.

I respond in the first place, that I know not if one must estimate by $\frac{1}{2 \cdot 2}$ or $\frac{1}{4}$, the probability that one will bring about *tails* or *heads* on the second toss. I agree that it is uncertain if one will play a second toss or not; & that the probability that one will play this second toss is $\frac{1}{2}$: but the probability that one will bring about *tails* or *heads* on the second toss, supposes *necessarily* that one will play this second toss; thus, to multiply the probability $\frac{1}{2}$ of bringing about *heads* or *tails* on the second toss (by supposing that one plays this second toss), by the *probability* $\frac{1}{2}$ that one will play this second toss, does it not regard all at once this second toss as having taken place before, & as being nonetheless simply probable? This seems to me to imply a contradiction. Without difficulty $\frac{1}{2}$ is the probability of bringing about *heads* in any toss, by supposing that one plays this toss; but if it is uncertain that one plays this toss, if the probability that one will play it, is $\frac{1}{2}$, then to multiply the first probability $\frac{1}{2}$ by the second $\frac{1}{2}$ is it not to multiply the one by the other two probabilities of different nature, a probability (namely the first) which always remains $= \frac{1}{2}$, & a probability (namely the second) which does not remain always $\frac{1}{2}$, but which becomes *certitude* as soon as one multiplies by the first? In fact the probability $\frac{1}{2}$ of bringing about *heads* or *tails*, supposes necessarily that one will play the toss; thus the combination of this probability with the second changes that in nature, & supposes it *certain*, from simply *probable* as it was before?

XXII.

I respond in the second place, that this manner of estimating the probabilities, is subject to all the difficulties of which we have spoken at the beginning of this Memoir. Because we suppose that one plays, for example, for one hundred tosses; the probability that *heads* happens only on the one hundredth toss, would be following this method $\frac{1}{2^{99}}$; this which supposes that the probability that *tails* will happen 99 times in sequence, is $\frac{1}{2^{99}}$. Now I demand if it is physically possible that *tails* happen 99 times in sequence; & if consequently one must not ($\S$ XII) regard the probability $\frac{1}{2^{99}}$ as equal to zero? If this is, it will follow; 1° that the rule is faulty, as least in the case where one plays a great number of tosses in sequence; 2° that it is at least very uncertain in the others, since there is no reason, for example, to not diminish the probability $\frac{1}{8}$ or $\frac{1}{16}$ of some smaller part, if the probability $\frac{1}{2^{99}}$ must be regarded as null.

XXIII.

I come now to the difficulties that one can make on the method that we have given Art. XVIII, to determine the ratio of the probabilities in the case where one plays at *heads* or *tails* in two tosses. One agrees first (see the *Encyclopedia* at the word *GAGEURE*) that the three tosses

Heads
Tails Heads
Tails Tails

are in truth the only possibilities; but one pretends that they are not equally; "because, one says, the probability of bringing about *heads* on the first toss is equal to that of bringing about *tails* on the first toss. Now the probability of bringing about *tails* on the first toss, is double of that of bringing about *tails* on the first toss & *heads* on the second, or *tails* on the first toss & *tails* on the second. Therefore &c."

In order to develop in what consists, according to me, the vice of this reasoning, I will borrow the language of the Logicians, & I will say that in this argument the *mean term* is not the same in the two Propositions. Because the mean term in the first Proposition, is the probability of bringing about *tails* on the first toss, *before having played this first toss*. In the second Proposition, the mean term is the probability of bringing about *tails* on the first toss, *compared to the probability of bringing about heads* or *tails on the second toss*. Now this last probability (that of bringing about *heads* or *tails* on the second toss) supposes that the first toss is played, & that it has given *tails*; thus this last probability supposes that the first probability (that of bringing about *tails* on the first toss) is no longer a *probability*, but a *certitude*. The *mean term* is therefore really different in the two Propositions. In a word there is this difference between the toss *heads* & the toss *tails*, arriving one or the other in the first toss, that the toss *heads* is not brought about on the second toss, instead that the toss *tails* brings about necessarily another; thus it is not necessary to compare first the probability of *heads* on the first toss, with that of *tails* on the same first toss, & next the probability of *tails* on the first toss, with the probability of *heads* or *tails* on the second toss; but the probability of *heads* on the first toss, with that of *tails* & *heads* on the first & second toss, or of *tails* & *tails* on the same first & second tosses.

XXIV.

I would not wish however to regard in all rigor the three tosses in question, as equally possible. Because 1° it could be in fact (& I am even carried to believe it), that the case *tails heads* was not exactly as possible as the case *heads* alone; but the ratio of the possibilities seems to me inappreciable. 2° It could be made further that the toss *tails heads* was slightly more possible than *tails tails*, by this reason alone that in the last the same result happens twice in sequence; but the ratio of the possibilities (suppose that they are unequal), is not easier to establish in this second case, than in the first. Thus it could very well be that in the proposed case, the ratio of the probabilities was neither 3 to 1, nor 2 to 1 (as we have supposed in the *Encyclopedia*) but one incommensurable or inestimable, mean between these two numbers. I believe now that this incommensurable will approach nearer to 2 than to 3, because again once there are only three possible cases, & not four. I believe likewise & by the same reasons, that in the case where one would play three tosses, the ratio of 3 to 1, what my method gives, is more near the truth; than the ratio 7 to 1, given by the ordinary method, & which seems to me exorbitant.

In order to fix well the state of the question, we take to the case of it where one plays to two tosses. It is first certain that the probability of bringing about *heads* on a first toss, is equal to that of bringing *tails* on the same first toss; the difficulty is reduced to namely; 1° what is the ratio of the probability of bringing about *tails* on the first toss, to the probability of bringing about *heads* on the second toss, when one will have brought *tails* on the first, & when consequently *there must be* a second toss; 2° if the probability of bringing about *tails* on the second toss, when one will have brought about *tails* on the first toss, is equal or slightly smaller than that of bringing *heads* on the second toss, when one will have brought about *tails* on the first toss; & if these probabilities are not equal, what is the ratio?

XXV.

When one plays more than two or three tosses, then the ratio of the possibilities or probabilities must yet be infinitely more difficult to determine. It is evident in fact that if one plays on four tosses, for example, it is more probable that one will bring about *heads* on the first toss, than *tails, tails, tails, tails* on four consecutive tosses. Now the ratio of these possibilities is again, according to me, inappreciable, although these possibilities are really different. I say more: it can be that *tails, tails, tails, heads* is more possible (§ XV) than *tails* 4 times in sequence: now how to compare these probabilities? How to assign their ratio?

XXVI.

It is by this consideration by the different possibility of these cases (when the number of casts is ever so little considerable) that I wish to respond to an objection which has been made to me, & which one can see in the Article *GAGEURE* of the Encyclopedia. There would follow, one says, an absurdity in my manner of computing the probabilities; namely, if one never could wager with advantage, on bringing about one of the faces A, of a die with three faces A, B, C, in as many tosses as one would wish. For let n be this number of tosses, one would find always that the probability is of $2^n - 1$ against 2^n.

For example, if $n = 3$, one will find that these favorable combinations are A, BA, CA, BBA, BCA, CCA, CBA; & that the unfavorable combinations are BBB, BBC, BCB, BCC, CBB, CBC, CCC, CCB; which gives the ratio of 7 to 8, or of $2^3 - 1$ to 2^3. This objection supposes that all the cases are equally possible in the enumeration made in my manner; now they are not; because A on the first toss is more possible, for example, than B four times in sequence. It is true that I believe difficult to assign the ratio to it, & that the ordinary theory of the Analysts on this object appears to me less satisfactory; but it suffices, in order to respond to the objection, that all the cases are not equally possible.

XXVII.

We conclude from all these reflections; 1 that if the rule that I have given in the *Encyclopedia* (for lack of knowing a better for it) in order to determine the ratio of probabilities in a game of *heads* & *tails*, is not rigorously correct, the ordinary rule to determine this ratio, is yet less; 2° that in order to attain a satisfactory theory of the calculus of probabilities, it would be necessary to resolve many Problems which are perhaps insoluble; namely, to assign the true ratio of the probabilities in the cases which are not equally possible, or which are able to not be regarded as such; to determine when the probability must be regarded as null; to fix next how one must estimate the expectation or the stake, according as the probability is more or less great.

XXVIII.

I speak not here of the considerations relative to the state & to the fortune of the players; & to the fortune of the players; essential considerations without doubt to make, but which would demand nearly as many rules as of particular cases. It is after these considerations that one has tried to resolve in Tome V of the Memoirs of Petersburg, the question proposed above Art. II. The views that one proposes on this, are fine & ingenious. But there were perhaps other more simple reflections to make on this question, more relative to the question taken in itself, & more independent of the state of the players; & these are, it seems to me, those which we have made in the beginning of this Memoir, & which had been born of our doubts on the calculus of probabilities. These doubts had seemed to me worthy of being proposed to the Mathematical Philosophers. I have every place to believe that there will be as frightened of them as myself, if they examine them without prejudice.

End of the tenth memoir.