REMARQUES
*sur l'utilité & l'étendue du principe par lequel on estime la probabilité des causes**

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In a preceding memoir we have recognized a proposition, enunciated by M. DE LA PLACE under the form of a principle, which serves as basis to the theory by which one estimates in a precise manner the probability of causes from the effects.[1]

The object of the actual memoir is to determine the extent of the applications which one can make of this principle, & to remark the importance.

FIRST SECTION
On the utility of the principle of estimation of the probability of the causes.

1. I remark first that the words *cause* & *effect* are taken here in an extremely general sense. Because the same principle of estimation would apply as well to *sign* & to *signifying*. This consideration tends to extend the applications of the theory, & it is under this point of view that makes sense the utility of it.

2. A more important remark is that the principle of M. DE LA PLACE prevents the nearly inevitable errors in the cases of all kinds to which one can be tempted to apply this theory. This can be proved first by a rather simple reasoning, & next by the fact.

3. By a rather simple reasoning, one is assured of the great utility of the general methods of calculation in order to prevent error. They have in mathematics the same

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[1]In order to not send the reader back to this preceding memoir, we will recall here the principles which are exposed there.

An effect having been produced by an unknown cause, the probability in favor of any determined cause, is proportional to the probability that the effect would have had by the action of this supposed cause acting.

Whence it follows that when one knows the probability of the effect by each of the possible causes, one can determine the probability of each of these causes by the effect which has been produced.

And if, after having determined the probability of each cause, one multiplies this probability by the probability to produce the effect by this same cause, one determines the probability of the return of this same effect.

These principles, of which a simple enunciation can not make understood the use, is deduced from the general principles of the theory of probabilities, thus as we have shown it in the memoir cited.

advantage, that the determination of the laws of nature has in physics, & that of the maxims of conduct in morals. In the particular cases, they serve in some way as beacon, & lead with more surety than all the resources of genius, deprived of this means of clarity.

4. Also henceforth one must expect at no point to see educated men fall into error more frequently, by calculating, according to the rules of Stochastic, the probability of causes, than by calculating that of the effects. But until the epoch where the method of this calculation has been definitely fixed, the fact proves that in the estimation of the causes, the errors have been much more frequent.

5. And first JAC. BERNOULLI & all those who have followed his march, have made, one must say it, only vain attempts to arrive to this estimation. Their methods, however good & useful that they were in some other regards, reposing not at all on the *etiological principle*, gave finally only the estimation of the effects by the cause. It is that which one can recognize by casting the eyes on the grand problem, in appearance experimental, which is resolved at the end of the *Ars conjectandi*, reprised by MOYVRE, BAYES & PRICE &c., & treated by these diverse mathematicians in a more exact manner, but not on some other principles. In this problem, the question is to determine according to the knowledge of the nature of the die, the probability that in playing a very great number of coups, one will obtain some results contained between certain limits, neighbors of a ratio which the faces of the die indicate. Thus one concludes in this problem from the cause to the effects, & not from the effects to the cause.

6. These Stochasticians are themselves separated in the end, but have not committed error. It would be difficult to make the complete enumeration of those who have had a worse lot. Unhappiest are without doubt those who, having had in view of the general theory (& not simply some particular application), have traced the rules of it in a faulty manner. As one owes much esteem besides in their work, we ourselves will abstain from citing them here.

7. In the particular applications, the errors are themselves multiplied. We will expose, by form of an example, only those which having already been revealed & generally known, can be remarked without being harmful.

8. In the evaluation of the value of testimony of two simultaneous witnesses, it appears that, until LAMBERT, one has not at all use of another artifice, than to take the complement of the formula employed for the successive testimony. One followed in this regard the trace of the appreciation of the conspiratory evidence, such as JAC. BERNOULLI had done it. If one had known the true method to estimate it from the causes, one would not have failed to examine before all if this case corresponded; & one would have seen the accord among the witnesses is an event posterior to any cause which has determined the dispositions: so that the question here is to estimate the cause by the effect. One would be thus fallen back all naturally & without effort into the method that LAMBERT has found by an effect of this rare sagacity which characterized his genius.

9. In a work of MR. HAYGARTH, entitled *Recherches sur les moyens de prévenir la petite vérole &c., traduit par* M. DE LA ROCHE *à Paris en 1786*, this physician

established on some solid arguments, which *if two persons are exposed for the first time to the contagion of small pox, it is rare that they both escape the malady;* & that *if three persons are exposed all at once, it is much more rare still that all three avoid it.*[2] But having wished to give more precision to his arguments, he appealed to a geometer of his friends, & he obtained from him the following results. "If there is one person out of twenty who can not take small pox, it is evident that for whoever has not had it, there are nineteen chances in order to be attacked by it, & one alone in order to avoid it. We have therefore place to believe, however violent that an epidemic of small pox be in a city, that if a child has not taken the malady, there is nineteen against one that he has not been exposed to the contagious miasma. If two in a family have escaped, the probability that both have not been exposed to the infection is more than four hundred against one; if there are three, it is more than eight thousand to one."

The translator (in recognizing the solidity of the thesis & of the general reasoning of its author) has well sensed that the friend of M. HAYGARTH was mislead in this application of the calculation of the probabilities, & he has demonstrated it by a judicious discussion.[3] It will suffice for our end to remark here that this error had been avoided by the etiological principle. Indeed, one sees first that it is a question of an estimation *a posteriori* & not *a priori*; the concern is to determine the probability of a cause by the effect, & not of an effect by its cause.

The event or known effect which serves as basis to this calculation, can be enunciated thus: *A determined person has not had small pox at all.* And for the following cases: *Two persons, three persons, ...* n *persons have not had small pox at all.*

One seeks the cause of this effect, & one has in view to determine the probability that this comes from that which *this person* (or these persons) *has* (or have) *not been exposed* (or exposed) *to the contagion*.

Admitting (as one must) that there is no spontaneous small pox, one will have for the probability of not taking small pox when one is not exposed, certitude or unity. And consequently also this same relative probability in favor of this cause.

Admitting next (with SAUVAGES) that of 20 persons exposed to the contagion, one alone escapes[4]), one will have $\frac{1}{20}$ relative probability in favor of this cause.

And as these two causes exhaust the field of possibility (seeing that one must say necessarily that the person has been exposed or that he has not been); it appears that one can in surety say that the probability that a single person exempt from the small pox has not been exposed to the contagion, is $\frac{20}{21}$, that two persons exempt all at once have

[2] *Translator's note*: *Researches on the means to prevent small pox &c.,* translated by Mr. de la Roche at Paris in 1786.

[3] I am going to transcribe here the end of the note of MR. DE LA ROCHE on the calculation of the friend of DR. HAYGARTH. "From that which out of twenty persons exposed to the contagion of small pox a single one escaped it, one can not conclude that there are odds of nineteen against one, that each person who has not had small pox has never been exposed to take it. There is no reason, no connection whatever between the consequence & the premise. If one has observed that out of twenty persons who gamble at a table of pharaon, there are nineteen of them who ruin themselves, one could not deduce from it that there are odds one against nineteen that each man of whom fortune is not disturbed, has not gambled at pharaon, nor that there are odds of nineteen against one, that this man is a player. This consequence would appear at first glance too absurd. It would not be more however than the preceding; because it is precisely the same reasoning which would lead to both."

[4] It is probable that this ratio of 20 to 1, determined by SAUVAGES, is too small. But it is not that which there is question to discuss here.

not been exposed, $\frac{400}{401}$, three, $\frac{8000}{8001}$.[5] Different results, but extremely reconciled, with those of the friend of M. HAYGARTH; although the principles from which the one derives them, are mal-assured & subjects to the objections that the translator himself has made.

This comes, I think, from this that the anonymous calculator has had some implicit sentiment of the etiological principle, which in this particular case, has sufficed in order to prevent greater deviations (§ 20).

10. It seems to me that these two examples, (one drawn from testimony, & the other from a contagious malady) suffices in order to justify our assertion; namely, that the fact proves that the recognition of the etiological principle & its formal enunciation can prevent many errors in the applications of this theory. I am going likewise to advance, that before the clear exposition of this principle, one would find very few examples of happy attempts in order to evaluate the probability of the causes by the effects, & that to the contrary one would find many unhappy attempts, & nearly as many falls as of steps.

SECOND SECTION
On the limits of this principle.

11. I wish to prove now that all this machinery of method, so beautiful & so useful, by which one arrives to calculate the probability of the causes by the effects, supposes an anterior estimation of this same probability, & that in particular in all the interesting applications that one can make of this calculus, we are always necessarily guided by an instinct of persuasion, inappreciable in degree, & that all our reasoning on this object depend on our confidence in a principle of belief only the calculus of probabilities can not estimate.

12. I will make a first remark, which is not particular to the estimation of causes, but which is related to all the consequences which one can draw from the general principles of Stochastic. Now, as it is well proven that the etiological principle derives from these common principles, & that all the art to estimate the probability of causes derives from the etiological principle; it is clear that all that which will be said on the principles of Stochastic in general, will be applied to this particular branch of the estimation of causes, which derives from it by a sequence of consequences.

I say therefore that there is in man a principle (which one can name *instinct of belief*) that each application of the calculus of the probabilities supposes. As much as one reasons in the abstract, one is not at all invited to render account of the reasons on which one bases the estimation of the probability of a chance. But in all the concrete cases or particulars, one can determine this probability only by way of experience. Now the past cases were not linked to the cases to come, we consider them as giving before the same results, which by this sentiment, insensible but irresistible, which make us admit the constancy of the laws of nature. If one takes the example of the die, one will see that in order to arrive to give to it the construction that the player has in view, the artist finally has been able to be guided only by some anterior experiences on some such aleatory instruments, & on the one in particular. When therefore as he

[5] See additional note at end.

hopes the same effects, it is founded on a foresight of which the reason can not be evaluated by the calculus. And it is in vain that one would wish to exit from this circle by remounting from cause to cause: because finally each probability which one will wish to estimate stochastically, will be reduced to this emblem. One determines the probability of life by some empirical tables: it is likewise of the probability of the meteorological phenomena, & others.

13. My second remark on this object is particular to the estimation of the probability of causes, & especially to that part of the theory by which one estimates the probability of the return of an observed effect.

In this problem (that MR. DE LA PLACE has resolved first, & that MR. L'HULIER has simplified & at the same time generalized) one begins by estimating the probability that the observed effect has been produced by each of the possible causes: then supposing certain that the same cause which has acted, will act; one infers from it the probability of the return of the event by either of these diverse causes. It is therefore only to the favor of this hypothesis: *the same cause which has acted, will act;* (or, *there is constancy in the laws of nature*); that this problem can be resolved. If one passes from the abstract to the concrete; if one envisions this problem in its useful applications; one will recognize that under this new reason, the conclusions of the stochastic calculus, in the estimation of the probability of the causes, depend on an inestimable principle of belief. They depended on it already, as all that which derives from the general principles of Stochastic: they depend on it more, as wishing to determine the influence of an observed effect on the probability of a like future event.

14. One would not know how absolutely to conceive by which path one could arrive to reason on some hypotheses of inconstancy in the laws of nature; or how one could claim to report total persuasion in that which is a result, expressed or tacit, of the calculus of probabilities.

THIRD SECTION

Comparison of some results of calculation with the perceived of common sense.

15. In the mixed sciences, these sorts of comparisons have many advantages. I am going therefore to remark how certain results of the etiological principle accords with common sense.

16. If one obtains a situation at the epoch where there has been no coup played; knowing the nature of the dice, one could estimate the probability to bring forth an *ace*, in playing with one of these dice, reputed equally possible. And this estimation being supposed made appropriately; if one obtains a situation in the epoch where a first coup has already given an *ace*, one must presume naturally that one is served with the die which has the most *ace* faces, rather than of the other. In a way that one has some tendency to believe that one is rather served of the die which has the most *ace* faces, or in its defect with the one which has the most of them after it, & thus in sequence. Whence there results, by admitting that one plays constantly with the same die, that there is a little more expectation to bring forth *ace*, than if one knew not at all the result of a first coup.

And in effect, if one compares, by the calculus, the probability *a priori*, & that *a posteriori*, under the same hypothesis; one will find always this one greater. (§ 26).

Example. Let $n = 2$ be the number of faces of each die. And let there be only two equally possible dice.

Number of *ace* faces of the I$^{\text{st}}$ die	1.
Number of *ace* faces of the II$^{\text{nd}}$ die	2.

Probability *a priori* to bring forth *ace* by either of these dice.

$$\frac{1}{2} \times \frac{1}{2} + \frac{1}{2} \times 1 = \frac{3}{4}.$$

Probability *a posteriori* (after a first coup has already brought forth *ace*) to bring forth *ace* by playing a new coup.

$$\frac{1}{3} \times \frac{1}{2} + \frac{2}{3} \times 1 = \frac{5}{6} > \frac{3}{4}.$$

Thus one can say, that in this first regard, the calculus does nothing than to evaluate the tendency in favor of the *ace*, as simple good sense would inspire, by virtue of the known result of a first experience.

17. If one compares the effect of this first experience on the opinion relative to the result of a new coup to play, in diverse circumstances; one will sense that the favor which results from it for *ace* must be the same, when the ratio of the faces of the die is the same, whatever be the number n of all the faces (which is the same for all the dice). In truth, when n is great with respect to the number of *ace* faces, both of the probabilities *a priori* & *a posteriori* are small, but the tendency that the experience gives to expect the return of the *ace* face, must be of an intensity proportional to this small probability.

This is also that which the calculation demonstrates (§ 27).

Example. Let $n = 12$.

Number of *ace* faces of the I$^{\text{st}}$ die	1.
Number of *ace* faces of the II$^{\text{nd}}$ die	2.

Probability *a priori*	$\frac{1}{2} \times \frac{1}{12} + \frac{1}{2} \times \frac{2}{12} = \frac{3}{24}.$
Probability *a posteriori* (*ace* having been brought forth)	$\frac{1}{3} \times \frac{1}{12} + \frac{2}{3} \times \frac{2}{12} = \frac{5}{36}.$

Now $\frac{3}{24} : \frac{5}{36} = \frac{3}{4} : \frac{5}{6}$. Ratio found in § 16.

18. If the ratio of the *ace*, of one & the other die differs in the cases which one compares; a first experience in which one has brought forth *ace*, must influence more on the presumed result of a new experience to make, there where this ratio is more unequal. Because when this ratio tends to equality, experience gives no longer any motive to believe in this return, because the reasoning on which this tendency is founded, (& that we have developed in § 16), finds no longer here its application.

The calculation is here in accord with common sense (§ 28).

First example. Number of *ace* faces of the I^st^ die 1.

Number of *ace* faces of the II^nd^ die 1.

Probability *a priori* $\frac{1}{2} \times \frac{1}{2} + \frac{1}{2} \times \frac{1}{2} = \frac{1}{2}$.

Probability *a posteriori* (*ace* having been brought forth) $\frac{1}{2} \times \frac{1}{2} + \frac{1}{2} \times \frac{1}{2} = \frac{1}{2}$.

Second example. Let $n = 14$.

Number of *ace* faces of the I^st^ die 4.

Number of *ace* faces of the II^nd^ die 7.

Probability *a priori* $\frac{1}{2} \times \frac{4}{14} + \frac{1}{2} \times \frac{7}{14} = \frac{11}{28}$.

Probability *a posteriori* (*ace* having been brought forth) $\frac{4}{11} \times \frac{4}{14} + \frac{7}{11} \times \frac{7}{14} = \frac{65}{154}$.

Now in the example of § 16 one had for the ratio of the *ace* in each die $1 : 2 < 4 : 7$.

And for the ratio of the, *a priori* & *a posteriori* probabilities one had $\frac{3}{4} : \frac{5}{6} < \frac{11}{28} : \frac{65}{154}$.

19. Finally the theory of estimation of the probabilities *a posteriori* furnishes a new & remarkable consequence: it is that *the hypothesis of ignorance of the causes, & the hypothesis of the knowledge of their nature, gives the same results only in the case where one estimates a simple probability*.

We suppose that I know the nature of a die, & that I know, for example, that the *ace* & *non-ace* faces are in number equal. I will have for the first coup the probability $\frac{1}{2}$ to bring forth *ace*.

Likewise, we suppose that one has played yet no coup with a die, which has some faces only of two kinds alone, which I will call *ace* & *non-ace*, & of which I am ignorant of the nature besides, I have the probability $\frac{1}{2}$ to bring forth *ace* in the first coup, rather than *non-ace*.

But if I have in view to bring forth *ace* two coups in sequence; the composed probability of one such event will be no longer the same under either hypothesis.

In the first, (that where the nature of the die is known) the probability in question will be $\frac{1}{2} \times \frac{1}{2} = \frac{1}{4}$.

In the second (that where the nature of the die is unknown), I will estimate the probability of the composed event *a posteriori* by the formula $\frac{p+1}{p+q+2}$ (in which p expresses the *ace*, q the *non-ace*). And the composed probability which will result from it, will be $\frac{1}{2} \times \frac{2}{3} = \frac{1}{3}$.

By the same process one finds that the cause being known, & the faces *ace*, *non-ace*, being in number equal; the probability to bring forth *ace* four times in sequence is $\frac{1}{16}$. But the cause being unknown, one will find this probability $= \frac{1}{5}$.

All this is quite natural. Because although for the first coup, there is no difference in the situations of the one who knows that the ratio of equality among the causes exists really, & the one who is absolutely ignorant what this ratio is: it is certain that since the second coup, there must be some difference between their positions: one has made some progress towards the knowledge of the cause, the other has made none from it.

Thus through some unknown causes, it is easier to suppose the sequences continues in a like effect, than through some known causes which act equally.

This will become sensible by an example. We pose that I am absolutely ignorant if a determined planet (*Mars*) contains, or does not contains some kind of organized being precisely equal to some one of those who inhabit our globe. We pretend an observer who is transported onto this planet. Before realizing his observation, there is the probability $\frac{1}{2}$ to meet one such kind. But after having met one of them since his first observation, there is no person who presumes only he will encounter two or three of them, with some degree of greater possibility. Thus, although there is only probability $\frac{1}{2}$ for the first event, there will be a composite probability for the subsequent, which will be stronger than if one would multiply together a sequence of fractions of which each was $= \frac{1}{2}$.

20. Taking the formula of estimation of the return of an event, (represented by the *ace* face brought forth with one die of an infinite number of faces), namely $\frac{p+1}{p+q+2}$ (p expressing the *ace*, q the *non-ace*); I observe that when p becomes great enough in order that unity (or its double) vanishes before it, this formula differs not at all sensibly from the one $\frac{p}{p+q}$. That is to say, that in this case, the estimation of the return of an event *a posteriori* differs not at all from that which one would make *a priori* in supposing that the experimental ratio is really the one of the causes. And this again is quite natural. Because one knows well that some very multiplied events gives a ratio on which one can count. It is not otherwise that one reasons on the tables of mortality, of meteorology, &c. It is thus that the geometer consulted by MR. HAYGARTH (§ 9) had reasoned (but without recognizing its limits). And this reasoning does not deceive, provided that (as we just said) 2 vanishes before p.

21. But in measure as p decreases, the *a priori* & *a posteriori* estimations differ. And the analogy that has frequently followed the calculators (until MR. DE LA PLACE) deceive much. One believes, in a number of limited experiences, to be able to substitute one of these estimations for the other, as one can do it when one relies on numerous experiences. And one falls into some considerable errors.

For example, if one has made a very great number of coups with an unknown die, & if one has observed as many *aces* as *non-aces*; one can estimate the probability of the return in one or in many coups of *ace* (to certain limits) by the same formula as one would estimate according to the knowledge of the die, supposed made by design in a manner to offer, among their heterogeneous faces, the ratio of equality. Thus making $p = q = 10000$, one would estimate, without sensible error, the probability to bring forth *ace* four coups in sequence by this formula $\left(\frac{1}{2}\right)^4 = \frac{1}{16}$. Because the formula *a posteriori* $\frac{p+1}{p+q+2}$ gives rigorously a fraction, which is the product of four others of which the first is $\frac{10001}{20002}$, & of which the three others differ from them only by the successive addition of a unit to each of their terms. Now $\frac{10001}{20002} = \frac{1}{2}$. And the addition of 3 units to each term alters not nearly the ratio of the two numbers greater than 10000 & 20000.

But if p decreases, the estimation *a posteriori* changes; & in this case the probability increases much.

For example, let $p = q = 1$. One will have the probability to bring forth *ace* four times in sequence $= \frac{2}{4} \cdot \frac{3}{5} \cdot \frac{4}{6} \cdot \frac{5}{7} = \frac{1}{7}$.

And in effect it is quite natural that one has less assurance of equality of chances, when one has played only two coups, that when one has played 20000 of them; & that the return of an *ace* influences more in the first case in order to presume others to come.

22. It is not at all particular to the case of equality among the possible chances. If $p = 20000$, $q = 30000$; the return of four successive *aces* could without risk be estimated by supposing that $p : q$ expresses the ratio of the causes. Consequently, the probability of this composite event could be said $= \left(\frac{2}{5}\right)^4 = \frac{16}{625} = \frac{1}{39}$ very nearly.

But if $p = 2$, $q = 3$; the formula *a posteriori* differs totally. The probability of the composite event grows. It becomes $= \frac{3}{7} \cdot \frac{4}{9} \cdot \frac{5}{9} \cdot \frac{6}{10} = \frac{1}{14}$.

23. This gives the explication of a kind of paradox remarked (without explanation) by MR. DE LA PLACE).[6] When one estimates the probability *a posteriori* to bring forth *ace* four times in sequence, after five coups played, on which one has brought it forth only two times; one finds this probability greater, than when one estimates *a priori* according to the known equality of the *aces* & of the *non-aces*. Indeed, in the first case, it is $= \frac{1}{14}$ (§ 22). In the second case, it is $\frac{1}{16}$. This appears shocking; since the event which has brought forth *ace* only two times out of five, must be presumed that it will return less often; & not the contrary, as it seems that the calculation indicates.

The explication of this paradox is this. If one had made no other experience, the nature of the die (which, by hypothesis, is determined only by experience) would be entirely unknown. Consequently, the probability to bring forth *ace* four times in sequence, would be $= \frac{1}{5}$ (§ 19). But since the experience made, this probability is no longer equal to $\frac{1}{14}$. Therefore it has in fact considerably diminished.

As for the case that one compares to it, where the cause is known; it can not be compared immediately, being of different nature. This case is similar to the one where one estimates *a posteriori* after a number of coups sufficing in order that 2 vanishes before p. Now in this case again, one will find that the experience, which brings forth only $2m$ *aces* out of $5m$ coups, has much weakened the probability of the return of four consecutive *aces*. In effect, when p & q increase much without changing in ratio, being always between them $= 2 : 3$; one finds the probability of four consecutive *aces* $= \frac{1}{39}$ nearly (§ 22); while, when $p = q$ (the one & the other much greater), one has this probability $= \frac{1}{16}$ (§ 21).

24. This is therefore nothing in all this than of very natural. And it appears well that the calculus, set to prove it from common sense, has resisted, & is shown solid & applicable.

FOURTH SECTION.

Some mathematical developments.

These developments refer to §§ 16, 17, 18.

25. 1° Known *Lemma. The sum of the squares of two real unequal quantities, is greater than their double product.*

Symbolically. Let a & b be two real unequal quantities. I affirm that $a^2 + b^2 > 2ab$.

[6] *Ecole normales, 6ième cahier.*

Demonstration.
$$aa + bb = \frac{(a+b)^2 + (a-b)^2}{2}$$
$$2ab = \frac{(a+b)^2 - (a-b)^2}{2}$$
$$\text{But, } (a+b)^2 + (a-b)^2 > (a+b)^2 - (a-b)^2$$
$$\text{therefore, } aa + bb > 2ab.$$

Corollary. $2(aa+bb) > (a+b)^2$. Namely, the double of the sum of the two squares is greater than the square of their sum.

2° *First application.* The double of the sum of the squares of three real unequal quantities is greater than the double of the sum of their products two by two.

Symbolically. Let $a,\ b,\ c$ be three real unequal quantities: I affirm that

$$2aa + 2bb + 2cc > 2ab + 2ac + 2bc.$$

Demonstr.
$$\begin{aligned} aa + bb \quad\; &> 2ab \\ aa + \quad\; cc &> 2ac \qquad (1^\circ) \\ bb + cc &> 2bc \end{aligned}$$

Demonstr. $2aa + 2bb + 2cc > 2ab + 2ac + 2bc$.

Corollary. $3(aa + bb + cc) > (a + b + c)^2$; namely, the triple of the sum of the squares of three quantities is greater than the square of their sum.

3° Thence one shows likewise: that, the triple of the sum of the square of four quantities, is greater than the double of the sum of their products two by two; & one concludes from it that the sum of the squares of four quantities taken four times is greater than the square of their sum.

Generally. The sum of the square of any number of real quantities which are not all equal among them, taken a number of times inferior by unity to the number of these quantities, is greater than the double of the sum of their products two by two.

Whence it follows, that, the sum of the squares of any number of real quantities which are not all equals among them, taken as many times as there are these quantities, is greater than the square of their sum.

26. *Application to* § 16. Let there be n dice, have each a number r faces; & of which the number of *ace* faces are respectively m', m'', m''', $m^{iv} \dots m^N$.

Let it be equally probable that, playing a coup, one makes it with any one of these dice. The probability to bring forth *ace* in this coup is

$$\frac{m' + m'' + m''' + \cdots + m^N}{r} + \frac{1}{n}$$

Let after having played a coup one has brought forth *ace*, the probability to bring forth anew *ace* with the die with which one has played is

$$\frac{m'm' + m''m'' + m'''m''' + \cdots + m^N m^N}{r(m' + m'' + m''' + \cdots + m^{N)})}.$$

Therefore, the probability to bring forth *ace* in a coup before having played any coup, is to the probability to bring forth *ace* after having brought it forth in a coup with the same die, in the ratio of

$$\frac{1}{n}(m' + m'' + m''' + \cdots + m^N) \text{ to } n\frac{m'm' + m''m'' + m'''m''' + \cdots + m^N m^N}{r(m' + m'' + m''' + \cdots + m^{N)}}$$

or of

$$(m' + m'' + m''' + \cdots + m^N)^2 \text{ to } n(m'm' + m''m'' + m'''m''' + \cdots + m^N m^N).$$

But (§ 25) the second of these quantities is greater than the first, therefore also the second probability is greater than the first. Namely, if one has already brought forth one *ace* with a die, it is more probable that one will bring forth anew *ace* with the same die by playing a coup more, that it is it only one will bring forth *ace* in a coup by playing with any die.

27. *Application to* § 17. The expression of the ratio that we just found (§ 26), is independent of the total number of faces; & this ratio remains the same, when the ratios of the number of the *ace* faces in the different dice remain the same.

ADDITIONAL NOTE
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that two persons exempt all at once have not been exposed $\frac{400}{401}$, *three* $\frac{8000}{8001}$. After the words *exempt all at once*, add, & *placed in the same circumstances*.*

*If the circumstances can differ, the probability of having been exposed not at all, which for a person is $\frac{20}{21}$, becomes for n persons $\left(\frac{20}{21}\right)^n$, according to the common rule of the composite probability. So that it decreases without limit by increasing n.

This result being contrary to the conjecture of Mr. Haygarth, it is equitable to suppose that this author had in view the hypothesis of the text, & not that of this note.

Perhaps one would bring together yet more of the intention of this author or of his calculator, if one determined the probability, that all the persons exempt all at once are not found all at once in the case of having been exposed to the contagion (these persons being able to be or not be in the same circumstances). In this case for n persons one would have the probability $1 - \left(\frac{1}{21}\right)^n$ in favor of the assumption that one of these persons at least has never been exposed to the contagion: a probability which increases rapidly & without limit by increasing n.

But if one reflected, one will see that this last aspect filled only very imperfectly the ulterior views of the author, in which the preceding aspects apply themselves better. These ulterior views are found clearly enunciated on page 32 of the work cited, where the author summarizing it expresses himself thus. "We can conclude thence, that if three persons or more, *at the same time & in the same place*, have all escaped from the small pox, they have not been exposed to its infection." This conclusion appears to put us precisely under the hypothesis adopted in the text of our memoir.