AVERTISSEMENT

PIERRE MONTMORT

This second Edition is by half more ample than the preceding; the order of it is also different. I have gathered in the first part all the Theorems on Combinations which were spread in the body of the book. I have added some to it. The principal additions are in the first part & towards the end of the fourth. I had omitted in the preceding edition the Demonstrations of the most difficult Problems, in the design to prick further the curiosity of the Reader, who often believes to have known that which he understands without pain. I have put them all in this here at the behest of some friends. Those of the Formulas on Treize will be found in the Latin notes of Mr. N. Bernoulli. I would not have been able to give betters.

The fifth part is a Compilation of Letters that I have received from Messers Bernoulli on the occasion of this Work, & of the responses that I have made to them. There is only one of Mr. J. Bernoulli, but very good. The others are from Mr. N. Bernoulli his nephew, worthy heir of the geometrical scholar, who is in this illustrious family.

There is no need that I give here praise of these Letters, they bear their recommendations with them. One will see that one can have nothing stronger in this genre. I hope that Geometers will be grateful to have sacrificed, by inserting these Letters in this Book, the vanity of the Author to the love that I have for the Public & for the perfection of the Sciences. One will find in these Letters & in the many responses new & very difficult researches of which we have made no mention in the body of this Work. There is in particular on Her, a game that I explain on page 278. The singular conditions of this game have given occasion to a dispute between Mr. Bernoulli & two of my friends, persons of much intelligence. Although I have finally taken part, I dare to be persuaded only the truth is on my side; the Reader will judge it. The question is curious, & I flatter myself that one will not at all find unuseful or too long that which I have written on all sides on this subject.

I do not know if having lacked or rather avoided many occasions which would naturally present themselves to combine in this Work the consideration of the Curves with pure Analysis, I must not fear that one blames me to have inserted into my Letter of 8 June 1712, some things which are of pure Geometry, & which have no relation to this Treatise: But beyond that I myself have made a rule to give my Letters & those of the Messers Bernoulli such as they have been written. I would have deprived the Public of many good things that one will see in the response of Mr. N. Bernoulli of 11 October 1712; this last reason has removed entirely my scruples.

Since that I have published this Essay, there has appeared two small Works which have much relation with this one here. The one has for title *De arte conjectandi in Jure*: This is a Latin Thesis that Mr. N. Bernoulli has submitted the year 1709 at Basel in taking the Degree of Bachelor in Law. There are many curious things & which merit to be read. If one does not find the same beauties there as in his Letters, it is that apparently the subject

---

Date: *Essay d'Analyse sur les Jeux de Hazard*, 1713.

Translated by Richard J. Pulskamp, Department of Mathematics and Computer Science, Xavier University, Cincinnati OH. August 26, 2009.

does not include it, or that the Author had not yet mediated these matters as profoundly as he has done since. The other has more relation with our Work, or rather it is formed on the same plan; it is a quite short Treatise, but excellent, which has for title, *De mensura sortis*.

Mr. Moivre gave it in the Philosophical Transactions at the beginning of the year 1711, but it has appeared only the past year. The Author has given me the honor to send me a Copy that I received at the beginning of the month of August of the last year.

Mr. Moivre has judged well that I would have need of his Book in order to respond to the Critique that he made of mine in his Foreward. The laudable intention that he has had to elevate & to give his Work value, has carried him to debase mine & to dispute to my methods the merit of novelty. As he has believed to be able to attack me without giving me my subject to complain of him, I believe to be able to respond to him without giving to him reason to complain of me. My intention is not at all to critique his Work; beyond that it is above criticism, one would be much annoyed to diminish the merit of it: this is too removed from our character; but because it is permitted to defend oneself, & to conserve one's property, I myself am proposed to respond to him: Here is what Mr. de Moivre exposes in his Avertissement.

> *"Huguenius primus, quod sciam, regulas tradidit ad istius generis Problematum solutionem quas nuperrimus Autor Gallus variis exemplis pulchre illustravit; sed non videntur viri clarissimi ea simplicitate ac generalitate usi fuisse, quam natura rei postulabat: etenim dum plures quantitates incognitatas usurpant, ut varias collusorum conditiones repraesentent, calculum suum nimis perplexum reddunt, dumque collusorum dexteritatem semper aequalem ponunt, doctrinam hanc ludorum intra limites nimis arctos continent."*[1]

There is lacking to these decisions only to be able to be sustained by good proofs; but where would Mr. Moivre have taken it? I have given general solutions especially where the nature of the Problem has permitted it or has demanded it: one can take the proof at the opening of the Book. It is true that I have given the particular solutions of the five Problems of Mr. Huygens & of some others; but I do not believe that it is always apropos to generalize all, principally that which is easy. Some particular solutions relax the Reader, & instruct the least, when, as happens often, the particular solution contains all the difficulties of the general; often nothing more easy than to generalize, it is an ostentation to always do it & without necessity; I swear that it happens often that a particular resolved example does not contain the path to the general solution; then it is necessary to raise, if one is able, from the particular case to the general; it is also that we believe to have done nearly throughout, I myself yield there to the judgment of the Readers.

I do not understand at all that which Mr. Moivre wishes to say when he reproaches me to have employed some unknowns where it was not necessary at all, & to have encumbered my calculation from it: I know only the single 5$^{\text{th}}$ Problem of Mr. Huygens on which this reproach is able to fall; I employ nearly throughout the method of Combinations, & I have put it solely into use in the first part which is the most considerable by its extent, & by the difficulty of the Problems. If I serve myself sometimes of Analysis, it is that I believe

[1]Huygens first, who I know, has related the rules to the solution of Problems of this kind which in recent times the French author with various examples has illustrated beautifully; but the illustrious men do not appear to have used that simplicity and generality, as the nature of the thing demanded: indeed while they use many unknown quantities, in order that they may represent the various conditions of the players, they render their calculation exceedingly intricate; and while they always set the skill of the players equal, they keep this theory of games within exceedingly narrow limits.

apropos, & that it is agreeable to attempt different ways, & useful to open other routes. If sometimes without serving myself of Analysis I employ some unknowns where one could absolutely be past them, as in Quinquenove, Lansquenet & others, it is in order to ease the imagination of the Reader; I would have been able to use it otherwise, but I have done it by design.

Mister Moivre reproaches me again that by supposing the forces of the Players always equals, I contain the science of probabilities in the Games within some too narrow limits; but beyond that the consideration of this inequality is rather useless in practice, because one does not know & because one can never know exactly the forces of the Players than *a posteriori*, quite imperfectly, & that this same never holds in the Games of pure chance. I know no Problem in which this pretended generality can make difficulty after all that which we have given. Mr. Moivre will see here by some Letters, of which the date is prior to the publication of his Book, that when I myself am advised of it it has not at all embarrassed me.[2]

Finally I can demand to Mr. Moivre why he affects to make the same reproaches to Mr. Huygens, by supposing as a certain thing that he is the first who has given rules for this kind of calculus, & that I have done nothing than to make some applications of it.

In every other matter than this here Mr. Moivre would do me an honor that I merit not at all, to put me beside Mr. Huygens; but as he does it only to attribute to us the same faults, & in order to say that I myself am limited to make some applications of the rules that Mr. Huygens gives, I believe to be excused from thanks; that he permits me on the contrary to call on his judgment, & to believe that I have better employed my time than he says it.

I am persuaded that I have drawn no help or that I have drawn very little from the rules supposed by Mr. Huygens, for the reason that I find none, nor any example may be able to furnish it, if it is not able to be the last Proposition; because the solution of the Problem where one seeks in how many coups one can wager in the end to bring forth sonnez,[3] proceeding only by leap & in fumbling, cannot serve for a model. That which gives to him the part of the Players to whom an unequal number of points are lackingon the game, has the same fault. His method, although natural, gives not at all the general solution, although one is able to find from it by following another route; & it is to these two Problems that the Treatise of Mr. Huygens is limited: his Lemma, although quite useful, is nearly an Axiom, being only a rule of common sense.

I do not dare seek what reasons Mr. Moivre is able to have had in order to envy me the small honor of having in some sort deepened a matter hardly touched upon, & entirely forgotten for sixty years. I will not limit myself to justify here that which I have advanced in my Preface touching the novelty of the matter that I treat. As it pleases to Mr. Moivre to doubt it, some Readers could be able to take on that occasion to believe that I have wished to impose to the Public, by giving for new that which is not it at all, & that which is found elsewhere.

As it is principally this appeal of novelty that has engaged me to write, I myself propose to enter into an exact detail of all that which is done on this matter, or which can have relation, so that one is in a state to judge it; & as one could believe that Mr. Moivre, better educated than me, has had reason to attribute the first views that one has had on this matter to Mr. Huygens rather than to Mr. Pascal, I will set again the Reader into a state to pronounce below.

---

[2] See my Letter of the first of March 1712.

[3] *Translators note*: To cast two 6's with two dice.

These facts being not at all completely personal, & having some relation to the History of Mathematics, of which the calculus of probabilities & of chances are going perhaps to become a considerable part, I flatter myself that this dissertation will not be disagreeable.

In 1654 one will propose to Mr. Pascal these two Problems.

1 ° . There lacks to two Players a certain number of points one demands their lots. 2 ° . One demands in how many coups one can undertake to bring forth sonnez with two dice: I do not know by what way Mr. Pascal resolved this last Problem; but his solution was correct, because he will find that he has the advantage to undertake it in twenty-five coups, but that there is the disadvantage to undertake it in 24 coups, this which is true, & will give however much scandal to Mr. le Chevalier de Meré, friend of Mr. Pascal, & good wit of these times, who was not able to agree to it. Here are the terms of Mr. Pascal in his Letter to Mr. Fermat of 29 July 1654.

> "He said to me therefore that he has found falsity in the numbers for this reason; if one undertakes to make a 6 with one die, there is advantage to undertake in four coups, as of 671 to 625. If one undertakes to make sonnez with two dice, there is disadvantage to undertake it in 24 coups; nevertheless 24 is to 36, which is the number of faces of two dice, as 4 is to 6 which is the number of faces of a die. Here is what was his great scandal, which made him to say haughtily that the propositions were not constants, & that Arithmetic was refuted."

It is from this Chevalier & also of this difficulty that Mr. Pascal wishes to speak without doubt in this same Letter, page 181 of the posthumous Works of Mr. Fermat, where one finds these remarkable words of a man such as Mr. Pascal.

> "I have not the time to send to you the demonstration of a difficulty which astonished much Mr. N. . . he is very good mind, but he is not Geometer; this is, as you know, a great fault."

There is place to believe that Mr. Pascal had resolved this Problem in the same manner as Mr. Huygens has done since. If his solution had been general, he would have apparently given it, & besides one did not know then the secret to use logarithms for the resolution of the equalities, this which is nonetheless absolutely necessary in order to resolve this Problem generally & methodically. That of the points appeared to him more difficult, & pleased him so much, that he will propose it to his friends Messers Fermat & Roberval. It did not appear that this last, quite inferior to Mr. Fermat, has resolved it: for Mr. Fermat he came to it in the end by serving himself with Combinations. Mr. Pascal who had not at all followed this route was entirely astonished from it, & sustained some time that it would not lead to the solution in the case of three or many Players. This dispute will give to them place to make diverse researches on the matter of Combinations, they both will find on this subject some very beautiful Theorems. One sees some of them in the posthumous Works that I just cited; but Mr. Pascal will penetrate the most forward, this is that which appears through his Treatise entitled *Triangle Arithmetique*, completely filled with reflections & discoveries on the figurate numbers of which I believe him the inventor, because he cites no person. As this small Book is rather rare, & as one finds all that which one would know before us on the calculus of chances & on Combinations, I am going to make an extract.

This Book is a Compilation of many small Treatises which were found printed at the death of Mr. Pascal among his papers; among many matters which he treats, here are those which have the most relation with ours.

The first Treatise is on the figurate numbers in general, it teaches their generation, & to find some properties.

One finds in the one which follows, entitled: *des Ordres Numeriques*, all that which one sees in the first part of our Essay until page 16. As Mr. Pascal employed not at all algebraic expressions, these are some canons where we give some formulas: the tour of demonstration is also quite different. I forgot to say that in regard to his beautiful Theorem:

> "A number of any order as this is being multiplied by the preceding root, & divided by the exponent of its order, gives for quotient the number of the following order which precedes this root."

Mr. Pascal associates Mr. Fermat to the honor of the invention.

Among all these small Treatises there are seen two quite short on Combinations, one Latin, the other French; nearly all is reduced to resolving in many ways what all return to the same thing the Problem which follows,

> "Datis duobus numeris inaequalibus invenire quot modis minor in maiore contineatur."[4]

One sees also a small Treatise which has for title: *Usage du Triangle Arithmetique, pour trouver les puissances des binomes & apotomes.* This Problem is quite curious, the Author adds the solution of this beautiful Problem,

> "Datis quotcumque numeris in progressione a quovis numero inchoante invenire quarumvis potestatum eorum summam."[5]

One can compare his solution with those that one finds here pages 19, 21 & 63.

Finally the Author resolved in three different ways the Problem of points between two Players who have an unequal number of points. This Problem is the first of this nature by which I know that any Geometer has ever thought: here is the first method. He begins with the case where one of the two Players would play for one point, the other for two. He determines next the case where each of the Players would play for two points, next the case where one would play for three points, & the other for two, & thus in sequence step by step, being able to find each case only by means of all the preceding, by beginning with the simplest case where each of the Players are lacking an equal number of points, this which gives to each of them the half of that which is in the game. He serves himself next in order to resolve this same Problem, with Combinations & with his Arithmetic Triangle; but it seems to me that these two methods are only one, the numbers that the Combinations give belonging to the figurate numbers of which the Arithmetic Triangle is one kind.

Since that time many persons have given some Treatises on Combinations, & have spoken of the figurate numbers; but I know none of them who has pushed these matters further than Mr. Pascal. The Authors who have written on it are, it seems to me, Fr. Prestet, Fr. Tacquet, Mr. Wallis. One is able to consult them. I do not believe that one finds anything considerable which goes beyond the discoveries of Mr. Pascal. It is true that Mr. Wallis has made a scholarly usage of the figurate numbers for the quadrature of curves in his Arithmetic of the infinite; but this likewise is yet common to it with Mr. Pascal who has made an admirable use in his Traité de la Roulette. The most beautiful Proposition of the Arithmetic of the infinite, & which is expressed thus in Latin:

---

[4]With two unequal numbers given to discover in how many ways the smaller may be contained in the greater.

[5]Given however many numbers in progression with any starting number to discover the sum of any power of them whatsoever.

> "Invenire rationem quam habet summa seriei cuiusvis numerorum figuratorum ab unitate incipientium ad summam totidem ultimo aequalium," [6]

is demonstrated only by induction by Mr. Wallis: one will find it here as Corollary of a very useful Proposition that we have resolved, &, I believe, well demonstrated, page 63. I will say on occasion, not having been able to say besides, that in the solution of this Problem I have done only to follow without knowing it, the views that the illustrious Mr. de Leibnitz has had since a long time, in order to find the sums of a series of number which will have a last difference constant: this is that which I have learned through the Book entitled: *Commercium Epistolicum*, &c. printed by order of the Messers of the Royal Society of England, who have given me the honor to send to me a Copy in the month of April of this year. The first part of this Book was then imprinted.

In 1657 Mr. Huygens gave his Treatise entitled: *Ratiocinia de Ludo Aleae*. One finds at the end of the Book of Mr. Chotten,[7] entitled: *Exertationes Geometricae*. I have nothing to add to that which I have already said, I will do only to observe that Mr. Huygens himself recognized that he has thought these Problems only the occasion of the solutions that some French Geometers had already given of them: Here are his terms:

> "Sciendum vero quod iampridem inter praestantissimos tota Gallia geometras calculus hic fuerit agitatus, ne quis indebitam mihi primae inventionis gloriam haec in re tribuat."[8]

But beyond that it is constant that Mr. Pascal is the first who has written on this matter, I doubt not at all that those who would read the Treatise of Mr. Huygens on one hand, & on the other the Treatises & Letters of Mr. Pascal who I have indicated, may agree that Mister Pascal has pushed furthest this matter, since having resolved both of same Problems, Mr. Pascal, beyond the method of Mr. Huygens that he has employed for the points, has again put into use that of Combinations which is much better, & has given place to some very great discoveries.

In 1685 Mr. Jacques Bernoulli proposed in the Journal of the Sçavans of France the two Problems which follow.

> "Two Players A & B play to who will bring forth first a certain point. A plays first one coup, B a coup; next A plays two of them, & B two; next A plays three of them & B three, & thus in sequence alternately; or else A plays first one coup & B plays two of them; next A plays three of them, next B plays four of them, & thus in sequence until one of the two Players has won, one demands their lot."

Mr. Bernoulli seeing that no person during the interval of five years had undertaken to resolve them, gave the solution of it in the Journal of Leipzig in the month of May 1690, but without analysis & without demonstration. A short time later Mr. Leibnitz, *vicem reddens*,[9] as he says, in order to render the parallel to Mr. Bernoulli who had given in this Journal the Analysis of the curve *descensus aequabilis*,[10] serving himself of the integral calculus

[6] To discover the ratio which the sum of any series of figurate numbers starting from unity has to the sum of as many equal to the last.

[7] Francis van Schooten.

[8] To be understood truly what long ago among most excellent Geometers in all France this calculus had been considered, that not anyone attribute to me the glory of first discovery that is not due in regard to the thing.

[9] delivering a duty

[10] of uniform descent, the brachistochrone.

to it, undertook to discover the basis of the solution of Mr. Bernoulli, this which was very good, but of a succinct manner, & which leaves, it seems to me, much to conjecture.

It was perhaps here the occasion to remark that the matter that I treat must not be too familiar to the Geometers, since of the Problems easy enough in comparison to the greatest part of those that one finds here, remain so many years without solution, although proposed by Geometers such as Mr. Huygens & Mr. Bernoulli. I could perhaps again take advantage against Mr. Moivre from the same bounty of his own Work, so superior to all that which Messers Huygens & Pascal have given in this genre; but I love better to leave to make these reflections to the Reader, than to make them myself.

I have found in the Philosophical Transactions a Memoir in which one is proposed to estimate the probability that gives the testimony of men, either that it is transmitted the oral way or by writing it; but what is one able to find on that? That if one hearsay gives $\frac{a}{b}$ of possibility, one hearsay of one hearsay will give $\frac{a}{b} \times \frac{c}{d}$ of possibility, if the testimony of the second is not of the same force, & $\frac{a^2}{b^2}$ if it has so much of so much authority, & other things of this nature, this is true & even evident; but what can one conclude from it, & how to make application of these Theories? I believe that this is impossible, this is however that which an English scholarly Geometer has undertaken, & infinitely beyond, of whom the honest & accommodating manners for me have prejudiced me in favor of his heart, so much that his excellent Works have given to me esteem for his intelligence. The Book of which I wish to speak has for title: *Philosophiae Christianae Principia Mathematica*: Mr. Craig is the Author of it. This Work is quite curious & has much relation to our matters in order to not say something here. The Author proposes principally to prove against the Jews the truth of the History of JESUS-CHRIST, & to demonstrate to the Libertines that the part that they take to prefer the pleasures of the world so slight & of so short duration, to the same uncertain expectation of the goods promised to those who would follow the Law of the Gospel, is not a reasonable part nor conformed to their true interests.

This last part seems to me well proven; but as it was too easy, the Author has adorned it with a quantity of beautiful & scholarly Theorems, in which he compares the duration of the pleasures with their intensity; but all this is only a game of a scholarly Geometry, which is formed from the difficulties in order to have the pleasure of surmounting them.

The execution of that which the Author is proposed in the first part is certainly impossible, & I cannot believe that the Author has good sense, when from his hypotheses he draws these amazing consequences.

> "Tanta itaque est hodie probabilitas Historiae Christi quantam habuisset ille qui ipsius Christi temporibus viva tantum voce eandem a 28 Discipulis acciperet."[11]

And this other:

> "Ergo necesse est ut veniat Christus antequam elabantur anni 1454 a nostro tempore, &c."[12]

Mr Craig has well seen without doubt that all these consequences were truths only by virtue of the arbitrary assumptions extended from the truth perhaps by half, by the third or by the fourth, &c. I say perhaps, because what way to know it? he would have been able by making other hypotheses, equally verisimilitudes, to find some quite different numbers.

[11] Thus the probability today of the History of Christ is as much as that one must have who alive in the times of Christ himself receives with a voice only the same from 28 Disciples.

[12] Therefore it is necessary that Christ come before 1454 years elapse from our time, &c.

For me I find the design of Mr. Craig laudable & pious, & the execution so joyous as it would be able to be; but I believe this Work much more proper to exercise some Geometers, than to convert the Jews or the incredulous. That which one can conclude more certain after the reading of this Work, is that the Author is very subtle, that he is a great Geometer, & that he has much spirit. The clarity of the Mathematics & the holy obscurity of the faith are of things too opposed: I do not believe that a person may succeed to ever make the mixture of it.

In 1679 when the Game of Bassette was in reign in France, & especially at the Court, Mr. Sauveur scholarly Geometer sought the chances of the Game, & gave some Tables of it that one finds in the Journal of Sçavans of this year: I have not at all spoken of it in my first Edition, because I was not instructed in it. I repair now with much pleasure the fault that I did then involuntarily: one finds in the Journal only some dry formulas without analysis & without demonstration. Mr. Sauveur has given me the pleasure to communicate them to me a short time after our analysis had been rendered public, & there is joined a demonstration of my formulas on Treize: all this was excellent & worthy of the reputation that he himself acquires by the good discoveries on the Theory of Music.

Jerome Cardan has given a Treatise *De Ludo Aleae*; but one finds only erudition & some moral reflections.

I have learned that Mr. Hudde & the famous Mr. Witt, Pensionaire of Holland, had given some calculations for the interest of life annuities which are proper to some persons of different ages. There is the appearance that that which they have given is a little different from that which I have reported from Mr. Halley, & suppose that one knows the different degrees of mortality of human kind. For the rest this matter has only a relation quite extended with this here: that which it has of calculation is quite easy, & depends nearly uniquely on the solution of this Problem that Mr. Leibnitz has resolved in 1683 in a very elegant manner.

> "To find the present value of any sum payable at the end of any number of years."

The great difficulty is to have some Tables exact parallels to that of which Mr. Halley is served for the foundation of his calculations. It would be wished that the observations were continued for a great number of years, & that one made similars to it in the many great Cities of Europe.

Caramuel has worked on the calculus of chances, but with little success; I have read his Treatise, entitled: ΚΥΒΕΙΑ, & I am of the sentiment of Mister N. Bernoulli who says that this Work is only a tissue of paralogisms.

I will end this Dissertation with a reflection of the illustrious Mr. Leibnitz; it is drawn from his Theodicy,[13] & confirms that which we have said in our Preface of the novelty & of the importance of the matter that we treat here.

> "One is not at all yet advised of this kind of Logic which must rule the weights of the truths, & which would be so necessary in the deliberations of importance."

And more below:

> "There is nothing more imperfect than our Logic when one goes beyond necessary arguments; & the most excellent Philosophers of our times, such as the Authors of the Art of Thinking, of the Research of the truth, & of the Essay on Understanding, have been quite extended from us to

[13] Discourse on the Conformity of the Faith with reason.

> make the true paths proper to aid this faculty which must make us weigh the appearances of the true & of the false, without speaking of the Art of invention, where it is yet more difficult to attain, & of which one has only some quite imperfect samples in Mathematics.

The authority of Mr. Leibnitz is in these matters a nearly complete proof; one knows that he is perfectly instructed in the state of the Sciences, & that no person works more usefully than he to perfect them.

I have found in the first volume of the Académie Royale de Berlin, what appeared two or three years ago where this Scholarly man speaks of a Chinese game which has much relation with our Chess; he proposes next some Problems on a game which has been fashionable in France it is twelve or fifteen years ago, which is named SOLITAIRE. Says Mr. Leibnitz,

> "Saepe notavimus, nusquam homines ingeniosiores esse quam in ludicris, atque ideo Ludos Mathematicorum curam mereri non per se, sed artis inveniendi causa. Ludi casus fortuiti inter alia prosunt ad aestimandos probabilitates, &c"[14]

And he adds on the occasion some views which he gives for SOLITAIRE.

> "Sed ego ad profectum inventricis artis ludendi artificia detexisse non ludum valde exercuisse laudarem."[15]

I subscribe in all to these reflections which appear to me very judicious; I have reported them because of the authority that Mr. Leibnitz himself has acquired among the People of Letters, & that he has so justly merited.

[14] We have noted often, on no occasion men to be more clever than at play, and therefore a Game to deserve the attention of Mathematicians not by itself, but because the arts must be invented. The cases of a game of chance are useful among themselves for probabilities to be estimated, &c.

[15] But I must recommend to the advancement of the art of invention to have revealed the artifices of gaming not to have practiced games vigorously.