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Mill's Conversion: The Herschel Connection

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Introduction

John Stuart Mill's *A System of Logic, Ratiocinative and Inductive, being a connected view of the principles of evidence, and the methods of scientific investigation* was the most popular and influential treatment of scientific method throughout the second half of the 19th century. As is well-known, there was a radical change in the view of probability endorsed between the first and second editions. There are three different conceptions of probability interacting throughout the history of probability:

> (1) Chance, or Propensity — for example, the bias of a biased coin.
>
> (2) Judgmental Degree of Belief — for example, the degree of belief one should have that the bias is between .6 and .7 after 100 trials that produce 81 heads.
>
> (3) Long-Run Relative Frequency — for example, proportion of heads in a very large, or even infinite, number of flips of a given coin.

It has been a matter of controversy, and continues to be to this day, which conceptions are basic. Strong advocates of one kind of probability may deny that the others are important, or even that they make sense at all.

In the first edition of 1843, Mill espouses a frequency view of probability that aligns well with his general material view of logic:

> Conclusions respecting the probability of a fact rest, not upon a different, but upon the very same basis, as conclusions respecting its certainly; namely, not our ignorance, but our knowledge: knowledge obtained by experience, of the proportion between the cases in which the fact occurs, and those in which it does not occur. … (Mill 1843 Ch. XVIII "Of the Calculation of Chances", p. 73)

The Bayesian views of Laplace are attacked already in the table of contents. The first section of Chapter XVIII, from which the preceding quotation is drawn, is entitled "The foundation of the doctrine of chances, as taught by Laplace, defective." And in the opening of the chapter itself, after quoting Laplace to the effect that probability has reference "partly to our ignorance, partly to our knowledge", Mill goes on to say:

> Such is this great mathematician's statement of the logical foundation upon which rests, according to him, the theory of chances: and if his unrivaled command over the means which mathematics supply for calculating the results of given data, necessarily implied an equally sure judgment of what the data ought to be, I should hardly dare give utterance to my conviction, that in this opinion he is entirely wrong … . (Mill 1843, p. 71)

Mill believes that the Bayesian-Laplacian ideas of a *chance* of a biased coin coming up heads, and of a *judgmental degree of belief* over the possible chance biases, to be updated on evidence, are mistakes! The only probabilities that make sense for Mill are frequencies of occurrences in a large number of trials. This is a position later defended by John Venn in his (1866) *The Logic of Chance*.

In a later chapter dealing with the testimony, "On the grounds of disbelief", Mill again takes the stance that Laplace, a good mathematician but a poor philosopher, has been led into error by philosophical mistakes: "Laplace again, falling into the same confusion …" (Mill 1843, p. 194) and "The mathematical reasoning which led Laplace into this logical error is too long to be here quoted." (Mill 1843, p. 196). What is at stake here is the special issue of the application of Bayes' theorem in analyzing the credibility of testimony.

This issue has a history going back to Hume's discussion of miracles. Many contemporaries objected that testimony can often convince one of events that antecedently appeared quite unlikely. You may believe reports that such-and-such a ticket has won a large lottery, or that there has been a large earthquake where none was expected. Why not believe reports of miracles? Without explicitly mentioning this literature, Laplace points out what is wrong with it. Likelihoods need to be carefully considered and factored in. This is discussed in Zabell (1988, p. 179).

There is a larger issue in the background. Interpretation of testimony is, in principle, no different from the interpretation of a medical test — or of any result of a scientific experiment, or indeed of the testimony of the senses. One must carefully consider the probabilities of different reports conditional on the hypotheses being true and conditional on it being false together with prior probabilities. For a discussion of what can go wrong if this is not done, see Ionnides (2005).

Yet, in the second edition of 1846, in the chapter on the calculation of chances, Mill retracts his criticism of Laplace. [I take all second-edition quotations from the scholarly cumulative edition of Robson, in Mill *Collected Works* (1963–1991), which details all changes from edition to edition of Mill's *Logic*.] In the second edition:

> This view of the subject was taken in the first edition of the present work; but I have since become convinced that the theory of chances, as conceived by Laplace and by mathematicians generally, has not the fundamental fallacy which I had ascribed to it. (Mill 1846, From Mill *Collected Works* vol. VII, p. 535)

Mill proceeds to endorse a thoroughly Bayesian theory of probability.

And in the later chapter on the grounds of disbelief, Mill again retracts:

> This argument of Laplace's, though I formerly thought it fallacious, is irrefragable in the case which he supposes, and in all others which that case fairly represents. (Mill 1846, From Mill *Collected Works* v. VII, p. 636)

What happened? Mill's scientific correspondents changed his mind. In his preface to the second edition, Mill gives credit:

> The only portions which have been materially changed are the chapter on the Calculation of Chances, and the latter part on the Grounds of Disbelief; on both of which topics the author has been indebted to Sir John Herschel, and to Mr. J. M. Macleod, for some important rectifications of his original conclusions. (Mill 1846, From Mill *Collected Works* v. VII, cxiv)

The input due to Macleod does not seem to be ascertainable, but the Mill-Herschel correspondence is largely preserved. Herschel's letters to Mill are in the library of the Royal Society, and they, together with Mill's replies, are available on microfilm. I am indebted to Katherine Marshall, librarian at the Royal Society, for high-resolution scans of Herschel's letters to Mill. Mill's replies have been transcribed, and the transcriptions appear in Mill's *Collected Works*. My main purpose here is to make the story more widely known, to set it in context, and to make the relevant manuscripts widely available as appendices to this article.

I should point out that this correspondence has been already referenced in the scholarly edition of Mill's *Logic* edited by J. M. Robson (Mill 1963–1991) as part of Mill's *Collected Works*. John V. Strong (1978) discusses the influence of Herschel, and quotes from an important letter from Herschel to Mill of December 1845, concerning the section on the calculation of chances. But there is more of the story to tell, and the additional details may be of interest to the reader.

1. Mill on Laplace

Mill's criticism of Laplace's basic theory in the first edition is contained in chapter XVIII, "Of the Calculation of Chances". The preceding chapter has the curious title "Of Chance, *and its Elimination*" (emphasis mine). Why are chances supposed to be eliminated? The reason is that Mill is mainly interested in chance as experimental error. Experimental error prevents Mill's methods of experimental inference — Agreement, Difference, and so on — being strictly applicable. Mill sees the answer as lying in the repetition of the experiments and averaging of the results, with the errors cancelling each other out. Will the errors really cancel out? Mill's discussion is qualitative, but at its end he sees that since the number of repetitions is necessarily finite, the doctrine of chances "or in a phrase of greater pretention the Theory of Probabilities" is relevant. "An attempt at a philosophical appreciation of that doctrine is, therefore, a necessary portion of our task" (Mill 1843 v. II, p. 69).

The next section moves immediately to a criticism of Laplace. It is based entirely on Laplace's 1840 *Essai philosophique*. Mill objects to ignorance playing any role in probability; frequency is everything.

> To pronounce two events equally probable, it is not enough that we should know that one or the other must happen, and should have no ground for conjecturing which. Experience must have shown that the two events are of equally frequent occurrence. (Mill 1843 v. II, p. 71)

To make his point he then introduces an example that pops up throughout the history of philosophical discussions of the nature of probability:

> Why, in tossing up a halfpenny, do we reckon it equally probable that we shall throw cross or pile? Because experience has shown that in any great number of throws, cross and pile are thrown about equally often; and that the more throws we make the more nearly the equality is perfect. …
>
> It would indeed require strong evidence to persuade any rational person that by a system of operations upon numbers, our ignorance can be coined into science…. (Mill 1843 v. II, p. 71)

Mill is contrasting probability ½ based on ignorance, which he takes to be Laplace's position, with probability based on relative frequency. (That he takes the former as Laplace's position is made clear when he reiterates the example with respect to a die: "In the cast of a die, the probability of an ace is one-sixth; not, as Laplace would say, because there are six possible throws, and because we do not know any reason why one should turn up rather than another…" [Mill 1843 v. II, p. 71].)

The same example is put forward by the philosopher Karl Popper as the "paradox of ideal evidence" in his 1959 *The Logic of Scientific Discovery* (new appendix ix, third note), where it is held to cause insuperable difficulties for the subjective theory of probability:

> Considerations of the "weight of evidence" lead, within the subjective theory of probability, to paradoxes which, in my opinion, are insoluble within the framework of that theory. (Popper 1959, p. 425)

There are two cases: probability ½ on little or no evidence, or probability ½ on ideal evidence. Popper thinks that degree-of-belief theories cannot distinguish between the cases. He takes this as a definitive objection to Carnap, just as Mill took it as a definitive objection to Laplace.

Mill also thinks that Bayes' theorem has limited applicability — it is valid for inference to causes but not to hypotheses. Neglect of this distinction is thought by Mill to have led Laplace into a great error regarding testimony:

> This error of Laplace has not been harmless. We shall see hereafter, in treating the Grounds of Disbelief, that he has been led by it into serious practical mistakes when attempting to pronounce upon the circumstances that render any statement incredible. (Mill 1843 v. II, p. 80)

Mill returns to this in his section on the grounds of disbelief (Mill 1843 v. II, p. 195). Laplace had begun his chapter on testimony with a simple exercise in using Bayes' theorem. He contrasts two cases. In both cases, a ball is drawn at random from an urn containing a thousand, and the result is reported by a witness who is known to lie one-tenth of the time. In each case, we wish to know whether the witness gave a true report.

In the first example, the witness is to report the number of the ball drawn, and reports number 79. In the second example, the urn contains 999 black balls and one white, and the witness reports a white ball was drawn. Laplace leads the reader through the computations that show that in the first case the probability that the ball really is 79 is 9/10, but in the second case the probability that the ball is really the white one is only 9/1008.

Mill cannot believe this:

> This appears to me entirely fallacious. It is evident, both from general reasoning and from specific experience, that the white ball will be drawn out exactly as often, in a large number of trials, as the Ticket No. 79 will; the two assertions, therefore, are on exactly the same level in point of credibility. There is one way of putting the case which, I think, must carry conviction to everyone. Suppose that the thousand balls are numbered, and that the white ball happens to be ticketed 79. Then the drawing of the white ball, and the drawing of No. 79, are the very same event; how then can one be credible and the other absolutely incredible? (Mill 1843 v. II, p. 195)

Mill, then, thinks his two examples, the halfpenny with unknown bias and the case in which ticket number 79 is the white one, constitute clear and convincing philosophical counterexamples to, respectively, Laplace's theory of the meaning of probability, and his application of probability theory to the credibility of testimony.

2. The correspondence between Herschel and Mill

Mill had sent Herschel a copy of the first edition in hopes that he would review it favorably. Herschel wrote back thanking Mill. On May 1, 1843, Mill wrote to Herschel acknowledging the thank-you note, saying that his experimental methods owe much to Herschel:

> You will find that the most important chapter of the book, that on the four Experimental methods, is little more than an expansion & a more scientific statement of what you had previously stated …. (Royal Society ms. HS 25/6/26) (Transcribed in Mill *Collected Works* v. XIII, letter 397) (referring to Herschel 1830)

Mill then proceeds to ask for criticism of his *System of Logic*:

> I should be very grateful if you could, without encroaching on time which is more valuably employed, note down some of the many errors I must have committed as well as of the important ideas I must have missed. (Royal Society ms. HS 25/6/26) (Transcribed in Mill *Collected Works* v. XIII, letter 397)

Herschel never reviewed Mill's book. He did, however, mention the matter in another letter to Mill. This letter was principally about Auguste Comte on the nebular hypothesis concerning the formation of the solar system. Herschel had ridiculed Comte in his presidential address to the British Association for the Advancement of Science on June 19, 1845. After pointing out Comte's errors, Herschel went on to say:

> I really should consider some apology needed for even mentioning an argument of the kind to such a meeting, were it not that this very reasoning, so ostentatiously put forward, and so utterly baseless, has been eagerly received among us* as the revelation of a profound analysis. (Herschel 1857, p. 667)

The asterisk is a footnote to Mill.

On July 9, Mill wrote to Herschel saying that he was gratified at being mentioned in Herschel's Presidential Address, but that he believed that Herschel was in error regarding Comte:

> I am writing … to call your attention to an act of injustice which you have, I am sure unintentionally, committed against the scientific reputation of a distinguished man. You have imputed to M. August Comte, not only a gross blunder in reasoning …. (Mill *Collected Works* v. XIII letter 464)

Herschel dismantles Comte's argument in his reply to Mill on July 10 (Royal Society ms. HS/25/6/26). This is not our concern here. The full story is told by a distinguished physicist and historian of science in Schweber (1991).

After discussing Comte, Herschel says that he had planned to review Mill's *System of Logic* frankly, but didn't have time to do so. His opinion of Mill's general philosophical point of view is indeed very positive, but he finds "the least felicitous portions of it, those in which points of physical science and mathematics are touched upon". He then offers to write Mill what the main points of the review would have said. A transcription of the full letter is given in Schweber 1991 (Appendix A, pp. 175–176). Schweber's transcription of the part of this letter relevant to our present concerns is included in Appendix 2.

On December 19, 1845, Mill wrote to take Herschel up on the offer:

> Some time ago, you did me the favour to intimate that you would have no objection to communicate to me some of your remarks on my "System of Logic," particularly those parts of it in which physical and mathematical subjects were adverted to. I have so little claim to ask you to take this trouble, that I am almost ashamed to remind you of your intention — but as I am informed by the publisher that he is about to prepare for a second edition, the

> advantage which I hope to derive from your criticisms would be peculiarly valuable if it could be afforded in time for that purpose. (Royal Society ms. HS 12/334) (Mill *Collected Works* v. XIII letter 476)

Herschel to Mill, Dec 22 (27), 1845 (Royal Society ms. HS/25/6/30) Folio 81 r. – 84 v.

Herschel obliges in a long letter to Mill headed December 22, 1845 (but ended December 27). He begins by saying that he read Mill's objections to Laplace "with great concern" and hopes that Mill will reconsider. Herschel takes Mill to task for misrepresenting Laplace on equipossibility. This is not just some mechanical principle. What cases are taken as equipossible is, according to Laplace, a matter of judgment. "Of course, by 'equally possible' he must mean equally possible concerning out limited judgment or conceptions" (Royal Society ms. HS/25/6/30, Folio 82 v.). Mill is, after all, a Newtonian determinist and believes that if we knew all the causes, the probability of an event would be one or zero. (Mill says so in his section on the "elimination of chance".) Alternatives would never be equally possible if we knew all. Herschel presses the point:

> The estimation of the elementary probabilities (or the determination of what shall be considered as equal probabilities) is a matter of common sense, which except in certain very simple cases must be open (as Laplace admits) to considerations of very great delicacy. Still it must always be a matter of opinion & judgment that these elementary events are equally possible or equally likely to happen — for after all what is likelihood? It is a judgment, an impression — whether founded on a hundred thousand trials or on a simple want of an apparent reason for preference. (Royal Society ms. HS/25/6/30, Folio 83 r.)

Herschel goes on to show that Mill's own frequentism does not account for cases on which he would like to base it:

> I do not suppose that in the history of any cardplayer's experience spades have actually turned up trumps exactly as often as hearts — no not by hundreds of times. Yet he believes the chance to be equal. Why so? Is not his belief here opposed to his experience? (HS/25/6/30, Folio 83 r.)

According to what Mill has said, it is.

And Herschel introduces analogical reasoning between similar setups (of the kind later discussed by de Finetti 1938) and asks how Mill's frequentism accounts for it:

> We judge the chances of a certain pair of dice from a million casts made with them (Suppose such a violent case.) How does that help us to bet on throwing sixes with another pair of dice? Have we tried a million pairs of dice, and thence by experience ascertained the chances of fairness or unfairness in a pair taken at random? Assuredly not. No man hesitates about a question of this kind. He reasons (and I contend justly & according to the true spirit of the calculus.) on the apparent equality of the chances — but always with a reserve "if the dice be cogged then indeed it is another affair." (HS/25/6/30, Folio 83 r.)

At this point Herschel breaks off. He says that because of the presence of holiday guests, he does not have time to go on to the question of testimony.

On December 29, Mill sent a short note in reply saying that he would reconsider, but that he still thinks he is right about testimony:

> I had already been convinced by other criticisms, that the chapter on which you comment required to be seriously reconsidered & that Laplace was not so far wrong as I had ventured to think him. The other point however,

> on which I differed from him, is one on which I have not hitherto been shaken, but I have not the smallest reluctance to acknowledge myself wrong on this also if it turns out that I am so. (HS 12/335) (Mill *Collected Works* v. XIII letter 477)

On February 20, 1846, Mill writes to Herschel, saying again that his publisher is preparing a second edition and he would be grateful for any further remarks (Mill *Collected Works* v. XIII letter 480). On March 30, Mill writes yet again, saying that volume one of the second edition has been printed and he is being pressed for the manuscript of volume two. He wants to get Laplace right, and will stop the printers to wait for Herschel's input (Mill *Collected Works* v. XIII letter 483).

Herschel to Mill April 2, 1846 (Royal Society ms. HS/ 25/6/32, Folio 87 r. – 89 r.)

Herschel first points out that Mill brings up all sorts of extraneous suggestions that are excluded by the explicit and precise assumptions that Laplace has made. The witness lies (or makes an error, if you please) with fixed probability 1/10. Then, to Mill's idea that the cases must be the same if we make ball number 79 the white ball, Herschel says that he is content with pointing out a difference: If someone is reporting color and draws other than 79 and lies, he must report white. If someone is reporting a number and draws other than 79 and lies, he may report any number other than the one that he drew. This is made explicit in the Bayesian calculation through which Laplace leads the reader, but it is evident that Mill has not worked through this reasoning (as he himself later remarks).

Herschel to Mill April 3 (Royal Society ms. HS 25/6/33, Folio 90 r.)

On the next day, Herschel wrote again, enclosing worked-out problems:

> It seems to me that so presented, the evidence is irresistible and I doubt not that you will perceive it to be so. — The conclusions agree exactly with Laplace, but I have added two deductions (Probls III & IV) which set the difference of the cases in a still stronger light.

The calendar of Herschel's correspondence (Crowe et. al. 1998) has the notation "[enclosure not found]". However, in the collection of Mill's replies to Herschel, there is a manuscript (Royal Society ms. HS/12/340). "Mill" is written in the upper right-hand corner. There are notations in other hands and other inks at the top, which invite consideration. There is an "H" in purple ink. There is a notation, which seems to be in pencil: "? in answer to letter of 8th Ap 46". But the only letter of April 8 is from Mill to Herschel, which we discuss next. Here is a little mystery. Is this the missing enclosure from Herschel? I think not. The handwriting appears to be Mill's rather than Herschel's. I believe that this is Mill's working through of these problems after seeing Herschel's enclosure.

Mill replied on April 8:

> Your second letter, as you anticipated, has convinced me. An analysis of the cases, such as you have given, is the last appeal where there is any doubt, & if I had resorted to it (which would have been more in conformity with my usual mode of working) I could not have fallen into the error which I committed, & which I am greatly indebted to you for causing me to rectify.

He adds that he has rewritten the earlier chapter on chances, "on which subject I now entirely agree with Laplace" (Royal Society ms. HS 12/339) (Mill *Collected Works* v. XIII letter 485).

3. Mill's revision

In the second edition of his work, Mill thoroughly revised the sections that Herschel commented on, and these are the principal revisions of the work. He even had the revisions reprinted as a pamphlet, possibly to send to those who had received the first edition (Mill *Collected Works* v. VII, lxxxi ftnt 87).

In the section on chance, Mill starts as in the first edition and repeats the objection about tossing a halfpenny. But he then proceeds to retract:

> This view of the subject was taken in the first edition of the present work; but I have since become convinced that the theory of chances, as conceived by Laplace and by mathematicians generally, has not the fundamental fallacy which I had ascribed to it. (Mill *Collected Works* v. VII, p. 535)

He then proceeds to a fully subjective, judgmental view of the meaning of probability:

> We must remember that the probability of an event is not a quality of the event itself, but a mere name for the degree of ground which we, or some one else, have for expecting it. The probability of an event to one person is a different thing from the probability of the same event to another, or to the same person after he has acquired additional evidence. The probability to me, that an individual of whom I know nothing but his name, will die within a year, is totally altered by my being told, the next minute, that he is in the last stage of a consumption. Yet this makes no difference in the event itself, nor in any of the causes on which it depends. Every event is in itself certain, not probable: if we knew all, we should either know positively that it will happen, or positively that it will not. But its probability to us means the degree of expectation of its occurrence, which we are warranted in entertaining by our present evidence. (Mill *Collected Works* v. VII, p. 535)

What if there is no "present evidence" at all? Mill goes on to endorse a principle of indifference in cases of complete ignorance. I quote a long passage to make clear how radical the change is from the first edition:

> Suppose that we are required to take a ball from a box, of which we only know that it contains balls both black and white, and none of any other colour. We know that the ball we select will be either a black or a white ball; but we have no ground for expecting black rather than white, or white rather than black. In that case, if we are obliged to make a choice, and to stake something on one or the other supposition, it will, as a question of prudence, be perfectly indifferent which; and we shall act precisely as we should have acted if we had known beforehand that the box contained an equal number of black and white balls. But though our conduct would be the same, it would not be founded on any surmise that the balls were in fact thus equally divided; for we might, on the contrary, know, by authentic information, that the box contained ninetynine balls of one colour, and only one of the other; still, if we are not told which colour has only one, and which has ninety-nine, the drawing of a white and of a black ball will be equally probable to us; we shall have no reason for staking anything on the one event rather than on the other; the option between the two will be a matter of indifference; in other words it will be an even chance. (Mill *Collected Works* v. VII, pp. 535–536)

He proceeds to illustrate the quantification of ignorance by indifference by balls of three colors, and then ends the discussion of the nature of chance on a pragmatic, judgmental note:

> The common theory, therefore, of the calculation of chances, appears to be tenable. Even when we know nothing except the number of the possible and mutually

> excluding contingencies, and are entirely ignorant of their comparative frequency, we may have grounds, and grounds numerically appreciable, for acting on one supposition rather than on another; and this is the meaning of Probability. (Mill *Collected Works* v. VII, pp. 536–537)

The meaning of probability is not frequency, but grounds for acting on one supposition rather than another.

In Chapter XXV, "On the grounds of disbelief", he now embraces Laplace's use of Bayes' theorem:

> This argument of Laplace's, though I formerly thought it fallacious, is irrefragable in the case which he supposes, and in all others which that ease fairly represents. (Mill *Collected Works* v. VII, p. 636)

He walks through the correct reasoning, and explains to the reader:

> White, then, is drawn, on an average, exactly as often as No. 79, but it is announced, without having been really drawn, 999 times as often as No. 79; the announcement, therefore, requires a much greater amount of testimony to render it credible. (Mill *Collected Works* v. VII, pp. 635–636)

In each case, he wishes to retain as much as he can of what he said before. In the section on chance, he insists that we should not be content with perfect ignorance, but should get as much evidence as possible. And he insists that frequency evidence from many trials is valuable. Laplace would not have disagreed.

In the chapter on testimony, Mill insists that Laplace's assumptions are implausible — "A person is far less likely to mistake, who has only one form of error to guard against, than if he had 999 different errors to avoid" — and concludes that "Laplace's argument, therefore, is faulty even as applied to his own case" (Mill *Collected Works* v. VII, p. 636).

The tone is rather different from that of Mill's letter of April 8 to Herschel. Laplace did not, of course, intend this case as a universal analysis of testimony, and indeed explored some variations to give examples of the use of probabilistic analysis. Herschel had already made this point to Mill in his letter of April 2.

4. Was Mill really converted?

Mill certainly was convinced that in the first edition he had gone too far, and his retractions persisted through all subsequent editions. But he was either not motivated, or too busy, to rethink the rest of the book from a Bayesian point of view. There were relevant ideas in the air. For instance, Laplace's Bayesian justification of the method of least squares (based on Gauss' justification of the normal distribution) dates from 1810. (See Stigler [1986, pp. 143 ff].) This treatment of experimental error could have been a natural part of any rethinking of Mill's *Logic*.

In 1866, John Venn set forth an uncompromising frequency view of probability in his *The Logic of Chance*. Charles Sanders Peirce, another uncompromising frequentist, reviewed it favorably in 1867:

> When this doctrine was first studied, probability seems to have been regarded as something inhering in the singular events, so that it was possible for Bernouilli to enounce it as a theorem (and not merely as an identical proposition), that events happen with frequencies proportional to their probabilities. That was a realistic view. Afterwards it was said that probability does not exist in the singular events, but consists in the degree of credence which ought to be reposed in the occurrence of an event. This is conceptualistic. Finally, probability is regarded as the ratio of the number of events in a certain part of an aggregate of them to the number in the whole aggregate. This is the nominalistic view. This last is the position of Mr. Venn and of the most advanced writers on the subject. The theory was perhaps first put forth by Mr. Stuart Mill; but his head became involved in clouds, and he relapsed.

Venn sent Mill a copy of his book, and Mill replied in a letter of February 4, 1868:

> Your general mode of viewing this class of questions is by far the best and most philosophical I have met with; and while there is evidence of a great agreement between us in our mode of regarding the great problems of inductive philosophy, you have, on this particular subject, thrown light upon many more points than space and time had allowed me to enter into. (Letter 1186A in *The Collected Works of John Stuart Mill vol.* XVI*: The Later Letters of John Stuart Mill*)

But Mill goes on to say there is some disagreement: "… you seem to go farther in rejecting the doctrines of mathematicians on the subject than even I do." Rejection of the use of Bayes' theorem seems to Mill to be a mistake:

> If I understand you rightly, you attach little value to the rule for determining the probability by which of several causes a known event has been produced, which rule seems to me to rest on solid grounds, and to be quite reconcileable with the principle that all evaluation of probabilities must depend on appropriate statistics.

But what are we to make of this passage from Mill? He does not object to Venn's theory of probability. But according to Venn's frequency theory of the meaning of probability, single-case probabilities have no meaning at all — while Mill, in the second edition and thereafter in all subsequent editions, endorsed a judgmental, degree-of-belief theory of the meaning of probability. Mill's language that we have already quoted — "We must remember that the probability of an event is not a quality of the event itself, but a mere name for the degree of ground which we, or some one else, have for expecting it …" — remains the same in the seventh edition, which contains a complementary footnote to Venn:

> For a fuller treatment of the many interesting questions raised by the theory of probabilities, I may now refer to a recent work by Mr. Venn, Fellow of Caius College, Cambridge, *The Logic of Chance* [London: Macmillan, 1866]; one of the most thoughtful and philosophical treatises on any subject connected with Logic and Evidence, which have been produced, in this or any other country, for many years. Some criticisms contained in it have been very useful to me in revising the corresponding chapters of the present work. In several of Mr. Venn's opinions, however, I do not agree. What these are will be obvious to any reader of Mr. Venn's work who is also a reader of this. (Mill *Collected Works* v. VII, p. 547)

And the clause of the letter, "all evaluation of probabilities *must* depend on appropriate statistics", sounds like backsliding from the concessions to Laplace caused by Herschel and others. It seems that Mill slips between frequency being the meaning of probability, to frequency being important evidence bearing on probability, to frequency being *essential* evidence for the evaluation of probabilities. Was Mill really converted to a Laplacian understanding of probability? On the surface, perhaps it appears that he was. But at best, the conversion was incomplete.

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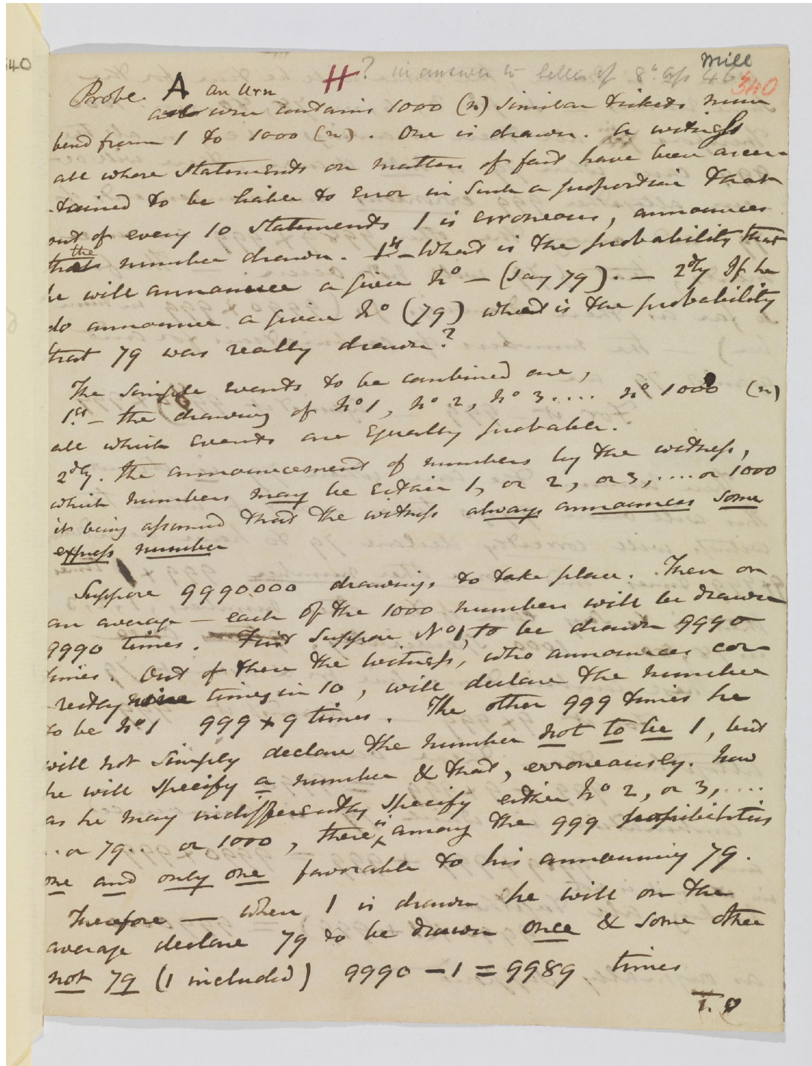
Note: Higher-resolution versions of all of the following scans are available from the author.

Appendix 1: Laplace worked out. Scan of (HS/12/340):



340

H? in answer to letter of 8 Ap 46 Mill 340

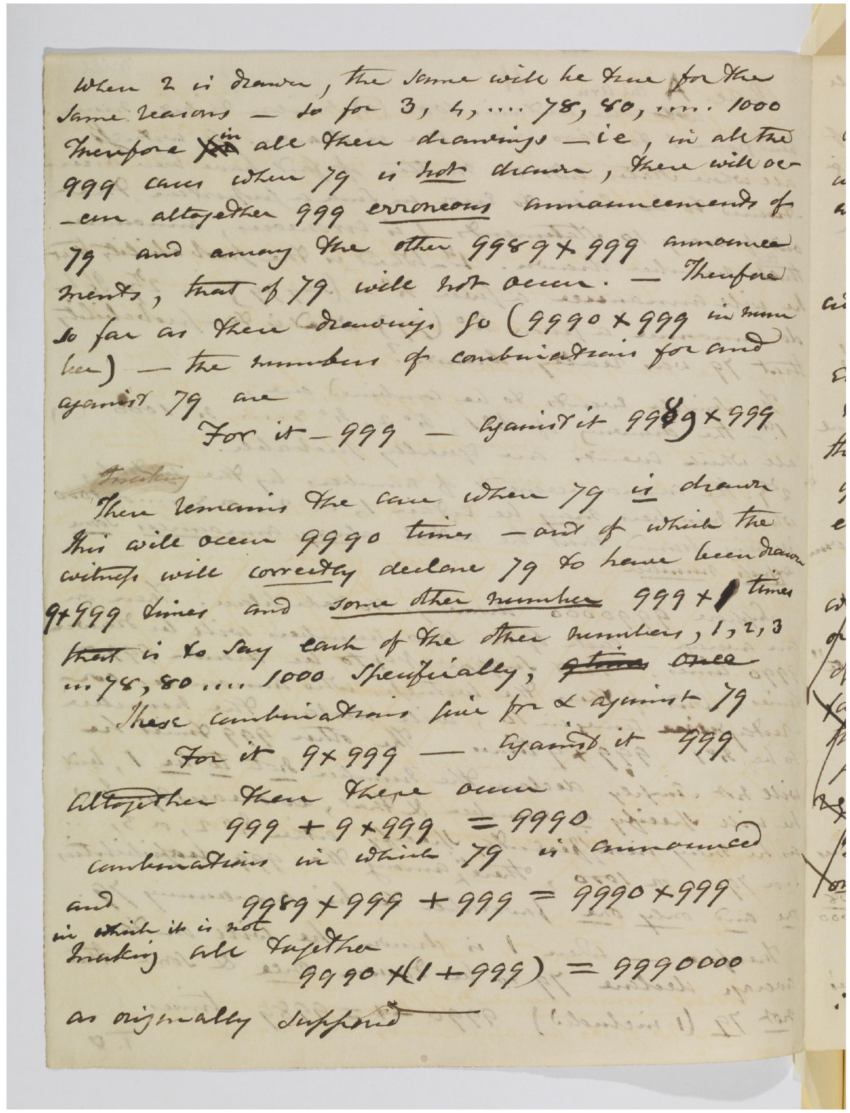
Probl. A An Urn contains 1000 (n) similar tickets numbered from 1 to 1000 (n). One is drawn. A witness all whose statements on matters of fact have been ascertained to be liable to Error in such a proportion that out of every 10 statements 1 is erroneous, announces the number drawn. 1st. What is the probability that he will announce a given No — (say 79). — 2dly If he do announce a given No (79) what is the probability that 79 was really drawn?

The simple events to be combined are,
1st — the drawing of No 1, No 2, No 3 .... No 1000 (n)
all which events are Equally probable.
2dly. The announcement of numbers by the witness, which numbers may be either 1, or 2, or 3, ... or 1000 it being assumed that the witness always announces some express number

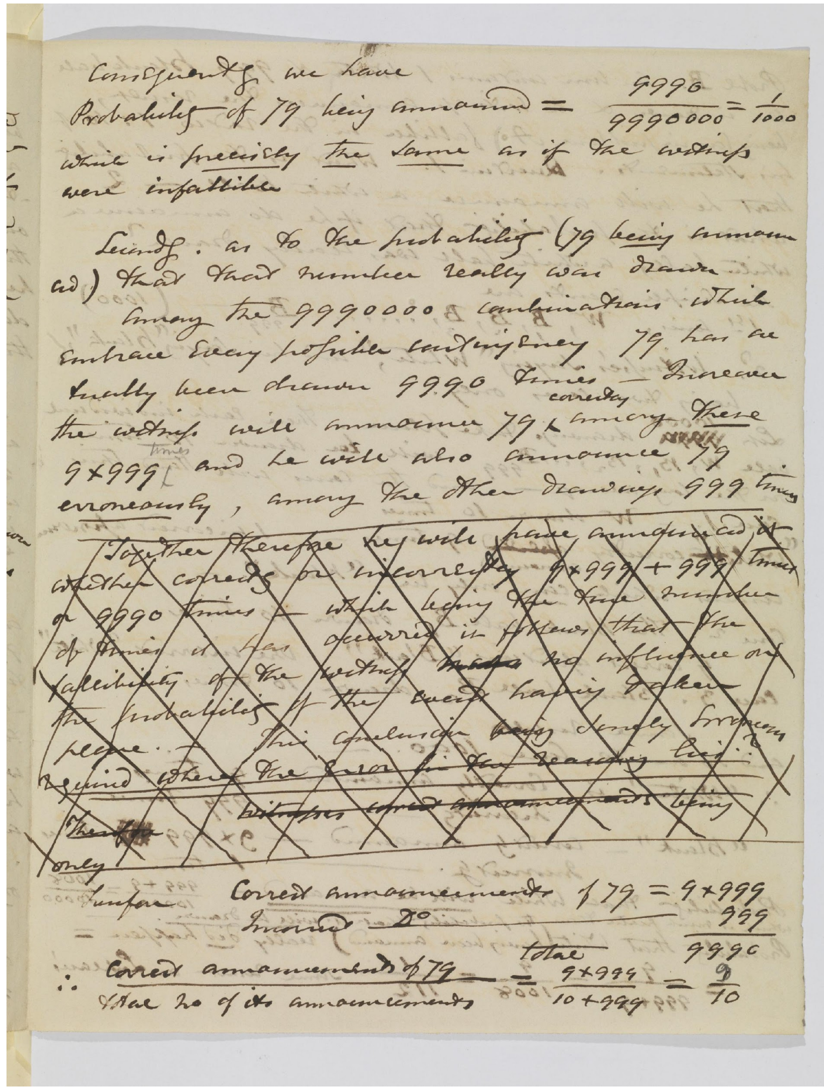
Suppose 9990,000 drawings to take place. Then on an average — each of the 1000 numbers will be drawn 9990 times. ~~First~~ Suppose No 1, to be drawn 9990 times. Out of these the witness, who announces correctly ~~nine~~ times in 10, will declare the number to be No 1 999 × 9 times. The other 999 times he will not simply declare the number not to be 1, but he will specify a number & that, erroneously. Now as he may indifferently specify either No 2, or 3, ... or 79 ... or 1000, there is among the 999 possibilities one and only one favorable to his announcing 79.

Therefore — when 1 is drawn he will on the average declare 79 to be drawn once & some other not 79 (1 included) 9990 − 1 = 9989 times

T.O


When 2 is drawn, the same will be true for the same reasons — so for 3, 4, ... 78, 80, ... 1000
Therefore in all these drawings — i.e., in all the 999 cases when 79 is not drawn, there will occur altogether 999 erroneous announcements of 79 and among the other 9989 × 999 announcements, that of 79 will not occur. — Therefore so far as these drawings go (9990 × 999 in number) — the numbers of combinations for and against 79 are
For it — 999 — Against it 9989 × 999

~~Breaking~~
There remains the case when 79 is drawn. This will occur 9990 times — and of which the witness will correctly declare 79 to have been drawn 9 × 999 times and some other number 999 × 1 times that is to say each of the other numbers, 1, 2, 3 ... 78, 80 .... 1000 specifically, ~~times~~ once
These combinations line for & against 79
For it 9 × 999 — Against it 999
Altogether then there occur
999 + 9 × 999 = 9990
combinations in which 79 is announced
and in which it is not
9989 × 999 + 999 = 9990 × 999
Breaking all together
9990 × (1 + 999) = 9990000
as originally supposed



Consequently we have

Probability of 79 being announced = $\frac{9990}{9990000} = \frac{1}{1000}$

which is precisely the same as if the witness were infallible

Secondly. as to the probability (79 being announced) that that number really was drawn.

Among the 9990000 combinations which embrace every possible contingency 79 has actually been drawn 9990 times — Moreover the witness will announce 79 correctly among these 9×999 times and he will also announce 79 erroneously, among the other drawings 999 times

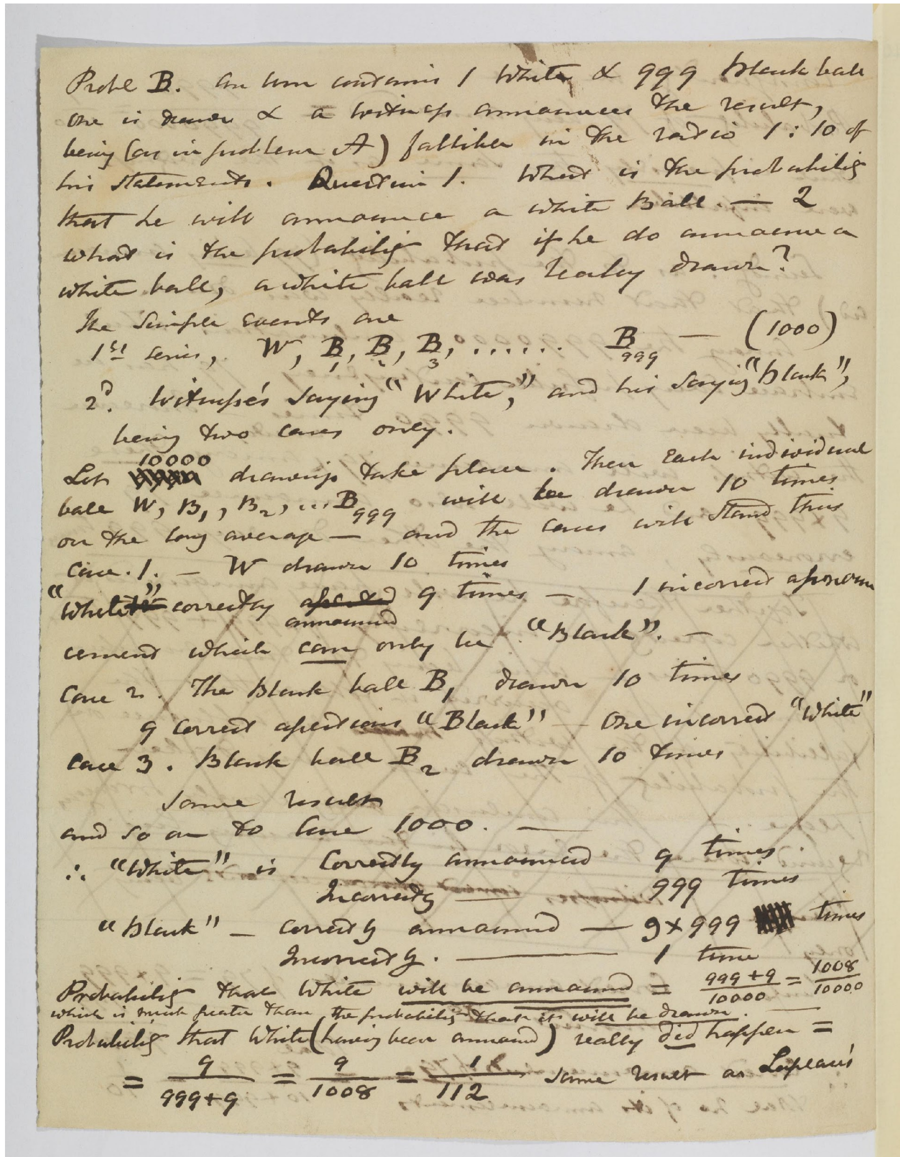
~~Together therefore he will have announced it whether correctly or incorrectly 9×999 + 999 times or 9990 times — which being the true number of times it has occurred it follows that the fallibility of the witness has no influence on the probability of the event having taken place. — This conclusion being [illegible] [illegible] [illegible] required where the error in the reasoning lies? witnesses correct announcements being Therefore only~~

Therefore Correct announcements of 79 = 9×999

Incorrect Do. = 999

total 9990

∴ $\frac{\text{Correct announcements of 79}}{\text{total no of its announcements}} = \frac{9\times999}{10\times999} = \frac{9}{10}$



Prob B. An urn contains 1 White & 999 Black ball
One is drawn & a witness announces the result,
being (as in problem A) fallible in the ratio 1 : 10 of
his statements. Question 1. What is the probability
that he will announce a white Ball. — 2
what is the probability that if he do announce a
white ball, a white ball was really drawn?
The simple events are
1st series, $W, B_1, B_2, B_3, \ldots\ldots B_{999}$ — (1000)
2°. witness's saying "White," and his saying "Black",
being two cases only.
Let ~~[illegible]~~ 10000 drawings take place. Then each individual
ball $W, B_1, B_2, \ldots B_{999}$ will be drawn 10 times
on the long average — and the cases will stand thus
Case. 1. — W drawn 10 times
"White" correctly ~~asserted~~ announced 9 times — 1 incorrect announcement which can only be "Black". —
Case 2. The Black ball B_1 drawn 10 times
9 correct assertions "Black" — One incorrect "White"
Case 3. Black ball B_2 drawn 10 times
Same result
and so on to Case 1000. —
∴ "White" is Correctly announced 9 times
Incorrectly — 999 times
"Black" — Correctly announced — 9×999 ~~[illegible]~~ times
Incorrectly. — 1 time
Probability that White will be announced $= \frac{999+9}{10000} = \frac{1008}{10000}$
which is much greater than the probability that it will be drawn. —
Probability that White (having been announced) really did happen =
$= \frac{9}{999+9} = \frac{9}{1008} = \frac{1}{112}$ Same result as Laplace's

Appendix 2: Herschel to Mill

Herschel to Mill July 10, 1845 (HS/25/6/26)

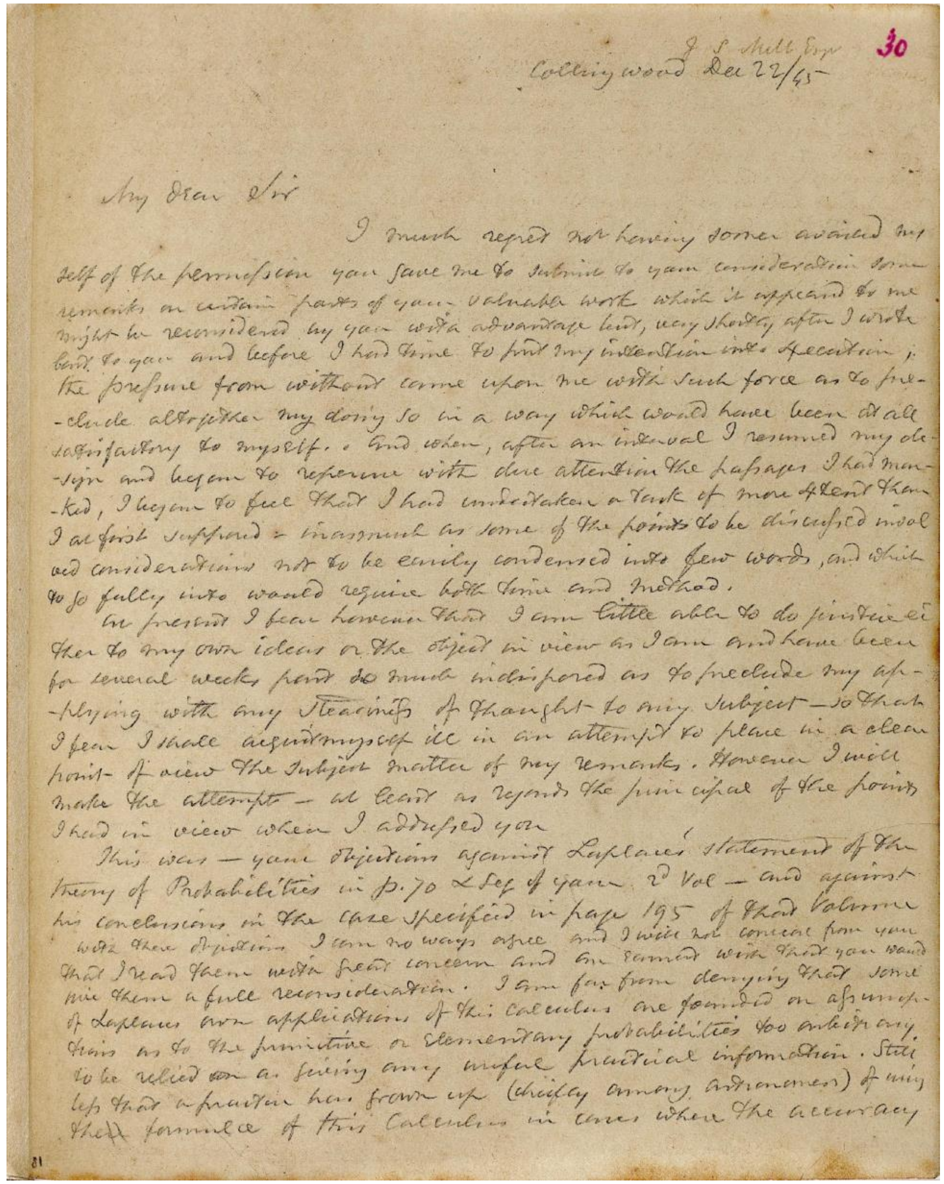
From the transcription in Schweber (1991) Appendix A, 175–176

(Letter begins with a discussion and criticism of Comte's "nebular hypothesis" in reply to an earlier letter from Mill.)

Returning to your own work — I hope you will excuse me if I remark (and the remark is in no way incompatible with the general high opinion I have formed & expressed of it in a Philosophical point of view) that I regard as the least felicitous portions of it, those in which points of physical science and mathematics are touched upon. I should have no objections, if you desire it, to specify some particular instances which have occurred to me inter legendum to which this remark applied, provided always, that I were distinctly understood as only pointing them out for your own reconsideration and not holding myself obliged to defend, or even to explain my objections against them should I be so unfortunate as to state them obscurely — a thing for which I have not time at my disposal. It was at one time my intention to have reviewed your book in the same sort of spirit that I did Whewell's (i.e. pointing out what I regarded as its defects with the same freedom as its merits.) but want of time prevented me. Now I cannot but fancy that it must be useful to an author of a Philosophical work to know what parts a possible Reviewer would have raised objections to.

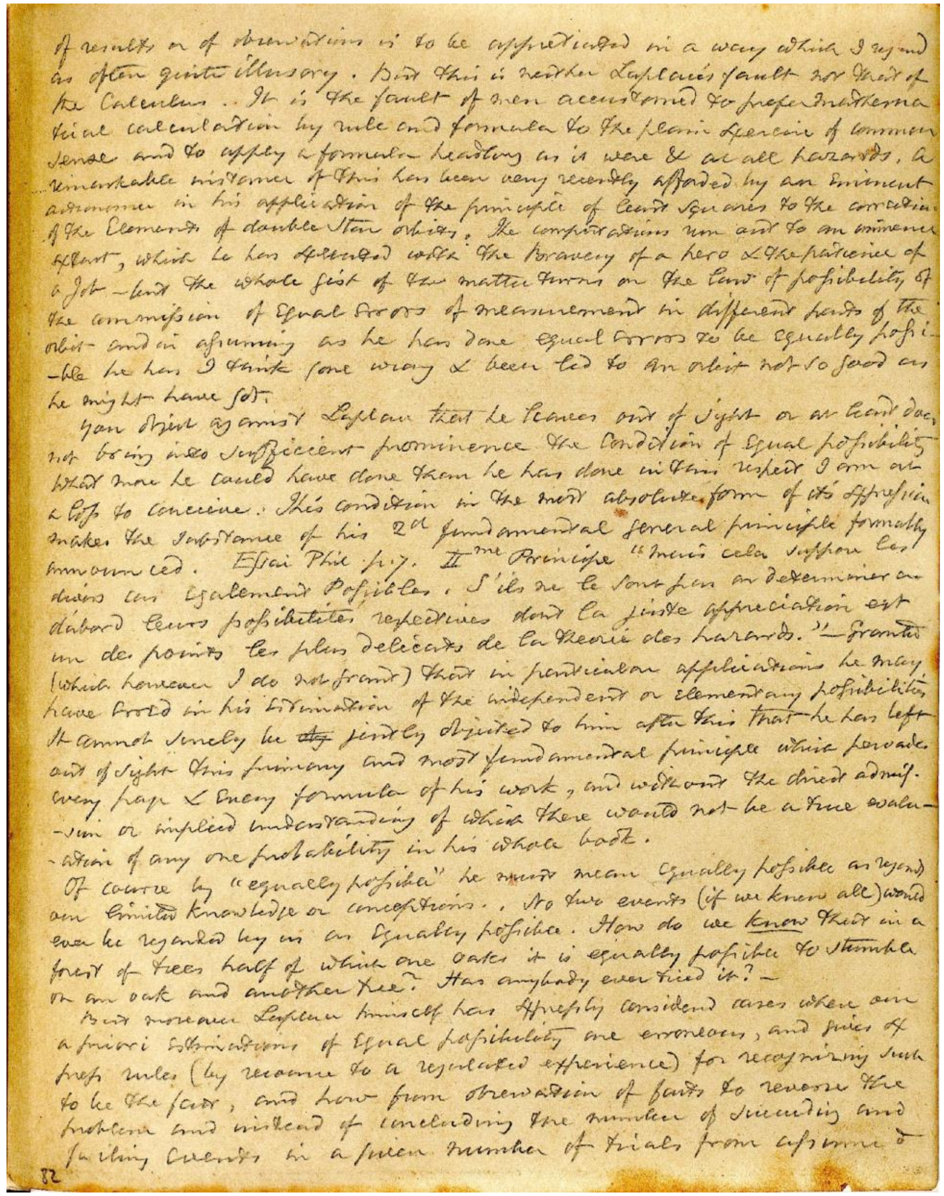
I remain
Dear Sir
Yours very truly
J. F. W. Herschel

Herschel to Mill Dec. 22 (27), 1845 (HS/25/6/30) Folio 81 r. – 84 v.


J S Mill Esqr 30
Collingwood Dec 22/45

My dear Sir

I much regret not having sooner availed my
self of the permission you gave me to submit to your consideration some
remarks on certain parts of your valuable work which it appeared to me
might be reconsidered by you with advantage but, very shortly after I wrote
back to you and before I had time to put my intention into execution,
the pressure from without came upon me with such force as to pre-
-clude altogether my doing so in a way which would have been at all
satisfactory to myself. And when, after an interval I resumed my de-
-sign and began to reperuse with due attention the passages I had mar-
-ked, I began to feel that I had undertaken a task of more extent than
I at first supposed – inasmuch as some of the points to be discussed invol
ved considerations not to be easily condensed into few words, and which
to go fully into would require both time and method.
At present I fear however that I am little able to do justice ei
ther to my own ideas or the object in view as I am and have been
for several weeks past so much indisposed as to preclude my ap-
-plying with any steadiness of thought to any subject – so that
I fear I shall acquit myself ill in an attempt to place in a clear
point of view the subject matter of my remarks. However I will
make the attempt – at least as regards the principal of the points
I had in view when I addressed you
This was – your objections against Laplace's statement of the
theory of Probabilities in p. 70 & seq of your 2d Vol – and against
his conclusions in the case specified in page 195 of that Volume
with these objections I can no ways agree and I will not conceal from you
that I read them with great concern and an earnest wish that you would
give them a full reconsideration. I am far from denying that some
of Laplaces own applications of this calculus are founded on assump-
tions as to the primitive or elementary probabilities too arbitrary
to be relied on as giving any useful practical information. Still
less that a practice has grown up (chiefly among astronomers) of using
the formulae of this Calculus in cases where the accuracy
81



of results or of observations is to be appreciated in a way which I regard as often quite illusory. But this is neither Laplace's fault nor that of the Calculus. It is the fault of men accustomed to prefer mathematical calculation by rule and formula to the plain exercise of common sense and to apply a formula headlong as it were & at all hazards. A remarkable instance of this has been very recently afforded by an eminent astronomer in his application of the principle of least squares to the correction of the Elements of double Star orbits. The computations run out to an immense extent, which he has executed with the bravery of a hero & the patience of a Job – but the whole gist of the matter turns on the law of possibility of the commission of equal errors of measurement in different parts of the orbit and in assuming as he has done equal errors to be equally possible he has I think gone wrong & been led to an orbit not so good as he might have got.

You object against Laplace that he leaves out of sight or at least does not bring into sufficient prominence the condition of equal possibility. What more he could have done than he has done in this respect I am at a loss to conceive. This condition in the most absolute form of its expression makes the substance of his 2d fundamental general principle formally announced. Essai Phil p. 7. II^me Principe "Mais cela suppose les divers cas également possibles. S'ils ne le sont pas on determinera d'abord leurs possibilités respectives dont la juste appreciation est un des points les plus delicats de la theorie des hazards." – Granted (which however I do not grant) that in particular applications he may have erred in his estimation of the independent or elementary possibilities it cannot surely be ~~ary~~ justly objected to him after this that he has left out of sight this primary and most fundamental principle which pervades every page & every formula of his work, and without the direct admission or implied understanding of which there would not be a true evaluation of any one probability in his whole book.

Of course by "equally possible" he must mean equally possible as regards our limited knowledge or conceptions. No two events (if we knew all) would ever be regarded by us as equally possible. How do we know that in a forest of trees half of which are oaks it is equally possible to stumble on an oak and another tree? Has anybody ever tried it? –

But moreover Laplace himself has expressly considered cases where our a priori estimations of equal possibility are erroneous, and gives us express rules (by recourse to a regulated experience) for recognizing such to be the case, and how from observation of facts to reverse the problem and instead of concluding the number of succeeding and failing events in a given number of trials from assumed

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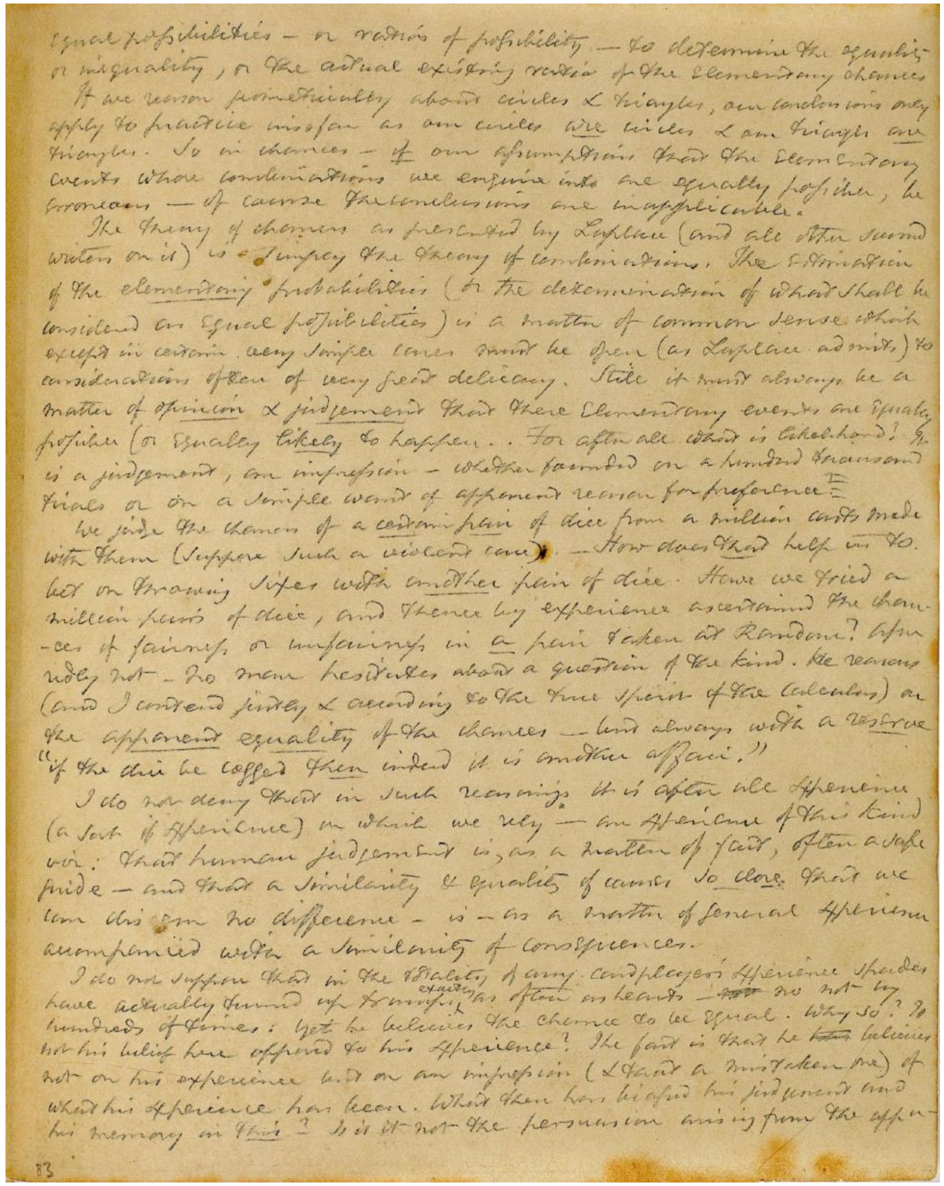

equal possibilities — or ratios of possibility — to determine the equality or inequality, or the actual existing ratio of the elementary chances

If we reason geometrically about circles & triangles, our conclusions only apply to practice insofar as our circles are circles & our triangles are triangles. So in chances — if our assumptions that the elementary events whose combinations we enquire into are equally possible, be erroneous — of course the conclusions are inapplicable.

The theory of chances as presented by Laplace (and all other sound writers on it) is simply the theory of combinations. The estimation of the elementary probabilities (or the determination of what shall be considered as equal possibilities) is a matter of common sense which except in certain very simple cases must be open (as Laplace admits) to considerations often of very great delicacy. Still it must always be a matter of opinion & judgement that these elementary events are equally possible (or equally likely to happen. For after all what is likelihood? It is a judgement, an impression — whether founded on a hundred thousand trials or on a simple want of apparent reason for preference.

We judge the chances of a certain pair of dice from a million casts made with them (suppose such a violent case). — How does that help us to bet on throwing sixes with another pair of dice. Have we tried a million pairs of dice, and thence by experience ascertained the chances of fairness or unfairness in a pair taken at random? Assuredly not — No man hesitates about a question of the kind. He reasons (and I contend justly & according to the true spirit of the calculus) on the apparent equality of the chances — but always with a reserve "if the dice be loaded then indeed it is another affair."

I do not deny that in such reasonings it is after all experience (a sort of experience) on which we rely — an experience of this kind viz: that human judgement is, as a matter of fact, often a safe guide — and that a similarity & equality of causes so close that we can discern no difference — is — as a matter of general experience accompanied with a similarity of consequences.

I do not suppose that in the totality of any cardplayer's experience spades have actually turned up trumps, exactly as often as hearts — no not by hundreds of times: yet he believes the chance to be equal. Why so? Is not his belief here opposed to his experience? The fact is that he believes not on his experience but on an impression (& that a mistaken one) of what his experience has been. What then has biased his judgement and his memory in this? Is it not the persuasion arising from the appa-

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mere absence of any assignable reason for preference. Now I think it is the sum total of all these coarse and erroneous impressions through out life, which make up the sort of experience on which we frame our conviction that where no difference of circumstances is apparent, the chances are equal, and that indifference of perception & intentional bias, results in indifference of events.

A total absence of all knowledge of the connexion of events — or so to speak, of the mechanism of the events is incompatible with that state of mind which leads us to assert an equal probability. — We must see enough of the case to get or make for ourselves an impression (perhaps an erroneous one) that there does exist a similarity of circumstance. In short an opinion or a judgement to be worth the name must be founded on something not merely negative.

I have not left myself space to enter into the other question agitated by you in p 195 about the credibility of the witness in Laplace's case. I must take another opportunity indeed to do so, as it will lead into some rather lengthy discussion, and my attention is at present distracted from the subject by company assembled here for Christmas. Ere long however I purpose to return to the charge, as I feel quite satisfied myself & hope to make it clear clear to you that Laplace is perfectly right. For the present allow me to remain

Dear Sir

Yours very truly

J. F. W. Herschel

Collingwood Dec 27 1845

86

My Dear Sir,

I will try to put, as briefly as I can, my case in favor of Laplace and against your argument in p. 195 vol. 2, et seq. I am sorry I have delayed to do so long. But first let me observe that you exaggerate Laplace's statement by making him say the witnesses' affirmation is incredible. (p. 195) I did not find such an expression in his own exposé of his view of the matter (*Essai Phil.* p 12, 13 14) It is a question of more or less probability and of the numerical degrees in which the probability is more or less. When first I read your passage I had not Laplace before me, but I noted in pencil the exaggerative effect of this word, which I felt afraid that Laplace would not have used, as I find on examination that he did not. In line 24 you carry this exaggeration still farther, by using the expression "absolutely incredible". Such phrases are out of place where numbers in all the rigor of geometrical strictness are under description.

In your mode of putting the subject before your readers all is estimative and indefinite. Laplace's is all strictly limited and numerically precise. You have omitted to refer to one of the most important features of Laplace's statement — that it is an ascertained fact that the witness (from whatever motives — moral, interested or capricious — it matters not) actually lies once on an average of every 10 of his statements. To this positive numerical estimate of his veracity you make little allusion. Yet the numerical result to which Laplace arrives is strictly a mathematical result of this datum 1/10 as that $(10000000000)^{1/10}$ is identical with the number 10. You put the case in p. 195 as of "an eye witness" — "a witness" (lines 18, 24) — of a person who might be influenced by love of the marvelous but who also might be rather influenced by an apparent marvel to enquire more minutely.

Laplace supposes no such person. He assumes for his witness a known to him — one who has been ascertained (no regard being had to motives) to tell a lie, knowing it to be such once in every 10 statements. The case where in addition to this, his known and notorious mendacity, he has a special motive of interest to lie in favor of 79 he considers separately, and shows that it makes his statement less probable.

You admit (p 197) that if the falsity of the assertion were a true cause for its being made and that there were no possible mode of accounting for a false assertion but by supposing that it is made precisely because of its falsity you do not see how Laplace's argument could be resisted. Now this is actually Laplace's direct assumption. He expressly excludes in his numerical evaluation of the probabilities pp 12, 13 all causes other than pure mendacity, such as possibility of a mistake, or self illusion of the witness in order to simplify his case. Pray observe the force of his very precise and carefully chosen words il trompe for a falsehood and il se trompe for a mistake. — In page 14 he goes with equal distinctness into a more complicated case in which as the alternative to intentional truth or falsehood, a third possibility, that of a mistake, self illusion or misinformation is introduced. And this gives him as possible combinations — viz 1^{st} Intentional truth but mistaken fact, 2^{nd} Intentional falsehood and mistaken fact (converting an intended lie into a truth), 3^{rd} Veracious intent and correctly observed fact, 4^{thly} Veracious intent but mistaken fact. This case he does not exemplify by precise numerical assumptions, but it is perfectly clear, if you put general algebraic symbols for the probabilities of falsehoods and mistakes and apply the principle no. 6 that the result must be as he says it is. It certainly does appear to me that (assuredly without intending it) you have given quite an erroneous impression of Laplace's meaning and reasoning. —

What says common sense in the latter? — Does not the known want of veracity in a witness increase our disbelief or diminish our belief in any statement he may make? And if he make a statement in itself highly improbable, does not his mendacity justify us in rejecting it altogether — not as incredible (i.e. as a thing that cannot be believed) but as unworthy of belief from his lips?

On reconsideration this last argument does not go to the point under consideration. But there is another way of putting the matter without meddling with character or motive. Let us suppose the witness to be of perfectly veracious intention but fallible. Now if 79 did not come up it must have been some other number. Then in choosing 79 out of 1, 2, ... 999, he was liable to mistake the probability that this mistake (supposing one committed) would lead him to 79 is 1/999.

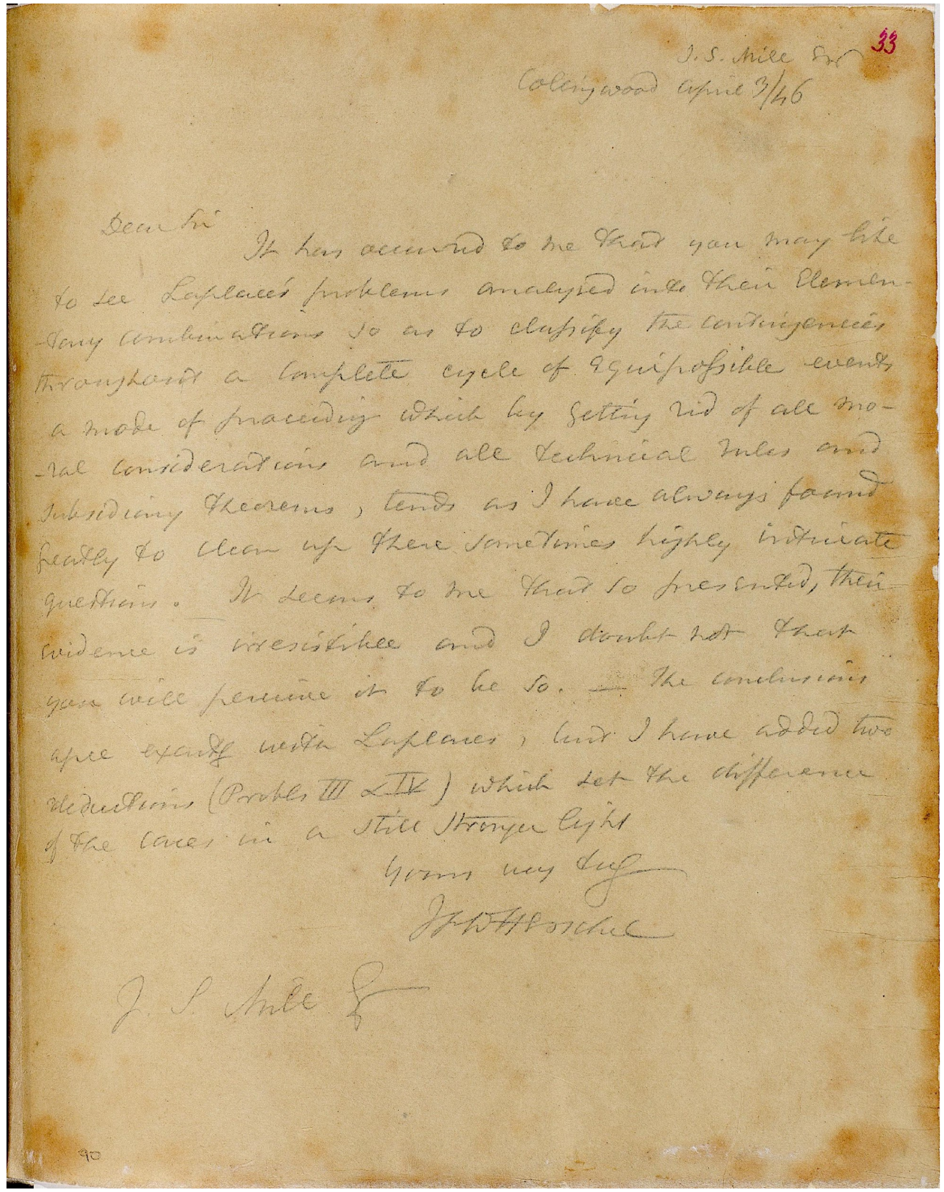
Whereas in the other case if a white ball did not come up it must have been a black ball which did and in saying white supposing a mistake he could have not said anything else. He was mistaken and the truth was black so his mistake must have been that he took a black ball for a white one. So that here his probability of saying white =1 or certainty.

Now this already established a difference between the two cases — which is all I contend for — since it is on the first of these differences that all Laplace's numerical reasoning turns. I have been forced to be somewhat lengthy, but I hope I have succeeded in at least conveying a clean impression of my own view of the case and remain Dear Sir

Yours very truly,

J. F. W. Herschel

Herschel to Mill April 3 1846 (HS/25/6/33) Folio 90 r.



J. S. Mill Esq 33

Collingwood April 3/46

Dear Sir

It has occurred to me that you may like to see Laplace's problems analysed into their elementary combinations so as to classify the contingencies throughout a complete cycle of equipossible events a mode of proceeding which by getting rid of all moral considerations and all technical rules and subsidiary theorems, tends as I have always found greatly to clear up these sometimes highly intricate questions. It seems to me that so presented, their evidence is irresistible and I doubt not that you will perceive it to be so. — The conclusions agree exactly with Laplace's, but I have added two deductions (Probls III & IV) which set the difference of the cases in a still stronger light

Yours very truly

JFW Herschel

J. S. Mill Esq

90

Dear Sir

It has occurred to me that you may like to see Laplace's problems analyzed into their elementary combinations so as to clarify the contingencies throughout a complete cycle of equiprobable events mode of proceeding which by getting rid of all moral considerations and all technical rules and subsidiary theories, tends as I have always found greatly to clean up these highly intricate questions. It seems to me that so presented, then evidence is irresistible and I doubt not that you will perceive it to be so. — The conclusions agree exactly with Laplace, but I have added two deductions (Probls III & IV) which set the difference of the cases in a still stronger light.

Yours very truly,
J. F. W. Herschel