Sur les principes
de la
THÉORIE DES GAINS FORTUITS[*]

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FIRST MEMOIR[1]

A theory being a sequence of propositions relative to one same subject & each proposition having a thesis & a hypothesis; the particular hypotheses in a theory must be those which determine the nature of the subject of which it is occupied.

Each subject being determined by a small number of general attributes which one could understand in one alone, the number of hypotheses particular to each theory must be reducible to a small number of general hypotheses or even to a single one.

Independently of the particular hypotheses of each theory, there is what is common to many of them. And there is what is common to all of them.

The *common notions* so-called axioms, all reducible to the distinction of Being & of Nothing, are the supposed hypotheses, or recognized formally in every theory.

The diverse propositions respecting the continuous & discrete quantity, or the theories of Geometry & of universal Arithmetic, are some hypotheses common to each of the exact sciences.

The principles of a theory are in general the enumeration of the hypotheses so much general as particular from which one is departed in order to found it.

The common notions are ordinarily implications & not expressed; it is often likewise of the hypotheses common to many theories; but it is the rule to enunciate formally the hypotheses particular to each theory, & it is thence that in a more restricted sense one has custom to call the *principles* of this theory. It is thus that I will employ this word.

If a theory would exist of which the principles were not enunciated formally; we would not have more certain means to discover them than the *analytic* method.

[1]This memoir offers an extension of the remark which terminates the preceding, & this reason determines to place it here, although its date is 11 April 1782.

I intend that way that which consists to decompose the consequences in order to recover the principles; that is to say to rise from the particular theses to the general hypotheses.

The last consequences offer some more divergent results & a more facile combination. And as in a consequent theory the principles are employed in the first propositions, the last analyzed consequences must recall to the first propositions where are found the elements which one seeks.

Such is the plan that I myself have traced relative to the theory of estimation of the accidental gain.

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The art of calculating the accidental events is not a century & a half old. Pascal & Wallis[2] appear to have traced the first rudiments. Huygens is, I believe, the first who has put the principles in his treatise on *la manière de raisonner aux jeux de hazard*.[3] The *Art de conjecturer*[4] of J. Bernoulli, of which this treatise of Huygens is part, appeared only after the *Essai d'analyse sur les jeux de hazard* by Montmort; but the posthumous work of J. Bernoulli was known by some extracts & must be envisioned as the first body of doctrine undertaken on this subject. The *Doctrine des hazards*[5] of Moivre, published in part in some detached dissertations, was finally collected & forms an accomplished theory. The later Geometers have generally worked on the same principles & have applied their methods. A Memoir of Mr. de la Place[6] is the sole example of them that I will cite, wishing to indicate here only the authors of whom he will make mention in this Memoir & who have served me as guides. This Geometer expresses himself by speaking of the equations in the finite differences. "The illustrious Mr. de la Grange is the first who has envisaged them under this reason, . . .this theory . . .is of the greatest usage in the science of probabilities."

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Since its origin the principles of this science were contested. The correspondence of Pascal & of Fermat proved it. The *art de conjecturer* resolves a difficulty noted by Pascal. The work of Montmort presents various of them. That which there is of the singular it is that this Geometer seems sometimes to ascribe them in the analysis; while the analysis (joining that this word is synonymous with algebra) is only a sequence of rigorous consequences of which one does not contest the premises. But it is chiefly the correspondence of Montmort & of Nic. Bernoulli (printed at the end of the work of the first) which offers some objects of prickly controversy. It is thence between them that one finds proposed the equivalent of this problem become famous under the name of

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[2] Wallis just as Pascal have posed the rules of combinations which are the foundation of this calculus. See Moivre *Miscell. Analyt.* L. VII. C.3.

[3] But one must not regard Huygens as the inventor of this calculus, which, as he himself observed, was already in use among the French geometers. This is that which the author of the *Discours sur la vie & les écrits de Pascal* has remarked with justice. p. 52.

Translator's note: The actual title is *De Ratiociniis in Ludo Aleae*.

[4] *Translator's note:* The actual title is *Ars Conjectandi*.

[5] *Translator's note:* The actual title is *Doctrine of Chances*.

[6] *Savans Étrangers*. T. VI. p. 631.

Problême de Pétersbourg since the Memoir of Dan. Bernoulli & Cramer inserted into those of the Academy of Petersburg (T.V.). The work of Moivre does not prevent all the difficulties. Mr. d'Alembert in T.II. of his *Opuscules Mathématiques* raised *doubts* on the principles of the calculus of Probabilities. Dan. Bernoulli responded to these doubts (*Mém. de Paris* 1760. p.28.). And in T. IV. of *Opusc. Mathém.* Mr. d'Alembert replied. Mr. Beguelin occupied himself with these doubts & in particular of the Problem of Petersburg in a Memoir inserted into those of the Academy of Berlin (year 1767.). A prize proposed some years before (in 1751.) by the Class of speculative philosophy on *the accidental events* was envisaged by the concurrences of which the pieces have been published only as a point of morals to which the calculus is not applicable. The Article *croix & pile* of the Encyclopedia gave place to Mr. d'Alembert to say a word on the uncertainty of the principles through which one estimates the accidental gains. Mr. Necker made on the subject some observations which found place in the article *Gageure*. Mr. de Buffon in his *Arithmetique morale* has seemed to think as Mr. d'Alembert in diverse regards. Quite recently Mr. d'Alembert has inserted in T.VII of his *Opusc. Mathém.* a Memoir in which he renews the same doubts & forms new objections against the solidity of the received principles.

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Here is that which I know touching the difficulties raised against the calculus of chances. I have thought that they had their source in the negligence with which one has determined the hypotheses of this calculus. I have therefore researched these hypotheses & this is that which is the object of this Memoir which I present with defiance & of which I am going to determine the object. It is uniquely Logic & not at all Geometry. I do not intend that way to renounce by the light & by the precision of mathematics; but even to the claim of adding nothing to this science.

My division is this one: I. I seek analytically the hypotheses on which one is founded in order to estimate accidental gains. And the *Art de conjecturer* of J. Bernoulli is the work to which I attach myself for this. II. Next I discuss the principles of each Author in particular. III. I examine until what point the analyzed hypotheses agree with that which is. IV. I apply these hypotheses to the solution of difficulties proposed against this calculus.

I believe that this is the route which it is necessary to follow in order to spread the light on a matter so interesting. And I would wish that some philosopher capable to create it had undertaken on this plan, this which I will execute without doubt in a too imperfect manner. One will not be offended, I hope, to see me discuss the reasonings of the greatest geometers without regard to their celebrity so well merited. The research of the truth is the only homage which one owes to the genius.

SECTION I$^{\text{st}}$
Research on the hypotheses.

§ 1. The first Problem of the *Art de conjecturer* has for object to determine the probability of events by experience. The solution of this Problem leads the Author to this consequence.

that if one would continue during eternity the observations of all the events (the probability is changing then to certitude) one would find that all things arrive in the Universe by some certain reasons & by virtue of a constant law of vicissitude; so that, even in the casual & accidental things, we are forced to admit a kind of necessity &, so to speak, of fatality.

But the Author offering in this work no observation on the nature of things, this truth can be only hypothetical.

Now all the Propositions of this work to this last Problem inclusively are some necessary consequences of Prop. III. P.I.[7]

Therefore Prop. III. P.I. contains the hypothesis that the Author enunciates here as consequence.

I must hasten myself to warn the Reader that I will justify this assertion in the 3rd Section of this Memoir, by analyzing the Problem of which there is concern. And I must say also that the consequence which I just cited is alleged by the Author with an expression of doubt which renders my conclusion less daring.

§ 2. Prop. III. P.I. offers a single hypothesis formally enunciated, as to all the subsequent Propositions, namely:
That all the chances are equally possible.

I abandon here my analysis in order to give some definitions. I pray that one receive them as arbitraries. And I hope that the rest of these reflections will prove that they are not it.

§ 3. A *chance* is an effect which is not actually proved by the testimony of sense.

Therefore it is a future or past effect, or if it is actual it is outside of the range of sense.

Of the equally possible effects are those which are produced by some equally efficient causes.

Causes are *simultaneous* or *successive*. All that which I will say of them under one of these relations will be able to be understood of the other by substituting the idea of space with that of time.

If one can assign no finite or infinite time during which m causes have produced each of the same number of effects, these causes will not be called equally efficient.

Therefore of the *equally possible* effects are those of which one can affirm that there exists a finite or infinite time t any whatever during which these effects are produced each the same number of times.

We suppose m causes & that the time t is the one which is necessary for m productive acts, if in the time t each of the m causes must necessarily produce an effect, these m causes & their effects will be so-called *equally necessary*.

§ 4. Here I resume my analysis & I apply myself to define these words *equally possible* by the usage that my Author makes of it.

I see therefore that the six casts of a die of six faces are so-called equally possible, when it has a perfectly cubical figure. (*Art. conj.* P.I. p. 20.)

§ 5. When a die has a perfectly cubical figure & when in general one has destroyed all the interior causes which could be able to determine a face in a sense with

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[7]This Proposition III. P.I. *Art. conj.* is enunciated in § 19. of this Section.

preference to the other faces, there is only the exterior causes which can produce this determination.

§ 6. If I anticipated these successive determinations with a full certitude, by supposing that each face falls an equal number of times in a given time & that each brings to me a determined gain, the method that Prop. III. P.I. *Art. conj.* indicates would give the mean gain of a single cast.

And in order to serve myself with the same method by setting aside as much as possible of the time or of the space, that is to say by reducing the fractions which express my expectation, it would be necessary that the time t was the one which is necessary in order to make six casts of a die.

Therefore Prop. III. P.I. *Art. conj.* supposes that the gains which six equally possible chances bring forth must be estimated by the mean gain of these six chances supposed equally necessary.

§ 7.   I$^{\text{st}}$ HYPOTHESIS.

Let m be the lucrative chances,[8] *we suppose that one of them must take place in a designated period, that these chances are mutually exclusive; let I perceive no cause which must determine one of these chances rather than the other. My expectation is equal to the mean gain of these m chances supposed equally necessary.*

I will call henceforth *qualified chances* those which will have the conditions enunciated in this hypothesis.

§ 8. If one examines slightly this assertion one will object to me that quite far to suppose equally necessary the six chances of a cubical die, to the contrary one puts in fact that if one plays six casts, the same face can fall six times.

My response is that when one plays more than one cast there is also more than six qualified chances. For example, a man plays with a cubical die two casts; if he brings forth the point *six* he wins an écu; if not nothing.

The two casts offer 36 qualified chances out of which I reason as if they were equally necessary & I find the mean gain or his expectation $= \frac{11}{36}$; all as if this man had bought all 36 tickets of a Lottery of which 25 blanks & 11 lots of an écu.

It follows from this remark that one can, in the principles of the calculus, make no reasoning on a Game by a trial without supposing at least two Games made; nor in two trials without supposing four Games &c.

§ 9. There are two kinds of games of chance. Some as the greater part of the Lotteries are such that all the possible chances take place necessarily; so that a man who would play a single time out of all the possible chances would be completely certain to make all the gains. The others, such as dice, Lotto, heads-tails &c. are in the contrary case. Since one estimates the expectation in these games here as in the preceding, one departs from a similar hypothesis.

§ 10. The calculation by which one estimates the accidental gains is absolutely the same as the *rule of alligation*, as J. Bernoulli observes. It follows thence that one

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[8]More or less.

supposes acquired all the gains of the qualified chances, that one mixes these gains mentally, & that one divided as many of the portions as one has conceived chances. I have said enough to confirm this first hypothesis.

I continue my analysis & after having discussed the hypothesis enunciated in Prop. III. P.I. *Art. conj.* I seek in this same acknowledged fundamental Proposition & in its Corollaries if it contains not at all some tacit & general hypothesis.

§ 11. Corollary 4. Prop. III.P.I. *Art. conj.* is this proposition;

If I have p chances to win a; q for b; r for x; the unknown x designating the expectation in this same game; one will find $x = \frac{pa+qb}{p+q}$.

This which signifies that all the chances which restore the same chance that I incur must be counted null in the calculation of my expectation.

This Corollary is employed in Prop. XIV.P.I. *Art. conj.* & in Probl. I. II. V. of the Appendix of this Part.

This explication & these citations have for end to prevent an equivocal which could be born in the comparison of this Corollary with Probl. LVI of the *Doctrine des hazards* of Moivre. It suffices to observe that x designates here the value of a chance & not the stake of a player. I will make at the end of this Memoir an observation relative to this distinction.

§ 12. If $q = 0$ (§ 11.); $x = \frac{pa+qb}{p+q} = a$. One would have been able to deduce immediately this Corollary of the principal Proposition. $x = \frac{pa+rx}{p+r}$. Therefore $x = a$.

§ 13. *Example* I. Pierre & Paul play at heads-tails with the condition that if Pierre brings forth heads, Paul will pay him an écu; if Pierre brings forth tails, the players will recommence to play with the same conditions. One demands the expectation of Pierre?

Let x be this expectation. Pierre has one chance to win an écu, one chance for x. Therefore $x = 1$ (§ 12.) Indeed $\frac{1}{2} + \frac{1}{4} + \frac{1}{8} \cdots + \frac{1}{2^\infty} = 1$.

Example II. Pierre & Paul play at dice with n similar dice of m faces, marked as ordinary according to the order of the natural numbers. The conditions of the game are that if Pierre brings forth rafle of the point b, Paul will give to him an écu; if Pierre brings forth any other point, the game will recommence under the same conditions. One demands the expectation of Pierre?

(The letters n, m, b will express some numbers whatever with the sole restriction that $b < m$.)

Let x be this expectation. Pierre has one chance to win an écu; $m^n - 1$ for x. Therefore $x = 1$ (§ 12.).

The infinite convergent sequence $\frac{1}{m^n} + \frac{m^n-1}{m^{2n}} + \frac{\overline{m^n-1}^2}{m^{3n}} \cdots + \frac{\overline{m^n-1}^\infty}{m^{n(\infty+1)}} = 1$, will have given the same result.

Example III. The denominations remaining the same as in the preceding Example, Pierre & Paul play with these same dice with the conditions that if Pierre brings forth rafle with the point b he will withdraw the stake which is one écu; if he brings forth rafle with the point c (c designating any number $< m$) Paul will withdraw the écu; if there comes any other point, the game will recommence under the same conditions. One demands the expectation of each of them, or that which each must pay to the other in order to withdraw it.

Let x be the expectation of Pierre. There is one chance to win an écu; one chance for *zero* & $m^n - 2$ chances for x.

Therefore $x = \frac{1}{2}$ (§ 11.)

Likewise the expectation of Paul $= \frac{1}{2}$.

One will have found likewise by summing the sequence $\frac{1}{m^n}$, $\frac{m^n-2}{m^{2n}}$ &c. of which the exponent is $\frac{m^n-2}{m^n}$ & the sum $= \frac{1}{2}$.

Remark Ist. In this 3rd Example the formula $\frac{pa+qb}{p+q}$ of § 11. is become $\frac{pa}{p+q}$, which is the case of Corollary I. Prop. III. *Art. conj.*

Remark 2nd. One can observer out of this 3rd Example that it is necessary to pay as much to play in the game which is enunciated as in order to play in the ordinary game of heads-tails.

§ 14. The solutions of the three Problems proposed in the preceding § can be true only by admitting outside the Ist hypothesis (§ 7.) a second which is here.

§ 15.   2nd HYPOTHESIS.

The value of a sum actually possessed is equal to the value of this same sum in future possession & which falls only at an indefinitely extended term.

§ 16. But these three solutions (§ 13.) are only three cases of Corollary 4. formally enunciated by J. Bernoulli. And this corollary is itself a consequence of the Prop. III. P.I. *Art. conj.*

Therefore Prop. III. P.I. *Art. conj.* supposes tacitly the hypothesis that I just enunciated.

§ 17. I join here an observation already made.

It follows from the Prop. III. P.I. *Art. conj.* that the chance at heads-tails between *zero* & m millions equivalent to $\frac{m}{2}$ millions.

§ 18.   3rd HYPOTHESIS.

The value of the money is exactly proportioned to its numerical quantity.

§ 19. Prop. III. P.I. *Art. conj.* extends according to the spirit of the note is this here:

If I have p chances to win a; q for b; r for c; s for d; &c. my expectation is $= \frac{pa+qb+rc+sd \,\&c.}{p+q+r+s \,\&c.}$

§ 20. Prop. I & II which precedes Prop. III. P.I. *Art. conj.* (§ *prec.*) are only some particular cases.

This Prop. III. P.I. *Art. conj.* is immediately derived from the definition of the word *expectation* which the Author calls the foundation of the theory.[9]

All the following Propositions are deduced from this here alone.

All the Authors who since Huygens are themselves occupied with the estimation of accidental gains, have taken this Proposition as proven or as evident, or else have proved it before all other.

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[9]*Hoc utar fundamento.* Prooem.

Therefore all the calculations by which one estimates the accidental gains suppose the three hypotheses which I have specified in §.§ 7. 15. 18.

§ 21. Thus all the results of this calculus are true only as much as one supposes

I. *That the accidental gains must be estimated by the mean product of the qualified chances* (§ 7.) *by supposing them equally necessary.*

II. *That it is equal to possessing an actual sum or to become possessing it after an indefinite time.*

III. *That the value of the money is exactly proportional to its numerical quantity.*

§ 22. In order to give some exactitude in the summary of these hypotheses, I am going to present them under a geometric point of view.

Suppose one moment that at the same instant one can not make two equal gains.
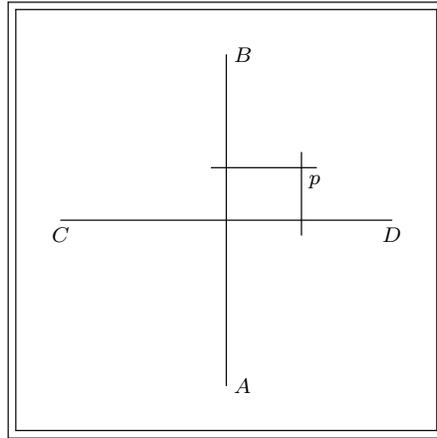
The infinite line AB represents the time. It is at the same time the place of all the null gains or $=$ zero.

The infinite line CD also cuts the same at right angles. It is the place of all the possible actual gains.

This being, CD divides the plane in two halves of which the one A is the place of all the past gains, the other B, the place of all the future gains.

Likewise AB divides the plane into two halves of which the one C is the place of all the negative gains, the other D, the place of all the positive gains.

And the plane $ABCD$ will be the place of all the possible gains.

So that the point p being given on this plane it will suffice to draw through this point some parallels to the straight lines AB, CD in order to know the gain & the time which it designates. Reciprocally a gain made at the time t being given, it will be always easy to represent it by a point p on this plane.

In order to correct the false assumption of the necessary inequality of all the simultaneous gains, we represent the number of equal gains made at this instant by a perpendicular to the plane $ABCD$ raised at this same point, that the letter p expresses the length of this perpendicular, & a that of the perpendicular lowered from the point p onto the line AB; the rectangle pa will designate the product of all the equal gains made at the point p.

We suppose many parallel points p', p'', p''', &c. all taken in the present, the sum of the rectangles pa', pa'', pa''' &c. will express the total gain.

And the mean gain will be $= \frac{pa' + pa'' + pa''' \text{ \&c.}}{p' + p'' + p''' \text{ \&c.}}$

Suppose all these points p', p'', p''', &c. taken in the past, suppose them taken in the future. The total gain & the mean gain will be estimated in the same manner.

Instead of supposing these points really indicated, admit that one alone will be it & that we do not see reason in order that one of the lines p', p'', p''', &c. or any point whatever of each of them be indicated.

It is convenient in this case to estimate the gain which results from it as in one of the three preceding cases.

SECTION IInd
Examination of the principles of different Authors.[10]

PRINCIPLE OF HUYGENS.

§ 1. *Definition* Ist. Many co-players are said to play *equitably* or at an *equal game* when 1∘. their stakes are equal. 2∘. When they incur the same chances. 3∘. When the sum of the gains made by all the players is necessarily equal to the sum of their stakes.

§ 2. *Definition* 2nd. The *expectation* of a player in a game of chance is the sum with which he could recommence to play at this same game, under the same conditions & equitably.

§ 3. LEMMA.

If one supposes a game where there are m qualified chances (Sect. I. § 7.), in order that this game can be played equitably, whatever be the gains of each chance, it is necessary $1°$. that the players are in the number of m. $2°$. That each of the players brings forth a different chance that from all the other co-players. $3°$. That these m co-players play in a single trial.

DEMONSTRATION.

1st *Point*. If one supposes more or less players as chances, the product of all the gains can not be foreseen with certitude; therefore the game can not be equal (§ 1.)

2nd *Point.* First it is evident that under this assumption, if the gains are made of the product of stakes, the game is equal. I say moreover that under each other assumption the game is unequal. — Let one deny it. — Since then he will have a gain a that no player bring forth; this gain is replaced by another b. Let be made $a - b = c$. And as I can give to the gains any value whatever (*hyp.*) I suppose them all different, finally that c is not $=zero$. Moreover admit that one can not find two other gains of which the difference $= c$. It will follow thence that the sum of all the gains will be no more the same than in the preceding case. Therefore the game will be unequal (*def.* I. § 1.).

3rd *Point*. If the co-players played at many trials, the subsequent players would not incur the same number of chances. And consequently the game will not be equal (def. I. § 1.)

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[10]This Section can be suppressed by the reader without being harmful to the sequence of ideas. It supposes that one has available the works which it analyzes.

COROLLARY.

If one wishes to estimate the expectation of a player in a game of m chances, of which each can bring forth any gain whatever, it will be necessary to suppose m co-players at the same game.

§ 4. *Remark.* Here is a conception of J. Bernoulli in order to make m co-players to play equitably in a single trial in a game of m chances.

Let the respective gains of each chance, a, b, c, &c. be such that their sum equals that of the stake of the players. Let one suppose each of the quantities a, b, c, &c. hidden apart in a hiding-place of which the players are unaware of it & that each of them takes one without choice.

§ 5. THEOREM.

If I have p qualified chances to win a; q for b; r for c; &c. my expectation is $= \frac{pa+qb+rc \ \&c.}{p+q+r \ \&c.}$.

DEMONSTRATION

The number of chances $= p + q + r$ &c. I must estimate the expectation whatever be the value of each gain. Therefore it is necessary to suppose $p+q+r$ &c. co-players (§ 3. Coroll.) The sum of all the possible gains & consequently the sum of the stakes of the co-players (§ 1.) $= pa + qb + rc$. The stake of one of the co-players is the expectation sought (§ 2.). Let x be this expectation. One will have

$$pa + qb + rc \ \&c. = px + qx + rx \ \&c.$$

& consequently $x = \frac{pa+qb+rc \ \&c.}{px+qx+rx \ \&c.}$ C.Q.F.D

COROLLARY 1st

Let $r = 0$, $p = q$. One will have $x = \frac{a+b}{2}$. This which is Prop. I. P.I. *Art. conj.*

COROLLARY 2nd

Let $p = q = r$; $x = \frac{a+b+c}{3}$. This which is Prop. II. P.I. *Art. conj.*

§ 6. I should at present motivate the modifications that I have brought to the exposition that Huygens made of this principle. But I persuade myself that this would be a task equally useless for those who will have meditated on it & for those who will not believe appropriate to do it.

I will content myself to observe that the demonstrations of Prop. II. III. P.I. *Art. Conj.* such as Huygens gives them, suppose either evident or demonstrated truths which are no more than these same Props.

In order to demonstrate the object of this observation, I will say that in Prop. II. P.I. *Art. Conj.* in order that the game was equal (by the terms of the definition of § 1. of that Section,) the arrangements among the three players that the demonstration supposes, would have ought to be made thus. Let these players be *L, M, N*. One agrees

that *L being vanquished*, *L* will win $a = 2x - b - c$; *M* will win b; *N* will win c. *M being vanquished*, *L* will win c; *M* will win $a = 2x - b - c$; *N* will win b. *N being vanquished*, *L* will win b; *M* will win c; *N* will win $a = 2x - b - c$.

One is therefore forced to agree that the demonstrations of these two Propositions lacked rigor. The origin of this vice of reasoning is in the shortcoming of a definition of the word *equal game*. This word being an element of the definition of *expectation*, the indetermination of the first has influence on this one here; such that without making violence to the expressions of Huygens, one could apply the idea of *expectation* to any sum whatever greater than the greatest of possible gains.

Without stopping to prove these assertions, I will limit myself to remark that a rapid glance deceives easily in an object of this kind, which escapes in attention only through its same simplicity.

§ 7.   PRINCIPLE OF J. BERNOULLI.

Each expected or can be called expectation that which he must infallibly obtain.
Such is the definition which J. Bernoulli substitutes to that of Huygens.

In order to prove Prop. III.P.I. *Art. conj.* (v. § 5.) this Author supposes $p + q + r$ &c. players in the manner explicated in § 4.

These players will obtain infallibly among them the sum of all the gains $pa + qb + rc$ &c. This sum is therefore their total expectation (*def.*)

But each player has an equal claim to this sum. Therefore each player must pay a like sum in order to purchase this claim here. That is to say that the stake must be $= \frac{pa + qb + rc \text{ \&c.}}{p + q + r \text{ \&c.}}$.

§ 8. Although this principle of J. Bernoulli appears to differ from the one of Huygens, this difference is only apparent.

The word *equal claim* signifies that the chances are qualified.

The assertion, that the players will *obtain infallibly* the sum of the gains, founded on the conception of § 4, is equivalent to the assertion of the equal necessity of the chances.

§ 9.   PRINCIPLE OF MONTMORT.

In the Remark respecting Lemma 1st of the *Essai d'Analyse sur les jeux de hazard*, one takes for evident Prop. III. P.I. *Art. conj.* (v. Sect. 1. § 19.).

§ 10.   PRINCIPLE OF MOIVRE.

The introduction to the treatise of the *Doctrine des hazards* begins thus. *The probability of an event is greater or less according as the number of chances by which it may happen compared to the whole number of chances by which it may happen or fail.* Thence the Author concludes the justice of the evaluation of a probability by a fraction. He passes next to the estimation of an accidental gain, which consists in multiplying the expected sum by the probability to obtain it. He demonstrates this principle by the

assumption of many persons who have the same claim to obtain it. The rest is only a development.

Perhaps one should have expressed in the passage that I just translated *equal* possibility of chances.

This fundamental idea is not defined. And that which I have said can suffice to establish the definitions of § 3. Sect. 1. When one says that many chances are equally possible, so one introduces the idea of time or of space, that is to say that they will never arrive.

I do not doubt that the oversight of this definition has no given place in some reasonings on the nature of chance, of which perhaps one will recognize the inutility (v. Sect. III § 12.) Moivre makes no mention of the last two hypotheses which I have specified.

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SECTION IIIrd

Application of the third hypothesis.

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§ 1.   1st HYPOTHESIS.

In order to judge if the first hypothesis of the calculus of the accidental gains is admissible, we look at how one helps oneself in order to apply the theory in practice. For this I am going to analyze the problem of J. Bernoulli of which I have cited a consequence in beginning this Memoir, that which will give me place to justify some assertions; I will try next to recognize the principles according to which the later Geometers have perfected the solution which this illustrious mathematician gives to it. I will depart thence in order to propose some observations tending to determine the degree of confidence which one must have in the results of this calculus & the cases in which it is applicable.

§ 2. *Let*[11] *a die of t faces, of which r white & $t - r = s$ black. I play with these dice nt times.*[12] *One counts the number of white trials. If its ratio to the number nt is $> \frac{r-1}{t}$ & $< \frac{r+1}{t}$, I win 1, if the contrary takes place, I win zero; one demands what is my expectation?*

Let $\alpha, \beta, \gamma, \delta$, &c. be the terms of the power nt of the binomial $r + s$.

The qualified chances are in this game to the number t^{nt}. I suppose them therefore equally necessary.

Each of these chances contain a certain number of white faces & many chances contain the same number of them, as I am going to express in order by writing under each number of white faces the number of chances which produce it.

White faces	0	1	2	$\cdots$	$nr - n$	$\cdots$	nr	$\cdots$	$nr + n$	$\cdots$	nt
Chances	α	β	γ	$\cdots$	λ	$\cdots$	ν	$\cdots$	ζ	$\cdots$	χ

All the chances placed between λ & ζ make me win, because $\frac{nr \pm n}{nt} = \frac{r \pm 1}{t}$. The others make me lose.

Let now the sum of all the terms contained between λ & $\zeta = M$.

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[11] Here as elsewhere I vary the form of the propositions which I analyze.

[12] One can imagine these dice as a prism turning on its axis.

Let the sum of all the other terms of the same sequence, namely $(\alpha + \beta + \gamma \cdots + \kappa) + (\sigma + \tau + \upsilon \cdots + \chi) = m$.

My expectation $= \frac{M}{M+m} = \frac{M}{t^{nt}}$.

§ 3. *Render my expectation in the preceding game so great that it surpasses $\frac{c}{c+1}$.*

That is to say that it is necessary to make $\frac{M}{M+m} > \frac{c}{c+1}$.

This is a purely mathematical Problem & susceptible to be resolved by increasing n.

Example 1$^{\text{st}}$. Let $t = 50$; $r = 30$; $nt = 25550$. One will have $\frac{M}{M+m} > \frac{1000}{1001}$. *Art. conj.* P. IV. *fin.*

Example 2$^{\text{nd}}$. If $n = \infty$, the expectation is infinite. *Ibid.*

§ 4. *Remark* I. A glance cast on the march to this solution will show that all the propositions of which it is composed depend on Prop. III. P.I. *Art. conj.* or are part of the theory of the discrete quantity. The sequence $\alpha, \beta, \gamma, \delta$, &c. is indicated by Prop. XII. P.I. *Art. conj.* which derives nearly immediately from Prop. III. P.I. (v. Sect. I. § 1.).

§ 5. *Remark* II. I have supposed t^{nt} chances equally necessary. I have found that the gains produced by these chances were M. I have taken their mean product $\frac{M}{t^{nt}}$ for my expectation.

If to the white & black faces of a die, I substitute two natural phenomena or two events whatsoever which are mutually exclusive, I will be able to make the same reasoning by departing from the same hypothesis.

For example; t designating the tropical year & n a very great number; if in the course of nt days, nr have been stormy, ns serene; I will be able to wager M against m or more than c against 1 that the true ratio of the stormy days to the serene days is contained within the limits $\frac{r \pm 1}{t}$. But it is necessary to make for this an assumption equivalent to the following three assumptions. The 1$^{\text{st}}$ that the ratio of these two kinds of days is the same each year. The 2$^{\text{nd}}$ that if one repeated these nt Experiences t^{nt} times, all the conceivable combinations among nt days (of which nr stormy, ns serene) would arrive necessarily. The 3$^{\text{rd}}$ that the one who wins only one time must pay the mean gain of the one who would have won t^{nt} times.

§ 6. *Remark* III. If $n = \infty$, that is to say if with the die one makes an infinite number of experiences, it is easy to prove that m becomes infinite, & M an infinity of a superior order.

One sees that the denomination of *certitude* given to this infinite probability in the alleged consequence at the beginning of this Memoir (Sect. I. § 1.) does not exclude a same infinite possibility of the contrary.

This expression signifies that if one mixed all the gains, those which are null being infinitely less numerous than the others, the mixture would not be altered so to speak.

The following Problem offers an application of this Remark.

§ 7. *One demands the probability that a given point (p) on a line, becomes the center of a certain circle which must necessarily have its center on this line, but of which one knows besides no other determination?*

The number of points on the line being infinite, this probability is infinitely small.

§ 8. *Objection.* Therefore its complement is infinitely great, that is to say that it is sure that the center in question will not fall on the given point. The same reasoning applying to all the points of the line, it is sure that the center in question will not fall on this line, that which is contradictory to the hypothesis.

Response. When one says that the probability against the point p is infinity, this signifies that if all the possible cases took place, that is to say if the center would fall successively on all the points of the given line; & if it was agreed that the sole point p would make me win *zero* & that all the others would make me win 1, I would win an infinite sum, & the mean gain of each trial would be $= \frac{\infty - 1}{\infty} = \frac{\infty}{\infty} = 1$, whence there results that my expectation would be the same as if the center had fallen outside of the line.

Some Authors have taken advantage of these expressions of the Geometers for lack of having paid attention to the sense that I just developed. *If there be a possibility that it MAY happen, the hazard is NOT infinite. The world therefore cannot &c.* Wollaston. *Religion of Nature.* Sect. V. p. 8.

§ 9. Moivre has resolved the Problem that J. Bernoulli had himself proposed (§ 2.) according to the same principles & has perfected only the calculation, to which he has given more precision; whence results the essential advantage to obtain some more narrow limits of the ratio than one wishes to determine.

§ 10. This author proves first that when one makes a great number of Experiences, the expectation to obtain a ratio which deviates itself from the true ratio is very small. This is the object of Probl. 72.73 *Doctr. of chances*, based on the 1$^{\text{st}}$ hypothesis (§ 7. Sect. 1$^{\text{st}}$) of which the Author draws some Corollaries to which he gives, if I am not mistaken, too much extension. On the subject Moivre himself proposes a difficulty to resolve. *Seeing the great power of chance, events can not be at the end of a long time to be arrived in a proportion different from that towards which they tend. Suppose, for example, that an event can equally arrive or not arrive, it is not possible that after 3000 Experiences this Event was arrived 2000 times & had missed 1000. It would be agreeable therefore to determine how much one can wager that so great a gap from the real proportion has not taken place.*— This Author responds that this is here the most difficult Problem of all the theory of Probabilities & he gives the solution which differs from that of J. Bernoulli, as I just said of it, only with more precision, by some skills of calculation & not by the principles. He arrives thus to the same conclusion as this last Geometer, namely that by taking some convenient & relatively very small limits, for the ratio of which one estimates the probability; & by multiplying indefinitely the experiences, one could wager a sum always greater & even infinity against one, to obtain a ratio contained between these limits.

§ 11. Here is how Moivre deduced from there a general consequence on the nature of chance. *One will find in each case that although chance produces some irregularities, however one could wager infinity that in the sequence of time these irregularities will have a null ratio in the recurrence of this order which results from the original design.*[13]

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[13]This remark makes the matter of the dedicatory Epistle of Moivre to Newton.

§ 12. If chance expresses unknown causes, it is only by studying these causes in Nature, that one can recognize if they have or not a regular march.

If to the contrary one understands by chance some causes which act all equally & which succeed themselves the one to the others in the most regular order, the contemplation of the generating effects by some parallel causes will lead us inevitably to rediscover in their causes this hypothetical arrangement.

Here is why these reasonings on the nature of chance founded on a purely mathematical theory seems to me to lack object.

They have given place to some risky consequences. "Since in the calculus of probabilities, says an estimable Author, it has been necessary that the stars follow an infinity of false routes before finding that which is combined with the universal system; I will be always grounded to say that the dogma of the existence of God is regarding atheism in the ratio of infinity to unity." *Phil. de la Nat.* T.V. p. 195.

§ 13. J. Bernoulli & Moivre suppose the true relation known & determine after this assumption the number of Experiences to make in order to obtain a ratio between two assigned limits. Messrs. Bayes & Price (*Trans. phil*. 1763. 64) had proposed a method to find this supposed unknown ratio. But the work of Mr. de le Place on this object dispenses me of an analysis of the others.

Although a mind accustomed to the abstractions & endowed with a strong attention can supply the demonstration of a proposition that this Author poses in principle, I believe myself obliged by the nature of these researches to give it here a few words uniting some remarks with them.

§ 14. *Two Urns A, B, contain some white & black tickets. I have drawn some tickets from one of the two. And I have found that the whites were to the blacks in a certain ratio r. I have drawn all those tickets out of A, or all out of B; one of these cases is as possible as the other. In Urn A the probability to obtain the ratio r is $\frac{K}{m}$; in urn B the probability to obtain this same ratio r is $\frac{K'}{m}$, one demands what is the probability that I have drawn out of Urn A; or what is the respective expectation of the two players of whom one could win 1 if I have drawn the ratio r out of A & the other could win 1 if I have drawn out of B this same ratio?*

To establish that it is equally possible to draw out of the two Urns, that is to say that out of $2m$ drawings, m are out of Urn A, m out of Urn B. Let x be the expectation of the player who wins if the ratio r is taken out of Urn A, let y be the expectation of the one who wins for Urn B. The 1$^{\text{st}}$ player has

K	chances to win	1
$m - K$	for	x
K'	for	0
$m - K'$	for	x.

Therefore $x = \frac{K}{K+K'}$. Likewise $y = \frac{K'}{K+K'}$. Therefore $x : y = K : K'$.

§ 15. *Remark* I. If one does not suppose the equal possibility to draw out of the two Urns, it will be necessary to make an assumption more difficult to express although more simple in appearance, namely that the chances which represent the letters K, K'

are equally possible. This is why I have preferred the first. And in order to enunciate in two words, I will call such Urns *equally possible*.

§ 16. *Remark* II. Instead of two Urns if one supposes many of them equally possible, one will find that the probabilities to have drawn out of each are among them as K, K', K'' &c.

§ 17. *Remark* III. In each Problem relative to the probabilities this that one seeks can be compared to the object of this question. It presents four conditions of which any three determine the fourth. Here are they under an interrogative form.

1. Having drawn m tickets from a single Urn what is the probability that I have drawn out of Urn A?

2. What is the probability for each Urn to bring forth the ratio r?

3. If I have obtained the ratio r what is the probability that I have drawn out of Urn A?

4. What is the ratio r which satisfies in the supposed known preceding probabilities?

§ 18. *Remark* IV. In the preceding Remark the 1$^{\text{st}}$ question tends to determine if the Urns are equally possible. Suppose that they are not, but that I know the ratio of their different possibilities, or the probability that a drawing of m tickets has been made in each of them; it will suffice to reduce these probabilities to the same denominator & to suppose a number of Urns equal to the sum of the Numerators in order to have some equally possible Urns. This Remark can be deduced from a Principle posed by Mr. de la Place in a preceding Memoir.[14]

§ 19. *Remark* V. As much as one leaves indeterminate the respective probabilities for each Urn to bring forth r, one has claim to suppose any number of equally possible Urns; because by making null the probability to draw the ratio r from certain Urns, it is as if one had declared them impossibles.

§ 20. *Remark* VI. m being the number of tickets drawn, let N express the all the tickets contained in an Urn, & let $r = \frac{a}{b}$; in order that the 2$^{\text{nd}}$ question of Remark III be not contradictory it is necessary that $a + b <$ or $= m$. Now under the assumption of Mr. de la Place $N = \infty$. Therefore under this assumption $a + b : N <$ or $= m : \infty$.

§ 21. Mr. de la Place is served by the principle that I just exposed (§ 14.) in order to resolve two Problems of which the end is to determine the ratio of the causes by Experience. The last Remark which the scholarly Memoir offers that I have under the eyes is relative to the 1$^{\text{st}}$ hypothesis of the calculation of the accidental gains & I am going to present it under a point of view relative to the object which occupies me.

§ 22. *In the game of Petersburg if the coin is not entirely just, or in general if the causes which determine heads are more or less efficient than those which determine tails, is the expectation of Pierre augmented or diminished?*

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[14]*Sav. Etr.* T. VI. p. 360.

In order to resolve this question it will be necessary to depart from the principle that the coin will tip as often for heads as for tails.

According to this principle Mr. de la Place determines the expectation of Pierre. And there results from its solution that if the players agree to stop at the 5$^{\text{th}}$ trial; the expectation is not at all changed by the falseness of the coin. If they agree to stop before the 5$^{\text{th}}$ trial, the expectation is less with the false coin; but it is greater if they stop later than the 5$^{\text{th}}$ trial.

§ 23. I am going to deduce this truth with a little more detail in the end 1°. to remark *why it is at the 5th trial very nearly that the expectation is equal; 2°. to determine what must be the falsity of the coin in order that the expectation of Pierre at the 6th trial is still the same as if the piece were just.*

The apparent contradiction between this last question & the formula of Mr. de la Place comes from this that in order to obtain this formula it has been necessary to neglect a negative quantity of a higher degree than the positive quantities neglected also, this which has necessarily increased the expectation a little, but by a quantity which one must regard as null when the falsity of the coin is very small.

§ 24. In order to resolve the proposed question (§ 24.) here is how I reason.

Let the Probability for heads be $\frac{a\pm1}{2a}$. Pierre has a chance for $\frac{a-1}{2a}$, a chance for $\frac{a+1}{2a}$. Thus estimating his expectation under both of these assumptions I will take the half of the sum & I will compare it with the expectation of Pierre in the case where the coin is just.

§ 25. The Game where one stops at the 5$^{\text{th}}$ trial offers $2^5 a^5$ qualified chances that I suppose equally necessary.

Number the chances of each kind with a just coin or the Probability $= \frac{1}{2}$.

$16a^5$	chances to win	1
$8a^5$	for	2
$4a^5$	for	4
$2a^5$	for	8
a^5	for	16
a^5	for	zero.

Number of the chances of each kind with a false coin or the Probability $= \frac{a\pm1}{2a}$.

$a.16a^4$	chances to win	1
$a^2 - 1.8a^3$	for	2
$a^3 - a.4a^4$	for	4
$a^4 - 1.2a$	for	8
$a^5 + 2a^3 - 3a$	for	16
$a^5 + 10a^3 + 5a$	for	zero.

§ 26. By comparing these two tables one sees that the gains in the 1$^{\text{st}}$ are greater than in the 2$^{\text{nd}}$ by the quantity $\overline{2a.8} + \overline{3a.16}$; this which increases the expectation by the quantity $\frac{64a}{2^5a^5} = \frac{2}{a^4}$, a negligible quantity when a is very great. But if $a < \sqrt{\frac{3}{2}}$, the gains of each kind in the $^{\text{st}}$ sum would be greater than the corresponding gains in the

2nd & the difference which would result from it in order that the expectation would be sensible.

§ 27. If the Game is in six trials one will find by an analogous process the sum of the gains produced by all the qualified chances in the case where the coin is just superior to the semi-sum of the gains in the two Games made with the false coin, by the quantity $5a^4.32 - 9a^2.32 - 32$. Whence there results that the expectation with a false piece will be the same as with a just piece if $5a^4 = 9a^2 + 1$, that is to say if $a = \sqrt{\frac{\sqrt{101}+9}{10}} = 1.38$ nearly, by giving to the roots a positive value.

The first formula of Mr. de la Place would have given the same result; because as in this formula x expresses the number of trials, $\frac{1 \pm \pi}{2}$ the probability, if one makes $x = 6$, & the formula $= x$, one will find $\pi = \sqrt{\frac{\sqrt{101}-9}{2}} = 0.73$ nearly; now $\frac{1.38 \pm 1}{2.138} = \frac{1 \pm 0.73}{2}$ nearly.

§ 28. The number of chances which give *zero* is constantly greater in the case where the coin is false. In general there is always advantage to serve oneself with an unequal coin when one wagers to bring forth many times in sequence one same face in the coin, because $\overline{a+1}^m + \overline{a-1}^m > 2a^m$, when $m > 1$.

For example, if $m = 2$, $a = 3$; that is to say that the Game is in two trials, and that the inequality of the Coin be such that one of the faces falls 2 times & the other only one time out of 3. One will have $\frac{\overline{a-1}^2 + \overline{a-1}^2}{2} = 10$; $4a^2 = 36$. The expectation to bring forth two consecutive homonymous chances $= \frac{10}{36} > \frac{1}{4}$.

This is also the consequence which one had been able to draw from Probl. 74. *Doctr. of chances* of Moivre.

§ 29. The solution of the preceding Problem reposes on the principle that there is an equal probability in order that the coin leans to heads or to tails. That which signifies that out of 2 Games the coin will tend one time for heads & one time for tails: in a way that the sole effect of this assumption is to represent heads & tails as equally possible, instead of being represented as equally necessary (Sect. I. § 3.). Thus one has only doubled the number of qualified chances & changed the order in which each face was supposed to fall; & if each qualified chance returned a like sum, the falsity of the coin would change nothing of the expectation, whether one won for heads or for tails.

One can by an analogous process defer indefinitely the term in which one fixes the equal possibility of the two events, because one can multiply indefinitely the time t (Sect. I. § 3.).

Thus one could say; let $\frac{a' \pm 1}{2a'}$ be the probability that the coin tend in favor of heads. Seeing no more reason for the sign $+$ as for the sign $-$; I estimate the expectation of Pierre in the one & the other assumption & the half-sum of these expectations will be the sought expectation; which will be found equal to that which had given the just coin or the probability $\frac{1}{2}$. And finally this calculation supposes always that out of $2m$ Games in heads-tails, there will be necessarily m of each kind.

§ 30. Depart from a different assumption; namely *that, whatever be the face for which Pierre wagers, the probability to bring forth this face is always a little greater.*

Let this probability be $\frac{1 \pm \pi}{2}$. Suppose with Mr. de la Place that the units given to the game of Petersburg are some coins of two écus; that x designates the number of trials

after which the players agree to stop; E the expectation of Pierre. We will find with this Geometer & by a summation of a very simple sequence $E = \frac{(1+\pi)[(1-\pi)^x-1]}{-\pi} = \frac{1+\pi}{\pi} - \left(\frac{1+\pi}{\pi}\right)(1-\pi)^x$.

Let x be infinity, & the quantity π finite & placed between the limits *zero* & 1; in this case the formula becomes $E = \frac{1+\pi}{\pi}$, a finite quantity.

§31. Suppose an instant that no other cause influences the expectation of Pierre we can reason thus:

Since the assumption of a slight tendency in favor of the face for which Pierre wagers gives a finite expectation; inversely if the expectation is finite, it is necessary to conclude that the face for which Pierre wagers has always a little more tendency to fall than the other.

Suppose, for example, that in the event $E = 5$; I will have $\pi = \frac{1}{4}$. And the probability to bring forth the face for which Pierre wagers $= \frac{5}{8}$. This which would suppose an unobserved effort of the part of the player in order to terminate the game.

§ 32. The setting aside we have made of each other cause of diminution of the expectation of Pierre is not natural; thus when the same as that which I just indicated in passing would not be admissible, it would be necessary to conclude nothing for the value of this expectation which my plan does not call me to actually evaluate; but one could, if I am not mistaken, apply to it one of the following reflections of which the object is much more general. They will tend to set some principles out of the art to determine the probability of the events by experience, to which the exposition that I am going to make made some methods in order that this must serve as supporting point.

§ 33. The calculus of probabilities applies to two objects, the games of chance & the events as much natural as politics.

Both are determined by some causes which are to us unknown in whole or in part.

But there is an essential difference between them that in the games we are ourselves in the number of causes acting & determininators of the events.

The end to which we act, is always to maintain a perfect equality.

In this effect $1°$ we destroy as much as there is in us the causes of interior inequality which could exist in the instruments of the game. $2°$ We will work also (sometimes without us avowing it) to destroy the exterior causes of inequality.

§ 34. Suppose that one plays at heads-tails an important sum, one will take care that the coin be quite just. In order to be assured of it the worker will not have means more sure than to destroy the exterior causes of inequality the most that will be possible by him & to test if in this case the coin turns alternately to heads & to tails, so that if one analyzes the sense of this expression *just coin*, one will find that it is a coin such as in balancing, as one makes it in the game, the exterior causes which bring forth heads or tails, it falls alternately on these two faces or nearly the thing. It is, if I am not mistaken, in this definition & in this remark so simple that there lies the solution of the difficulties made at the occasion of the Problem of Peterbourg, thus I will indicate besides. After having seen the work of the worker, reflect on the action of the players. As the first balances the exterior causes in order to know if the coin is just, the players in their turn envisioning the coin as just will not have another view than to be assured of the equality of the exterior causes. Displeased perhaps with the ordinary

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precautions, they will replace the hands by a spring which launches the coin. But if the coin were put on the spring all the trials with the same side, it seems that one would be harmful to the equality which one has in view. One would come therefore to set the coin alternately on heads & on tails before making the spring act. In general, one will remark that the manifest or unnoticed efforts of the players tend to make succeed a coup of heads to a coup of tails & reciprocally. This which perhaps is the cause of an effect observed by Messrs. d'Alembert & de Buffon, namely *that the consecutive homonymous chances are less frequent than all other kinds of chance.* If one limits oneself to admit in the coins an interior or exterior inequality according to the principles exposed above (§ 29.), one arrives to a result diametrically opposed to this observation (§ 28.), whence I conclude not that the observation is false but that the hypothesis is not natural. The observation that I just made & that which I have indicated just now (§ 31.) can be false without that this last conclusion be absurd. But I am going to give two or three examples which will indicate *that in many cases the consideration of the possible inequality of the consecutive homonymous chances or in general of the regular chances must enter into the calculation of the estimation of the accidental gains.*

§ 35. I suppose that a blind man draws at random from a pile of marked pieces & that he makes 1024 packets of 10 pieces. Could anyone affirm that none of the packets will offer ten pieces turned to the same side? or that if the blind man repeated m times this Experience, he will make less than m packets of this kind? In order to respond to this question there is only experience which can serve as guide to us. And if the blind man always drew from the same pile, if he raised & set constantly each piece with the same precautions, after having seen the nature of m times 1024 packets, I could presume the nature of the following. Until here I do not think that a sensible man believed to be able to affirm anything on the possibility of homonymous chances. One sees therefore that it is only in certain causes dependent on us & particular to each kind of game that one must seek the reason of their least possibility, if it takes place. As the experiences in order to assure of us this point of discussion in each particular game are as delicate as their object is subtle, I refer to some articles that which I have to say on experience & I limit myself to a remark on pharaon in confirmation of the preceding observation.

§ 36. When the Punters would observe this chance 1111122223333 &c. to approach which is very unfavorable to them, without doubt they would find the order of the chance in design or by making the cards to mix or by changing the game. The Banker would do the same with it if he saw this here 12345 &c to approach. Thence there would result a kind of impossibility to obtain such chances & all those which resemble them much. The expectation of the Banker is increased by the diminution of the number of possible cases, but it is diminished more than proportionally by the curtailment of some more lucrative chances. And it is easy to imagine such games where these observations would have more influence. Here at least one can not deny that it has a little of it & that one must modify in consequence Probl. XXXIII. *Doctr. of chances* of Moivre.

§ 37. To this subject I will say a word in passing of a general effect observed by the players & denied by those who do not play. The first designate this effect by the words

good luck & *bad luck* & seem to attribute to it thus a supernatural cause. The players occupied frequently during some consecutive days & nights to make an immense sequence of experiences on a unique object to which they give all their attention, perceive some recurrences of effect that of others can not notice; little curious of knowing of it the causes provided that they can profit from it, they content themselves with an obscure word in order to express an idea that they have neither need nor perhaps the capacity to analyze. The refined & passionate players are those who these observations strike; the vulgar players imitate them, mechanically, without principles, at the pleasure of a blind routine & their superstitious practices become so much more ridiculous as they attach more importance. The first could reduce to principles the art which they profess & their rules to lead us would give the key to their systems on good luck. Some examples will clarify my thought.

§ 38. If a banker at pharaon rejects a game because it carries bad luck to him, can he not come to that which he sees to approach some ruinous combinations? He changes hands in order to make cut: is this not at all that the same hand produces more frequently some even or odd cuts? e.g. if this chance approaches 11223344 &c. this is the cut which decides his lot. Each player follows a certain system, his lot must therefore vary according to the system of his adversary. If, for example, at pharaon I am in the usage to make much of double stakes, I would lose to play with a banker who would mix not at all & I would have bad luck with him. In any game in which the banker distributes the cards at the whim of the Punters, as in Ferme,[15] if I am preceded by a player who has for system to take at each trial a number of very different cards, the chance will change so brusquely for me that my combinations will be useless; this is perhaps for that which the place which an able player occupies can not be indifferent to him. And if it is true, as one can conclude from an expression of Mr. Dusaulx, that an experienced player can recognize a player by his look, I do not see that one is right to affirm the impossibility of a rapport between the gain of a player & the figure of his neighbor. The good luck or bad luck days will be those where some parallel circumstances are found to compete.

§ 39. Here is enough of it on the application of the first hypothesis of the calculus in the games of chance; the application of this doctrine to the uses of life, to the events & to the phenomena is each interesting otherwise. *This is the only useful part of this science, the sole one worthy to occupy seriously the philosophers.*[16]

§ 40. The end of the Experiences is to be assured of the future by the past. The object of nt made Experiences is to indicate the nature of t Experiences to make.

One can present under this point of view all the applications of the calculus of Probabilities in the research on causes; in the principle of Mr. de la Place e.g. one can express the question of Problem (§ 14.) by a future form by supposing that one draws anew out of the same Urn &c.

If in the Problem of J. Bernoulli (§ 2.) one changed n times dice, if in the Principle of Mr. de la Place (§ 14.) one can not draw m times in sequence out of the same Urn, the solutions would be impossible & the conclusions defectives.

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[15]This game is explained in the *Essai d'analyse sur les jeux de hazard.* 2nd Edition p. 280.

[16]*Sav. Étrang.* T. VI. *Préf.*

Therefore when the question is of the probability of some phenomenon; if I have nt Experiences, in order to be able to draw some analogous conclusions to those of these Propositions relatively to t Experiences, it is necessary that I have made n times t Experiences under similar circumstances; this is to say that each time where I have seen to arrive t effects, the acting causes have been the same. Therefore this calculus finds its application only in the periodical return of identical or supposed such causes.

Some Examples will clarify my observation.

§ 41. The application of the Problem of J. Bernoulli to the proportion of serene & stormy days (§ 5.) will be very nearly exact if $t = 365$ days $5^{\mathrm{h}}\, 48'\, 4''$ &c. that is to say in a tropical year precisely, because then t Experiences will be made n times under the same circumstances very nearly or under the influence of similar causes; I will have played with the same dice. Or if I applied to this matter the principle of Mr. de la Place, I would have drawn out of the same urn.

If n is very great with respect to t, if $t > x$ & if the number of Experiences is for example $nt - x = nt$ sensibly, the conclusion will not be altered by a sensible quantity.

If on the contrary $nt < t$, this condition will be immediately chanced. This is the case of a Being strange to our Planet who having observed six or eight months of the year, would claim to conclude for the six or four others the probability of the ratio of the serene days to the stormy.

§ 42. I observe some Games in the petit-palet without seeing the players, but also their two places *A*, *B*. After nt Games am I able to determine the probability to win for each place if he himself makes further t Games?

If each place were occupied constantly by the same player, so that there were only two antagonists of whom the respective addresses remained always the same, this periodic return of causes would permit the application of the Problem of J. Bernoulli; but if the forces of the players change perpetually, nt Experiences make one alone only when this number becomes infinity; whence it follows that $n < t$ & that I am not able to draw any conclusion from these Experiences.

In truth in this particular case the number of players & of their respective forces not being infinity, one could by virtue of some assumption form a conclusion more or less defective.

§ 43. Suppose now that not knowing always if the same players occupy the same places, I note in the gain of each place some periodic returns, in a way, for example, that out of t Games *A* wins nearly always r Games; *B*, s Games. Let $r + s = t = 1$; $nt = n = \tau = \rho + \sigma$; let ν be much $> \tau$; if I made $\nu\tau$ Experiences, of which $\nu\rho$ gave $\frac{r}{s}, \nu\sigma$ each other ratio, I could conclude from it the Probability of the ratio $\frac{\rho}{\sigma}$ for τ new Games, on condition that at each τ Experiences the same causes returned periodically; otherwise I can conclude nothing more of the observed periodic return.

And as it is easy to see that if one pushed further these assumptions one could obtain always the analogous results, I conclude that *in order to be able to apply to the research of the causes the calculus of Probabilities, it is necessary to suppose some periodic returns of causes of which Experience alone can assure us, & that at least to commit a circle, it is necessary to admit the Experience as certitude or inappreciable probability & as last basis of all our conjectures.*

§ 44. It is easy to make the application of this remark to all the cases in which on applies the calculus in order to determine the probability of the causes.

I will content myself to observe that if one does not suppose a recurrence of the same causes of errors in many observations, one would not be able to estimate the probability of one of these observations. Whence it follows that from one instrument & from one observer to another one can make no induction, without being assured by Experience of this periodic recurrence.

In this subject I will permit myself a short digression relative to the method proposed by Mr. de la Place in order to take the mean probability among three observations of one same phenomenon. The distances of these observations to the true point being the abscissas of a curve, their respective probabilities can be represented by the ordinates of this curve. It appears that these ordinates decrease in deviating from the true point. The law according to which this diminution takes place being known, the mean or *maximum* of probability will be placed at the point which being supposed true gives three probabilities of which the product is the greatest. The hypothesis preferred by Mr. de la Place is that the ratio of two infinitely small consecutive differences is equal to the one of the corresponding ordinates.

In reflecting on the construction of this great Geometer, I have believed that in the usage one would be able to replace it by a simpler operation. $1°$. The part of the curve which is extended from both sides to beyond the observations most extended from the true point become useless in this construction. $2°$. I observe next that some physically imperceptible instants are the only ones of which it is necessary to take account. $3°$. Finally I believe that one would not know how to affirm in a general manner that the probability to commit an error of observation is constantly smaller when the error is greater. In effect, suppose three observers occupied to determine the instant of one same phenomenon; each having taken account of the inequalities produced by the causes which are known to them, such as its position, refraction, the known faults of his instrument &c., if these three observers neglected no element, it is clear that their observations would coincide; it is therefore these ignored or inappreciable elements which it is the question to estimate. Now there is only experience which can teach the degree of probability that there is for each observer to commit such or such error produced by this unknown cause. Whatever be this cause, we can compare it to a slight deviation of the alidade.[17] $1°$. It will be contained between certain limits; call 1 the greatest arc of aberration. $2°$. The arcs of error neighboring 1 are apparently more rare. But $3°$. perhaps & even probably those which are very near to *zero* will return also quite rarely, so that the greatest probabilities would be found placed between these two extremes.

It is according to these reflections that I have tried to find a practical method based on experience & which was preferable to that of the arithmetic means without being

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[17]*Translator's note*: An alidade is the needle in a sextant or quadrant.

much less simple.

A						V					B
	1	2	3	4	5	6	5	4	3	2	1
1st	a				b		c				
2nd		a				b		c			
3rd			a				b		c		

Let AB be the line of time; V the true point; the points A, B, two limits such that to the right & to the left the observer passes them not at all, in a way that the length of the line AB is determined by the supposed known ability of the observer & by the also known difficulty or delicacy of the observation. Let each division of the line AB designate a time t so small that its extremes are sensibly confounded. I suppose the probabilities diminish in the ratio 6, 5, 4 &c. that is to say as the ordinates of two straight lines departing from the points A & B. Making three observations, there is concern only of applying them into these divisions in a manner that the products of the three numbers which they indicate are the greatest possible.

Example. Let $t = \frac{1}{4}''$; let the three observations be $a = 0''$, $b = 1''$, $c = 1\frac{1}{2}''$. I make the successive Tests indicated in the Figure.

1st Test	1.5.5=25
2nd	2.6.4=48
3rd	3.5.3=45

Of which Tests there results that the second position of the point V is the most probable & that the instant of the phenomenon to which I must fix myself is $1''$, since every other assumption, before or after, gives a lesser probability.

Now let one multiply as much as one will wish the divisions of the line AB; let one render them as small as one will wish; let one change the hypothesis of the straight line in that which one will judge the truest, that is to say let one substitute in the sequence of natural numbers, the triangular, square, or such others following a constant or irregular Law. If Experience proves that the Law according to which the probabilities of error diminish in deviating from the true point, is constant & continuous, it will be necessary to determine this *maximum* probability, according to the principle exposed, by means of a curve which represents this Law. But if (as I believe that this will be the most ordinary case) if, I say, the numbers which experience will oblige to place in each division do not follow a fixed progression, the method of groping that I just indicated will be the only one which one can employ. And there will be diverse ways to simplify it that it would be superfluous & out of place to note here.

Each observer will sense that the determination of the line AB is in his power; it will not be likewise of the probability of each error, or of the numbers to place in each division: that will appear first impractical. However it seems that a long sequence of experiences can teach that the inappreciable elements cause one time out of m such given error. And if one accords this, it is easy enough to assign the probability that it is necessary to express in each division. Moreover this method or such other destined to replace the arithmetic means appears necessary to use only in some most delicate observations.

§ 45. I resume the example of the game of petit-palet (§ 42.), I myself suppose certain that the two places *A*, *B*, are constantly occupied each by the same player. And I note that still in this case the first hypothesis of the calculus is not applicable, because

although all the ratios between *zero* & 1 are conceivable, we know that the ratios neighboring the one of equality take place much more frequently, &, if this question is of importance, I would not be able to assume that some equally possible Urns contained each kind of ratio.

This remark made on a frivolous game finds, if I am not mistaken, its application in Nature.

§ 46. Some animals enter & exit through the door of a park where an observer is placed. He knows by knowledge certain that the ratio of the entering to the exiting is one of equality. One demands what is the probability that after 3600 observations of entered & of exits, the number of incoming will be to that of the exiting in the limited ratio $\frac{1800\pm30}{1800\mp30}$?

Response $\frac{2}{3}$. (See Moivre *Doctr. Ch.* App. Probl. 73. Cor. 4).

§ 47. *Remark* I. Consequently the Probability for each other ratio will be $\frac{1}{3}$. And the observer must wager 1 against 2 that the limited ratio will not take place.

But the end of these entrances & exits is to replace almost immediately each animal that one takes away.

Therefore the observer always loses & if any number whatever of such wagers were made, all those who would wager against the limited ratio would necessarily lose.

§ 48. *Remark* II. This Problem could have been able to be enunciated thus. *In a mortal specie of which the population changes nearly not at all, what is the probability that by making m Experiences the ratio of the deaths to the births will be outside the limits $\frac{1}{2} \pm x$?*

§ 49. A being strange to our Planet, imprisoned in a cavern under the equinoctial line, who communicates by the light of the sun only through an air-hole, has seen a very great number of times the day succeed to the night. One demands how much he must wager that by making 3600 new experiences the number of the days will be to the one of the nights in a relation $> 1830, < 1790$?

By reasoning as Nic. Bernoulli on the relation of the males to the females, the observer will decide that the relation of the days to the nights is a relation of equality; as this consequence is true, it will be found in this regard in the same position of the hypothesis of J. Bernoulli (§ 2) & of Moivre (§ 4); that is to say that he will know only $r : s = 1 : 1$. Whence he will conclude that if he makes 3600 Experiences, he can wager 1 against 2 that the number of the days will be outside the limits 1800 ± 30.

The observer will lose always.

§ 50. It appears therefore that it is necessary to pay attention not only to the sum $\left(\frac{nr}{ns}\right)$ of all the ratios, but to each ratio $\left(\frac{r}{s}\right)$ in particular.

§ 51. The falsity of the estimation in the two cases above comes from this that in these two cases the hypothesis of equality necessitating of the chances is not at all applicable. Because it is absurd to say that if one observes 2^{2n} times 24^h under the equinoctial line, one will see one time $2n$ nights & one time $2n$ days consecutively; in other terms, it is absurd to say that the probability for $2n$ consecutive nights $= \frac{1}{2^{2n}}$. This probability should appear to be counted null & the chance which gives it impossible. This remark can easily be extended to other chances.

§ 52. Likewise when Nic. Bernoulli estimates what is the probability that the ratio of the females to the males will be, for 14000 births in the same place, between the limits $\frac{6800\pm163}{7200\mp163}$, & when he finds that one can wager 44 against 1 in favor of one such ratio; is it not part of a false principle in supposing possible that all this number was of girls or all of boys?

§ 53. According to the formula of J. Bernoulli which corresponds to the problem of § 2. if $r : s = 100 : 1$, after around a billion & a half experiences, one can wager much more than 10 against 1 in favor of a limited ratio $\frac{100\pm1}{10000}$. If the question was, for example, of boatmen insured at the price of 1 p.C., this consequence, although it offers nothing of absurdity, would not seem legitimate, because it sets on a principle which one knows not how to admit, namely that the chance of one & a half billion of successively castaway seamen is as possible as any determined chance of the same number of men alternately rescued & castaway.

§ 54. There results from these examples *that before assuring a wager on the conceivable or qualified chances, it is necessary to be certain that they are really equally possibles & that they take place in nature as in our understanding.* Now experience alone can instruct us on the real possibility; therefore the *experience envisioned as certitude or inappreciable probability is the only foundation on which one can seat a conjecture.*

§ 55. It appears therefore that it is necessary for great precautions in order to apply the calculus of probabilities to the accidental events independent of us. The calculus supposes *some periodic returns of causes & an equal necessity in the chances of which the sole experience can assure us.*

§ 56. But if one demands hereafter what is the foundation of our confidence in experience & which authorizes us to envision it as certitude or inappreciable probability; I do not believe that one can find in it other than an *analogy* which is the principle of all our actions & of those of all sentient beings, although we know well that it carries on a ruinous foundation.

> *A parvis quod enim consuerant cernere semper*
> *Alterno tenebras & lucem tempore gigni,*
> *Non erat ut fieri posset mirarier unquam,*
> *Nec diffidere ne terras aeterna teneret*
> *Nox, in perpetuum detracto lumine solis.*[18]

<div style="text-align: right">Lucr. Book V. v. 975</div>

The certitude that the ignorant man has to see the sun to rise, is not based on the equal possibility of all the conceivable successions of day & of night, a possibility totally unknown to the scholar as to the ignorant; this certitude is born of this that one awaits naturally the return of an effect of which one has been witness: one supposes tacitly that the same causes act without ceasing, that Nature follows the same Laws;

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[18]Accustomed as they were from infancy to seeing the alternate birth of darkness and light, they could never have been struck with amazement or misgiving whether the withdrawal of the sunlight might not plunge the earth in everlasting night.

& as the duration of the man so much individual as specie interconnected & linked is quite short, there are well some cases where one has no occasion to be assured of the falsity of this principle.

§ 57. In order to summarize; it is therefore only by the examination of the really possible causes & not by the nomenclature of the conceivable causes, that one can estimate the probability of an effect. When these causes are independent of us & unknown, there remains to us only doubt; when the impossibility of some causes or their unequal possibility is recognized, it is evident that these causes must be reduced to their just value. In order to make this examination & in order to carry a judgment, we have no other rule than the analogy envisioned as certitude or inappreciable probability; & the Experience which must found this analogy is nearly always imperfect.

There is therefore some rules to trace in this regard & a kind of *Logic of the art of conjecture*, which would demand perhaps a study more consistent & a discussion more regular than that which one has accorded to it until here.

§ 58.   2nd HYPOTHESIS.

I will say a little thing on the two last hypotheses of the calculus of accidental gains, seeing that they are only partial simple abstractions.

When researching in it the probability of an event one has no intention to realize some wager on this object & that in general the times in which one can hope that it will take place is indifferent to the object of which one is occupied, the second hypothesis of the calculus of the accidental gains merit not at all to be noticed; it is without doubt the reason which has prohibited that it was not it; because no part is found enunciated in the exposition of the general principles of the calculus of the probabilities, although this oversight gives place to some difficulties of which this hypothesis would have furnished the solution.

§ 59. One will not confound without doubt the influence of the times on the value of the expectation with the object of the calculus of Moivre on the duration of the game, nor with a remark of Mr. de Buffon on the times employed to make a great number of expectations in the game of Petersburg.

The theory of Moivre on the duration of the game[19] has for end only to estimate the Probability of this duration in diverse circumstances, without regard to the consequence which results from it with respect to the value of the expectation; so much the more has it not for end to exclude or to under appreciate certain chances.

As for the Remark of Mr. de Buffon[20], it is still more estranged from the second hypothesis than I have specified, as I will have occasion on the remark made in the 4th Part of this Memoir.

§ 60. The rate of interest of the money seems ought to determine the price of the time in all the cases where the question is of future gains, as one is served in order to estimate the value of the pensions on many heads. And it will be without doubt more exact to suppose, conformably to a remark of Mr. de l'Alembert, that this interest

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[19]*Doctr. of chances Probl. LVIII.*
[20]*Arithm. mor.*

increases perpetually in each instant below any given quantity. Whence it follows that if x represents the time, y the value of a sum a payable after the time x, one will have $x = \ln y$; so that the ordinates of a logarithm of which a would be the ordinate corresponding to $x = zero$, would indicate the value decreasing from this sum payable in diverse times.

§ 61. But as ordinarily the first elements of the time must be neglected & are insensible for us, the consideration of the least value of a future sum acquires importance only when the time to which it falls is found at a certain distance from the present moment. It would be therefore à propos to fix some limits short of & beyond it of which one was excused from having regard.

Call t the time which in the estimation of a chance can be counted null. There will be a time mt which must be counted infinity.

The question is to determine the value of t & of m.

But these values vary according to the circumstances of a difficult evaluation & which escapes from the general expressions.

One sees well that if the question concerns a game of chance such as those which one plays commonly, one must regard as null the time necessary to some Games & as infinite a time rather short. Because finally there is no player at all at heads-tails, for example, who wished to pass some entire months to follow a chance & this here resolves the kind of difficulty which would be born of the Problems indicated Sect. I. §§ 13. *and following* & of others similar which one would propose.

If the question is of a game of State, of assurances, &c. some centuries would be able to be envisioned as a finite quantity.

Between these two limits, the mean chances for the duration would be estimated consequently according to the common rules, or by means of a continuing formula such as I just indicated.

One would find thus, for example, in the game of Sect. I. § 13. the expectation of Pierre equal to the sum of a sequence a little less than unity, as that seems reasonable by setting aside the falsity of the first hypothesis.

§ 62.   3rd HYPOTHESIS.

The third hypothesis having made the object of a Memoir of Dan. Bernoulli,[21] I will not permit myself observations on this subject; although one can make some objections against the construction of this illustrious Geometer, it seems that it is necessary to admit or renounce in a general expression of the value of the wealth of the fortune in their relation with our enjoyments.

§ 63. Mr. de Buffon has proposed himself the same problem in his *Essai d'Arithmétique morale*, but it seems to me that his views are too general. Dan. Bernoulli is departing from a unity, namely the fortune of each man. Mr. de Buffon finds a formula which departs from *zero*, that which gives place to some inadmissible consequences & which I have raised in a preceding Memoir.

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[21]*Mém. de Pétersbourg.* T. V.

§ 64. The game of Petersburg is served with a rule to this illustrious Naturalist. Here is how I imagine that one would be able to attempt to infer departing from this game in order to evaluate money. 1°. Set aside the first two hypotheses of the calculus of accidental gains; suppose that the third is the only one which causes the different appreciation of the stake of Pierre according as one calculates it or as one estimates it by the rules of common sense. 2°. This being, suppose again that after this last estimate, the stake of Pierre is worth only 5, while by the calculus it is infinite. 3°. Suppose finally that the relative price of the money follows a geometric progression in simple exponent commencing at *zero*.

Since all the absolute values of the money being contained between *zero* & *infinity*, all its relative values will be contained between *zero* & *five*. The question in no longer now but to find the Geometric sequence of which the sum is $= 5$.

But if one reflects that the falsity of the first hypothesis is that which in the fact diminishes most subtly the stake of Pierre; that when even that one would set it aside, that of the second hypothesis would diminish it again infinitely; that the value of five écus for the stake is not a quite sure principle; finally that the supposition of the value relative to the money increasing as a geometric sequence in simple exponent & commencing at *zero*, is a gratuitous assumption & even inadmissible; one will be able only to reject the consequence which one drew from these premises. And the game of Petersbourg will appear, if I am not mistaken, little proper to base the estimation of the wealth of the fortune.

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I reserve for another Lecture the 4th Part of this Memoir, which will contain the application of these three hypotheses to the solution of some difficulties.