Thomas Bayes, "An Essay toward solving a Problem in the Doctrine of Chances." Philosophical Transactions of the Royal Society of London 53 (1764), pp. 370–418.

quòdque ſolum, certa nitri ſigna præbere, ſed plura concurrere debere, ut de vero nitro producto dubium non relinquatur.

LII. *An Eſſay towards ſolving a Problem in the Doctrine of Chances. By the late Rev. Mr.* Bayes, *F. R. S. communicated by Mr.* Price, *in a Letter to* John Canton, *A. M. F. R. S.*

Dear Sir,

Read Dec. 23, 1763.

I Now ſend you an eſſay which I have found among the papers of our deceaſed friend Mr. Bayes, and which, in my opinion, has great merit, and well deſerves to be preſerved. Experimental philoſophy, you will find, is nearly intereſted in the ſubject of it; and on this account there ſeems to be particular reaſon for thinking that a communication of it to the Royal Society cannot be improper.

He had, you know, the honour of being a member of that illuſtrious Society, and was much eſteemed by many in it as a very able mathematician. In an introduction which he has writ to this Eſſay, he ſays, that his deſign at firſt in thinking on the ſubject of it was, to find out a method by which we might judge concerning the probability that an event has to happen, in given circumſtances, upon ſuppoſition that we know nothing concerning it but that, under the ſame circum-

circumſtances, it has happened a certain number of times, and failed a certain other number of times. He adds, that he ſoon perceived that it would not be very difficult to do this, provided ſome rule could be found according to which we ought to eſtimate the chance that the probability for the happening of an event perfectly unknown, ſhould lie between any two named degrees of probability, antecedently to any experiments made about it; and that it appeared to him that the rule muſt be to ſuppoſe the chance the ſame that it ſhould lie between any two equidifferent degrees; which, if it were allowed, all the reſt might be eaſily calculated in the common method of proceeding in the doctrine of chances. Accordingly, I find among his papers a very ingenious ſolution of this problem in this way. But he afterwards conſidered, that the *poſtulate* on which he had argued might not perhaps be looked upon by all as reaſonable; and therefore he choſe to lay down in another form the propoſition in which he thought the ſolution of the problem is contained, and in a *ſcholium* to ſubjoin the reaſons why he thought ſo, rather than to take into his mathematical reaſoning any thing that might admit diſpute. This, you will obſerve, is the method which he has purſued in this eſſay.

Every judicious perſon will be ſenſible that the problem now mentioned is by no means merely a curious ſpeculation in the doctrine of chances, but neceſſary to be ſolved in order to a ſure foundation for all our reaſonings concerning paſt facts, and what is likely to be hereafter. Common ſenſe is indeed ſufficient to ſhew us that, from the obſervation of what has in former inſtances been the conſequence of a certain caufe or action, one may make a judgment what is likely to be the confequence of it another time, and that the larger number of experiments we have to fupport a conclufion, fo much the more reafon we have to take it for granted. But it is certain that we cannot determine, at leaft not to any nicety, in what degree repeated experiments confirm a conclufion, without the particular difcuffion of the beforementioned problem; which, therefore, is neceffary to be confidered by any one who would give a clear account of the ftrength of *analogical* or *inductive reafoning*; concerning, which at prefent, we feem to know little more than that it does fometimes in fact convince us, and at other times not; and that, as it is the means of cquainting us with many truths, of which otherwife we muft have been ignorant; fo it is, in all probability, the fource of many errors, which perhaps might in fome meafure be avoided, if the force that this fort of reafoning ought to have with us were more diftinctly and clearly underftood.

Thefe obfervations prove that the problem enquired after in this effay is no lefs important than it is curious. It may be fafely added, I fancy, that it is alfo a problem that has never before been folved. Mr. De Moivre, indeed, the great improver of this part of mathematics, has in his *Laws of chance* *, after Bernoulli, and to a greater degree of exactnefs, given rules to find the probability there is, that if a very great number of trials be made concerning any event,

* See Mr. De Moivre's *Doctrine of Chances*, p. 243, &c. He has omitted the demonftrations of his rules, but thefe have been fince fupplied by Mr. Simpfon at the conclufion of his treatife on *The Nature and Laws of Chance*.

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the proportion of the number of times it will happen, to the number of times it will fail in thofe trials, fhould differ lefs than by fmall affigned limits from the proportion of the probability of its happening to the probability of its failing in one fingle trial. But I know of no perfon who has fhewn how to deduce the folution of the converfe problem to this; namely, " the number of times an unknown event " has happened and failed being given, to find the " chance that the probability of its happening fhould " lie fomewhere between any two named degrees of " probability." What Mr. De Moivre has done therefore cannot be thought fufficient to make the confideration of this point unneceffary: efpecially, as the rules he has given are not pretended to be rigoroufly exact, except on fuppofition that the number of trials made are infinite; from whence it is not obvious how large the number of trials muft be in order to make them exact enough to be depended on in practice.

Mr. De Moivre calls the problem he has thus folved, the hardeft that can be propofed on the fubject of chance. His folution he has applied to a very important purpofe, and thereby fhewn that thofe a remuch miftaken who have infinuated that the Doctrine of Chances in mathematics is of trivial confequence, and cannot have a place in any ferious enquiry *. The purpofe I mean is, to fhew what reafon we have for believing that there are in the conftitution of things fixt laws according to which events happen, and that, therefore, the frame of the world muft be

* See his Doctrine of Chances, p. 252, &c.

the effect of the wiſdom and power of an intelligent cauſe; and thus to confirm the argument taken from final cauſes for the exiſtence of the Deity. It will be eaſy to ſee that the converſe problem ſolved in this eſſay is more directly applicable to this purpoſe; for it ſhews us, with diſtinctneſs and preciſion, in every caſe of any particular order or recurrency of events, what reaſon there is to think that ſuch recurrency or order is derived from ſtable cauſes or regulations in nature, and not from any of the irregularities of chance.

The two laſt rules in this eſſay are given without the deductions of them. I have choſen to do this becauſe theſe deductions, taking up a good deal of room, would ſwell the eſſay too much; and alſo becauſe theſe rules, though of conſiderable uſe, do not anſwer the purpoſe for which they are given as perfectly as could be wiſhed. They are however ready to be produced, if a communication of them ſhould be thought proper. I have in ſome places writ ſhort notes, and to the whole I have added an application of the rules in the eſſay to ſome particular caſes, in order to convey a clearer idea of the nature of the problem, and to ſhew how far the ſolution of it has been carried.

I am ſenſible that your time is ſo much taken up that I cannot reaſonably expect that you ſhould minutely examine every part of what I now ſend you. Some of the calculations, particularly in the Appendix, no one can make without a good deal of labour. I have taken ſo much care about them, that I believe there can be no material error in any of them; but ſhould there be any ſuch errors, I am the only perſon who ought to be conſidered as anſwerable for them.

Mr.

Mr. Bayes has thought fit to begin his work with a brief demonſtration of the general laws of chance. His reaſon for doing this, as he ſays in his introduction, was not merely that his reader might not have the trouble of ſearching elſewhere for the principles on which he has argued, but becauſe he did not know whither to refer him for a clear demonſtration of them. He has alſo made an apology for the peculiar definition he has given of the word *chance* or *probability*. His deſign herein was to cut off all diſpute about the meaning of the word, which in common language is uſed in different ſenſes by perſons of different opinions, and according as it is applied to *paſt* or *future* facts. But whatever different ſenſes it may have, all (he obſerves) will allow that an expectation depending on the truth of any *paſt* fact, or the happening of any *future* event, ought to be eſtimated ſo much the more valuable as the fact is more likely to be true, or the event more likely to happen. Inſtead therefore, of the proper ſenſe of the word *probability*, he has given that which all will allow to be its proper meaſure in every caſe where the word is uſed. But it is time to conclude this letter. Experimental philoſophy is indebted to you for ſeveral diſcoveries and improvements; and, therefore, I cannot help thinking that there is a peculiar propriety in directing to you the following eſſay and appendix. That your enquiries may be rewarded with many further ſucceſſes, and that you may enjoy every every valuable bleſſing, is the ſincere wiſh of, Sir,

your very humble ſervant,

Newington-Green,
Nov. 10, 1763.

Richard Price.

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PROBLEM.

Given the number of times in which an unknown event has happened and failed: *Required* the chance that the probability of its happening in a single trial lies somewhere between any two degrees of probability that can be named.

SECTION I.

DEFINITION 1. Several events are *inconsistent*, when if one of them happens, none of the rest can.

2. Two events are *contrary* when one, or other of them must; and both together cannot happen.

3. An event is said to *fail*, when it cannot happen; or, which comes to the same thing, when its contrary has happened.

4. An event is said to be determined when it has either happened or failed.

5. The *probability of any event* is the ratio between the value at which an expectation depending on the happening of the event ought to be computed, and the value of the thing expected upon it's happening.

6. By *chance* I mean the same as probability.

7. Events are independent when the happening of any one of them does neither increase nor abate the probability of the rest.

PROP. 1.

When several events are inconsistent the probability of the happening of one or other of them is the sum of the probabilities of each of them.

Suppose

Suppose there be three such events, and which ever of them happens I am to receive N, and that the probability of the 1st, 2d, and 3d are respectively $\frac{a}{N}$, $\frac{b}{N}$, $\frac{c}{N}$. Then (by the definition of probability) the value of my expectation from the 1st will be a, from the 2d b, and from the 3d c. Wherefore the value of my expectations from all three will be $a + b + c$. But the sum of my expectations from all three is in this case an expectation of receiving N upon the happening of one or other of them. Wherefore (by definition 5) the probability of one or other of them is $\frac{a+b+c}{N}$ or $\frac{a}{N} + \frac{b}{N} + \frac{c}{N}$. The sum of the probabilities of each of them.

Corollary. If it be certain that one or other of the three events must happen, then $a + b + c = N$. For in this case all the expectations together amounting to a certain expectation of receiving N, their values together must be equal to N. And from hence it is plain that the probability of an event added to the probability of its failure (or of its contrary) is the ratio of equality. For these are two inconsistent events, one of which necessarily happens. Wherefore if the probability of an event is $\frac{P}{N}$ that of it's failure will be $\frac{N-P}{N}$.

PROP. 2.

If a person has an expectation depending on the happening of an event, the probability of the event is to the probability of its failure as his loss if it fails to his gain if it happens.

Suppose a person has an expectation of receiving N, depending on an event the probability of which

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is $\frac{P}{N}$. Then (by definition 5) the value of his expectation is P, and therefore if the event fail, he loses that which in value is P; and if it happens he receives N, but his expectation ceases. His gain therefore is N—P. Likewise since the probability of the event is $\frac{P}{N}$, that of its failure (by corollary prop. 1) is $\frac{N-P}{N}$. But $\frac{P}{N}$ is to $\frac{N-P}{N}$ as P is to N—P, i. e. the probability of the event is to the probability of it's failure, as his loss if it fails to his gain if it happens.

PROP. 3.

The probability that two subsequent events will both happen is a ratio compounded of the probability of the 1st, and the probability of the 2d on supposition the 1st happens.

Suppose that, if both events happen, I am to receive N, that the probability both will happen is $\frac{P}{N}$, that the 1st will is $\frac{a}{N}$ (and consequently that the 1st will not is $\frac{N-a}{N}$) and that the 2d will happen upon supposition the 1st does is $\frac{b}{N}$. Then (by definition 5) P will be the value of my expectation, which will become b if the 1st happens. Consequently if the 1st happens, my gain by it is b—P, and if it fails my loss is P. Wherefore, by the foregoing proposition, $\frac{a}{N}$ is to $\frac{N-a}{N}$, i. e. a is to N—a as P is to b—P. Wherefore (componendo inversè) a is to N as P is to b. But the ratio of P to N is compounded of the ratio of P to b, and that of b to N. Wherefore the same

ſame ratio of P to N is compounded of the ratio of a to N and that of b to N, i. e. the probability that the two ſubſequent events will both happen is compounded of the probability of the 1ſt and the probability of the 2d on ſuppoſition the 1ſt happens.

Corollary. Hence if of two ſubſequent events the probability of the 1ſt be $\frac{a}{N}$, and the probability of both together be $\frac{P}{N}$, then the probability of the 2d on ſuppoſition the 1ſt happens is $\frac{P}{a}$.

PROP. 4.

If there be two ſubſequent events to be determined every day, and each day the probability of the 2d is $\frac{b}{N}$ and the probability of both $\frac{P}{N}$, and I am to receive N if both the events happen the 1ſt day on which the 2d does; I ſay, according to theſe conditions, the probability of my obtaining N is $\frac{P}{b}$. For if not, let the probability of my obtaining N be $\frac{x}{N}$ and let y be to x as N—b to N. Then ſince $\frac{x}{N}$ is the probability of my obtaining N (by definition 1) x is the value of my expectation. And again, becauſe according to the foregoing conditions the 1ſt day I have an expectation of obtaining N depending on the happening of both the events together, the probability of which is $\frac{P}{N}$, the value of this expectation is P. Likewiſe, if this coincident ſhould not happen I have an expectation of being reinſtated in my former circumſtances, i. e. of receiving that which in value is x depending

pending on the failure of the 2d event the probability of which (by cor. prop. 1) is $\frac{N-b}{N}$ or $\frac{y}{x}$, becauſe y is to x as $N-b$ to N. Wherefore ſince x is the thing expected and $\frac{y}{x}$ the probability of obtaining it, the value of this expectation is y. But theſe two laſt expectations together are evidently the ſame with my original expectation, the value of which is x, and therefore $P+y=x$. But y is to x as $N-b$ is to N. Wherefore x is to P as N is to b, and $\frac{x}{N}$ (the probability of my obtaining N) is $\frac{P}{b}$.

Cor. Suppoſe after the expectation given me in the foregoing propoſition, and before it is at all known whether the 1ſt event has happened or not, I ſhould find that the 2d event has happened; from hence I can only infer that the event is determined on which my expectation depended, and have no reaſon to eſteem the value of my expectation either greater or leſs than it was before. For if I have reaſon to think it leſs, it would be reaſonable for me to give ſomething to be reinſtated in my former circumſtances, and this over and over again as often as I ſhould be informed that the 2d event had happened, which is evidently abſurd. And the like abſurdity plainly follows if you ſay I ought to ſet a greater value on my expectation than before, for then it would be reaſonable for me to refuſe ſomething if offered me upon condition I would relinquiſh it, and be reinſtated in my former circumſtances; and this likewiſe over and over again as often as (nothing being known concerning the 1ſt event) it ſhould appear that the 2d had happened. Notwithſtanding therefore this diſcovery that the 2d event

event has happened, my expectation ought to be esteemed the same in value as before, i. e. x, and consequently the probability of my obtaining N is (by definition 5) still $\frac{x}{N}$ or $\frac{P}{b}$ *. But after this discovery the probability of my obtaining N is the probability that the 1st of two subsequent events has happened upon the supposition that the 2d has, whose probabilities were as before specified. But the probability that an event has happened is the same as the probability I have to guess right if I guess it has happened. Wherefore the following proposition is evident.

PROP. 5.

If there be two subsequent events, the probability of the 2d $\frac{b}{N}$ and the probability of both together $\frac{P}{N}$, and it being 1st discovered that the 2d event has happened, from hence I guess that the 1st event has also happened, the probability I am in the right is $\frac{P}{b}$ †.

PROP.

* What is here said may perhaps be a little illustrated by considering that all that can be lost by the happening of the 2d event is the chance I should have had of being reinstated in my former circumstances, if the event on which my expectation depended had been determined in the manner expressed in the proposition. But this chance is always as much *against* me as it is *for* me. If the 1st event happens, it is *against* me, and equal to the chance for the 2d event's failing. If the 1st event does not happen, it is *for* me, and equal also to the chance for the 2d event's failing. The loss of it, therefore, can be no disadvantage.

† What is proved by Mr. Bayes in this and the preceding proposition is the same with the answer to the following question. What is the probability that a certain event, when it happens, will

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PROP. 6.

The probability that ſeveral independent events ſhall all happen is a ratio compounded of the probabilities of each.

For from the nature of independent events, the probability that any one happens is not altered by the happening or failing of any of the reſt, and conſequently the probability that the 2d event happens on ſuppoſition the 1ſt does is the ſame with its original probability; but the probability that any two events happen is a ratio compounded of the probability of the 1ſt event, and the probability of the 2d on ſuppoſition the 1ſt happens by prop. 3. Wherefore the probability that any two independent events both happen is a ratio compounded of the probability of the 1ſt and the probability of the 2d. And in like manner conſidering the 1ſt and 2d event together as one event; the probability that three independent events all happen is a ratio compounded of the probability that the two 1ſt both happen and the probability of the 3d. And thus you

be accompanied with another to be determined at the ſame time? In this caſe, as one of the events is given, nothing can be due for the expectation of it; and, conſequently, the value of an expectation depending on the happening of both events muſt be the ſame with the value of an expectation depending on the happening of one of them. In other words; the probability that, when one of two events happens, the other will, is the ſame with the probability of this other. Call x then the probability of this other, and if $\frac{b}{N}$ be the probability of the given event, and $\frac{p}{N}$ the probability of both, becauſe $\frac{p}{N} = \frac{b}{N} \times x$, $x = \frac{p}{b}$ = the probability mentioned in theſe propoſitions.

may

may proceed if there be ever ſo many ſuch events; from whence the propoſition is manifeſt.

Cor. 1. If there be ſeveral independent events, the probability that the 1ſt happens the 2d fails, the 3d fails and the 4th happens, &c. is a ratio compounded of the probability of the 1ſt, and the probability of the failure of the 2d, and the probability of the failure of the 3d, and the probability of the 4th, &c. For the failure of an event may always be conſidered as the happening of its contrary.

Cor. 2. If there be ſeveral independent events, and the probability of each one be a, and that of its failing be b, the probability that the 1ſt happens and the 2d fails, and the 3d fails and the 4th happens, &c. will be $abba$, &c. For, according to the algebraic way of notation, if a denote any ratio and b another, $abba$ denotes the ratio compounded of the ratios a, b, b, a. This corollary therefore is only a particular caſe of the foregoing.

Definition. If in conſequence of certain data there ariſes a probability that a certain event ſhould happen, its happening or failing, in conſequence of theſe data, I call it's happening or failing in the 1ſt trial. And if the ſame data be again repeated, the happening or failing of the event in conſequence of them I call its happening or failing in the 2d trial; and ſo on as often as the ſame data are repeated. And hence it is manifeſt that the happening or failing of the ſame event in ſo many diffe- trials, is in reality the happening or failing of ſo many diſtinct independent events exactly ſimilar to each other.

PROP. 7.

If the probability of an event be a, and that of its failure be b in each ſingle trial, the probability of its happening p times, and failing q times in $p+q$ trials is $E\,a^p\,b^q$ if E be the coefficient of the term in which occurs $a^p\,b^q$ when the binomial $\overline{a+b}|^{b+q}$ is expanded.

For the happening or failing of an event in different trials are ſo many independent events. Wherefore (by cor. 2. prop. 6.) the probability that the event happens the 1ſt trial, fails the 2d and 3d, and happens the 4th, fails the 5th, &c. (thus happening and failing till the number of times it happens be p and the number it fails be q) is $abbab$ &c. till the number of a's be p and the number of b's be q, that is; 'tis $a^p\,b^q$. In like manner if you conſider the event as happening p times and failing q times in any other particular order, the probability for it is $a^p\,b^q$; but the number of different orders according to which an event may happen or fail, ſo as in all to happen p times and fail q, in $p+q$ trials is equal to the number of permutations that $aaaa\ bbb$ admit of when the number of a's is p, and the number of b's is q. And this number is equal to E, the coefficient of the term in which occurs $a^p\,b^q$ when $\overline{a+b}|^{p+q}$ is expanded. The event therefore may happen p times and fail q in $p+q$ trials E different ways and no more, and its happening and failing theſe ſeveral different ways are ſo many inconſiſtent events, the probability for each of which is $a^p\,b^q$, and therefore by

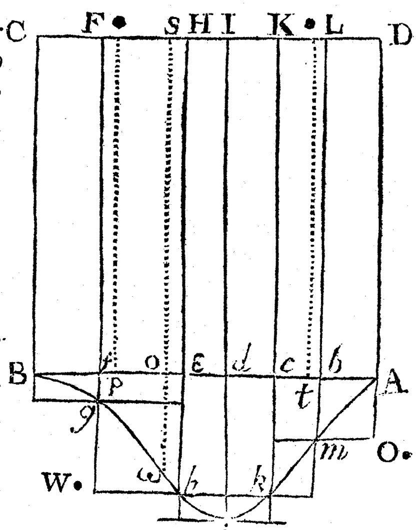
prop.

prop. 1. the probability that ſome way or other it happens p times and fails q times in $p+q$ trials is E $a^p\, b^q$.

SECTION II.

Poſtulate. 1. I Suppoſe the ſquare table or plane ABCD to be ſo made and levelled, that if either of the balls *o* or W be thrown upon it, there ſhall be the ſame probability that it reſts upon any one equal part of the plane as another, and that it muſt neceſſarily reſt ſomewhere upon it.

2. I ſuppoſe that the ball W ſhall be 1ſt thrown, and through the point where it reſts a line *os* ſhall be drawn parallel to AD, and meeting CD and AB in *s* and *o*; and that afterwards the ball O ſhall be thrown $p+q$ or n times, and that its reſting between AD and *os* after a ſingle throw be called the happening of the event M in a ſingle trial. Theſe things ſuppoſed,



Lem. 1. The probability that the point *o* will fall between any two points in the line AB is the ratio of the diſtance between the two points to the whole line AB.

Let any two points be named, as *f* and *b* in the line AB, and through them parallel to AD draw *fF*, *b*L meeting CD in F and L. Then if the rectangles C*f*, F*b*, LA are com-

commenfurable to each other, they may each be divided into the fame equal parts, which being done, and the ball W thrown, the probability it will reft fomewhere upon any number of thefe equal parts will be the fum of the probabilities it has to reft upon each one of them, becaufe its refting upon any different parts of the plane A C are fo many inconfiftent events; and this fum, becaufe the probability it fhould reft upon any one equal part as another is the fame, is the probability it fhould reft upon any one equal part multiplied by the number of parts. Confequently, the probability there is that the ball W fhould reft fomewhere upon F *b* is the probability it has to reft upon one equal part multiplied by the number of equal parts in F*b*; and the probability it refts fomewhere upon C*f* or LA, i.e. that it dont reft upon F *b* (becaufe it muft reft fomewhere upon A C) is the probability it refts upon one equal part multiplied by the number of equal parts in C *f*, L A taken together. Wherefore, the probability it refts upon F *b* is to the probability it dont as the number of equal parts in F *b* is to the number of equal parts in C *f*, L A together, or as F *b* to C *f*, L A together, or as *f b* to B *f*, A *b* together. Wherefore the probability it reft upon F *b* is to the probability it dont as *f b* to B *f*, A *b* together. And *(componendo inverfe)* the probability it refts upon F *b* is to the probability it refts upon F *b* added to the probability it dont, as *f b* to A B, or as the ratio of *f b* to A B to the ratio of A B to A B. But the probability of any event added to the probability of its failure is the ratio of equality; wherefore, the probability it reft upon F *b* is to the ratio of equality as the ratio of *f b* to A B to the ratio of A B to A B, or the ratio of equality; and therefore the probability it reft upon

F *b*

F b is the ratio of fb to AB. But *ex hypothesi* according as the ball W falls upon F b or not the point o will lie between f and b or not, and therefore the probability the point o will lie between f and b is the ratio of fb to AB.

Again; if the rectangles Cf, Fb, LA are not commensurable, yet the last mentioned probability can be neither greater nor less than the ratio of fb to AB; for, if it be less, let it be the ratio of fc to AB, and upon the line fb take the points p and t, so that pt shall be greater than fc, and the three lines Bp, pt, tA commensurable (which it is evident may be always done by dividing AB into equal parts less than half cb, and taking p and t the nearest points of division to f and c that lie upon fb). Then because Bp, pt, tA are commensurable, so are the rectangles Cp, Dt, and that upon pt compleating the square AB. Wherefore, by what has been said, the probability that the point o will lie between p and t is the ratio of pt to AB. But if it lies between p and t it must lie between f and b. Wherefore, the probability it should lie between f and b cannot be less than the ratio of pt to AB, and therefore must be greater than the ratio of fc to AB (since pt is greater than fc). And after the same manner you may prove that the forementioned probability cannot be greater than the ratio of fb to AB, it must therefore be the same.

Lem. 2. The ball W having been thrown, and the line os drawn, the probability of the event M in a single trial is the ratio of Ao to AB.

For, in the same manner as in the foregoing lemma, the probability that the ball o being thrown shall

rest

reſt ſomewhere upon D *o* or between A D and *so* is is the ratio of A *o* to A B. But the reſting of the ball *o* between A D and *s o* after a ſingle throw is the happening of the event M in a ſingle trial. Wherefore the lemma is manifeſt.

P R O P. 8.

If upon B A you erect the figure B *g h i k m* A whoſe property is this, that (the baſe B A being divided into any two parts, as A *b*, and B *b* and at the point of diviſion *b* a perpendicular being erected and terminated by the figure in *m*; and *y*, *x*, *r* repreſenting reſpectively the ratio of *b m*, A *b*, and B *b* to A B, and E being the the coefficient of the term in which occurs $a^p\,b^q$ when the binomial $\overline{a+b}|^{p+q}$ is expanded) $y = \mathrm{E}\,x^p\,r^q$. I ſay that before the ball W is thrown, the probability the point *o* ſhould fall between *f* and *b*, any two points named in the line A B, and withall that the event M ſhould happen *p* times and fail *q* in $p + q$ trials, is the ratio of *f g h i k m b*, the part of the figure B *g h i k m* A intercepted between the perpendiculars *f g*, *b m* raiſed upon the line A B, to C A the ſquare upon A B.

D E M O N S T R A T I O N.

For if not; 1ſt let it be the ratio of D a figure greater than *f g h i k m b* to C A, and through the points *e*, *d*, *c* draw perpendiculars to *f b* meeting the curve A *m i g* B in *h*, *i*, *k*; the point *d* being ſo placed that *d i* ſhall be the longeſt of the perpendi-

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culars terminated by the line fb, and the curve $A m i g B$; and the points e, d, c being so many and so placed that the rectangles, bk, ci, ei, fb taken together shall differ less from $fghikmb$ than D does; all which may be easily done by the help of the equation of the curve, and the difference between D and the figure $fghikmb$ given. Then since di is the longest of the perpendicular ordinates that insist upon fb, the rest will gradually decrease as they are farther and farther from it on each side, as appears from the construction of the figure, and consequently eh is greater than gf or any other ordinate that insists upon ef.

Now if Ao were equal to Ae, then by lem. 2. the probability of the event M in a single trial would be the ratio of Ae to AB, and consequently by cor. Prop. 1. the probability of it's failure would be the ratio of Be to AB. Wherefore, if x and r be the two forementioned ratios respectively, by Prop. 7. the probability of the event M happening p times and failing q in $p+q$ trials would be $E x^p r^q$. But x and r being respectively the ratios of Ae to AB and Be to AB, if y is the ratio of eh to AB, then, by construction of the figure AiB, $y = E x^p r^q$. Wherefore, if Ao were equal to Ae the probability of the event M happening p times and failing q in $p+q$ trials would be y, or the ratio of eh to AB. And if Ao were equal to Af, or were any mean between Ae and Af, the last mentioned probability for the same reasons would be the ratio of fg or some other of the ordinates insisting upon ef, to AB. But eh is the greatest of all the ordinates that insist upon ef. Wherefore, upon supposition the point should lie

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any where between f and e, the probability that the event M happens p times and fails q in $p+q$ trials can't be greater than the ratio of eb to AB. There then being these two subsequent events, the 1st that the point o will lie between e and f, the 2d that the event M will happen p times and fail q in $p+q$ trials, and the probability of the 1st (by lemma 1st) is the ratio of ef to AB, and upon supposition the 1st happens, by what has been now proved, the probability of the 2d cannot be greater than the ratio of eb to AB, it evidently follows (from Prop. 3.) that the probability both together will happen cannot be greater than the ratio compounded of that of ef to AB and that of eb to AB, which compound ratio is the ratio of fb to CA Wherefore, the probability that the point o will lie between f and e, and the event M happen p times and fail q, is not greater than the ratio of fb to CA. And in like, manner the probability the point o will lie between e and d, and the event M happen and fail as before, cannot be greater than the ratio of ei to CA. And again, the probability the point o will lie between d and c, and the event M happen and fail as before, cannot be greater than the ratio of ci to CA. And lastly, the probability that the point o will lie between c and b, and the event M happen and fail as before, cannot be greater than the ratio of bk to CA. Add now all these several probabilities together, and their sum (by Prop. 1.) will be the probability that the point will lie somewhere between f and b, and the event M happen p times and fail q in $p+q$ trials. Add likewise the correspondent ratios together, and their sum will be the ratio of the sum of the antecedents to to their common consequent, i. e. the ratio of fh, ei, ci, bk together to CA; which ratio is less than that of D to CA, because D is greater than fh, ei, ci, bk together. And therefore, the probability that the point o will lie between f and b, and withal that the event M will happen p times and fail q in $p+q$ trials, is *less* than the ratio of D to CA; but it was supposed the same which is absurd. And in like manner, by inscribing rectangles within the figure, as eg, dh, dk, cm, you may prove that the last mentioned probability is *greater* than the ratio of any figure less than $fghikmb$ to CA.

Wherefore, that probability must be the ratio of $fghikmb$ to CA.

Cor. Before the ball W is thrown the probability that the point o will lie somewhere between A and B, or somewhere upon the line AB, and withal that the event M will happen p times, and fail q in $p+q$ trials is the ratio of the whole figure AiB to CA. But it is certain that the point o will lie somewhere upon AB. Wherefore, before the ball W is thrown the probability the event M will happen p times and fail q in $p+q$ trials is the ratio of AiB to CA.

PROP. 9.

If before any thing is discovered concerning the place of the point o, it should appear that the event M had happened p times and failed q in $p+q$ trials, and from hence I guess that the point o lies between any two points in the line AB, as f and b, and consequently that the probability of the event M in a single trial was somewhere between the ratio of Ab to AB and that of Af to AB: the probability I am in

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the right is the ratio of that part of the figure A *i* B defcribed as before which is intercepted between perpendiculars erected upon A B at the points *f* and *b*, to the whole figure A *i* B.

For, there being thefe two fubfequent events, the firft that the point *o* will lie between *f* and *b*; the fecond that the event M fhould happen *p* times and fail *q* in *p* + *q* trials; and (by cor. prop. 8.) the original probability of the fecond is the ratio of A *i* B to C A, and (by prop. 8.) the probability of both is the ratio of *f g h i m b* to C A; wherefore (by prop. 5) it being firft difcovered that the fecond has happened, and from hence I guefs that the firft has happened alfo, the probability I am in the right is the ratio of *f g h i m b* to A *i* B, the point which was to be proved.

Cor. The fame things fuppofed, if I guefs that the probability of the event M lies fomewhere between *o* and the ratio of A *b* to A B, my chance to be in the right is the ratio of A *b m* to A *i* B.

SCHOLIUM.

From the preceding propofition it is plain, that in the cafe of fuch an event as I there call M, from the number of times it happens and fails in a certain number of trials, without knowing any thing more concerning it, one may give a guefs whereabouts it's probability is, and, by the ufual methods computing the magnitudes of the areas there mentioned, fee the chance that the guefs is right. And that the fame rule is the proper one to be ufed in the cafe of an event concerning the probability of which

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we abſolutely know nothing antecedently to any trials made concerning it, ſeems to appear from the following conſideration; viz. that concerning ſuch an event I have no reaſon to think that, in a certain number of trials, it ſhould rather happen any one poſſible number of times than another. For, on this account, I may juſtly reaſon concerning it as if its probability had been at firſt unfixed, and then determined in ſuch a manner as to give me no reaſon to think that, in a certain number of trials, it ſhould rather happen any one poſſible number of times than another. But this is exactly the caſe of the event M. For before the ball W is thrown, which determines it's probability in a ſingle trial, (by cor. prop. 8.) the probability it has to happen p times and fail q in $p + q$ or n trials is the ratio of A i B to C A, which ratio is the ſame when $p + q$ or n is given, whatever number p is; as will appear by computing the magnitude of A i B by the method * of fluxions. And conſequently before the place of the point o is diſcovered or the number of times the event M has happened in n trials, I can have no reaſon to think it ſhould rather happen one poſſible number of times than another.

In what follows therefore I ſhall take for granted that the rule given concerning the event M in prop. 9. is alſo the rule to be uſed in relation to any event concerning the probability of which nothing

* It will be proved preſently in art. 4. by computing in the method here mentioned that A i B contracted in the ratio of E to 1 is to C A as 1 to $\overline{n+1} \times E$: from whence it plainly follows that, antecedently to this contraction, A i B muſt be to C A in the ratio of 1 to $n + 1$, which is a conſtant ratio when n is given, whatever p is.

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at all is known antecedently to any trials made or observed concerning it. And ſuch an event I ſhall call an unknown event.

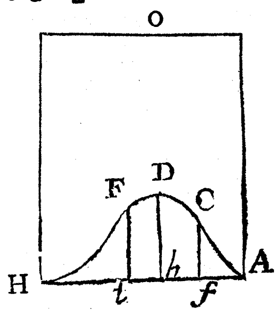
Cor. Hence, by ſuppoſing the ordinates in the figure $A\,i\,B$ to be contracted in the ratio of E to one, which makes no alteration in the proportion of the parts of the figure intercepted between them, and applying what is ſaid of the event M to an unknown event, we have the following propoſition, which gives the rules for finding the probability of an event from the number of times it actually happens and fails.

P R O P. 10.

If a figure be deſcribed upon any baſe A H (Vid. Fig.) having for it's equation $y = x^p r^q$; where y, x, r are reſpectively the ratios of an ordinate of the figure inſiſting on the baſe at right angles, of the ſegment of the baſe intercepted between the ordinate and A the beginning of the baſe, and of the other ſegment of the baſe lying between the ordinate and the point H, to the baſe as their common conſequent. I ſay then that if an unknown event has happened p times and failed q in $p+q$ trials, and in the baſe A H taking any two points as f and t you erect the ordinates fC, tF at right angles with it, the chance that the probability of the event lies ſomewhere between the ratio of Af to A H and that of At to A H, is the ratio of $tFCf$, that part of the before-deſcribed figure which is intercepted between the two ordinates, to A C F H the whole figure inſiſting on the baſe A H.

This is evident from prop. 9. and the remarks made in the foregoing ſcholium and corollary.

5 Now



Now, in order to reduce the foregoing rule to practice, we muſt find the value of the area of the figure deſcribed and the ſeveral parts of it ſeparated, by ordinates perpendicular to its baſe. For which purpoſe, ſuppoſe $AH = 1$ and HO the ſquare upon AH likewiſe $= 1$, and Cf will be $= y$, and $Af = x$, and $Hf = r$, becauſe y, x and r denote the ratios of Cf, Af, and Hf reſpectively to AH. And by the equation of the curve $y = x^p r^q$ and (becauſe $Af + fH = AH$) $r + x = 1$. Wherefore $y = x^p \times \overline{1-x}|^q = x^p - qx^{p+1} + q \times \frac{q-1}{2} \times x^{p+2} - q \times \frac{q-1}{2} \times \frac{q-2}{3} \times x^{p+3} +$ &c. Now the abſciſſe being x and the ordinate x^p the correſpondent area is $\frac{x^{p+1}}{p+1}$ (by prop. 10. caſ. 1. Quadrat. Newt.) * and the ordinate being qx^{p+1} the area is $\frac{qx^{p+2}}{p+2}$; and in like manner

* 'Tis very evident here, without having recourſe to Sir Iſaac Newton, that the fluxion of the area ACf being $y\dot{x} = x^p\dot{x} - qx^{p+1}\dot{x} + q \times \frac{q-1}{2} x^{p+2}\dot{x}$ &c. the fluent or area itſelf is $\frac{x^{p+1}}{p+1} - q \times \frac{x^{p+2}}{p+2} + q \times \frac{q-1}{2} \times \frac{x^{p+3}}{p+3}$ &c.

ner of the rest. Wherefore, the abscisse being x and the ordinate y or $x^{p} - qx^{p+1} + \&c.$ the correspondent area is $\frac{x^{p+1}}{p+1} - \frac{q}{p+2} \times x^{p+2} + q \times \frac{q-1}{2} \times \frac{x^{p+3}}{p+3} - q \times \frac{q-1}{2} \times \frac{q-2}{3} \times \frac{x^{p+4}}{p+4} + \&c.$ Wherefore, if $x = Af = \frac{Af}{AH}$ and $y = Cf = \frac{Cf}{AH}$, then $ACf = \frac{ACf}{HO} = \frac{x^{p+1}}{p+1} - \frac{q}{p+2} \times x^{p+2} + q \times \frac{q-1}{2} \times \frac{x^{p+3}}{p+3} - \&c.$

From which equation, if q be a small number, it is easy to find the value of the ratio of ACf to HO. and in like manner as that was found out, it will appear that the ratio of HCf to HO is $\frac{r^{q+1}}{q+1} - p \times \frac{r^{q+2}}{q+2} + p \times \frac{p-1}{2} \times \frac{r^{q+3}}{q+3} - p \times \frac{p-1}{2} \times \frac{p-2}{3} \times \frac{r^{q+4}}{q+4}$ &c. which series will consist of few terms and therefore is to be used when p is small.

2. The same things supposed as before, the ratio of ACf to HO is $\frac{x^{p+1}\,r^{q}}{p+1} + \frac{q}{p+1} \times \frac{x^{p+2}\,r^{q-1}}{p+2} + \frac{q}{p+1} \times \frac{q-1}{p+2} \times \frac{x^{p+3}\,r^{q-2}}{p+3} + \frac{q}{p+1} \times \frac{q-1}{p+2} \times \frac{q-2}{p+3} \times \frac{x^{p+4}\,r^{q-3}}{p+4} +$ &c.

&c. $+ \frac{x^{n+1}}{n+1} \times \frac{q}{p+1} \times \frac{q-1}{p+2} \times$ &c. $\times \frac{1}{n}$ where $n = p+q$. For this ſeries is the ſame with $\frac{x^{p+1}}{p+1} - q \times \frac{x^{p+2}}{p+2}$ &c. ſet down in Art. 1ſt. as the value of the ratio of A Cf to H O; as will eaſily be ſeen by putting in the former inſtead of r its value $1-x$, and expanding the terms and ordering them according to the powers of x. Or, more readily, by comparing the fluxions of the two ſeries, and in the former inſtead of $\dot{r}$ ſubſtituting $-\dot{x}$ *.

* The fluxion of the firſt ſeries is $x^p r^q \dot{x} + \frac{q x^{p+1} r^{p-1} \dot{r}}{p+1} + \frac{q x^{p+1} r^{q-1} \dot{x}}{p+1} + q \times \frac{q-1}{p+1} \times \frac{x^{p+2} r^{q-2} \dot{r}}{p+2} + \frac{q}{p+1} \times \frac{q-1}{p+2} \times x^{p+2} r^{q-2} \dot{x} + \frac{q}{p+1} \times \frac{q-1}{p+2} \times \frac{q-3}{p+3} \times x^{p+3} r^{q-3} \dot{r}$ &c. or, ſubſtituting $-\dot{x}$ for $\dot{r}$, $x^p r^q \dot{x} - \frac{q x^{p+1} r^{q-1} \dot{x}}{p+1} + \frac{q x^{p+1} r^{q-1} \dot{x}}{p+1} - q \times \frac{q-1}{p+1} \times \frac{x^{p+2} r^{q-2} \dot{x}}{p+2} + q \times \frac{q-1}{p+1} \times \frac{x^{p+2} r^{q-2} \dot{x}}{p+2}$ &c. which, as all the terms after the firſt deſtroy one another, is equal to $x^p r^q \dot{x} = x^p \times \overline{1-x}|^q \dot{x} = x^p \dot{x} \times \overline{1 - qx + q \times \frac{q-1}{2} x^2}$ &c. $= x^p \dot{x} - q x^{p+1} \dot{x} + q \times \frac{q-1}{2} x^{p+2} \dot{x}$ &c. $=$ the fluxion of the latter ſeries or of $\frac{x^{p+1}}{p+1} - q \times \frac{x^{p+2}}{p+2}$ &c. The two ſeries therefore are the ſame.

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3. In like manner, the ratio of H Cf to H O is $\frac{r^{q+1}\, x^{p}}{q+1} + \frac{p}{q+1} \times \frac{r^{q+2}\, x^{p-1}}{q+2} + \frac{p}{q+1} \times \frac{p-1}{q+2} \times \frac{r^{q+3}\, x^{p-2}}{q+3} +$ &c.

4. If E be the coefficient of that term of the binomical $\overline{a+b}|^{p+q}$ expanded in which occurs $a^p\, b^q$, the ratio of the whole figure A C F H to H O is $\frac{1}{n+1} \times \frac{1}{E}$, n being $= p + q$. For, when Af = A H $x = 1$, $r = 0$. Wherefore, all the terms of the ſeries ſet down in Art. 2. as expreſſing the ratio of A Cf to H O will vaniſh except the laſt, and that becomes $\frac{1}{n+1} \times \frac{q}{p+1} \times \frac{q-1}{p+2} \times$ &c. $\times \frac{1}{n}$. But E being the coefficient of that term in the binomial $\overline{a+b}|^{n}$ expanded in which occurs $a^p\, b^q$ is equal to $\frac{p+1}{q} \times \frac{p+2}{q-1} \times$ &c. $\times \frac{n}{1}$. And, becauſe Af is ſuppoſed to become = A H, A Cf = A C H. From whence this article is plain.

5. The ratio of A Cf to the whole figure A C F H is (by Art. 1. and 4.) $\overline{n+1} \times E \times \frac{x^{p+1}}{p+1} - q \times \frac{x^{p+2}}{p+2} + q \times \frac{q-1}{2} \times \frac{x^{p+3}}{p+3}$ &c. and if, as x expreſſes the ratio of Af to A H, X ſhould expreſs the ratio of At to A H; the ratio of A Ft to A C F H would be $\overline{n+1} \times E \times \frac{X^{p+1}}{p+1} - q\frac{X^{p+2}}{p+2} + q \times \frac{q-1}{2} \times \frac{X^{p+3}}{p+3}$ — &c. and conſequently the ratio of tF Cf to A C F H is $\overline{n+1} \times E \times^{d}$ into the difference between

between the two series. Compare this with prop. 10. and we shall have the following practical rule.

RULE 1.

If nothing is known concerning an event but that it has happened p times and failed q in $p+q$ or n trials, and from hence I guess that the probability of its happening in a single trial lies somewhere between any two degrees of probability as X and x, the chance I am in the right in my guess is $\overline{n+1} \times E \times^{d}$ into the difference between the series $\frac{X^{p+1}}{p+1} - q\frac{X^{p+2}}{p+2} + q \times \frac{q-1}{2} \times \frac{X^{p+3}}{p+3}$ — &c. and the series $\frac{x^{p+1}}{p+1} - q\frac{x^{p+2}}{p+2} + q \times \frac{q-1}{2} \times \frac{x^{p+3}}{p+3}$ — &c. E being the coefficient of $a^p\, b^q$ when $\overline{a+b}|^n$ is expanded.

This is the proper rule to be used when q is a small number; but if q is large and p small, change every where in the series here set down p into q and q into p and x into r or $1-x$, and X into R $= 1-X$; which will not make any alteration in the difference between the two serieses.

Thus far Mr. Bayes's essay.

With respect to the rule here given, it is further to be observed, that when both p and q are very large numbers, it will not be possible to apply it to practice on account of the multitude of terms which the serieses in it will contain. Mr. Bayes, therefore, by an investigation which it would be too tedious to give here, has deduced from this rule another, which is as follows.

RULE 2.

If nothing is known concerning an event but that it has happened p times and failed q in $p+q$ or n trials, and from hence I guess that the probability of its happening in a single trial lies between $\frac{p}{n}+z$ and $\frac{p}{n}-z$; if $m^2=\frac{n^3}{pq}$ $a=\frac{p}{n}$, $b=\frac{q}{n}$, E the coefficient of the term in which occurs $a^p b^q$ when $\overline{a+b}|^n$ is expanded, and $\Sigma=\frac{n+1}{n}\times\frac{\sqrt{2pq}}{\sqrt{n}}\times E\,a^p\,b^q\times{}^d$ by the series $mz-\frac{m^3z^3}{3}+\frac{n-2}{2n}\times\frac{m^5z^5}{5}-\frac{\overline{n-2}\times\overline{n-4}}{2n\times 3n}\times\frac{m^7z^7}{7}+\frac{n-2}{2n}\times\frac{n-4}{3n}\times\frac{n-6}{4n}\times\frac{m^9z^9}{9}$ &c. my chance to be in the right is greater than $\frac{2\Sigma}{1+2Ea^pb^q+\frac{2Ea^pb^q}{n}}$ * and less than $\frac{2\Sigma}{1-2Ea_pb^q-\frac{2Ea^pb^q}{n}}$. And if $p=q$ my chance is 2Σ exactly.

* In Mr. Bayes's manuscript this chance is made to be greater than $\frac{2\Sigma}{1+2Ea^pb^q}$ and less than $\frac{2\Sigma}{1-2Ea^pb^q}$. The third term in the two divisors, as I have given them, being omitted. But this being evidently owing to a small oversight in the deduction of this rule, which I have reason to think Mr. Bayes had himself discovered, I have ventured to correct his copy, and to give the rule as I am satisfied it ought to be given.

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In order to render this rule fit for uſe in all caſes it is only neceſſary to know how to find within ſufficient nearneſs the value of E $a^p b^q$ and alſo of the ſeries $mz - \frac{m^3 z^3}{3}$ &c*. With reſpect to the former Mr. Bayes has proved that, ſuppoſing K to ſignify the ratio of the quadrantal arc to it's radius, E $a^p b^q$ will be equal to $\frac{\sqrt{n}}{2\sqrt{Kpq}}$ × by the *ratio* whoſe *hyperbolic* logarithm is $\frac{1}{12} \times \overline{\frac{1}{n} - \frac{1}{p} - \frac{1}{q}} - \frac{1}{360} \times \overline{\frac{1}{n^3} - \frac{1}{p^3} - \frac{1}{q^3}} + \frac{1}{1260} \times \overline{\frac{1}{n^5} - \frac{1}{p^5} - \frac{1}{q^5}} - \frac{1}{1680} \times \overline{\frac{1}{n^7} - \frac{1}{p^7} - \frac{1}{q^7}} + \frac{1}{1188} \times \overline{\frac{1}{n^9} - \frac{1}{p^9} - \frac{1}{q^9}}$ &c. where the numeral coefficients may be found in the following manner. Call them A, B, C, D, E, &c. Then $A = \frac{1}{2.2.3} = \frac{1}{3.4}$. $B = \frac{1}{2.4.5} - \frac{A}{3}$. $C = \frac{1}{2.6.7} - \frac{10B + A}{5}$. $D = \frac{1}{2.8.9} - \frac{35C + 21B + A}{7}$. $E = \frac{1}{2.10.11} - \frac{126C + 84D + 36B + A}{9}$. $F = \frac{1}{2.12.13} -$

* A very few terms of this ſeries will generally give the hyperbolic logarithm to a ſufficient degree of exactneſs. A ſimilar ſeries has been given by Mr. De Moivre, Mr. Simpſon and other eminent mathematicians in an expreſſion for the ſum of the logarithms of the numbers 1, 2, 3, 4, 5 to x, which ſum they have aſſerted to be equal to $\frac{1}{2}\log. c + \overline{x + \frac{1}{2}} \times \log. x - x + \frac{1}{12x} - \frac{1}{360x^3} + \frac{1}{1260x^5}$ &c. c denoting the circumference of a circle whoſe radius is unity. But Mr. Bayes, in a preceding paper in this volume, has demonſtrated that, though this expreſſion will very nearly approach to the value of this ſum when only a proper number of the firſt terms is taken, the whole ſeries cannot expreſs any quantity at all, becauſe, let x be what it will, there will be always a part of the ſeries where it will begin to diverge. This obſervation, though it does not much affect the uſe of this ſeries, ſeems well worth the noticeof mathematicians.

$\frac{462 D + 330 C + 165 E + 55 B + A}{11}$ &c. where the coefficients of B, C, D, E, F, &c. in the values of D, E, F, &c. are the 2, 3, 4, &c. higheſt coefficients in $\overline{a+b}|^7$, $\overline{a+b}|^9$, $\overline{a+b}|^{11}$, &c. expanded; affixing in every particular value the leaſt of theſe coefficents to B, the next in magnitude to the furtheſt letter from B, the next to C, the next to the furtheſt but one, the next to D, the next to the furtheſt but two, and ſo on *.

With reſpect to the value of the ſeries $mz - \frac{m^3z^3}{3} + \frac{n-2}{2n} \times \frac{m^5z^5}{5}$ &c. he has obſerved that it may be calculated directly when mz is leſs than 1, or even not greater than $\sqrt{3}$: but when mz is much larger it becomes impracticable to do this; in which caſe he ſhews a way of eaſily finding two values of it very nearly equal between which it's true value muſt lie.

The theorem he gives for this purpoſe is as follows.

Let K, as before, ſtand for the ratio of the quadrantal arc to its radius, and H for the ratio whoſe hyperbolic logarithm is $\frac{2^2-1}{2n} - \frac{2^4-1}{360n^3} + \frac{2^6-1}{1260n^5} - \frac{2^8-1}{1680n^7}$ &c. Then the ſeries $mz - \frac{m^3z^3}{3}$ &c. will be greater or leſs than the ſeries $\frac{Hn}{n+1} \times \frac{\sqrt{K}}{\sqrt{2}} - \frac{n}{n+2} \times$

$$\frac{\overline{1-\frac{2m^2z^2}{n}}|^{\frac{n}{2}+1}}{2mz} + \frac{n^2}{n+2} \times \frac{\overline{1-\frac{2m^2z^2}{n}}|^{\frac{n}{2}+2}}{n+4 \times 4m^3z^3} +$$

* This method of finding theſe coefficients I have deduced from the demonſtration of the third lemma at the end of Mr. Simpſon's Treatiſe on the Nature and Laws of Chance.

$$\frac{3n^3}{n+2}\times\frac{\overline{1-\frac{2m^2z^2}{n}}\Big|^{\frac{n}{2}+3}}{\overline{n+4}\times\overline{n+6}\times 8m^5z^5}+\frac{3\times 5\times n^4}{n+2}\times\frac{\overline{1-\frac{2m^2z^2}{n}}\Big|^{\frac{n}{2}+4}}{\overline{n+4}\times\overline{n+6}\times\overline{n+8}\times 16z^7m^7}$$

— &c. continued to any number of terms, according as the laſt term has a poſitive or a negative ſign before it.

From ſubſtituting theſe values of E $a^p b^q$ and $mz - \frac{m^3z^3}{3}+\frac{n-2}{2n}\times\frac{m^5z^5}{5}$ &c. in the 2d rule ariſes a 3d rule, which is the rule to be uſed when mz is of ſome conſiderable magnitude.

RULE 3.

If nothing is known of an event but that it has happened p times and failed q in $p+q$ or n trials, and from hence I judge that the probability of it's happening in a ſingle trial lies between $\frac{p}{n}+z$ and $\frac{p}{n}-z$ my chance to be right is *greater* than

$$\frac{\sqrt{Kpq}\times b}{2\sqrt{Kpq}+bn^{\frac{1}{2}}+bn^{-\frac{1}{2}}}\times\overline{2H-\frac{\sqrt{2}}{\sqrt{K}}\times\frac{n+1}{n+2}\times\frac{1}{mz}\times\overline{1-\frac{2m^2z^2}{n}}\Big|^{\frac{n}{2}+1}}$$

and *leſs* than $\frac{\sqrt{Kpq}\times b}{2\sqrt{Kpq}-bn^{\frac{1}{2}}-bn^{-\frac{1}{2}}}$ multiplied by the 3 terms

$$2H-\frac{\sqrt{2}}{\sqrt{K}}\times\frac{n+1}{n+2}\times\frac{1}{mz}\times\overline{1-\frac{2m^2z^2}{n}}\Big|^{\frac{n}{2}+1}+\frac{\sqrt{2}}{\sqrt{K}}\times\frac{n}{n+2}\times\frac{n+1}{n+4}\times\frac{1}{2m^3z^3}\times\overline{1-\frac{2m^2z^2}{n}}\Big|^{\frac{n}{2}+2}$$

where m^2, K, b and H ſtand for the quantities already explained.

An

An APPENDIX.

CONTAINING

An Application of the foregoing Rules to some particular Cases.

THE first rule gives a direct and perfect solution in all cases; and the two following rules are only particular methods of approximating to the solution given in the first rule, when the labour of applying it becomes too great.

The first rule may be used in all cases where either p or q are nothing or not large. The second rule may be used in all cases where mz is less than $\sqrt{3}$; and the 3d in all cases where $m^2 z^2$ is greater than 1 and less than $\frac{n}{2}$, if n is an even number and very large. If n is not large this last rule cannot be much wanted, because, m decreasing continually as n is diminished, the value of z may in this case be taken large, (and therefore a considerable interval had between $\frac{p}{n} - z$ and $\frac{p}{n} + z$,) and yet the operation be carried on by the 2d rule; or mz not exceed $\sqrt{3}$.

But in order to shew distinctly and fully the nature of the present problem, and how far Mr. Bayes has carried the solution of it; I shall give the result of this solution in a few cases, beginning with the lowest and most simple.

Let

Let us then firſt ſuppoſe, of ſuch an event as that called M in the eſſay, or an event about the probability of which, antecedently to trials, we know nothing, that it has happened *once*, and that it is enquired what concluſion we may draw from hence with reſpect to the probability of it's happening on a *ſecond* trial.

The anſwer is that there would be an odds of three to one for ſomewhat more than an even chance that it would happen on a ſecond trial.

For in this caſe, and in all others where q is nothing, the expreſſion $\overline{n+1} \times \overline{\frac{X^{p+1}}{p+1} - \frac{x^{p+1}}{p+1}}$ or $X^{p+1} - x^{p+1}$ gives the ſolution, as will appear from conſidering the firſt rule. Put therefore in this expreſſion $\overline{p+1} = 2$, $X = 1$ and $x = \frac{1}{2}$ and it will be $1 - \overline{\tfrac{1}{2}}^2$ or $\frac{3}{4}$; which ſhews the chance there is that the probability of an event that has happened once lies ſomewhere between 1 and $\frac{1}{2}$; or (which is the ſame) the odds that it is ſomewhat more than an even chance that it will happen on a ſecond trial *.

In the ſame manner it will appear that if the event has happened twice, the odds now mentioned will be ſeven to one; if thrice, fifteen to one; and in general, if the event has happened p times, there will be an odds of $2^{p+1} - 1$ to one, for *more* than an equal chance that it will happen on further trials.

Again, ſuppoſe all I know of an event to be that it has happened ten times without failing, and the

* There can, I ſuppoſe, be no reaſon for obſerving that on this ſubject unity is always made to ſtand for certainty, and $\frac{1}{2}$ for an even chance.

enquiry to be what reaſon we ſhall have to think we are right if we gueſs that the probability of it's happening in a ſingle trial lies ſomewhere between $\frac{16}{17}$ and $\frac{2}{3}$, or that the ratio of the cauſes of it's happening to thoſe of it's failure is ſome ratio between that of ſixteen to one and two to one.

Here $p+1=11$, $X=\frac{16}{17}$ and $x=\frac{2}{3}$ and $X^{p+1} - x^{p+1} = \overline{\tfrac{16}{17}}|^{11} - \overline{\tfrac{2}{3}}|^{11} = .5013$ &c. The anſwer therefore is, that we ſhall have very nearly an equal chance for being right.

In this manner we may determine in any caſe what concluſion we ought to draw from a given number of experiments which are unoppoſed by contrary experiments. Every one ſees in general that there is reaſon to expect an event with more or leſs confidence according to the greater or leſs number of times in which, under given circumſtances, it has happened without failing; but we here ſee exactly what this reaſon is, on what principles it is founded, and how we ought to regulate our expectations.

But it will be proper to dwell longer on this head.

Suppoſe a ſolid or die of whoſe number of ſides and conſtitution we know nothing; and that we are to judge of theſe from experiments made in throwing it.

In this caſe, it ſhould be obſerved, that it would be in the higheſt degree improbable that the ſolid ſhould, in the firſt trial, turn any one ſide which could be aſſigned before hand; becauſe it would be known that ſome ſide it muſt turn, and that there was an infinity of other ſides, or ſides otherwiſe marked, which it was equally likely that it ſhould turn. The firſt throw only ſhews that *it has* the ſide then thrown, without giving any reaſon to think that it has it any one number of times rather than any other. It will appear, therefore, that *after* the firſt throw and not before, we ſhould be in the circumſtances required by the conditions of the preſent problem, and that the whole effect of this throw would be to bring us into theſe circumſtances. That is: the turning the ſide firſt thrown in any ſubſequent ſingle trial would be an event about the probability or improbability of which we could form no judgment, and of which we ſhould know no more than that it lay ſomewhere between nothing and certainty. With the ſecond trial then our calculations muſt begin; and if in that trial the ſuppoſed ſolid turns again the ſame ſide, there will ariſe the probability of three to one that it has more of that ſort of ſides than of *all* others; or (which comes to the ſame) that there is ſomewhat in its conſtitution diſpoſing it to turn that ſide ofteneſt: And this probability will increaſe, in the manner already explained, with the number of times in which that ſide has been thrown without failing. It ſhould not, however, be imagined that any number of ſuch experiments can give ſufficient reaſon for thinking that it would *never* turn any other ſide. For, ſuppoſe it has turned the ſame ſide in every trial a million of times. In theſe circumſtances there would be an improbability that it had *leſs* than 1.400,000 more of theſe ſides than all others; but there would alſo be an improbability that it had *above* 1.600,000 times more. The chance for the latter is expreſſed by $\frac{1600000}{1600001}$ raiſed to the millioneth power ſubſtracted from unity, which is equal to .4647 &c. and the chance for the former is equal to $\frac{1400000}{1400001}$ raised to the same power, or to .4895; which, being both less than an equal chance, proves what I have said. But though it would be thus improbable that it had *above* 1.600,000 times more or *less* than 1.400,000 times *more* of these sides than of all others, it by no means follows that we have any reason for judging that the true proportion in this case lies somewhere between that of 1.600,000 to one and 1.400,000 to one. For he that will take the pains to make the calculation will find that there is nearly the probability expressed by .527, or but little more than an equal chance, that it lies somewhere between that of 600,000 to one and three millions to one. It may deserve to be added, that it is more probable that this proportion lies somewhere between that of 900,000 to 1 and 1.900,000 to 1 than between any other two proportions whose antecedents are to one another as 900,000 to 1.900,000, and consequents unity.

I have made these observations chiefly because they are all strictly applicable to the events and appearances of nature. Antecedently to all experience, it would be improbable as infinite to one, that any particular event, before-hand imagined, should follow the application of any one natural object to another; because there would be an equal chance for any one of an infinity of other events. But if we had once seen any particular effects, as the burning of wood on putting it into fire, or the falling of a stone on detaching it from all contiguous objects, then the conclusions to be drawn from any number of subsequent events of the same kind would be to be determined in the same manner with the conclusions just mentioned relating to the constitution of the solid I have

supposed

ſuppoſed. ——— In other words. The firſt experiment ſuppoſed to be ever made on any natural object would only inform us of one event that may follow a particular change in the circumſtances of thoſe objects; but it would not ſuggeſt to us any ideas of uniformity in nature, or give us the leaſt reaſon to apprehend that it was, in that inſtance or in any other, regular rather than irregular in its operations. But if the ſame event has followed without interruption in any one or more ſubſequent experiments, then ſome degree of uniformity will be obſerved; reaſon will be given to expect the ſame ſucceſs in further experiments, and the calculations directed by the ſolution of this problem may be made.

One example here it will not be amiſs to give.

Let us imagine to ourſelves the caſe of a perſon juſt brought forth into this, world and left to collect from his obſervation of the order and courſe of events what powers and cauſes take place in it. The Sun would, probably, be the firſt object that would engage his attention; but after loſing it the firſt night he would be entirely ignorant whether he ſhould ever ſee it again. He would therefore be in the condtion of a perſon making a firſt experiment about an event entirely unknown to him. But let him ſee a ſecond appearance or one *return* of the Sun, and an expectation would be raiſed in him of a ſecond return, and he might know that there was an odds of 3 to 1 for *ſome* probability of this. This odds would increaſe, as before repreſented, with the number of returns to which he was witneſs. But no finite number of returns would be ſufficient to produce abſolute or phyſical certainty. For let it be ſuppoſed that he has ſeen it return at regular and ſtated intervals a million of times. The concluſions this would warrant would be ſuch as follow ——— There would be the odds of the millioneth power of 2, to one, that it was likely that it would return again at the end of the uſual interval. There would be the probability expreſſed by .5352, that the odds for this was not *greater* than 1.600,000 to 1; And the probability expreſſed by .5105, that it was not *leſs* than 1.400,000 to 1.

It ſhould be carefully remembered that theſe deductions ſuppoſe a previous total ignorance of nature. After having obſerved for ſome time the courſe of events it would be found that the operations of nature are in general regular, and that the powers and laws which prevail in it are ſtable and parmanent. The conſideration of this will cauſe one or a few experiments often to produce a much ſtronger expectation of ſucceſs in further experiments than would otherwiſe have been reaſonable; juſt as the frequent obſervation that things of a ſort are diſpoſed together in any place would lead us to conclude, upon diſcovering there any object of a particular ſort, that there are laid up with it many others of the ſame ſort. It is obvious that this, ſo far from contradicting the foregoing deductions, is only one particular caſe to which they are to be applied.

What has been ſaid ſeems ſufficient to ſhew us what concluſions to draw from *uniform* experience. It demonſtrates, particularly, that inſtead of proving that events will *always* happen agreeably to it, there will be always reaſon againſt this concluſion. In other words, where the courſe of nature has been the moſt conſtant, we can have only reaſon to reckon upon a recurrency of events proportioned to the degree of this

this constancy; but we can have no reason for thinking that there are no causes in nature which will *ever* inrerfere with the operations of the causes from which this constancy is derived, or no circumstances of the world in which it will fail. And if this is true, supposing our only *data* derived from experience, we shall find additional reason for thinking thus if we apply other principles, or have recourse to such considerations as reason, independently of experience, can suggest.

But I have gone further than I intended here; and it is time to turn our thoughts to another branch of this subject: I mean, to cases where an experiment has sometimes succeeded and sometimes failed.

Here, again, in order to be as plain and explicit as possible, it will be proper to put the following case, which is the easiest and simplest I can think of.

Let us then imagine a person present at the drawing of a lottery, who knows nothing of its scheme or of the proportion of *Blanks* to *Prizes* in it. Let it further be supposed, that he is obliged to infer this from the number of *blanks* he hears drawn compared with the number of *prizes*; and that it is enquired what conclusions in these circumstances he may reasonably make.

Let him first hear *ten* blanks drawn and *one* prize, and let it be enquired what chance he will have for being right if he guesses that the proportion of *blanks* to *prizes* in the lottery lies somewhere between the proportions of 9 to 1 and 11 to 1.

Here taking $X = \frac{11}{12}$, $x = \frac{9}{10}$, $p = 10$, $q = 1$, $n = 11$, $E = 11$, the required chance, according to the first rule,

rule, is $\overline{n+1} \times E$ into the difference between $\overline{\frac{X^{p+1}}{p+1} - \frac{qX^{p+2}}{p+2}}$ and $\overline{\frac{x^{p+1}}{p+1} - \frac{qx^{p+2}}{p+2}} = 12 \times 11$

$$\times \overline{\frac{\left(\frac{11}{12}\right)^{11}}{11} - \frac{\left(\frac{11}{12}\right)^{12}}{12}} - \overline{\frac{\left(\frac{9}{10}\right)^{11}}{11} - \frac{\left(\frac{9}{10}\right)^{12}}{12}} = .07699$$

&c. There would therefore be an odds of about 923 to 76, or nearly 12 to 1 *against* his being right. Had he guessed only in general that there were less than 9 blanks to a prize, there would have been a probability of his being right equal to .6589, or the odds of 65 to 34.

Again, suppose that he has heard 20 *blanks* drawn and 2 *prizes*; what chance will he have for being right if he makes the same guess?

Here X and x being the same, we have $n = 22$, $p = 20$, $q = 2$, $E = 231$, and the required chance equal to $\overline{n+1} \times E \times \overline{\frac{X^{p+1}}{p+1} - \frac{qX^{p+2}}{p+2} + q \times \frac{q-1}{2} \times \frac{X^{p+3}}{p+3}}$

$$\overline{- \frac{x^{p+1}}{p+1} - \frac{qx^{p+2}}{p+2} + q \times \frac{q-1}{2} \times \frac{x^{p+3}}{p+3}} = .10843 \text{ \&c.}$$

He will, therefore, have a better chance for being right than in the former instance, the odds against him now being 892 to 108 or about 9 to 1. But should he only guess in general, as before, that there were less than 9 blanks to a prize, his chance for being right will be worse; for instead of .6589 or an odds of near two to one, it will be .584, or an odds of 584 to 415.

Suppose,

Suppose, further, that he has heard 40 *blanks* drawn and 4 *prizes*; what will the before-mentioned chances be?

The answer here is .1525, for the former of these chances; and .527, for the latter. There will, therefore, now be an odds of only $5\frac{1}{2}$ to 1 against the proportion of blanks to prizes lying between 9 to 1 and 11 to 1; and but little more than an equal chance that it is less than 9 to 1.

Once more. Suppose he has heard 100 *blanks* drawn and 10 *prizes*.

The answer here may still be found by the first rule; and the chance for a proportion of blanks to prizes *less* than 9 to 1 will be .44109, and for a proportion *greater* than 11 to 1 .3082. It would therefore be likely that there were not *fewer* than 9 or *more* than 11 blanks to a prize. But at the same time it will remain unlikely * that the true proportion should lie between 9 to 1 and 11 to 1, the chance for this being .2506 &c. There will therefore be still an odds of near 3 to 1 against this.

From these calculations it appears that, in the circumstances I have supposed, the chance for being right in guessing the proportion of *blanks* to *prizes* to be nearly the same with that of the number of *blanks*

* I suppose no attentive person will find any difficulty in this. It is only saying that, supposing the interval between nothing and certainty divided into a hundred equal chances, there will be 44 of them for a less proportion of blanks to prizes than 9 to 1, 31 for a greater than 11 to 1, and 25 for some proportion between 9 to 1 and 11 to 1; in which it is obvious that, though one of these suppositions must be true, yet, having each of them more chances against them than for them, they are all separately unlikely.

drawn in a given time to the number of prizes drawn, is continually increaſing as theſe numbers increaſe; and that therefore, when they are conſiderably large, this concluſion may be looked upon as morally certain. By parity of reaſon, it follows univerſally, with reſpect to every event about which a great number of experiments has been made, that the cauſes of its happening bear the ſame proportion to the cauſes of its failing, with the number of happenings to the number of failures; and that, if an event whoſe cauſes are ſuppoſed to be known, happens oftener or ſeldomer than is agreeable to this concluſion, there will be reaſon to believe that there are ſome unknown cauſes which diſturb the operations of the known ones. With reſpect, therefore, particularly to the courſe of events in nature, it appears, that there is demonſtrative evidence to prove that they are derived from permanent cauſes, or laws originally eſtabliſhed in the conſtitution of nature in order to produce that order of events which we obſerve, and not from any of the powers of chance*. This is juſt as evident as it would be, in the caſe I have inſiſted on, that the reaſon of drawing 10 times more *blanks* than *prizes* in millions of trials, was, that there were in the wheel about ſo many more *blanks* than *prizes.*

But to proceed a little further in the demonſtration of this point.

We have ſeen that ſuppoſing a perſon, ignorant of the whole ſcheme of a lottery, ſhould be led to conjecture, from hearing 100 *blanks* and 10 prizes drawn,

* See Mr. De Moivre's Doctrine of Chances, pag. 250.

that

that the proportion of *blanks* to *prizes* in the lottery was ſomewhere between 9 to 1 and 11 to 1, the chance for his being right would be .2506 &c. Let now enquire what this chance would be in ſome higher caſes.

Let it be ſuppoſed that *blanks* have been drawn 1000 times, and prizes 100 times in 1100 trials.

In this caſe the powers of X and x riſe ſo high, and the number of terms in the two ſerieſes $\frac{X^{p+1}}{p+1} - \frac{qX^{p+1}}{p+2}$ &c. and $\frac{x^{p+1}}{p+1} - \frac{qx^{p+2}}{p+2}$ &c. become ſo numerous that it would require immenſe labour to obtain the anſwer by the firſt rule. 'Tis neceſſary, therefore, to have recourſe to the ſecond rule. But in order to make uſe of it, the interval between X and x muſt be a little altered. $\frac{10}{11} - \frac{9}{10}$ is $\frac{1}{110}$, and therefore the interval between $\frac{10}{11} - \frac{1}{110}$ and $\frac{10}{11} + \frac{1}{110}$ will be nearly the ſame with the interval between $\frac{9}{10}$ and $\frac{11}{12}$, only ſomewhat larger. If then we make the queſtion to be; what chance there would be (ſuppoſing no more known than that blanks have been drawn 1000 times and prizes 100 times in 1100 trials) that the probability of drawing a blank in a ſingle trial would lie ſomewhere between $\frac{10}{11} - \frac{1}{110}$ and $\frac{10}{11} + \frac{1}{110}$ we ſhall have a queſtion of the ſame kind with the preceding queſtions, and deviate but little from the limits aſſigned in them.

The anſwer, according to the ſecond rule, is that this chance is greater than $\frac{2\Sigma}{1 - 2Ea^pb^q + \frac{2Ea^pb^q}{n}}$

and

and less than $\frac{2\Sigma}{\overline{1-2Ea^p b^q - \frac{2Ea^p b_q}{n}}}$, E being $\frac{n+1}{n}$ $\times \frac{\sqrt{2pq}}{\sqrt{n}} \times Ea^p p^q \times mz - \frac{m^3 z^3}{3} + \frac{n-2}{2n} \times \frac{m^5 z^5}{5}$ &c.

By making here $1000 = p$ $100 = q$ $1100 = n$ $\frac{1}{110} = z$, $m = \frac{\sqrt{n^3}}{\sqrt{pq}} = 1.048808$, $Ea^p b^q = \frac{h}{2} \times \frac{\sqrt{n}}{\sqrt{Kpq}}$, h being the ratio whose hyperbolic logarithm is $\frac{1}{12} \times \overline{\frac{1}{n} - \frac{1}{p} - \frac{1}{q}} - \frac{1}{360} \times \overline{\frac{1}{n^3} - \frac{1}{p^3} - \frac{1}{q^3}} + \frac{1}{1260} \times \overline{\frac{1}{n^5} - \frac{1}{p^5} - \frac{1}{q^5}}$ &c. and K the ratio of the quadrantal arc to radius; the former of these expressions will be found to be .7953, and the latter .9405 &c. The chance enquired after, therefore, is greater than .7953, and less than .9405. That is; there will be an odds for being right in guessing that the proportion of blanks to prizes lies *nearly* between 9 to 1 and 11 to 1, (or *exactly* between 9 to 1 and 1111 to 99) which is greater than 4 to 1, and less than 16 to 1.

Suppose, again, that no more is known than that *blanks* have been drawn 10,000 times and *prizes* 1000 times in 11000 trials; what will the chance now mentioned be?

Here the second as well as the first rule becomes useless, the value of mz being so great as to render it scarcely possible to calculate directly the series $\overline{mz - \frac{m^3 z^3}{3} + \frac{n-2}{2n} \times \frac{m^5 z}{5}}$ - &c. The third rule, therefore, must be used; and the information it gives us is, that the required chance is greater than .97421, or more than an odds of 40 to 1.

By

By calculations ſimilar to theſe may be determined univerſally, what expectations are warranted by any experiments, according to the different number of times in which they have ſucceeded and failed; or what ſhould be thought of the probability that any particular cauſe in nature, with which we have any acquaintance, will or will not, in any ſingle trial, produce an effect that has been conjoined with it.

Moſt perſons, probably, might expect that the chances in the ſpecimen I have given would have been greater than I have found them. But this only ſhews how liable we are to error when we judge on this ſubject independently of calculation. One thing, however, ſhould be remembered here; and that is, the narrowneſs of the interval between $\frac{9}{10}$ and $\frac{11}{12}$, or between $\frac{10}{11}+\frac{1}{110}$ and $\frac{10}{11}-\frac{1}{110}$. Had this interval been taken a little larger, there would have been a conſiderable difference in the reſults of the calculations. Thus had it been taken double, or $z=\frac{1}{55}$, it would have been found in the fourth inſtance that inſtead of odds againſt there were odds for being right in judging that the probability of drawing a blank in a ſingle trial lies between $\frac{10}{11}+\frac{1}{55}$ and $\frac{10}{11}-\frac{1}{55}$.

The foregoing calculations further ſhew us the uſes and defects of the rules laid down in the eſſay. 'Tis evident that the two laſt rules do not give us the required chances within ſuch narrow limits as could be wiſhed. But here again it ſhould be conſidered, that theſe limits become narrower and narrower as q is taken larger in reſpect of p; and when p and q are equal, the exact ſolution is given in all caſes by the ſecond rule. Theſe two rules therefore afford a direction a direction to our judgment that may be of considerable use till some person shall discover a better approximation to the value of the two series's in the first rule †.

But what most of all recommends the solution in this *Essay* is, that it is compleat in those cases where information is most wanted, and where Mr. De Moivre's solution of the inverse problem can give little or no direction; I mean, in all cases where either p or q are of no considerable magnitude. In other cases, or when both p and q are very considerable, it is not difficult to perceive the truth of what has been here demonstrated, or that there is reason to believe in general that the chances for the happening of an event are to the chances for its failure in the same *ratio* with that of p to q. But we shall be greatly deceived if we judge in this manner when either p or q are small. And tho' in such cases the *Data* are not sufficient to discover the exact probability of an event, yet it is very agreeable to be able to find the limits between which it is reasonable to think it must lie, and also to be able to determine the precise degree of assent which is due to any conclusions or assertions relating to them.

† Since this was written I have found out a method of considerably improving the approximation in the 2d and 3d rules by demonstrating that the expression $\dfrac{2\Sigma}{1 + 2\,\mathrm{E}\,a^p b^q + \dfrac{2\,\mathrm{E}\,a^p b^q}{n}}$ comes almost as near to the true value wanted as there is reason to desire, only always somewhat less. It seems necessary to hint this here; though the proof of it cannot be given.

LIII. *An*