Journ@l Electronique d'Histoire des Probabilités et de la Statistique

Electronic Journ@l for History of Probability and Statistics

Vol 8; Décembre/December 2012

www.jehps.net

De Morgan and Laplace: A Tale of Two Cities

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Il était fort préoccupé, ce soir-là, de notre conversation très longue sur le système des probabilités de Laplace. Je me souviens qu'il tenait sous le bras ce livre, que nous avions en grande estime, et dont il était souvent tourmenté. –

Alfred de Vigny, *Servitude et grandeur militaires.*

1 Prequel

After the death of Newton (in 1727), the retirement of James Stirling (in 1736), and the eventual departure of de Moivre (in 1754), mathematics in England went into decline.[1] France had its Lagrange, Legendre, and Laplace; Switzerland its Euler and Daniel Bernoulli; Germany its Gauss; but no figure of comparable stature was to be found in England up to the outbreak of the Napoleonic wars. British mathematics was indeed "isolated, by the dead hand of Newton and the superior quality of the followers of Leibniz, from the development of analysis in continental Europe" (Plackett, 1989, p. 163).

Reform came in the wake of the Industrial Revolution, beginning in 1812 with the birth of the Cambridge "Analytical Society", a group of students and Fellows at the University devoted to the advocacy of "the principles of pure D-ism as opposed to the Dot-age of the University" (as put by Charles Babbage, 1961, p. 25, the reference being to the conflicting notations of Leibniz and Newton); its founding members including Charles Babbage, John Herschel and George Peacock. It later incorporated in 1832 as the Cambridge

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Philosophical Society. Its members, in particular Peacock, played an important role in modernizing mathematics at Cambridge. (This was just part of a more general movement, including the founding of the Astronomical Society of London in 1820, the Society for the Diffusion of Useful Knowledge in 1826, and the British Association for the Advancement of Science in 1831.)

Cambridge up to that time had not been receptive to the ideas of the continental mathematicians. Some idea of this is afforded by a revealing experience of Babbage's:

> I went to my public tutor Hudson, to ask the explanation of one of my mathematical difficulties [concerning something in Lacroix's textbook on calculus]. He listened to my question, said it would not be asked in the Senate House [that is, would not be the subject of examination], and was of no sort of consequence, and advised me to get up the earlier subjects of the university studies.
>
> After some little while I went to ask the explanation of another difficulty from one of the lecturers. He treated the question just in the same way. I made a third effort to be enlightened about what was really a doubtful question, and felt satisfied that the person I addressed knew nothing of the matter, although he took some pains to disguise his ignorance.
>
> I thus acquired a distaste for the routine of the studies of the place, and devoured the papers of Euler and other mathematicians, scattered through innumerable volumes of the academies of Petersburgh, Berlin, and Paris, which the libraries I had recourse to contained.
>
> Under these circumstances it was not surprising that I should perceive and be penetrated with the superior power of the notation of Leibnitz. [Babbage, 1961, p. 23]

The voices of reform in the Analytical Society had considerable authority despite the youth and relative lack of standing of its members: Herschel, for example, was the "senior wrangler" in 1812 (that is, had the highest score on the Mathematical Tripos, the examination to which Hudson was referring), followed by Peacock. The effects of the resulting curricular reforms were eventually felt: examination of the list of senior wranglers in the initial decades of the century reveals few names of distinction in mathematics,[2] but in later decades this changed, as the ranks of the senior wranglers came to include many distinguished mathematicians and physicists.[3]

Thus when Augustus de Morgan studied at Cambridge in the 1820s, it was at a time of intellectual ferment, and a missionary fervor for spreading the new math and science to the masses (the function of the Society for the Diffusion of Useful Knowledge).

2 Augustus De Morgan

Augustus De Morgan had a most unconventional university career.[4] True, it began conventionally enough, as a student at Trinity College, Cambridge, where De Morgan received his BA in 1827. Regarded as one of the best students in mathematics then at Cambridge, his performance on the Tripos (fourth wrangler) was however a disappointment: like Babbage he was too interested in continental mathematics and too little on what would be asked in the Senate House. This by itself would not have prevented him from enjoying an academic career at Cambridge; but religious scruples (specifically, his refusal to take a then required theological examination) prevented him from obtaining either an MA or Cambridge Fellowship. Instead, in 1828 De Morgan was appointed (at the age of 22!) to the first Chair of Mathematics at the newly founded (and, more importantly, religiously neutral) University College London.[5]

After only thee years, however, De Morgan resigned in 1831 on a matter of academic principle (the failure of the University to respect academic tenure), only to return in 1836 as an emergency replacement after the untimely death of his successor. Here he remained until 1867, when he once again resigned (once again on a matter of academic principle), and died just a few years later, in 1871. Thus for four decades De Morgan was the primary instructor of mathematics at one of the two main academic institutions in London (the other being King's College London).

Throughout his career De Morgan was an energetic, conscientious, and captivating teacher, as well as a prolific author of both scholarly and popular works on mathematics, logic, science, and history. Although he made important original contributions to both mathematics and logic, the bulk of his writings show him in his role as an outstanding expositor of mathematics (including many textbooks).

Consideration of these elements of his biography suggests why De Morgan was a natural popularizer of Laplace in Britain. First, given his extensive (and then unusual) familiarity with continental mathematics, he was one of the few British mathematicians actually familiar with Laplace (and other

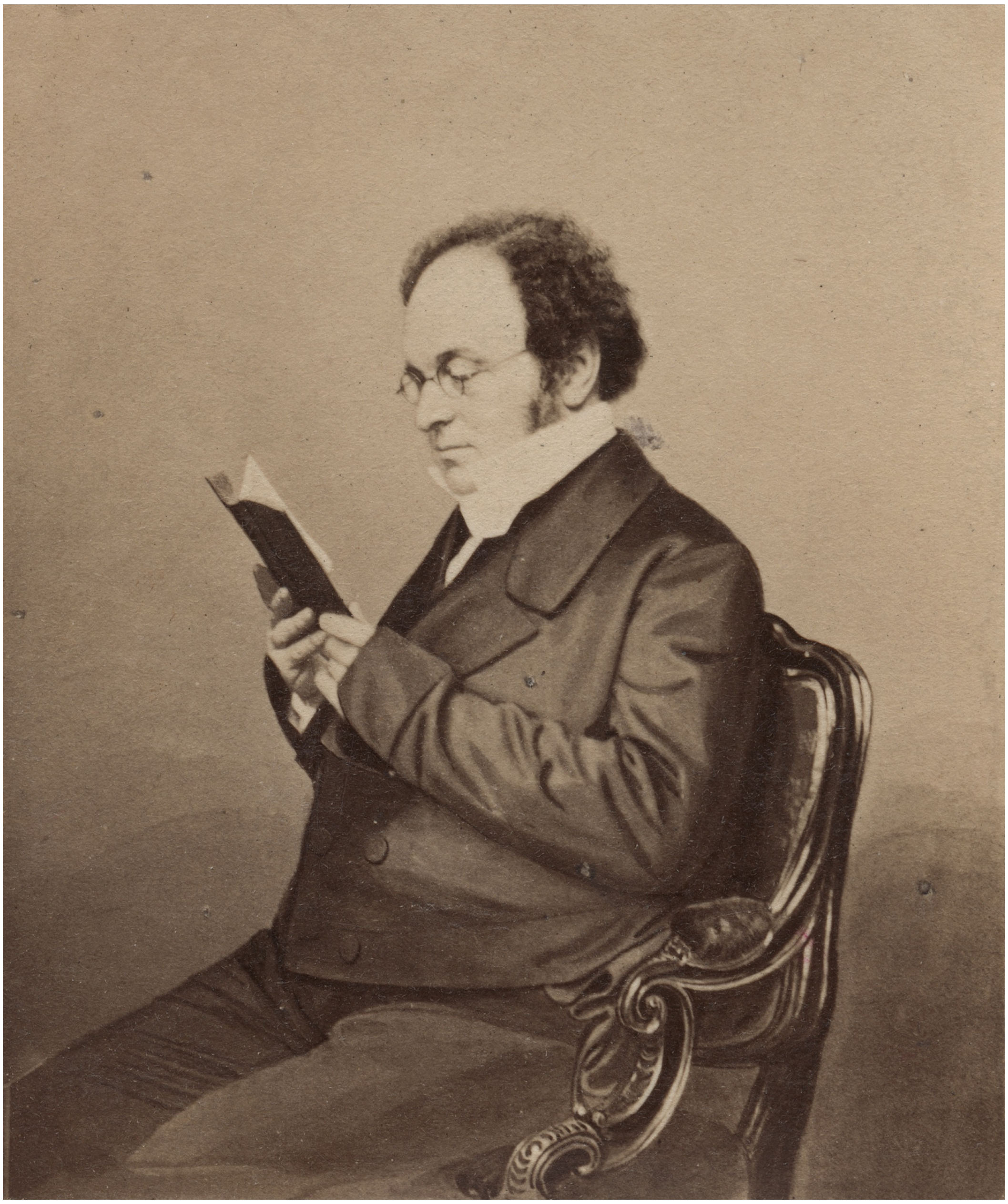

Figure 1: Augustus De Morgan
(Courtesy National Portrait Gallery, London; Artist: Mayall)

continental writers on probability). Second, his zest for exposition made him willing to undertake the onerous task of writing a long (98 page) encyclopedia article about the technical aspects of mathematical probability. Third, his dedication to teaching led him to follow this up with popular books and essays aimed at a wider audience. Finally, his broad interests ensured that these writings dealt not only with the mathematics of probability, but also its applications to other areas such as logic and the philosophy of science. It is to this body of work that we now turn.

3 Books and papers on probability

The preponderance of De Morgan's writings on mathematical probability occurred during the relatively short period from 1836 to 1838.

3.1 Theory of Probabilities

In 1836–7 De Morgan wrote a long article on "Theory of Probabilities" for the *Encyclopedia Metropolitana* (for the dates, see De Morgan, 1882, p. 92). A work of synthesis, it was the most mathematically detailed work on mathematical probability and its applications written in English since Abraham De Moivre's classic *Doctrine of Chances* (1st ed., 1718; 3rd ed., 1756).[6] Although it draws on many sources (in particular, Lubbock and Drinkwater, 1830), it demonstrates an extensive familiarity with Laplace's *Théorie analytique*, and was the first presentation of many of Laplace's results in a form digestible by an English-reading audience. The agreement with the publishers reads in part:

> A Mathematical Treatise on the Theory of Probabilities; containing such development of the application of Mathematics to the said Theory as shall to him (the Author) seem fit, and in particular such a view of the higher parts of the subject as laid down by Laplace in his work entitled *Théorie des Probabilités*, as can be contained in a reasonable compass, regard being had to the extent and character of the Mathematical portions of the said work. [De Morgan, 1882, p. 92]

The writing of the article was a considerable undertaking on De Morgan's part, and marks the start of his serious interest in the subject. Initially published separately (De Morgan, 1837a), it appeared as part of Volume 2 of the *Encyclopedia Metropolitana* in 1838, later reprinted in 1845.

In his article De Morgan did not unthinkingly parrot Laplace's results. Indeed, on a purely technical level Laplace can seldom have had a more careful and critical reading of his *Théorie analytique*. For example, sometimes we see De Morgan simplifying Laplace's treatment, as in the derivation of the distribution of a sum of discrete uniform random variables (Article 44, p. 410); sometimes drawing on material in Laplace's earlier papers but not included in his book, as in the discussion of the multinomial rule of succession (Articles 48–49, pp. 413–4415); sometimes stressing the role of a subtle condition, as in the analysis of Waldegrave's problem, where it is assumed at least one other player has already won a game (Article 52, p. 418); sometimes changing notation relating to the use of infinitesmals (Article 106, p. 447); sometimes streamlining proofs by not repeating essentially the same derivation again (Article 125, pp. 452–3); sometimes noting that a result is asymptotic rather than exact (Article 148, p. 460); and sometimes noting an actual error, as in the solution of the Buffon needle problem, where the master inadvertently maximizes a fluctuation when the minimum is actually called for (Article 172, p. 468). Two examples are briefly noted here.

3.1.1 The convolution of a discrete sum

Chapter 2, Article 13, pp. 257–260 of the *Théorie analytique* considers drawing a ball with replacement from an urn containing balls numbered $0, 1, \ldots n$, and asks for the probability that the sum of the numbers after i drawings is s. In modern terminology one seeks a convolution formula giving the distribution of a sum of independent random variables each having a discrete uniform distribution. This problem was first asked and answered by Montmort and De Moivre (independently), and later discussed by Simpson and Lagrange; see Todhunter (1865, Articles 148–149, 364, and 987) and Hald (1998, pp. 34–36, 42, 55–56) for the history the problem. Laplace's interest also includes an extension to the case of continuous uniform variates, and using the resulting formula to assess the significance of the different inclinations of the planets to the ecliptic.

De Morgan discusses this problem in his Articles 44–47 (including the continuous extension and planetary application), but draws instead on the derivation in Lubbock and Drinkwater (1830, pp. 12–14) in preference to "the very unwieldy method of Laplace, which nevertheless is an excellent deduction from something like inspection of cases". The simpler technique is to use generating functions, reading off the coefficient of x^s in the expansion of

$$(1 + x + \cdots + x^n)^i = (1 - x^{n+1})^i (1 - x)^{-i};$$

one can also employ the binomial theorem to express this coefficient as an alternating sum. (De Moivre, 1738, pp. 35–36, and 1756, pp. 39–40, gives several examples illustrating use of the formula.)

Presumably instances such as this, where Laplace omits any mention of a problem's extensive past history, led De Morgan—as discussed below—to take him to task for not mentioning the (other) giants on whose shoulders he stood.

3.1.2 The fallacy of the transposed conditional

The *fallacy of the transposed conditional* refers to using (or confusing) the conditional probability $P(A|B)$ in place of $P(B|A)$. It has an ancient lineage and many distinguished victims, including – as noted by De Morgan – both Laplace and Poisson.

Here is an example that may help to make the distinction between the two conditional probabilities clear. Suppose you are playing draw poker, H_0 is the hypothesis that the cards are being dealt fairly (and therefore randomly), H_1 that some form of cheating is taking place, and E that your opponent has just been dealt a straight flush. Then $P(E \mid H_0)$ (the chances of getting a straight flush if the cards are dealt at random) is an objective quantity, susceptible of calculation, and is in fact quite small: 40/2,598,960, or about 1 chance in 65,000 (assuming aces can count either high or low). In contrast $P(H_0 \mid E)$ is a subjective quantity, whose value will depend on one's opponent: it will have one value if your opponent is the Archbishop of Canterbury, quite another if Doc Holliday or Maverick.

Just after his *Encyclopedia Metropolitana* article appeared, De Morgan published a short paper (De Morgan, 1837c) noting Laplace (and Poisson) had committed an error of this kind, interpreting a direct probability (the normal approximation to the binomial) as if it were an inverse one. (That is, if p is the probability of an event occurring in a series of Bernoulli trials, and A_n the number of times the event occurs in n trials, the charge is one of confusing (a) the conditional probability, *given* p, that $A_n - np$ lies between limits $-l$ and l, for (b) the conditional probability, *given* A_n, that p lies between the corresponding limits.[7]) To demonstrate the two probabilities are different, De Morgan gave a derivation of the second, inverse probability using the standard uniform prior for p ordinarily employed by Laplace. (Ironically, De Morgan himself almost immediately fell victim to the same fallacy in his 1838 *Essay*; see Rice (2003) and Rice and Seneta (2005), who discuss the examples and issues in great detail.)

3.2 The Dublin Review

After completing his article, De Morgan wrote a review of the *Théorie analytique des probabilités* for the *Dublin Review* (De Morgan, 1837b). This brought Laplace's work to the attention of an even wider public, and it is here that De Morgan provides us with his overall view of the work and its author.[8]

How widely read was Laplace's *Théorie analytique des probabilités* in England and the Continent at that time? De Morgan tells us: very little.

> Now, even meaning by the world the mathematical world, there is not a sufficient proportion of that little public which has read the work in question, to raise any such collective sound as a cry either on one side or the other. The subject of the work is, in its higher parts, comparatively isolated and detached, though admitted to be of great importance in the sciences of observation. The pure theorist has no immediate occasion for the results, as results, and therefore contents himself in many instances with a glance at the processes, sufficient for admiration, though hardly so for use. The practical observer and experimenter obtains a knowledge of results and nothing more, well knowing in most cases, that the analysis is above his reach. We could number upon the fingers of one hand, all the men we know *in Europe* who have *used* the results in their *published* writings in a manner which makes it clear that they could both *use* and *demonstrate.*

This is valuable testimony indeed. It suggests caution in interpreting mere citation of the *Théorie analytique des probabilités*, or application of its results. Then as now, citation can often be merely for purposes of adornment.[9]

This benign neglect did not reflect the value of Laplace's work. To the contrary: for Laplace, De Morgan had only the highest appreciation:

> Of all the masterpieces of analysis, this is perhaps the least known; it does not address its powers to the consideration of a vast and prominent subject, such as astronomy or optics, but confines itself to a branch of enquiry of which the first principles are so easily mastered (in appearance), that the student who attempts the higher parts feels almost deprived of his rights when he begins to encounter the steepness of the subsequent ascent. The *Théorie des Probabilités* is the Mont Blanc of mathematical analysis; but the mountain has this advantage over the book, that there are guides always ready near the former, whereas the student has been left to his own method of encountering the latter.

Despite this high opinion, De Morgan was a careful and critical reader of Laplace, and he did not hesitate to note a number of what he perceived to be defects of both style and substance in the *Théorie analytique*. These included:

3.2.1 Lack of elegance

As someone who valued elegance and craftsmanship in doing mathematics, De Morgan found Laplace most wanting in this category:

> The genius of Laplace was a perfect sledge hammer in bursting purely mathematical obstacles; but, like that useful instrument, it gave neither finish nor beauty to the results. In truth, in truism if the reader please, Laplace was neither Lagrange nor Euler, as every student is made to feel. The second is power and symmetry, the third power and simplicity; the first is power without either symmetry or simplicity. But, nevertheless, Laplace never attempted the investigation of a subject without leaving upon it the marks of difficulties conquered: sometimes clumsily, sometimes indirectly, always without minuteness of design or arrangement of detail; but still his end is obtained, and the difficulty is conquered.

3.2.2 Cut-and-paste writing

De Morgan was a lively and effective lecturer in the classroom, and his own writing is informed by the attempt to make material accessible, both by paying attention to conceptual difficulties and the use of practical examples. His judgement of Laplace's exposition here was severe:

> The arrangement will seem simple and natural, but there is a secret which does not appear immediately, and refers to a point which distinguishes this and several other works from most of the same magnitude. The work is not an independent treatment of the subject, but a collection of memoirs taken *verbatim* from those which the author had previously inserted in the Transactions of the Academy of Sciences. Thus in the volume for 1782, appears a paper on the valuation of functions of very high numbers, with an historical and explanatory introduction. Now this introduction being omitted, the rest of the memoir is, substantially, and for the most part word for word, inserted in the work we are now

> describing. And the same may be said of other memoirs published at a later period: so that the *Théorie des Probabilités*, first published in 1812, may be considered as a collection of the various papers which had appeared in the Transactions cited from 1778 up to 1812.[p. 351]

One disadvantage of this procedure (not unknown in our own day), as De Morgan noted elsewhere (1837a, pp. 452–3) is that sometimes derivations are unnecessarily repeated in different sections of the *Théorie analytique.*

But such an approach can sometimes also lead to structural problems. Anyone who has struggled with Laplace's discussion of generating functions will appreciate De Morgan's critique:

> [T]he difficulty of the subject is materially increased by the practice of placing general descriptions at the beginning, instead of the end. Our present work begins with a tremendous account of the theory of generating functions, which we doubt not has deterred many a reader, who has imagined that it was necessary to master this first part of the work before proceeding to the rest. And why is this obstacle placed in the way? Because there was an old memoir ready to reprint from. And where in the subsequent part of the work is it used? In some isolated problems connected with gambling, which in the first place might be omitted without rendering the material part of the work more difficult; and in the second place are applications of the theory of generating functions of so simple a character, that the preliminaries connected with it might be discussed in two pages. And in what future part of the work do the very tedious (though skilful) methods of development become useful which are formally treated in the introductory chapter? Nowhere. Hence the reader may begin to suspect that the difficulty of this work does not lie entirely in the subject, but is to be attributed in great part to the author's method. That such difficulty is in part wholesome, may be very true; but it is also discouraging, unless the student be distinctly informed upon its cause and character. [pp. 353–4]

3.2.3 Imprecision

On a number of occasions De Morgan expresses asperity that Laplace was not more careful in his statement of conditions or hypotheses. For example, in his *Encyclopedia Metropolitana* article, after noting an unstated implicit condition De Morgan adds:

> Laplace (p. 240) has omitted all allusion to this circumstance; and the omission is highly characteristic of his method of writing. No one was more sure of giving the result of an analytical process correctly, and no one ever took so little care to point out the various small considerations on which correctness depends. His *Théorie des Probabilités* is by very much the most difficult mathematical work we have ever met with, and principally from this circumstance: the *Mécanique Céleste* has its full share of the same sort of difficulty; but the analysis is less intricate. [De Morgan 1837a, Section 52, p. 418, footnote]

Compare Todhunter's assessment (1865, pp. 536–539.)

3.2.4 Failure to cite previous literature

But perhaps De Morgan's harshest criticism had to do with Laplace's failure to carefully document his indebtedness to the work of his predecessors. As a mathematician who was particularly interested in the history of his subject, De Morgan regarded citation as an important obligation:

> The first duty of a mathematical investigator, in the manner of stating his results, is the most distinct recognition of the rights of others; and this is a duty which he owes as much to himself as to others. ... That such attention to the rights of others is due to those others, need hardly be here insisted on. [De Morgan, 1837b, p. 348]

De Morgan saw this in part as a national trait: "there runs throughout most of the modern writings of the French school, a thorough and culpable indifference to the necessity of clearly stating how much has been done by the writer himself, and how much by his predecessors". In doing so De Morgan did not see this as an arising from nationality partiality: "on the contrary, they are most impartially unfair both to their own countrymen and to foreigners; we may even say, that, to a certain extent, they behave properly to the latter, while of each other they are almost uniformly neglectful". Laplace was merely the "most striking example of this disingenuous practice"; and De Morgan pointed to an example of this in the *Mécanique Céleste*, where Laplace failed to properly credit the work of Lagrange.

De Morgan saw this as being of particular importance when assessing Laplace's work in mathematical probability:

> The preceding remarks have a particular bearing upon the *Théorie des Probabilités*, for it is in this work that the author has furnished

> the most decided proof of grand originality and power. It is not that the preceding fault is avoided; for to whatever extent De Moivre, Euler, or any other, had furnished either isolated results, or hints as to method of proceeding, to precisely that same extent have their names been suppressed. Nevertheless, since less had been done to master the difficulties of this subject than in the case of the theory of gravitation, it is here that Laplace most shines as a creator of resources. It is not for us to say that, failing such predecessors as he had (Newton only excepted), he would not by his own genius have opened a route for himself. Certainly, if the power of any one man would have sufficed for the purpose, that man might have been Laplace. As it is, we can only, looking at the *Théorie des Probabilités*, in which he is most himself, congratulate the student upon the fact of more symmetrical heads having preceded him in his *Mécanique Céleste*. Sharing, as does the latter work, in the defects of the former, what would its five volumes have presented if Laplace had had no forerunner? [pp. 349–50]

By the end, De Morgan's irritation is almost palpable:

> The short historical notice and general explanation is omitted, in consequence, we suppose, of the humiliation which the writer of a treatise would feel, were he compelled to name another man. The extravagance of an original memoir lights the candle at both ends; not only is an author permitted to say clearly where he ends, but also where he began. Did Stirling give a result which might have afforded a hint as to the direction in which more was to be looked for? Laplace may and does confess it in the Transactions of the Academy. But the economy of a finished work will not permit such freedoms; and while on the one hand the student has no direct reason for supposing that there ever *will* be any body but Laplace, he has, on the other, no means of knowing that there ever *was* any body but Laplace. [p. 353]

De Morgan's annoyance was understandable for a very personal reason. His essay in the *Dublin Review* was written shortly after he completed his article for the *Encyclopedia Metropolitana*. While writing it De Morgan was just coming to grips with the considerable literature of probability, and he had assumed at the time—wrongly as it turned out—that any unattributed result in the *Théorie analytiques* was due to Laplace. But as De Morgan delved deeper into the subject he realized to his consternation that this was far from being the case. Thus, when his own *Essay on Probabilities* appeared the next year (1838, discussed below), he added the following unusual footnote of retraction:

> The solution of Laplace [to the problem of the duration of play] gives results for the most part in precisely the same form as those of De Moivre, but, according to Laplace's usual custom, no predecessor is mentioned. Though generally aware that Laplace, (and too many others, particularly among French writers) was much given to this unworthy species of suppression, I had not any idea of the extent to which it was carried until I compared his solution of the problem of the duration of play, with that of De Moivre. Having been instrumental (in my mathematical treatise on Probabilities, in the *Encyclopedia Metropolitana*) in attributing to Laplace more than his due, having been misled by the suppressions aforesaid, I feel bound to take this opportunity of requesting any reader of that article to consider every thing there given to Laplace as meaning simply that it is to be found in his work, in which, as in the *Mécanique Céleste*, there is enough originating from himself to make any reader wonder that one who could so well afford to state what he had taken from others, should have set an example so dangerous to his own claims. [De Morgan, 1838b, First Appendix, pp. ii–iii, footnote.]

These criticisms were later echoed by Todhunter and others.

In the end, De Morgan sees his critical assessment and exposition of Laplace as being of benefit both to the student and Laplace himself:

> In pointing out, therefore, the defects of the work in question in detaching them from the subject, and laying them upon the author—taking care at the same time to distinguish between the high praise which is due to the originality and invention of the latter, and the expression of regret that he should, like Newton, have retarded the progress of his most original views by faults of style and manner—we conceive that we are doing good service, not only to the subject itself, but even to the fame of its investigator. If, at the same time, we can render it somewhat more accessible to the student, and help to create a larger class of readers, we are forwarding the creation of the opinion that the results of this theory, in its more abstruse parts, may and should be made both practical and useful, even in the restricted and commercial

sense of the former term. Such must be the impression of all who have examined the evidence for this theory. [pp. 350–351]

3.3 *An Essay on Probabilities*

In 1838 De Morgan published a popular reworking of the material in his 1837 encyclopedia article, entitled *An Essay on the Theory of Probabilities.* This was designed primarily to present the *results* of the mathematical theory in a form accessible to an audience with only limited mathematical abilities, together with applications to inductive inference and insurance. (When the publishers of the *Encyclopedia Metropolitana* learned of this, they expressed concern, and De Morgan had to go to great lengths to attempt to convince them that the book was an entirely different affair from the encyclopedia article.[10])

Although Laplace made many contributions to the mathematical theory of probability, De Morgan ascribed great importance to one in particular :

> Laplace, armed with the mathematical aid given by De Moivre, Stirling, Euler, and others, and being in possession of the inverse principle already mentioned, succeeded both in the application of this theory to more useful species of questions, and in so far reducing the difficulties of calculation that very complicated problems may be put, as to method of solution, within the reach of an ordinary arithmetician. His contribution to the science was a general method (the analytical beauty and power of which would alone be sufficient to give him a high rank among mathematicians) for the solution of all questions in the theory of chances which would otherwise require large numbers of operations. The instrument employed is a table (marked Table I. in the Appendix to this work), upon the construction of which the ultimate solution of every problem may be made to depend. [pp. vii–viii]

Thus De Morgan views Laplace's primary contribution to the theory of probability (note he is careful to add "armed with the mathematical aid given by De Moivre, Stirling, Euler, and others") to be the central limit theorem. A substantial part of his *Essay* is therefore devoted to making these results accessible to a wide public:

> To understand the demonstration of the method of Laplace would require considerable mathematical knowledge; but the manner of using his results may be described to a person who possesses no more than a common acquaintance with decimal fractions. To

> reduce this method to rules, by which such an arithmetician may have the use of it, has been one of my primary objects in writing this treatise. I am not aware that such an attempt has yet been made: if, therefore, the fourth, and part of the fifth chapters of this work, should be found *difficult*, let it be remembered that the attainment of such results has hitherto been *impossible*, except to those who have spent a large proportion of their lives in mathematical studies. [p. viii]

(Note this absence of earlier guides testifies to the very limited penetration the Laplacian theory had had into the English literature prior to the time De Morgan wrote.)

Much of the later part of De Morgan's book is devoted to the theory of annuities (as were the later editions of De Moivre's *Doctrine of Chances*), and he stresses the practical uses of the theory here (as opposed to, say, the analysis of games of chance). Nevertheless, he also viewed theory as having a more general importance:

> The considerations contained in this volume have, in my opinion, a species of value which is not directly derived from the use which may be made of them as an aid to the solution of problems, whether pecuniary or not. Those who prize the higher occupations of intellect see with regret the tendency of our present social system, both in England and America, with regard to opinion upon the end and use of knowledge, and the purpose of education. Of the thousands who, in each year, take their station in the different parts of busy life, by far the greater number have never known real mental exertion; and, in spite of the variety of subjects which are crowding upon each other in the daily business of our elementary schools, a low standard of utility is gaining ground with the increase of the quantity of instruction, which deteriorates its quality. All information begins to be tested by its *professional* value; and the knowledge which is to open the mind of fourteen years old is decided upon by its fitness to manure the money-tree. [p. xiii]

3.3.1 Predicting the unpredictable

There is a particularly interesting aspects of De Morgan's treatment of the "rule of succession" (the term is due to Venn) that deserves brief mention here. Unlike most other discussions, which limited themselves to the dichotomous case of an event and its negation, De Morgan discusses the multinomial generalization.[11] This states that if there are t categories, n_j observations in each, and $n = n_1 + \cdots + n_t$, then the probability that the next observation will be of the j-th type is

$$\frac{n_j + 1}{n + t}.$$

This appears in Laplace (1781)—but not the *Théorie analytique*—as well as Lubbock (1830). But then De Morgan goes on to discuss a most interesting refinement that can only be found in Laplace's 1781 paper: if the possibility of entirely new outcomes of a type not previously known is included, then the rule of succession becomes

$$\frac{n_j + 1}{n + t + 1}.$$

This is a special case of what came to be termed in the 1980s the "Hoppe urn model" or "chinese restaurant process"; see Zabell (1992b, 1997).

3.4 *Formal Logic*

After 1838 De Morgan turned increasingly to logic, summarizing his views in his 1847 work, *Formal Logic: or, the Calculus of Inference, Necessary and Probable*. The book is unusual in that it presents probabilistic inference as part of formal logic. It is is still worth reading today, if only for its vigorous style.[12]

Three chapters of the *Formal Logic* deal with probability: Chapter 9 ("On Probability"), Chapter 10 ("On probable Inference"), and – to some extent – Chapter 11 ("On Induction"). Chapter 9 summarizes the most basic properties of probability, but is of considerable interest in its defense of Laplace's subjective viewpoint. It argues first (pp. 172–3) that "by degree of probability we really mean, or ought to mean, degree of belief"; and then (pp. 174–182) that such degrees of belief are capable of measurement (to what extent psychological phenomena are capable of measurement became a topic of considerable dispute in the 19th century; see, e. g., Stigler, 1986, Chapter 7). Chapter 10 is a reworking of the classical theory of testimony, a subject De Morgan touched on in some of his papers on logic from this period (De Morgan, 1849 and 1856).

Perhaps the most important direct influence of De Morgan's book and related papers was on George Boole, who devoted the later part of his book *An Investigation into the Laws of Thought* (1854) to a discussion of the theory of probability, inductive inference, and the reliability of judgements by courts and assemblies.

3.4.1 John Venn's *The Logic of Chance*

De Morgan's views regarding the nature of probability were later directly challenged by John Venn (1834–1923) in his 1866 book *The Logic of Chance*.[13] In a footnote to his sixth chapter, "The subjective side of probability. Measurement of belief", written for the 3rd, 1888 edition, Venn testifies to the ascendance of the Laplacian viewpoint, saying the chapter was:

> Originally written in somewhat of a spirit of protest against what seemed to me the prevalent disposition to follow De Morgan in taking too subjective a view of the science. [Venn, 1888, p. 118]

Venn argued instead for the "material" versus the "conceptualist" view of logic; that is, "with that which regards it as taking cognisance of laws of things and not of the laws of our own minds in thinking about things" (1888, p. x). His *Logic* was the first book in English expounding the frequency view of probability,—but it was also the last of any kind in English devoted to the foundations of probability until the appearance of Keynes's *Treatise* in 1921. Nor did it have much impact on the statistical profession either: both Edgeworth and Karl Pearson were, at least in their foundational views, Bayesians. The overthrown of the Laplacean edifice would have to wait for the 20th century and the rise of Fisher and Neyman.

4 Students of De Morgan

De Morgan had several students of note (for example, J. J. Sylvester and W. K. Clifford), but two of these, Isaac Todhunter (1820–1884) and William Stanley Jevons (1835–1882) are of interest here, both because of their contributions to probability, and, in particular, the diffusion in Britain of knowledge of, and appreciation for, Laplace. Their respective contributions in this area, however, were of very different kinds.

4.1 Isaac Todhunter

Isaac Todhunter is of interest to us because of his comprehensive *History of the Mathematical Theory of Probability from the Time of Pascal to that of Laplace* (1865). Its title obviously singles out Laplace as a milestone in the history of the subject. Before turning to this remarkable book, however, a few words about Todhunter himself and his debt to De Morgan seem in order.

4.1.1 Life

Todhunter began his studies at University College London (BA 1842, MA 1844), attending lectures by Sylvester and De Morgan. It is not unusual for a teacher to have an important influence on a student, but in this case the influence on Todhunter was profound. His brother, T. H. Todhunter, tells us: "for [De Morgan] his admiration was unbounded, and by whom he was induced to enter at Cambridge. ... The determining factors of his course of life must, I think, be taken to be his connexion with Mr. Austin [an early schoolmaster] and the influence of Prof. De Morgan" (Mayor, 1884, pp. 263–264).

Thus encouraged, Todhunter went on to study at St. John's College, Cambridge (once again as an undergraduate!) in 1844.[14]

The result was a highly successful career: senior wrangler and winner of both the first Smith's and Burney Prizes in 1848, Todhunter was elected a Fellow of St. John's and went on to a career as a prolific author of both textbooks and scholarly tomes, as well as becoming a Fellow of the Royal Society in 1862 and winner of the Adams Prize in 1871. His students at St. John's included Leslie Stephen, P. G. Tait, and John Venn.[15] He resigned his Fellowship at St. John's in 1864 in order to marry, (reflecting yet another limitation of the the 19th century Oxbridge educational system); but was made Honorary Fellow of his College ten years later (a distinction he highly prized).

Driven in part by the necessities of the invisible hand, Todhunter became a prolific and highly successful author of basic mathematical textbooks (see generally Barrow-Green, 2001). He wrote dozens of such books, on subjects ranging from algebra, geometry, and trigonometry, to the differential and integral calculus, and mechanics, some going into fifteen or more editions, many used in other countries, either in the English original or translated into languages such as Italian and Chinese.

If he had only written such elementary textbooks, Todhunter would be a largely forgotten figure today. But in addition to these, he also wrote several highly regarded histories of mathematical subjects: the calculus of variations, probability, "theories of attraction and the figure of the earth", and elasticity and strength of materials (the later edited posthumously by a young Karl Pearson). Here again one can see the influence of De Morgan, who was responsible for "that interest in the history and bibliography of science, in moral philosophy and logic, which determined the course of his riper studies" (Mayor, 1884, p. 181).[16]

4.1.2 Mathematical History

Let us now turn to Todhunter's comprehensive *History of the Mathematical Theory of Probability from the Time of Pascal to that of Laplace.*[17]

This was a remarkable summary of virtually all relevant literature in probability, in all European languages, up to and including Laplace. Its title and organization emphasized Laplace's central position in the subject, and its summaries of both his papers and the *Théorie analytique* made this material much more accessible to the interested mathematician.

Todhunter's book remains very useful even today. It may not exactly be rivetting, but if you want to know exactly what someone wrote and where, you will likely find it there, clearly stated (and also virtually free of typographical errors). Historians in later periods have usually looked back on it with great respect. Keynes (1921, p. 472), himself a prodigious bibliophile and often acid in his judgment, says of Todhunter that his bibliography "and also his commentary and analysis are complete and exact,—a work of true learning, beyond criticism". Anders Hald, in the first chapter of his own very impressive history of probability and statistics (1990, pp. 8), although not uncritical of certain aspects of Todhunter's methodology, is also at pains to make clear his respect: Todhunter is the "unquestioned authority on the early history of probability theory"; his book a "masterpiece" and "an invaluable handbook". Indeed, it is a remarkable tribute that writing exactly 125 years after Todhunter, Hald begins his book by setting out his "Principles of Exposition" and "Plan of this Book", and then immediately turns to "A Comparison with Todhunter's Book", thereby passing over all possible intervening competitors!

De Morgan's influence is evident throughout Todhunter's book. Quite apart from his repeated reference to De Morgan (sixteen times according to the index), Todhunter reports and usually endorses De Morgan's major criticisms of Laplace: for example, noting Laplace's carelessness in his treatment of Waldegrave's problem (p. 539, quoting De Morgan); noting, but criticizing, De Morgan's 1837 paper (p. 557); noting Laplace's error in the analysis of the Buffon needle problem (p. 591, but observing De Morgan has not himself realized the correct result *is* given in the first edition of the *Théorie analytique*); criticizing Laplace's treatment of the mean duration of marriages as "very obscure" (p. 602) and, after an examination from several different vantage points, citing De Morgan's correction (p. 605); and taking Laplace to task in several places—either explicitly or implicitly—for failure to cite his predecessors (pp. 468, 553, and 612).

4.2 William Stanley Jevons: the ballot box of nature

If Isaac Todhunter played an important role in making Laplace's purely technical results more accessible to the interested mathematician, another of De Morgan's students, William Stanley Jevons, played a very different role in popularizing Laplace's philosophical views among a much larger audience throughout Britain.

Jevons entered University College London in 1859, studying mathematics and logic under De Morgan. Although primarily an economist and logician, Jevons's 1874 book *The Principles of Science* was an important milestone in the philosophy of inductive inference. It took an enthusiastically Laplacian view of the process, championing the use of the rule of succession as an explanation of inductive inference. Keynes thought Bacon, Hume, and Mill "are the principal names . . . with which the history of induction ought to be associated. The next place is held by Laplace and Jevons" (Keynes, 1921, p. 295). In a celebrated passage in his book, Jevons wrote:

> Nature is to us like an infinite ballot box, the contents of which are being continually drawn, ball after ball, and exhibited to us. Science is but the careful observation of the succession in which balls of various character present themselves (Jevons, 1877, p. 150).

The appropriateness of this analogy was at the heart of the classical analysis of induction in probabilistic terms.

5 Discussion

Augustus De Morgan was the beneficiary of a recent reform movement in English mathematics, one which made it natural for him to turn—much more so than most of his contemporaries—to continental sources for mathematical inspiration. This, together with a practical streak and interest in annuities, led him in turn to study, closely and critically, the works of Laplace in mathematical probability. But where others might confine themselves to writing arcane papers to be read only by those in their immediate invisible college, De Morgan preferred to act primarily as an expositor, writing technical, popular, and philosophical books on probability and logic, as well as papers, reviews, and numerous short encyclopedia entries. This helped make Laplace's work accessible to an entire generation, mathematicians and non-mathematicians alike, and consolidated the Bayesian view of statistical and inductive inference in England, making it the preferred one until the 20th century (when it came under attack, for differing reasons, by Keynes, Fisher, Egon Pearson, and Jerzy Neyman).

De Morgan's preeminence as an authority was certainly recognized by his contemporaries. Boole (1854, p. 375) for example, refers to De Morgan as "one who, of English writers, has most fully entered into the spirit and the methods of Laplace"; and as late as 1895, *The Encyclopedia Britannica* entry on De Morgan opined that his 1837 encyclopedia article "is still the most complete mathematical treatise on the subject in the English language, and said his *Essay on Probabilities* was "still much used, being probably the best simple introduction to the theory in the English language". (The entry is initialed "W. S. J.", and was presumably written by Jevons.)

But of all the things that De Morgan wrote concerning Laplace, however, the most perversely persistent seems to be his criticisms regarding citation practice. For example, earlier this very year, Sharon Bertsch McGrayne (2012, p. 34), in her book on Bayes's theorem, writes:

> The English mathematician Augustus de Morgan wrote in *The Penny Cyclopaedia* of 1839 that Laplace failed to credit the work of others, the accusation was repeated without substantiation for 150 years until a detailed study by Stigler concluded it was groundless.

Unraveling what is going on here is a bit of a detective story in its own right. As has been seen, the citation charge goes back even further than 1839, to De Morgan (1837b and 1838), and is far from groundless. What does Stigler's outstanding 1978 study, on which McGrayne draws, say regarding this point?

> Since his death, Laplace has been accused sporadically of a multitude of sins, including political and personal expediency and borrowing others' results without citation. These charges seem to originate with an ill-considered article on Laplace by Augustus De Morgan in the *Penny Cyclopedia*, published in 1839, and if true they could substantially bias any analysis based on citation counts. However, the writers who have repeated these accusations have never substantiated the charges, and, based on my own detailed investigation of Laplace's work in probability and mathematical statistics, I would side with Pearson in dismissing them as groundless. [Stigler, 1978, p. 247, footnotes omitted.]

But as Stigler explains (pp. 239–240), his paper does *not* cover Laplace's treatises (as well as certain papers prior to 1812, and all publications from

1812 on). Stigler advances cogent methodological reasons for doing so in the context of his own study, but the relevance for us here is that De Morgan's criticisms, as he makes clear in his *Dublin Review* screed (p. 353, quoted above) refer primarily to Laplace's citation practices in his treatises (from which De Morgan draws his examples) rather than the original memoirs that are the subject of Stigler's study.

Certainly Karl Pearson does not entirely acquit Laplace on this specific charge: after discussing the *Mécanique céleste* ("it is a pity that Laplace was not more careful to be generous to his compeers, marking off his own from other men's contributions to the mechanics of the planetary system"), Pearson goes on to add:

> The remarks which the *Mécanique céleste* calls forth apply as strongly, if not more so, to the *Théorie analytique des Probabilités.* Laplace put together all that was known of the subject in his day, and immensely added to and developed his material. But only those intimately acquainted with what Montmort, De Moivre, the Bernoullis, Condorcet and Lagrange had achieved, can fully grasp how much he owed to them not only for fundamental principles but for suggestions for further research.

This was precisely De Morgan's point.

But there is more to the story. Stigler also refers to accusations of a "multitude of sins, including political and personal expediency", that "seem to originate with" De Morgan's 1839 *Penny Cyclopedia* article. The link between the two was in fact made by Pearson, but it is instructive to see exactly what Pearson (1929, pp. 208–209) says:

> Where again, I ask, did these smaller English historians draw their characterization of Laplace as a time-server and a futile politician?
>
> Probably, I believe, from an article on Laplace by Augustus De Morgan in the *Penny Encyclopaedia* of 1835 [sic]. That distinguished mathematician had a fatal bent towards damaging the scientific and moral reputations of greater mathematicians. I need only cite his treatment of both Newton and Laplace.

Ah! —now all becomes clear. Pearson has in mind the characterizations of Laplace as "as a time-server and a futile politician" when referring to De Morgan's 1839 article, *not* De Morgan's remarks about citation.[18]

Nevertheless, even in the case of the last Pearson urges viewing the matter in context: treatises are different from papers; Laplace's intended audience was well aware of the prior accomplishments of others, no one criticizes Euclid for his silence on what came before, our practices now are different from what they were then, and so on. These are all valid points.

Attribution and citation of the works of one's predecessors could be a most uncertain affair in the eighteenth century. Laplace's failure to cite his predecessors, however, is a complex matter, often bordering on questions of style. Many of the examples in Laplace's *Théorie analytique* and *Essai philosophique*, for instance, were designed to correct past misunderstandings and errors in the work of others. It is only seldom, however, that Laplace directly refers to this literature. The examples are clearly chosen with this past history in mind, but Laplace is content to give the correct analysis, and draw the proper conclusions, while he disdains to catalogue the history of error.[19]

Acknowledgement: I thank Laurent Mazliak for many helpful comments and suggestions on an earlier draft of this paper.

Notes

[1]This was certainly the perception of the English themselves; Rouse Ball (1889, Chapter 6, p. 99), for example, says that for the period immediately after Newton "except for [Waring] I can recall the names of no Cambridge men whose writings at this distance of time are worth more than a passing notice", and refers to"the quality of the mathematical work produced in this period" as being "so mediocre". G. H. Hardy (1926, p. 63), writing 37 years after Rouse Ball was, if anything, even harsher: "Since Newton, England has produced no mathematician of the very highest rank". Hardy attributed this largely to the malign influence of the Mathematical Tripos, a sentiment that De Morgan would, as will be seen, no doubt have approved.

[2]For example, in the ten years preceding the founding of the Analytical Society (1802–1811), five of the senior wranglers were lawyers, jurists, or politicians (Thomas Starkie, 1803; Jonathan Frederick Pollock, 1806; Henry Bickersteth, 1808; Edward Hall Alderson, 1809; William Henry Maule, 1810), three clergymen (Thomas Penny White, 1802; John Kaye, 1804; Henry Gipps, 1807), and one a railroad entrepreneur (Thomas Edward Dicey, 1811). This is not to say that many of these did not have careers of distinction, quite the contrary: Pollock was Attorney General, Alderson, Baron of the Exchequer, Bickersteth, Master of the Rolls; Kaye was Master of Christ's College, Cambridge (1814–1830), and later Vice-Chancellor of the University. But virtually none had serious connections with mathematics, per se. The one apparent exception – Thomas Turton, 1805 – is in fact the exception that proves the rule: quite apart from his present-day obscurity, Turton served as Lucasian Professor for only five years (1822–1827), until resigning it for the more suitable Regius Professor of Divinity (1827–1842) and a variety of later ecclesiastical preferments. (Frederick Pollock, who actually *wrote* on mathematics, did so only a half-century later, in a series of papers in the *Philosophical Transactions*, most notably on the the so-called "Pollock octahedral numbers conjecture".) See generally Neale (1907).

[3]For example, during the period 1820–1900, a short list would include G. B. Airey, 1823; James Challis, 1825; R. L. Ellis, 1840; G. G. Stokes, 1841; Arthur Cayley, 1842; J. C. Adams, 1843; Isaac Todhunter, 1848; P. G. Tait, 1852; E. J. Routh, 1854; J. W. Strutt (Lord Rayleigh), 1865; E. W. Hobson, 1878; Joseph Larmor, 1880; A. R. Forsyth, 1881; A. C. Dixon, 1886; H. F. Baker, 1887; Thomas Bromwich, 1895.

[4]The basic source of information for much of De Morgan's life is the memoir of his wife Sophie (De Morgan, 1882). For a briefer but very readable account (and also that of Todhunter's), see MacFarlane (1916).

[5] London University (today University College London) was founded in 1826, and together with King's College (today King's College London), incorporated as part of the University of London in 1836.

[6]Looking back at the English literature since De Moivre, Galloway (1839, pp. 14–15) was unimpressed: "English treatises on the general theory of probability have neither been numerous, nor, with one or two exceptions, very important"; and only mentions Simpson (1740), Dodson (1748), and Lubbock and Drinkwater (1830), presumably as honorable exceptions. De Morgan's treatise, in contrast, is "by far the most valuable work in the language" and, in a "very able production . . . has treated the subject in its utmost generality, and embodied, within a moderate compass, the substance of the great work of Laplace".

[7]The lack of justification for such an inversion was at the center of the much later dispute concerning R. A. Fisher's theory of *fiducial inference*; see Zabell (1992a).

[8]De Morgan's discussion of the *Théorie analytique des probabilités* appeared in the April and July 1837 issues of the *Dublin Review*. The article, as was the practice of the journal, is unsigned but known to be by De Morgan. (It is included in a list of De Morgan's writings compiled after his death by his wife Sophia De Morgan (De Morgan, 1882, p. 406); and was later acknowledged by the *Dublin Review* itself, in a "General List of Articles: 1836–1896, vol. 118 (1896) pp. 467–520, at p. 468.)

[9]In some cases, of course, this can be constructively noted, as in Keynes's famous disclaimer at the beginning of the bibliography for his *Treatise on Probability*:

> I have not read all these books myself, but I have read more of them than it would be good for any one to read again. There are here enumerated many dead treatises and ghostly memoirs. The list is too long, and I have not always successfully resisted the impulse to add to it in the spirit of a collector. There are not above a hundred of these which it would be worth while to preserve,—if only it were securely ascertained which these hundred are. At present a bibliographer takes pride in numerous entries; but he would be a more useful fellow, and the labours of research would be lightened, if he could practise deletion and bring into existence an accredited *Index Expurgatorius*. But this can only be accomplished by the slow mills of the collective judgment of the learned; and I have already indicated my own favourite authors in copious footnotes to the main body of the text. [Keynes, 1921, pp. 472–473 of the 1973 edition]

[10]Indeed, in Sophia De Morgan's memoir of her husband, a pamphlet is mentioned:

> The advertisement of the 'Essay' alarmed the editor of the 'Encyclopaedia Metropolitana,' who, being unable to understand that a profound Mathematical work full of definite integration was altogether a different thing from a popular essay requiring only decimal fractions, and mainly devoted to life

contingencies, accused the writer of having infringed the rights of the proprietors of the Encyclopedia, by publishing what he said 'might be deemed a second edition of the treatise,' and threatened, or implied a threat of prosecution. The author, who was more amused than annoyed by this want of perception in the publisher, ex- plained to him very clearly the respective characters of the works, but failed to make him understand how widely they differed. He then proposed arbitration, he being willing to pay whatever damages should be judged proportionate to their loss to the supposed injured parties; or, in the event of the decision being in his favour, that a sum of money should be given by them to some charity, as amends for the trouble given and the false aspersions made. This last proposal being rejected, the author of the Treatise and Essay published a little pamphlet in explanation, which showed to all who cared to understand the question that the publisher's ignorance of its nature had led him into what my husband called 'wasting a good deal of good grumbling,' but which was in truth an unjust imputation on himself. [De Morgan, 1882, pp. 92–3.]

Copies of the pamphlet ("Remarks on an accusation made by the proprietors of the 'Encyclopaedia Metropolitana' against the author of an 'Essay on probabilities'," London, 1838) are listed in the catalogues of the libraries of both the Royal Astronomical Society and the London Institution.

[11]For De Morgan's discussion of the rule, see De Morgan, 1837a, Articles 48–49, pp. 413–415); and his 1838 *Essay*, pp. 64–68.

[12]A brief quotation will illustrate De Morgan's astringent wit. Regarding belief, De Morgan says

> When we speak of belief in common life, we always mean that we consider the object of belief more likely than not: the state of mind in which we rather reject than admit, we call disbelief. When the mind is quite unbalanced either way, we have no word to express it, because the state is not a popular one.

He then adds in a footnote:

> Many minds, and almost all uneducated ones, can hardly retain an intermediate state. Put it to the first comer, what he thinks on the question whether there be volcanoes on the unseen side of the moon larger than those on our side. The odds are, that though he has never thought of the question, he has a pretty stiff opinion in three seconds.

[13]Venn's *Logic* went through three editions (1866, 1876, and 1888); important changes were made in the second and third editions. In particular, Venn's statement in the third edition (1888, p. 119) that Edgeworth held "a view not substantially different from mine, but expressed with a somewhat different emphasis", is remarkable, given Edgeworth's Bayesian view of statistical inference. But Venn's views had shifted in important ways, presumably due to Edgeworth's influence, who Venn thanks (1888, p. xviii) "for many discussions, oral and written, and for his kindness in looking through the proof-sheets".

[14]Such a double BA path was by no means unique to Todhunter, reflecting in part the then limited resources for teaching preparatory mathematics in some schools at precollegiate levels. Numa Edward Hartog (1846–1871), for example, one of De Morgan's last students, after graduating from University College London in 1864, also went on

to Cambridge to take a second BA (Trinity College, 1869). (Despite being both Senior Wrangler and winner of a Smith's Prize in his graduating year, Hartog was unable to stay on at Cambridge as a Fellow due to his being Jewish. This is said to have played a part in the passing of the Test Act of 1871, which removed such impediments.)

[15]For Venn, see Barrow-Green (2001, p. 185). Todhunter stressed preparation for the Tripos, which Venn thought too restrictive.

[16]For De Morgan's interest in the history of science, see Rice (1996).

[17]Not surprisingly, Todhunter thanks De Morgan in his preface (together with Boole) for his assistance ("the kind interest which he has taken in my work, for the loan of scarce books, and for the suggestion of valuable references").

[18]And it is certainly true that De Morgan's further animadversions on Laplace in his *Penny Cyclopedia* entry seem disproportionate both in tone and length.

[19]See Zabell (1988) from which the language in this paragraph is taken.

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