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STUDIES IN THE HISTORY OF PROBABILITY AND STATISTICS

IX. THOMAS BAYES'S ESSAY TOWARDS SOLVING A PROBLEM IN THE DOCTRINE OF CHANCES*

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THOMAS BAYES—A BIOGRAPHICAL NOTE

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Bayes's paper, reproduced in the following pages, must rank as one of the most famous memoirs in the history of science and the problem it discusses is still the subject of keen controversy. The intellectual stature of Bayes himself is measured by the fact that it is still of scientific as well as historical interest to know what Bayes had to say on the questions he raised. And yet such are the vagaries of historical records, that almost nothing is known about the personal history of the man. *The Dictionary of National Biography*, compiled at the end of the last century, when the whole theory of probability was in temporary eclipse in England, has an entry devoted to Bayes's father, Joshua Bayes, F.R.S., one of the first six Nonconformist ministers to be publicly ordained as such in England, but it has nothing on his much more distinguished son. Indeed, the note on Thomas Bayes which is to appear in the forthcoming new edition of the *Encyclopedia Britannica* will apparently be the first biographical note on Bayes to appear in a work of general reference since the *Imperial Dictionary of Universal Biography* was published in Glasgow in 1865. And in treatises on the history of mathematics, such as that of Loria (1933) and Cantor (1908), notice is taken of his contributions to probability theory and to mathematical analysis, but biographical details are lacking.

The Reverend Thomas Bayes, F.R.S., author of the first expression in precise, quantitative form of one of the modes of inductive inference, was born in 1702, the eldest son of Ann Bayes and Joshua Bayes, F.R.S. He was educated privately, as was usual with Nonconformists at that time, and from the fact that when Thomas was 12 Bernoulli wrote to Leibniz that 'poor de Moivre' was having to earn a living in London by teaching mathematics, we are tempted to speculate that Bayes may have learned mathematics from one of the founders of the theory of probability. Eventually Thomas was ordained, and began his ministry by helping his father, who was at the time stated, minister of the Presbyterian meeting house in Leather Lane, off Holborn. Later the son went to minister in Tunbridge Wells at the Presbyterian Chapel on Little Mount Sion which had been opened on 1 August 1720. It is not known when Bayes went to Tunbridge Wells, but he was not the first to minister on Little Mount Sion, and he was certainly there in 1731, when he produced a tract entitled 'Divine Benevolence, or an attempt to prove that the Principle End of the Divine

* Thomas Bayes's famous Essay is so often referred to in current statistical literature, but so rarely studied because of the difficulty of access, that the Editors have felt justified in reprinting it in the *Biometrika* History of Probability and Statistics series.

Providence and Government is the happiness of His Creatures'. The tract was published by John Noon and copies are in Dr Williams's library and the British Museum. The following is a quotation:

[p. 22]: I don't find (I am sorry to say it) any necessary connection between mere intelligence, though ever so great, and the love or approbation of kind and beneficent actions.

Bayes argued that the principal end of the Deity was the happiness of His creatures, in opposition to Balguy and Grove who had, respectively, maintained that the first spring of action of the Deity was Rectitude, and Wisdom.

In 1736 John Noon published a tract entitled 'An Introduction to the Doctrine of Fluxions, and a Defence of the Mathematicians against the objections of the Author of the Analyst'. De Morgan (1860) says: 'This very acute tract is anonymous, but it was always attributed to Bayes by the contemporaries who write in the names of the authors as I have seen in various copies, and it bears his name in other places.' The ascription to Bayes is accepted also in the British Museum catalogue.

From the copy in Dr Williams's library we quote:

[p. 9]: It is not the business of the Mathematician to dispute whether quantities do in fact ever vary in the manner that is supposed, but only whether the notion of their doing so be intelligible; which being allowed, he has a right to take it for granted, and then see what deductions he can make from that supposition. It is not the business of a Mathematician to show that a strait line or circle can be drawn, but he tells you what he means by these; and if you understand him, you may proceed further with him; and it would not be to the purpose to object that there is no such thing in nature as a true strait line or perfect circle, for this is none of his concern: he is not inquiring how things are in matter of fact, but supposing things to be in a certain way, what are the consequences to be deduced from them; and all that is to be demanded of him is, that his suppositions be intelligible, and his inferences just from the suppositions he makes.

[p. 48]: He [i.e. the Analyst = Bishop Berkeley] represents the disputes and controversies among mathematicians as disparaging the evidence of their methods: and, Query 51, he represents Logics and Metaphysics as proper to open their eyes, and extricate them from their difficulties. Now were ever two things thus put together? If the disputes of the professors of any science disparage the science itself, Logics and Metaphysics are much more to be disparaged than Mathematics; why, therefore, if I am half blind, must I take for my guide one that can't see at all?

[p. 50]: So far as Mathematics do not tend to make men more sober and rational thinkers, wiser and better men, they are only to be considered as an amusement, which ought not to take us off from serious business.

This tract may have had something to do with Bayes's election, in 1742, to Fellowship of the Royal Society, for which his sponsors were Earl Stanhope, Martin Folkes, James Burrow, Cromwell Mortimer, and John Eames.

William Whiston, Newton's successor in the Lucasian Chair at Cambridge, who was expelled from the University for Arianism, notes in his Memoirs (p. 390) that 'on August the 24th this year 1746, being Lord's Day, and St. Bartholomew's Day, I breakfasted at Mr Bay's, a dissenting Minister at Tunbridge Wells, and a Successor, though not immediate, to Mr Humphrey Ditton, and like him a very good mathematician also'. Whiston goes on to relate what he said to Bayes, but he gives no indication that Bayes made reply.

According to Strange (1949) Bayes wished to retire from his ministry as early as 1749, when he allowed a group of Independents to bring ministers from London to take services in his chapel week by week, except for Easter, 1750, when he refused his pulpit to one of these preachers; and in 1752 he was succeeded in his ministry by the Rev. William Johnston, A.M., who inherited Bayes's valuable library. Bayes continued to live in Tunbridge Wells until his death on 17 April 1761. His body was taken to be buried, with that of his father, mother, brothers and sisters, in the Bayes and Cotton family vault in Bunhill Fields, the Nonconformist burial ground by Moorgate. This cemetery also contains the grave of Bayes's friend, the Unitarian Rev. Richard Price, author of the *Northampton Life Table* and object of Burke's oratory and invective in *Reflections on the French Revolution*, and the graves of John Bunyan, Samuel Watts, Daniel Defoe, and many other famous men.

Bayes's will, executed on 12 December 1760, shows him to have been a man of substance. The bulk of his estate was divided among his brothers, sisters, nephews and cousins, but he left £200 equally between 'John Boyl late preacher at Newington and now at Norwich, and Richard Price now I suppose preacher at Newington Green'. He also left 'To Sarah Jeffery daughter of John Jeffery, living with her father at the corner of Fountains Lane near Tonbridge Wells, £500, and my watch made by Elliott and all my linen and wearing apparell and household stuff.'

Apart from the tracts already noted, and the celebrated Essay reproduced here, Bayes wrote a letter on Asymptotic Series to John Canton, published in the *Philosophical Transactions of the Royal Society* (1763, pp. 269–271). His mathematical work, though small in quantity, is of the very highest quality; both his tract on fluxions and his paper on asymptotic series contain thoughts which did not receive as clear expression again until almost a century had elapsed.

Since copies of the volume in which Bayes's essay first appeared are not rare, and copies of a photographic reprint issued by the Department of Agriculture, Washington, D.C., U.S.A., are fairly widely dispersed, the view has been taken that in preparing Bayes's paper for publication here some editing is permissible. In particular, the notation has been modernized, some of the archaisms have been removed and what seem to be obvious printer's errors have been corrected. Sometimes, when a word has been omitted in the original, a suggestion has been supplied, enclosed in square brackets. Otherwise, however, nothing has been changed, and we hope that while the present text should in no sense be regarded as definitive, it will be easier to read on that account. All the work of preparing the text for the printer was most painstakingly and expertly carried out by Mr M. Gilbert, B.Sc., A.R.C.S. Thanks are also due to the Royal Society for permission to reproduce the Essay in its present form.

In writing the biographical notes the present author has had the friendly help of many persons, including especially Dr A. Fletcher and Mr R. L. Plackett, of the University of Liverpool, Mr J. F. C. Willder, of the Department of Pathology, Guy's Hospital Medical School, and Mr M. E. Ogborn, F.I.A., of the Equitable Life Assurance Society. He would also like to thank Sir Ronald Fisher, for some initial prodding which set him moving, and Prof. E. S. Pearson, for patient encouragement to see the matter through to completion.

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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.

Read 23 *December* 1763

Dear Sir,

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 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 [provide] 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 cause or action, one may make a judgment what is likely to be the consequence of it another time, and that the larger [the] number of experiments we have to support a conclusion, so much the more reason we have to take it for granted. But it is certain that we cannot determine, at least not to any nicety, in what degree repeated experiments confirm a conclusion, without the particular discussion of the beforementioned problem; which, therefore, is necessary to be considered by any one who would give a clear account of the strength of *analogical* or *inductive reasoning*; concerning, which at present, we seem to know little more than that it does sometimes in fact convince us, and at other times not; and that, as it is the means of [a]cquainting us with many truths, of which otherwise we must have been ignorant; so it is, in all probability, the source of many errors, which perhaps might in some measure be avoided, if the force that this sort of reasoning ought to have with us were more distinctly and clearly understood.

These observations prove that the problem enquired after in this essay is no less important than it is curious. It may be safely added, I fancy, that it is also a problem that has never before been solved. 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 exactness, given rules to find the probability there is, that if a very great number of trials be made concerning any event, the proportion of the number of times it will happen, to the number of times it will fail in those trials, should differ less than by small assigned limits from the proportion of the probability of its happening to the probability of its failing in one single trial. But I know of no person who has shewn how to deduce the solution of the converse 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 should lie somewhere between any two named degrees of probability.' What Mr De Moivre has done therefore cannot be thought sufficient to make the consideration of this point unnecessary: especially, as the rules he has given are not pretended to be rigorously exact, except on supposition that the number of trials made are infinite; from whence it is not obvious how large the number of trials must be in order to make them exact enough to be depended on in practice.

Mr De Moivre calls the problem he has thus solved, the hardest that can be proposed on the subject of chance. His solution he has applied to a very important purpose, and thereby shewn that those are much mistaken who have insinuated that the Doctrine of Chances in mathematics is of trivial consequence, and cannot have a place in any serious enquiry.† The purpose I mean is, to shew what reason we have for believing that there are in the constitution of things fixt laws according to which events happen, and that, therefore, the frame of the world must be 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

* See Mr De Moivre's *Doctrine of Chances*, p. 243, etc. He has omitted the demonstrations of his rules, but these have been since supplied by Mr Simpson at the conclusion of his treatise on *The Nature and Laws of Chance*.

† See his *Doctrine of Chances*, p. 252, etc.

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 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 valuable blessing, is the sincere wish of, Sir,

your very humble servant,

Richard Price

Newington-Green,
10 *November* 1763

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 there be three such events, and whichever of them happens I am to receive N, and that the probability of the 1st, 2nd, and 3rd are respectively a/N, b/N, c/N. Then (by the definition of probability) the value of my expectation from the 1st will be a, from the 2nd b, and from the 3rd 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 $(a+b+c)/N$ or $a/N+b/N+c/N$. 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 P/N that of it's failure will be $(N-P)/N$.

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 is 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 P/N, that of its failure (by corollary prop. 1) is $(N-P)/N$. But P/N is to $(N-P)/N$ 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 2nd on supposition the 1st happens.

Suppose that, if both events happen, I am to receive N, that the probability both will happen is P/N, that the 1st will is a/N (and consequently that the 1st will not is $(N-a)/N$) and that the 2nd will happen upon supposition the 1st does is 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, a/N is to $(N-a)/N$, i.e. a is to $N-a$ as P is to $b-P$. Wherefore (*componendo inverse*) 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 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 2nd on supposition the 1st happens.

Corollary. Hence if of two subsequent events the probability of the 1st be a/N, and the probability of both together be P/N, then the probability of the 2nd on supposition the 1st happens is P/a.

Prop. 4

If there be two subsequent events to be determined every day, and each day the probability of the 2nd is b/N and the probability of both P/N, and I am to receive N if both the events happen the first day on which the 2nd does; I say, according to these conditions, the probability of my obtaining N is P/b. For if not, let the probability of my obtaining N be x/N and let y be to x as $N-b$ to N. Then since 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 first day I have an expectation of obtaining N depending on the happening of both the events together, the probability of which is 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 on the failure of the 2nd event the probability of which (by cor. prop. 1) is $(N-b)/N$ or y/x, because y is to x as $N-b$ to N. Wherefore since x is the thing expected and 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 x/N (the probability of my obtaining N) is 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 2nd 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 2nd 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 2nd had happened. Notwithstanding therefore this discovery that the 2nd 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 x/N or 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 2nd 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.

* What is here said may perhaps be a little illustrated by considering that all that can be lost by the happening of the 2nd 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 2nd event's failing. If the 1st event does not happen, it is *for* me, and equal also to the chance for the 2nd event's failing. The loss of it, therefore, can be no disadvantage.

Prop. 5

If there be two subsequent events, the probability of the 2nd b/N and the probability of both together P/N, and it being first discovered that the 2nd event has happened, from hence I guess that the 1st event has also happened, the probability I am in the right is P/b.*

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 2nd 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 2nd 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 2nd. And in like manner considering the 1st and 2nd events 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 3rd. And thus you 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 2nd fails, the 3rd fails and the 4th happens, etc. is a ratio compounded of the probability of the 1st, and the probability of the failure of the 2nd, and the probability of the failure of the 3rd, and the probability of the 4th, etc. 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 2nd fails, and the 3rd fails and the 4th happens, etc. will be $abba$, etc. 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 2nd 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[rent] trials, is in reality the happening or failing of so many distinct independent events exactly similar to each other.

* 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 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 b/N be the probability of the given event, and p/N the probability of both, because $p/N = (b/N) \times x$, $x = p/b =$ the probability mentioned in these propositions.

Prop. 7

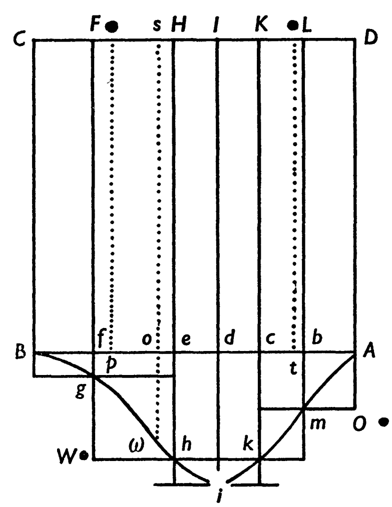
If the probability of an event be a, and that of its failure be b in each single trial, the probability of its happening p times, and failing q times in $p+q$ trials is Ea^pb^q if E be the coefficient of the term in which occurs a^pb^q when the binomial $(a+b)^{p+q}$ is expanded.

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 2nd and 3rd, and happens the 4th, fails the 5th, etc. (thus happening and failing till the number of times it happens be p and the number it fails be q) is *abbab* etc. till the number of a's be p and the number of b's be q, that is; 'tis a^pb^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^pb^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^pb^q when $(a+b)^{p+q}$ 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^pb^q, and therefore by prop. 1 the probability that some way or other it happens p times and fails q times in $p+q$ trials is Ea^pb^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 first 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, bL meeting CD in F and L. Then if the rectangles Cf, Fb, LA are commensurable to each other, they may each be divided into the same equal parts, which being done, and the ball W thrown, the probability it will rest somewhere upon any number of these equal parts will be the sum of the probabilities it has to rest upon each one of them, because its resting upon any different parts of the plane AC are so many inconsistent events; and this sum, because the probability it should rest upon any one equal part as another is the same, is the probability it should rest upon any one equal part multiplied by the number of parts. Consequently, the probability there is that the ball W should rest somewhere upon Fb is the probability it has to rest upon one equal part multiplied by the number of equal parts in Fb; and the probability it rests somewhere upon Cf or LA, i.e. that it does not rest upon Fb (because it must rest somewhere upon AC) is the probability it rests upon one equal part multiplied by the number of equal parts in Cf, LA taken together. Wherefore, the probability it rests upon Fb is to the probability it does not as the number of equal parts in Fb is to the number of equal parts in Cf, LA together, or as Fb to Cf, LA together, or as fb to Bf, Ab together. Wherefore the probability it rests upon Fb is to the probability it does not as fb to Bf, Ab together. And (*componendo inverse*) the probability it rests upon Fb is to the probability it rests upon Fb added to the probability it does not, as fb to AB, or as the ratio of fb to AB to the ratio of AB to AB. But the probability of any event added to the probability of its failure is the ratio of equality; wherefore, the probability it rests upon Fb is to the ratio of equality as the ratio of fb to AB to the ratio of AB to AB, or the ratio of equality; and therefore the probability it rests upon Fb is the ratio of fb to AB. But *ex hypothesi* according as the ball W falls upon Fb 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 somewhere upon Do or between AD and so is the ratio of Ao to AB. But the resting of the ball o between AD and so after a single throw is the happening of the event M in a single trial. Wherefore the lemma is manifest.

Prop. 8

If upon BA you erect the figure $BghikmA$ whose property is this, that (the base BA being divided into any two parts, as Ab, and Bb 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 bm, Ab, and Bb to AB, and E being the coefficient of the term in which occurs $a^p b^q$ when the binomial $(a+b)^{p+q}$ is expanded) $y = Ex^p r^q$. 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 AB, and withall that the event M should happen p times and fail q in $p+q$ trials, is the ratio of $fghikmb$, the part of the figure $BghikmA$ intercepted between the perpendiculars fg, bm raised upon the line AB, to CA the square upon AB.

DEMONSTRATION

For if not; first let it be the ratio of D a figure greater than *fghikmb* to CA, and through the points *e, d, c* draw perpendiculars to *fb* meeting the curve *AmigB* in *h, i, k*; the point *d* being so placed that *di* shall be the longest of the perpendiculars terminated by the line *fb*, and the curve *AmigB*; and the points *e, d, c* being so many and so placed that the rectangles, *bk, ci, ei, fh* 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 $Ex^p r^q$. 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, $y = Ex^p r^q$. 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 anywhere between *f* and *e*, the probability that the event M happens p times and fails q in $p+q$ trials cannot be greater than the ratio of *eh* to AB. There then being these two subsequent events, the 1st that the point *o* will lie between *e* and *f*, the 2nd that the event M will happen p times and fail q in $p+q$ trials, and the probability of the first (by lemma 1) is the ratio of *ef* to AB, and upon supposition the 1st happens, by what has been now proved, the probability of the 2nd cannot be greater than the ratio of *eh* 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 *eh* to AB, which compound ratio is the ratio of *fh* 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 *fh* 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 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 anything 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 the right is the ratio of that part of the figure AiB described as before which is intercepted between perpendiculars erected upon AB at the points f and b, to the whole figure AiB.

For, there being these two subsequent events, the first that the point o will lie between f and b; the second that the event M should happen p times and fail q in $p+q$ trials; and (by cor. prop. 8) the original probability of the second is the ratio of AiB to CA, and (by prop. 8) the probability of both is the ratio of $fghimb$ to CA; wherefore (by prop. 5) it being first discovered that the second has happened, and from hence I guess that the first has happened also, the probability I am in the right is the ratio of $fghimb$ to AiB, the point which was to be proved.

COR. The same things supposed, if I guess that the probability of the event M lies somewhere between 0 and the ratio of Ab to AB, my chance to be in the right is the ratio of Abm to AiB.

Scholium

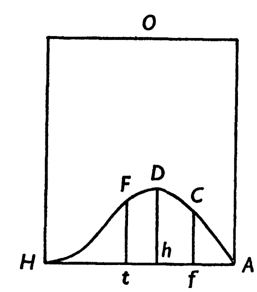
From the preceding proposition it is plain, that in the case of such an event as I there call M, from the number of times it happens and fails in a certain number of trials, without knowing anything more concerning it, one may give a guess whereabouts it's probability is, and, by the usual methods computing the magnitudes of the areas there mentioned, see the chance that the guess is right. And that the same rule is the proper one to be used in the case of an event concerning the probability of which 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 AiB to CA, which ratio is the same when $p+q$ or n is given, whatever number p is; as will appear by computing the magnitude of AiB 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 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 AiB 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.

Prop. 10

If a figure be described upon any base AH (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 AH 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 AH and that of At to AH, is the ratio of $tFCf$, that part of the before-described figure which is intercepted between the two ordinates, to $ACFH$ the whole figure insisting on the base AH.



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

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

$$y = x^p(1-x)^q$$
$$= x^p - qx^{p+1} + \frac{q(q-1)\,x^{p+2}}{2} - \frac{q(q-1)\,(q-2)\,x^{p+3}}{2.3} + \text{etc.}$$

Now the abscisse being x and the ordinate x^p the correspondent area is $x^{p+1}/(p+1)$ (by prop. 10, cas. 1, Quadrat. Newt.)† and the ordinate being qx^{p+1} the area is $qx^{p+2}/(p+2)$; and

* It will be proved presently in art. 4 by computing in the method here mentioned that AiB contracted in the ratio of E to 1 is to CA as 1 to $(n+1)E$: from whence it plainly follows that, antecedently to this contraction, AiB must be to CA in the ratio of 1 to $n+1$, which is a constant ratio when n is given, whatever p is.

† 'Tis very evident here, without having recourse to Sir Isaac Newton, that the fluxion of the area ACf being

$$y\dot{x} = x^p\dot{x} - qx^{p+1}\dot{x} + \frac{q(q-1)}{2}\,x^{p+2}\dot{x} - \text{etc.}$$

the fluent or area itself is

$$\frac{x^{p+1}}{p+1} - \frac{qx^{p+2}}{p+2} + \frac{q(q-1)\,x^{p+3}}{2(p+3)} - \text{etc.}$$

in like manner of the rest. Wherefore, the abscisse being x and the ordinate y or $x^p - qx^{p+1} + \text{etc.}$ the correspondent area is

$$\frac{x^{p+1}}{p+1} - \frac{qx^{p+2}}{p+2} + \frac{q(q-1)\,x^{p+3}}{2(p+3)} - \frac{q(q-1)\,(q-2)\,x^{p+4}}{2\,.\,3(p+4)} + \text{etc.}$$

Wherefore, if $x = Af = Af/(AH)$, and $y = Cf = Cf/(AH)$, then

$$ACf = \frac{ACf}{HO} = \frac{x^{p+1}}{p+1} - \frac{qx^{p+2}}{p+2} + \frac{q(q-1)\,x^{p+3}}{2(p+3)} - \text{etc.}$$

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

$$\frac{r^{q+1}}{q+1} - \frac{pr^{q+2}}{q+2} + \frac{p(p-1)\,r^{q+3}}{2(q+3)} - \frac{p(p-1)\,(p-2)\,r^{q+4}}{2\,.\,3(q+4)} + \text{etc.}$$

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

$$\frac{x^{p+1}r^q}{p+1} + \frac{qx^{p+2}r^{q-1}}{(p+1)(p+2)} + \frac{q(q-1)\,x^{p+3}r^{q-2}}{(p+1)(p+2)(p+3)} + \frac{q(q-1)\,(q-2)\,x^{p+4}r^{q-3}}{(p+1)(p+2)(p+3)(p+4)} + \text{etc.} + \frac{x^{n+1}q(q-1)\ldots 1}{(n+1)(p+1)(p+2)\ldots n},$$

where $n = p+q$. For this series is the same with $x^{p+1}/(p+1) - qx^{p+2}/(p+2) + \text{etc.}$ set down in Art. 1st as the value of the ratio of ACf to HO; 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 $-\dot{x}$.*

3. In like manner, the ratio of HCf to HO is

$$\frac{r^{q+1}x^p}{q+1} + \frac{pr^{q+2}x^{p-1}}{(q+1)(q+2)} + \frac{p(p-1)\,r^{q+3}x^{p-2}}{(q+1)(q+2)(q+3)} + \text{etc.}$$

* The fluxion of the first series is

$$x^p r^q \dot{x} + \frac{qx^{p+1}r^{q-1}\dot{r}}{p+1} + \frac{qx^{p+1}r^{q-1}\dot{x}}{p+1} + \frac{q(q-1)\,x^{p+2}r^{q-2}\dot{r}}{(p+1)(p+2)} + \frac{q(q-1)\,x^{p+2}r^{q-2}\dot{x}}{(p+1)(p+2)} + \frac{q(q-1)\,(q-2)\,x^{p+3}r^{q-3}\dot{r}}{(p+1)(p+2)(p+3)} + \text{etc.}$$

or, substituting $-\dot{x}$ for $\dot{r}$,

$$x^p r^q \dot{x} - \frac{qx^{p+1}r^{q-1}\dot{x}}{p+1} + \frac{qx^{p+1}r^{q-1}\dot{x}}{p+1} - \frac{q(q-1)\,x^{p+2}r^{q-2}\dot{x}}{(p+1)(p+2)} + \frac{q(q-1)\,x^{p+2}r^{q-2}\dot{x}}{(p+1)(p+2)} - \text{etc.}$$

which, as all the terms after the first destroy one another, is equal to

$$x^p r^q \dot{x} = x^p(1-x)^q\,\dot{x} = x^p\dot{x}\left[1 - qx + q\frac{(q-1)}{2}x^2 - \text{etc.}\right]$$

$$= x^p\dot{x} - qx^{p+1}\dot{x} + \frac{q\,(q-1)\,x^{p+2}}{2}\,\dot{x} - \text{etc.}$$

$$= \text{the fluxion of the latter series, or of } \frac{x^{p+1}}{p+1} - \frac{qx^{p+2}}{p+2} + \text{etc.}$$

The two series therefore are the same.

4. If E be the coefficient of that term of the binomial $(a+b)^{p+q}$ expanded in which occurs $a^p b^q$, the ratio of the whole figure $ACFH$ to HO is $\{(n+1)E\}^{-1}$, n being $= p+q$. For, when $Af = AH, x = 1, r = 0$. Wherefore, all the terms of the series set down in Art. 2 as expressing the ratio of ACf to HO will vanish except the last, and that becomes

$$\frac{q(q-1)\ldots 1}{(n+1)(p+1)(p+2)\ldots n}.$$

But E being the coefficient of that term in the binomial $(a+b)^n$ expanded in which occurs $a^p b^q$ is equal to

$$\frac{(p+1)(p+2)\ldots n}{q(q-1)\ldots 1}.$$

And, because Af is supposed to become $= AH$, $ACf = ACH$. From whence this article is plain.

5. The ratio of ACf to the whole figure $ACFH$ is (by Art. 1 and 4)

$$(n+1)E\left[\frac{x^{p+1}}{p+1} - \frac{qx^{p+2}}{p+2} + \frac{q(q-1)x^{p+3}}{2(p+3)} - \text{etc.}\right]$$

and if, as x expresses the ratio of Af to AH, X should express the ratio of At to AH; the ratio of AFt to $ACFH$ would be

$$(n+1)E\left[\frac{X^{p+1}}{p+1} - \frac{qX^{p+2}}{p+2} + \frac{q(q-1)X^{p+3}}{2(p+3)} - \text{etc.}\right]$$

and consequently the ratio of $tFCf$ to $ACFH$ is $(n+1)E$ multiplied into the difference 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 $(n+1)E$ multiplied into the difference between the series

$$\frac{X^{p+1}}{p+1} - \frac{qX^{p+2}}{p+2} + \frac{q(q-1)X^{p+3}}{2(p+3)} - \text{etc.}$$

and the series

$$\frac{x^{p+1}}{p+1} - \frac{qx^{p+2}}{p+2} + \frac{q(q-1)x^{p+3}}{2(p+3)} - \text{etc.}$$

E being the coefficient of $a^p b^q$ when $(a+b)^n$ 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 everywhere 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 series.

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 series 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 $(p/n)+z$ and $(p/n)-z$; if $m^2 = n^3/(pq)$, $a = p/n$, $b = q/n$, E the coefficient of the term in which occurs a^pb^q when $(a+b)^n$ is expanded, and

$$\Sigma = \frac{(n+1)\sqrt{(2pq)}}{n\sqrt{n}} Ea^pb^q$$

multiplied by the series

$$mz - \frac{m^3z^3}{3} + \frac{(n-2)\,m^5z^5}{2n\,.\,5} - \frac{(n-2)\,(n-4)\,m^7z^7}{2n\,.\,3n\,.\,7} + \frac{(n-2)\,(n-4)\,(n-6)\,m^9z^9}{2n\,.\,3n\,.\,4n\,.\,9} - \text{etc.}$$

my chance to be in the right is greater than

$$\frac{2\Sigma}{1+2Ea^pb^q+2Ea^pb^q/n}\,*$$

and less than

$$\frac{2\Sigma}{1-2Ea^pb^q-2Ea^pb^q/n},$$

and if $p = q$ my chance is 2Σ exactly.

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 Ea^pb^q and also of the series $mz - \frac{1}{3}m^3z^3 + \text{etc.}$ With respect to the former Mr Bayes has proved that, supposing K to signify the ratio of the quadrantal arc to its radius, Ea^pb^q will be equal to $\frac{1}{2}\sqrt{n}/\sqrt{(Kpq)}$ multiplied by the *ratio*, $[h]$, whose *hyberbolic* logarithm is

$$\frac{1}{12}\left[\frac{1}{n}-\frac{1}{p}-\frac{1}{q}\right] - \frac{1}{360}\left[\frac{1}{n^3}-\frac{1}{p^3}-\frac{1}{q^3}\right] + \frac{1}{1260}\left[\frac{1}{n^5}-\frac{1}{p^5}-\frac{1}{q^5}\right] - \frac{1}{1680}\left[\frac{1}{n^7}-\frac{1}{p^7}-\frac{1}{q^7}\right] + \frac{1}{1188}\left[\frac{1}{n^9}-\frac{1}{p^9}-\frac{1}{q^9}\right] - \text{etc.}\dagger$$

* In Mr Bayes's manuscript this chance is made to be greater than $2\Sigma/(1+2Ea^pb^q)$ and less than $2\Sigma/(1-2Ea^pb^q)$. 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.

† 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 DeMoivre, 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

$$\tfrac{1}{2}\log c + (x+\tfrac{1}{2})\log x - x + \frac{1}{12x} - \frac{1}{360x^3} + \frac{1}{1260x^5} - \text{etc.}$$

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 always be 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 notice of mathematicians.

where the numeral coefficients may be found in the following manner. Call them A, B, C, D, E etc. Then

$$A=\frac{1}{2.2.3}=\frac{1}{3.4},\quad B=\frac{1}{2.4.5}-\frac{A}{3},\quad C=\frac{1}{2.6.7}-\frac{10B+A}{5},$$

$$D=\frac{1}{2.8.9}-\frac{35C+21B+A}{7},\quad E=\frac{1}{2.10.11}-\frac{126C+84D+36B+A}{9},$$

$$F=\frac{1}{2.12.13}-\frac{462D+330C+165E+55B+A}{11}\text{ etc.}$$

where the coefficients of B, C, D, E, F, etc. in the values of D, E, F, etc. are the 2, 3, 4, etc. highest coefficients in $(a+b)^7$, $(a+b)^9$, $(a+b)^{11}$, etc. expanded; affixing in every particular value the least of these coefficients 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

$$mz-\tfrac{1}{3}m^3z^3+\frac{(n-2)m^5z^5}{2n.5}\text{ etc.}$$

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 its 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

$$\frac{2^2-1}{2n}-\frac{2^4-1}{360n^3}+\frac{2^6-1}{1260n^5}-\frac{2^8-1}{1680n^7}+\text{etc.}$$

Then the series $mz-\frac{1}{3}m^3z^3+$ etc. will be greater or less than the series

$$\frac{Hn\sqrt{K}}{(n+1)\sqrt{2}}-\frac{n\left(1-\frac{2m^2z^2}{n}\right)^{\frac{1}{2}n+1}}{(n+2)\,2mz}+\frac{n^2\left(1-\frac{2m^2z^2}{n}\right)^{\frac{1}{2}n+2}}{(n+2)\,(n+4)\,4m^3z^3}$$
$$-\frac{3n^3\left(1-\frac{2m^2z^2}{n}\right)^{\frac{1}{2}n+3}}{(n+2)\,(n+4)\,(n+6)\,8m^5z^5}+\frac{3.5.n^4\left(1-\frac{2m^2z^2}{n}\right)^{\frac{1}{2}n+4}}{(n+2)\,(n+4)\,(n+6)\,(n+8)\,16m^7z^7}-\text{etc.}$$

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 Ea^pb^q and

$$mz-\frac{m^3z^3}{3}+\frac{(n-2)}{2n}\frac{m^5z^5}{5}\text{etc.}$$

in the second rule arises a third rule, which is the rule to be used when mz is of some considerable magnitude.

* 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 ond Laws of Chance.*

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 its happening in a single trial lies between $p/n+z$ and $p/n-z$ my chance to be right is *greater* than

$$\frac{\frac{1}{2}\sqrt{(Kpq)}\,h}{\sqrt{(Kpq)}+hn^{\frac{1}{2}}+hn^{-\frac{1}{2}}}\left\{2H-\frac{\sqrt{2}\,(n+1)\,(1-2m^2z^2/n)^{\frac{1}{2}n+1}}{\sqrt{K}\,(n+2)\,mz}\right\}$$

and *less* than

$$\frac{\frac{1}{2}\sqrt{(Kpq)}\,h}{\sqrt{(Kpq)}-hn^{\frac{1}{2}}-hn^{-\frac{1}{2}}}\left\{2H-\frac{\sqrt{2}\,(n+1)\,(1-2m^2z^2/n)^{\frac{1}{2}n+1}}{\sqrt{K}\,(n+2)\,mz}+\frac{\sqrt{2}\,n\,(n+1)\,(1-2m^2z^2/n)^{\frac{1}{2}n+2}}{\sqrt{K}\,(n+2)\,(n+4)\,2m^3z^3}\right\}$$

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

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 third in all cases where m^2z^2 is greater than 1 and less than $\frac{1}{2}n$, 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 $p/n-z$ and $p/n+z$), and yet the operation be carried on by the second 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 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

$$(n+1)\left\{\frac{X^{p+1}}{p+1}-\frac{x^{p+1}}{p+1}\right\} \quad \text{or} \quad X^{p+1}-x^{p+1}$$

gives the solution, as will appear from considering the first rule. Put therefore in this expression $p+1=2, X=1$ and $x=\frac{1}{2}$ and it will be $1-(\frac{1}{2})^2$ 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 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 $X^{p+1}-x^{p+1}=(\frac{16}{17})^{11}-(\frac{2}{3})^{11}=0{\cdot}5013$ etc. The answer therefore is, that we shall have very nearly an equal chance for being right.

* 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.

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 beforehand; 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 has *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 1,600,000/1,600,001 raised to the millioneth power subtracted from unity, which is equal to 0·4647 etc and the chance for the former is equal to 1,400,000/1,400,001 raised to the same power, or to 0·4895; 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 0·527, 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, beforehand 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. 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 condition 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 0·5352, that the odds for this was not *greater* than 1,600,000 to 1; and the probability expressed by 0·5105, 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 permanent. 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 constancy; but we can have no reason for thinking that there are no causes in nature which will *ever* interfere 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, is $(n+1)E$ multiplied by the difference between

$$\left\{\frac{X^{p+1}}{p+1}-\frac{qX^{p+2}}{p+2}\right\} \quad \text{and} \quad \left\{\frac{x^{p+1}}{p+1}-\frac{qx^{p+2}}{p+2}\right\} = 12.11.\left\{\left[\frac{(\frac{11}{12})^{11}}{11}-\frac{(\frac{11}{12})^{12}}{12}\right]-\left[\frac{(\frac{9}{10})^{11}}{11}-\frac{(\frac{9}{10})^{12}}{12}\right]\right\} = 0{\cdot}07699 \text{ etc.}$$

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 0·6589, 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

$$(n+1)E\left\{\left[\frac{X^{p+1}}{p+1}-\frac{qX^{p+2}}{p+2}+\frac{q(q-1)X^{p+3}}{2(p+3)}\right]-\left[\frac{x^{p+1}}{p+1}-\frac{qx^{p+2}}{p+2}+\frac{q(q-1)x^{p+3}}{2(p+3)}\right]\right\} = 0{\cdot}10843 \text{ etc.}$$

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 0·6589 or an odds of near two to one, it will be 0·584, or an odds of 584 to 415.

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

The answer here is 0·1525, for the former of these chances; and 0·527, 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 0·44109, and for a proportion *greater* than 11 to 1, 0·3082. 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 0·2506 etc. 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* 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, 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 0·2506 etc. Let [us] 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 series

$$\frac{X^{p+1}}{p+1}-\frac{qX^{p+2}}{p+2}\text{ etc.}\quad\text{and}\quad\frac{x^{p+1}}{p+1}-\frac{qx^{p+2}}{p+2}\text{ etc.}$$

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

$$\frac{2\Sigma}{1+2Ea^pb^q+\dfrac{2Ea^pb^q}{n}}$$

and less than

$$\frac{2\Sigma}{1-2Ea^pb^q-2E\dfrac{a^pb^q}{n}}$$

Σ being

$$\frac{(n+1)\sqrt{(2pq)}}{n\sqrt{n}}Ea^pb^q\left\{mz-\frac{m^3z^3}{3}+\frac{(n-2)\,m^5z^5}{2n.5}-\text{etc.}\right\}$$

* 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.

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

By making here $1000 = p$, $100 = q$, $1100 = n$, $\frac{1}{110} = z$,

$$mz = z\sqrt{\left(\frac{n^3}{pq}\right)} = 1{\cdot}048808, \quad Ea^pb^q = \tfrac{1}{2}h\frac{\sqrt{n}}{\sqrt{(Kpq)}},$$

h being the ratio whose hyperbolic logarithm is

$$\frac{1}{12}\left[\frac{1}{n}-\frac{1}{p}-\frac{1}{q}\right]-\frac{1}{360}\left[\frac{1}{n^3}-\frac{1}{p^3}-\frac{1}{q^3}\right]+\frac{1}{1260}\left[\frac{1}{n^5}-\frac{1}{p^5}-\frac{1}{q^5}\right]-\text{etc.}$$

and K the ratio of the quadrantal arc to radius; the former of these expressions will be found to be 0·7953, and the latter 0·9405 etc. The chance enquired after, therefore, is greater than 0·7953, and less than 0·9405. 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 11,000 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

$$\left\{mz-\frac{m^3z^3}{3}+\frac{(n-2)\,m^5z^5}{2n\,.\,5}-\text{etc.}\right\}$$

The third rule, therefore, must be used; and the information it gives us is, that the required chance is greater than 0·97421, or more than an odds of 40 to 1.

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 to our judgment that may be of considerable use till some person shall discover a better approximation to the value of the two series 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 second and third rules by demonstrating that the expression $2\Sigma/\{1+2Ea^pb^q+2Ea^pb^q/n\}$ 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.