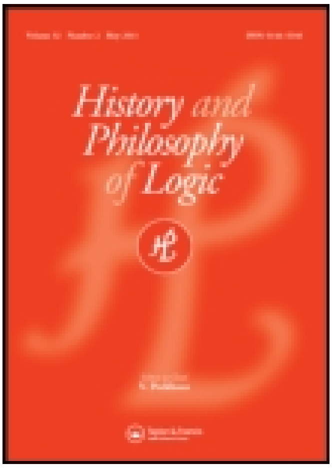

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'Everybody makes errors': The intersection of De Morgan's Logic and Probability, 1837-1847

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'Everybody makes errors': The intersection of De Morgan's Logic and Probability, 1837–1847

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For Ivor Grattan-Guinness on the occasion of his retirement.

The work of Augustus De Morgan on symbolic logic in the mid-nineteenth century is familiar to historians of logic and mathematics alike. What is less well known is his work on probability and, more specifically, the use of probabilistic ideas and methods in his logic. The majority of De Morgan's work on probability was undertaken around 1837–1838, with his earliest publications on logic appearing from 1839, a period which culminated with the publication of his *Formal Logic* in 1847. This article examines the overlap between his work on probability theory and logic during the earliest period of his interest in both.

1. Introduction

There can be very few logicians today to whom the name of De Morgan is completely unknown. The principal reason for this, of course, is his association with the fundamental principles, now inextricable from a first course in logic, namely,

$$\neg(a \vee b) = \neg a \wedge \neg b \text{ and } \neg(a \wedge b) = \neg a \vee \neg b$$

or, in set theoretic notation,

$$(A \cup B)' = A' \cap B' \text{ and } (A \cap B)' = A' \cup B'$$

Yet, as is so often the case in the history of ideas, these rules are now known universally as De Morgan's Laws, despite the fact that he did not invent them[1] and would be unfamiliar with their current formulations.[2]

De Morgan is more justifiably remembered, primarily by historians of logic, for his significant work on developing the logic of relations (*Merrill 1990*; *Hawkins 1979*; *Kneale and Kneale 1962*, p. 427). Indeed, he seems to have been the first logician to notice that relational propositions, such as 'Alice is the daughter of Bill', cannot be deduced (or even stated) by conventional syllogistic means. In examining this gap in the literature, De Morgan effectively founded a new branch of the subject and, applying his results to classical logic, revealed that the syllogism was 'one case, and one case only, of the *composition of relations*' (*De Morgan 1860a*, p. 331). De Morgan was thus the first logician not only to stress the deficiency of classical syllogistic logic

1 One might say that they are an example of 'Stigler's Law of Eponymy', namely: 'No scientific discovery is named after its original discoverer' (*Stigler 1999*, p. 277).

2 De Morgan enunciated them as 'The contrary of an aggregate is the compound of the contraries of the aggregants: the contrary of a compound is the aggregate of the contraries of the components' (*De Morgan 1858*, p. 208; *De Morgan 1860b*, p. 41; compare also *De Morgan 1847*, p. 116).

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but also to view it as a special case of his logic of relations. Yet although this foundational work was later described as 'indispensable and of permanent value' (*Lewis 1918*, p. 50), it was never a finished theory, being left for others to develop more fully. Nevertheless, it is widely regarded as being his most significant logical contribution (*Peirce 1931*, p. 562; *Heath 1966*, p. xx; *Panteki 1992*, pp. 422–423; *Grattan-Guinness 2000*, p. 32).

De Morgan's work in logic has often been overshadowed by that of Boole, who worked on very similar areas[3] at roughly the same time. (Indeed, as De Morgan pointed out,[4] their key works *Formal Logic* and Boole's *Mathematical Analysis of Logic* were published simultaneously.) Although Boole's work was of greater depth, his initial publications on the subject were inspired, at least in part, by De Morgan's work (*Heath 1966*, p. xv; *Laita 1979*; *Grattan-Guinness 2000*, pp. 32, 40). Yet while they shared many common goals and interests in the realm of logic, their correspondence reveals that they differed considerably in how they realised them (*Smith 1982*, pp. 22–29).

A key feature (and point of overlap) in both De Morgan and Boole's logic was the use of mathematics and mathematical concepts, such as symbolic notation, to facilitate procedures. It was this aspect that resulted in De Morgan's work achieving a considerable level of fame (and notoriety) in its day, since the very notion of introducing mathematics into pure logic was an anathema to many practicing logicians (chiefly philosophers) (*Panteki 1992*, pp. 424–425). One of his most vehement critics was the Scottish philosopher Sir William Hamilton, who in 1847 mistakenly accused De Morgan of plagiarising the idea of quantifying the predicate. De Morgan replied vigorously in his defense, initiating a sustained discussion on the merits of their contrasting approaches to the subject. The ensuing debate itself was largely fruitless, but a significant outcome was the stimulus it provided Boole to 'resume the almost-forgotten thread of former inquiries' (*Boole 1847*, p. 1). Thus, an indirect consequence of De Morgan's use of mathematics in his logic was Boole's public entrance into the subject (*Laita 1979*).

Despite the attention that De Morgan's use of mathematics in his logic received, virtually nothing has been written on his use of a specific branch of mathematics, namely probability theory, in his logical research. That this aspect of his 'mathematisation' of logic has gone unnoticed is surprising for several reasons, but in particular because we nowadays regard logic and probability as being inextricably linked via the area of set theory. Considering the obvious overlaps between these disciplines, surely the fact that De Morgan actively sought to employ probability theory in his logic would have warranted some attention? But it appears not.

In recent years, scholars have kept De Morgan's logical works in print and academic circulation. For example, Peter Heath edited a well-presented collection of his logical essays in 1966, which contains practically everything that De Morgan published on the subject (*De Morgan 1966*). In 1992, Maria Panteki produced a significant dissertation on the connections between logic, algebra and differential equations from 1800 to 1860, which included a substantial chapter on the mathematical aspects of De Morgan's logic (*Panteki 1992*, pp. 407–492). Yet, for

3 Although Boole used quite different means; for example, there were no relations in Boole's logic.

4 When communicating Boole's posthumous paper, *Boole 1868*, De Morgan noted that *The Mathematical Analysis of Logic* 'appeared on the same day as my *Formal Logic*' (*Boole 1868*, p. 396n).

reasons entirely consistent with their respective projects, both Heath and Panteki omit consideration of De Morgan's work on probability.[5]

This article will therefore examine De Morgan's use of probability in his early work on logic, also giving a rare example of his (mis-)use of logic in his work on probability, before concluding with some possible explanations as to why probability is absent from his later logical works. It begins with an overview of his contributions to the two distinct fields.

2. De Morgan's work on probability

De Morgan's principal work on probability was a book-length article on the subject in the *Encyclopedia Metropolitana*[6] (*De Morgan 1837a*), in which he provided one of the first detailed commentaries on Laplace's seminal *Théorie analytique des probabilités* (*1812*). While it contained no original results, De Morgan's treatise was the first full-length exposition of Laplacean probability to be published in Britain, and as such, it constituted the first major work on modern probability theory to appear in the English language.[7]

From this article, as well as a lengthy review of Laplace in the *Dublin Review*, it is clear that De Morgan regarded the *Théorie* as the most difficult and demanding mathematics he had ever encountered. He described it as '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'[8] (*De Morgan 1837c*, p. 347). Nevertheless, through De Morgan's thoughtful presentation and elucidation of many of the complex proofs, his encyclopedia article served not only as a summary and digest of Laplace's *Théorie*, but also functioned as a translation of Laplace's work, both linguistically and mathematically.

It is a testament to De Morgan's mastery of the material that he was not only able to point out and simplify many often superfluous complexities in Laplace's notoriously opaque work, but he was also able to detect and correct several mistakes (*De Morgan 1837a*, pp. 410, 418, 447, 452–453, 460, 468; *De Morgan 1847*, p. 188). Indeed one error, committed by both Laplace and Poisson, warranted the

5 In Heath's 1966 edited collection of De Morgan's logical works, there are at least four occasions where the editor has excised discussions of probability from the text (*De Morgan 1966*, pp. 1, 9, 22, 206). Due to the thematic constraints of her study, Panteki also did not discuss De Morgan's use of probability in his logic, although she did mention that he 'proposed the substitution of "the numerical theory of probability" for the "old doctrine of modals"' (*Panteki 1992*, p. 426).

6 There is often some confusion about the precise dates of articles written for the *Encyclopedia Metropolitana*, due to the sprawling nature of that publication. From its inception in 1817, articles were contributed (and often published separately) for nearly 30 years, until its eventual completion in 1845. Thus, De Morgan's entry on probability first appeared in 1837, but is often erroneously dated as 1845, which is when it was finally bound together with the other *Encyclopedia* entries.

7 De Morgan also contributed a variety of articles on logical and probabilistic topics to the *Penny Cyclopedia* between 1833 and 1846. Although these entries are anonymous, they are attributed to him in *S. E. De Morgan 1882* (p. 415 *passim*).

8 In a footnote to his *Metropolitana* article, De Morgan wrote of Laplace: '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 ...' (*De Morgan 1837a*, p. 418n).

composition of a research paper, which De Morgan presented to the Cambridge Philosophical Society in 1837 (*De Morgan 1837b*).

In 1838, he published a more popular work, a book entitled *An Essay on Probabilities* (*De Morgan 1838*), as volume 107 of Dionysius Lardner's *Cabinet Cyclopedia* series. Containing probabilistic methods for use in insurance problems, this book was designed for those engaged in actuarial work, a profession still in its infancy in early nineteenth-century Britain. As the first book of its kind on the subject, it remained highly regarded in insurance literature for well over a generation.

De Morgan's probabilistic work also evinced a knowledge and appreciation of mathematical statistics. This is particularly evident in his *Essay*, which, in a number of places, contains analysis of data from various publications, including a couple of recent works by Quetelet (*De Morgan 1838*, pp. 120, 165–181, 191–197, 211–212). In his *Metropolitana* article, he also devoted a considerable portion to applications of the theory of probability to questions involving real data, including a detailed discussion of the method of least squares (*De Morgan 1837a*, pp. 440–464). This interest resurfaced a quarter of a century later with a substantial paper in which he attempted to simplify the mathematics underlying the theory of errors of observation (*De Morgan 1861c*).

De Morgan's contributions, then, to probability theory in Britain were twofold. First, he provided the British with the first full treatment of Laplacean probability theory in their own language, at the same time showing himself to be one of the only mainstream British proponents of the subject during the mid-nineteenth century; and second, by advocating its practical utility in the growing field of insurance he helped establish it as the basis of modern actuarial methods, which it has remained ever since.

3. De Morgan's early work on logic

De Morgan's interest in logic arose principally from his teaching of Euclidean geometry, and in particular the difficulties experienced in training students to construct abstract mathematical proofs. Indeed, his first publication on the subject, *First Notions of Logic* (*De Morgan 1839*), was designed especially for the use of his students prior to embarking on a course of geometry. Upon investigating the subject further, however, he soon came to realise two key weaknesses of the accepted logical method. First, very few refinements had been made to the conventional modes of logical reasoning, with the result that the traditional Aristotelian method of the syllogism had remained virtually unchanged in almost two thousand years. Second, there appeared to be no obvious link between logical (i.e. syllogistic) and mathematical reasoning. He later wrote:

> We know that mathematicians care no more for logic than logicians for mathematics. The two eyes of exact science are mathematics and logic: the mathematical sect puts out the logical eye, the logical sect puts out the mathematical eye; each believing that it sees better with one eye than with two. *De Morgan 1868* (p. 71)

Deploring the mutual antipathy between logicians and mathematicians then commonplace in Britain, he addressed these issues in the first of five major papers on the subject (*De Morgan 1846*), presented to the Cambridge Philosophical Society in 1846. It was this paper that sparked the long and drawn-out controversy with Hamilton and his followers over the quantification of the predicate and other matters.

This debate was fueled by the publication of his *Formal Logic De Morgan 1847*,[9] which remains the fullest, yet by no means the most developed, account of his logical system.

Perhaps the most noticeable feature of both his first paper and the *Formal Logic* book was the introduction of a symbolic notation to abbreviate the length of propositions and facilitate logical deductions. This notation would change considerably throughout De Morgan's oeuvre, but in this early period at least, it was pretty constant. Using a mixture of brackets and dots, he represented basic propositions such as

$X\,)\,Y$	Every X is Y
$X\!:\!Y$	Some Xs are not Ys
$X.Y$	No X is Y
XY	Some Xs are Ys (*De Morgan 1847*, p. 60)

Introducing the concept of 'contrary' terms, which he denoted by lower case letters x, y, he was able to form equivalent statements to those above so that, for example,

$X)Y = X.y = y)x$
$X\!:\!Y = Xy = y\!:\!x$
$X.Y = X)y = Y)x$
$XY = X\!:y = Y\!:x$ (*De Morgan 1847*, p. 61)

He would then use this (somewhat clumsy) notation to undertake logical deductions and deduce further propositions. However, he soon realised that such methods do not always lead to correct conclusions, for example

(1) $XY + ZY = XZ$ and (2) $XY + Z\!:Y = X\!:Z$

$$\text{i.e.}\quad \frac{\begin{array}{c}\text{Some } X\text{s are } Y\text{s}\\ \text{Some } Z\text{s are } Y\text{s}\end{array}}{\therefore \text{Some } X\text{s are } Z\text{s}} \qquad\qquad \frac{\begin{array}{c}\text{Some } X\text{s are } Y\text{s}\\ \text{Some } Z\text{s are not } Y\text{s}\end{array}}{\therefore \text{Some } X\text{s are not } Z\text{s}}$$

As both of these (invalid) deductions illustrate, and as De Morgan was quick to notice, any Aristotelian syllogism of the above form is incapable of deducing precise quantities. In an attempt to remedy this defect, De Morgan introduced his 'numerically definite syllogism'. Suppose our 'universe of discourse' (another concept he introduced) contains υ objects, of which ξ are Xs, η are Ys, and ζ are Zs, let mXY signify that there are m Xs among the Ys, and $mX : nY$ mean that there are m Xs which are not among n Ys. Two possible cases would then be:

(1) Of the η Ys, there are m Xs and n Zs (i.e. *some* Xs are Ys and *some* Zs are Ys).
(2) Of the η Ys, there are m Xs and s Ys are not among n Zs.

In case (1), m, $n \leqslant \eta$, although if $m + n \leqslant \eta$, nothing can be inferred. However, if $m + n > \eta$, we would have

(3) $mXY + nYZ = (m + n - \eta)XZ$.

9 This work contains De Morgan's account of the controversy as an appendix (pp. 297–323).

In other words, *some* Xs are Zs.

In case (2), if $m + s > \eta$, then there would be $(m + s - \eta)$ Ys that are Xs and are not among the n Zs. This gives

(4) $mXY + nZ : sY = (m + s - \eta)X : nZ.$

Alternatively, suppose that $n + s > \eta$, and that no n Zs are in the s Ys. Since $n + s > \eta$, then $n > \eta - s$, which is the number of Ys left. Therefore $n - (\eta - s)$ of the n Zs cannot be any of the Ys, and consequently, they cannot be any of the m Xs which are Ys. So

(5) $mXY + nZ : sY = mX : (n + s - \eta)Z.$

Both (4) and (5) are, of course, 'numerically definite' versions of (2).

A further refinement was introduced by introducing the notation m,XY to represent the statement 'exactly m Xs are Ys'. Since it was obvious that

$$m, XY = mXY + (\eta - m)xY$$

simple algebraic manipulation and the use of equation (3) gave

$$\begin{aligned} m, XY + n, ZY &= (mXY + (\eta - m)xY) + (nZY + (\eta - n)zY) \\ &= (mXY + nZY) + ((\eta - m)xY + (\eta - n)zY) \end{aligned}$$

(6) $\qquad = (m + n - \eta)XZ + (\eta - m - n)xz.$

De Morgan's creation of a numerically definite syllogism was a significant innovation. For the first time, given specific numerical information about certain propositions, precise calculations could be made via the syllogism. Furthermore, these deductions could be made purely symbolically, without any reference to significance or meaning. Of course, the real problem with De Morgan's method was that it involved a certain degree of uncertainty. In other words, case (1) would only produce equation (3), if $m + n > \eta$; so perhaps an alternative statement of syllogism (1) would be:

(7) Some Xs are Ys
Some Zs are Ys
$\therefore$ There is some probability that some Xs are Zs

It was here that De Morgan began to apply the methods of probability theory to his newly-created system of numerically definite logic.

4. De Morgan's use of probability in his logic

In his early works, De Morgan argued strongly that the inclusion of probability as part of logic would clarify the validity of certain deductions. He contrasted traditional logic—where the inability to formally demonstrate a statement by accepted methods would lead to its automatic rejection as false—with mathematics, in which a variety of techniques existed to establish the veracity or fallacy of a statement. As he said, the logician

> when his premises do not yield their inference legitimately, drops that inference as a fallacy: and few indeed are the books which speak of the distinction between an invalid inference and a false conclusion in terms which shew that the same distinction is a well recognized topic of the subject. It is, I think, for the mathematician to try to correct the habit arising out of this omission, namely, the confusion between paralogism and falsehood: and also to introduce his notions of probability, so as to establish some little power of discriminating between the various degrees of fallacy which are all called by one name, whether that name be falsehood or not. *De Morgan 1846* (p. 385)

His belief in the importance of probability in the realm of logic is illustrated by the subtitle of his *Formal Logic*—'The calculus of inference, necessary and probable'. Yet he was keenly aware that its inclusion would be controversial:

> Many will object to this theory as extralogical. But I cannot see on what definition, founded on real distinction, the exclusion of it can be maintained. . . . Without pretending that logic can take cognizance of the probability of any given matter, I cannot understand why the study of the effect which partial belief of the premises produces with respect to the conclusion, should be separated from that of the consequences of supposing the former to be absolutely true. . . . I should maintain, against those who would exclude the theory of probability from logic, that, call it by what name they like, it should accompany logic as a study. *De Morgan 1847* (p. v)

To illustrate the utility of probability in logic, he returned to the consideration of syllogism (7). As he noted, we would be unable to draw any further conclusions from any syllogism of this sort using traditional means, yet, with the aid of probability, he was able to show that a further step was possible.

Taking η as the total number of Ys, m the number of Xs which are Zs, and n the number of Zs which are Ys, he attempted to find the probability that some Ys will be both Xs and Zs, given that the distribution of Xs and Zs among the Ys is completely unknown. (He took it for granted that $m + n \leqslant \eta$, since if $m + n > \eta$, then by (6), it would be certain that some Xs would be Zs.) As he put it: 'when from "some Ys are Xs and some Ys are Zs" we decline to admit that some Xs are Zs, what is the chance that we reject a truth?' (*De Morgan 1846*, p. 385).

Using combinatorics, he argued that we have ${}^{\eta}C_m$ ways of choosing m Ys to be Xs, and ${}^{\eta-m}C_n$ to choose n Zs from the remaining $\eta - m$ Ys. Thus there are ${}^{\eta}C_m \times {}^{\eta-m}C_n$ ways in which the assertion 'Some Xs are Zs' could be false, out of a total number of ${}^{\eta}C_m \times {}^{\eta}C_n$ possible cases. Therefore the probability that the statement is false would be

$$\frac{{}^{\eta-m}C_n}{{}^{\eta}C_n} = \frac{(\eta - m)!(\eta - n)!}{\eta!(\eta - m - n)!}$$

Assuming $(\eta - m - n)$ to be large, he then used Stirling's Formula

$$n! \sim \sqrt{2\pi n}e^{-n}n^n$$

to obtain

$$\sqrt{\frac{(\eta-m)(\eta-n)}{\eta(\eta-m-n)}}\,\frac{(\eta-m)^{\eta-m}(\eta-n)^{\eta-n}}{\eta^{\eta}(\eta-m-n)^{\eta-m-n}}=\sqrt{\frac{(1-\mu)(1-v)}{1-\mu-v}}\left(\frac{(1-\mu)^{1-\mu}(1-v)^{1-v}}{(1-\mu-v)^{1-\mu-v}}\right)^{\eta}$$

where $\mu = m/\eta \leqslant 1$ and $v = n/\eta \leqslant 1$. Since $(1-\mu)$, $(1-v)$, and $(1-\mu-v) \leqslant 1$, he deduced that, for large enough η, this value would be negligibly small. Thus

$$P(\text{the statement 'Some } X\text{s are } Z\text{s' is false}) \approx 0.$$

He concluded: 'The result is, that if η be very considerable, and if a perceptible fraction of the Ys be Xs, and a perceptible fraction Zs, ... then we are justified in treating it as a moral certainty that some Xs are Zs' (*De Morgan 1847*, p. 225).

For De Morgan, probability was not an objective concept. In his system, it represented the amount of confidence a rational mind would have regarding the likelihood of a forthcoming event, a claim, or anything on which absolute knowledge did not exist. Writing in 1837, he said:

> What we mean, then, by an event being probable or improbable, is this; that with regard to that event the mind of the spectator is in a state of disposition either to doubt or believe its happening; which evidently depends in no way upon the event itself, but upon the whole train of previous ideas and associations which the mind of the spectator possesses upon such circumstances as he thinks similar. Therefore it is wrong to speak of any thing being probable or improbable in itself. The same thing may be *really* probable to one person and improbable to another. *De Morgan 1837a* (p. 394)

He thus equated the probability of an event with the degree of belief a reasonable person would assign to it. The choice of numerical scale used to measure this degree of belief was, he said, arbitrary. Indeed, he gave examples of the Fahrenheit temperature scale (32–212) and the real number line from −1 to 1, before adopting the conventional 0–1 scale of measurement (*De Morgan 1847*, pp. 182–183). Having thus defined p, his measure of probability of (or belief in) an event, to be between 0 and 1, he then added the further definition of *testimony*, q, to be

$$q = 2p - 1$$

so that

$$p = \frac{1}{2}(q+1) \quad (\textit{De Morgan 1846},\ 393;\ \textit{De Morgan 1847},\ 183).$$

And since $0 \leqslant p \leqslant 1$, it followed that $-1 \leqslant q \leqslant 1$. Furthermore, if $p > \frac{1}{2}$, he defined its corresponding q to represent the probability that the assertion of an individual about the event is true, sometimes calling it the *authority* value for this event.

Thus if a person assigned the probability of $\frac{2}{5}$ to an event, the testimony, $-\frac{1}{5}$, being negative would be against this belief. However, a probability of $\frac{4}{5}$ would produce a positive authority value of $\frac{3}{5}$ in favor of this belief. Obviously, an event with a 50% likelihood of happening (i.e. $p = \frac{1}{2}$) would result in no authority for the conclusion whatsoever. Using the established rules of mathematical probability, De Morgan was now able to discuss the compounding and derivation of partial beliefs via the subject of probable inference.

In his 1846 paper, he treated the problem of evaluating the truth of conflicting propositions, by means of his probabilistic system, claiming: 'Writers on logic have made no effort to apply the mathematical theory of probabilities to the balance of arguments' (*De Morgan 1846*, p. 393). One of several problems investigated was to find the respective probabilities of the truth of a conclusion and its contradiction, given various arguments for and against, and the testimony for each (*1846*, p. 398). Let

$a = P$(an argument proves its conclusion)
$b = P$(an argument proves its contradiction)
$\mu = P$(an argument for the conclusion is true[10])
$=$ testimony for the conclusion
$1-\mu = P$(an argument for the contradiction is true)
$=$ testimony for the contradiction.

If we let a be represented by m valid cases and n invalid ones, then $a = \frac{m}{m+n}$. Similarly, if we let b be represented by p valid cases and q invalid ones, then $b = \frac{p}{p+q}$. Likewise, if μ is represented by v truths and w falsehoods, then $\mu = \frac{v}{v+w}$ and $1 - \mu = \frac{w}{v+w}$. By simple combinatorics, there are mqv cases in which the argument for the conclusion is valid and true, npw cases in which the argument against is valid and the conclusion false, nqv cases in which both arguments for and against are invalid and the conclusion true, and nqw in which both are invalid and the conclusion false. Thus:

$$P(\text{the conclusion is true}) = \frac{(m+n)qv}{(m+n)qv + (p+q)nw} = \frac{(1-b)\mu}{(1-b)\mu + (1-a)(1-\mu)}$$

and

$$P(\text{the conclusion is false}) = \frac{(p+q)nw}{(m+n)qv + (p+q)nw} = \frac{(1-a)(1-\mu)}{(1-b)\mu + (1-a)(1-\mu)}$$

He was able to show that these expressions produce several logically consistent results. For example, if $a = 1$, then P(conclusion true) $= 1$ and P(conclusion false) $= 0$, and if $b = 1$, then P(conclusion true) $= 0$ and P(conclusion false) $= 1$. Also, if $a = b$, then $P(\text{true}) = \mu$ and $P(\text{false}) = 1-\mu$, i.e. if the opposing arguments are equally strong, they will have no effect on the testimony. Indeed, given that the arguments for and against the conclusion are equally strong, we should expect these resulting probabilities of truth and falsity to both equal $\frac{1}{2}$.[11]

To provide a mathematical justification of this intuition a few pages later, De Morgan introduces calculus into the discussion. Letting $d\mu$ be the probability that the testimony lies between μ and $d\mu$, he gave the corresponding chance of the conclusion being true as

10 Note that 'for the *truth* of either side, it is not essential that the argument for it should be valid, but only that the argument against it should be invalid' (*De Morgan 1847*, p. 205).

11 Note however, that if $a = b = 1$, we get the meaningless probability values of $\frac{0}{0}$. But since the probabilities represented by a and b are mutually contradictory, they can never equal 1 simultaneously.

$$\frac{(1-b)\mu \, \mathrm{d}\mu}{(1-b)\mu + (1-a)(1-\mu)}.$$

Integrating between 0 and 1 to get the full probability of the conclusion's truth gave him

$$\begin{aligned} P(\text{conclusion is true}) &= \int_0^1 \frac{(1-b)\mu \, \mathrm{d}\mu}{(1-b)\mu + (1-a)(1-\mu)} \\ &= \frac{(1-b)}{(a-b)} \left(1 - \frac{(1-a)\log\frac{(1-b)}{(1-a)}}{(a-b)} \right) \\ &= \frac{r}{r-1} \left(1 - \frac{\log r}{r-1} \right), \end{aligned}$$

where $r = \frac{1-b}{1-a}$. Letting $a \to b$ is tantamount to finding the limit as $r \to 1$ of the above probability, and as expected

$$\lim_{r\to 1} \frac{r}{r-1} \left(1 - \frac{\log r}{r-1} \right) = \frac{1}{2}.$$

In his early work on logic, De Morgan was clearly trying to show that the techniques of mathematical probability could be employed to evaluate the likelihood of logical deductions. He thus viewed probability as an area of formal logic concerned with the investigation of the rules whereby less-than-certain propositions affect the likelihood of other related statements.[12] However, as the examples given above illustrate, when it came to using probability as a tool to do logic, one would hardly claim that they *facilitated* the process. But then, that was not the intention: De Morgan was interested merely in showing that probabilistic methods *could* be used, not in refining the mathematical techniques. As Panteki has observed: 'De Morgan followed throughout his writings a conceptual approach, showing much more interest in elucidating first principles than in inventing new rigorous technical processes' (*Panteki 1992*, p. 490). And this was clearly true for his work in applying probability to logic.[13]

5. De Morgan's (mis-)use of logic in his probability

Towards the end of his career, in 1861, De Morgan contributed a number of articles to the *English Cyclopedia* including substantial entries on both logic and probability. While both are interesting pieces in their own right, the latter is particularly noteworthy because of the inclusion of the following intriguing paragraph:

> There are no questions in the whole range of applied mathematics which require such close attention, and in which it is so difficult to escape error, as those which occur in the theory of probabilities ... and, of all subjects, there is no one in which writers of every grade have so frequently or so strangely made mistakes of mere

12 Although supported by some contemporaries, such as the mathematical physicist William Donkin (*Donkin 1851*), this view was later challenged by the logician John Venn (*Venn 1962*, pp. ix, 119n, 122–123).

13 For a relevant discussion of De Morgan's ideas on probable inference, see *Hailperin 1996* (pp. 91–107).

> inadvertence. One was pointed out about twenty years ago ('Camb. Phil. Trans.') into which both Laplace and Poisson had fallen, one after the other; but the discoverer of their slip proved himself signally liable to greater ones a very little while after. ('Cab. Cyclop: Probability and Life Insurance,' p. 28.). *De Morgan 1861b* (cols. 778 – 779)

The last sentence is particularly interesting, not only because it refers to an alleged mistake committed by two of the nineteenth-century's finest mathematicians, but also because of the anonymity of 'the discoverer of their slip'. But the identity is revealed in a letter from De Morgan to his friend, the Irish mathematician, Sir William Rowan Hamilton,[14] written on 26 August 1853. In this letter, De Morgan refers to his 1838 *Essay on Probabilities*, which Hamilton was currently reading.

> I see you did not notice, in the part you have read, a thundering error, on the simplest point possible. Nor has anyone else noticed it, except myself, who have printed a sarcasm against 'a recent writer' that I may be able to prove I found it out before anybody. *Graves 1889* (p. 459)

It appears that Hamilton was similarly unable to detect De Morgan's mistake, as two weeks later the latter was writing to reveal its location: 'My great error is in page 28 of the little treatise' (*Graves 1889*, p. 461). Thus, it would seem, De Morgan had spotted a logical error committed by both Laplace and Poisson, wrote a paper revealing that error, and then soon after, committed a similar mistake in his *Essay on Probabilities*. So what was the error in question, and how did it occur?

The particular mistake to which De Morgan referred was a simple and common oversight in conditional probability. In both Laplace's *Théorie analytique des probabilités* (*Laplace 1812*, p. 279). and Poisson's *Recherches sur la probabilité des jugements* (*Poisson 1837*, p. 209), both authors 'assume in effect that the probability of the equation $\phi(x,y) = a$, where x is given and y presumed, must be the same as in the case where y is given and x presumed' (*De Morgan 1837b*, p. 424). In other words, given two events X and Y, they infer that the probability of X given Y equals the probability of Y given X. To correct this oversight, De Morgan published a short paper in the *Transactions of the Cambridge Philosophical Society* (*De Morgan 1837b*), in which he provided an analysis of the mathematics underling their mistake.

The irony was, as he in fact noticed, that not more than a year later, he committed a similar 'thundering error' himself, which he actually published in *two* sources. Moreover, the error is of the kind very often fallen into by beginners in probability. Suppose there are n objects, numbered 1, 2,. . .,n. If one is selected and replaced, then another selected and replaced, and so on, what is the probability that i selections will produce a total result of s? This question is easy to answer for the finite case, but as the number of objects becomes infinitely large, calculus is required to evaluate the probabilities. In his 1837 article for the *Encyclopedia Metropolitana*, De Morgan found the general formula for the probability to be

$$(8) \quad \frac{1}{i!}\left\{\left(\frac{s}{n}\right)^i - i\left(\frac{s}{n}-1\right)^i + \frac{i(i-1)}{2}\left(\frac{s}{n}-2\right)^i - \dots\right\}.$$

14 Not to be confused with De Morgan's great philosophical opponent, Sir William Hamilton.

He then applied it to a problem from astronomy, taken from an example given by Laplace. Taking the number of planets (excluding the earth, but including the known asteroids) to be ten,[15] he considered the inclination of the plane of each planet's orbit to that of the earth, which he called the ecliptic. He then says: 'The inclinations of these ten orbits are so small that their sum is only 91.4187° (French metrical system)[16] less than one alone might be (100°), if the orbits of the planets were the work of chance' (*De Morgan 1837a*, p. 412). Supposing the planets' orbital inclinations to be the result of pure chance, so that any angular inclination from 0 to 100° would be equally likely, he asked for the probability of finding ten planets such that their combined angular inclinations are less than 92°.

Taking $s = 92°$, $n = 100°$ and $i = 10$ in formula (8), and disregarding all but the first term, he obtained a probability of

$$\frac{1}{10!}\left(\frac{92}{100}\right)^{10} \text{ or } \cdot 00000012$$

But, he continued, if there were a *reason* for the observed inclinations (i.e. if it were not pure chance that they were so configured), the above probability should be 1. 'Consequently,' he said, 'it is 1 : ·00000012, or about ten million to one that there was a necessary cause in the formation of the solar system for the inclinations being what they are' (*De Morgan 1837a*, p. 412).

A similar discussion (albeit less mathematical and more philosophical) is contained in the first chapter of his popular *Essay on Probabilities* for actuaries. In his opening discussion of probability, he devoted a section to combating the notion that the subject promotes anti-deism. His chief illustrative example was a less technically presented consideration of the planets' orbital characteristics. He considered two planetary phenomena—their orbital inclinations, as described above; and the fact that all planets thus far discovered moved in the same direction around the sun—and tried to determine the likelihood of the existence of some creative power or design in bringing it about. 'Taking it then as certain that the preceding phenomena would have followed from design, if such had existed, ... we proceed to inquire what prospect there would have been of such a concurrence of circumstances, if a state of pure chance had been the only antecedent' (*De Morgan 1838*, p. 27).

He first addressed the question of finding the probability of 11 planets moving in the same direction around the sun. Given that there are only two ways a planet can go, this gives a probability value of $\frac{1}{2^{11}} = \frac{1}{2048}$, or 2047 to 1 against, given that the choice was purely random. Next, he turned to the probability of ten planetary orbits being inclined to the ecliptic at a combined angle of less than 92 (metric) degrees. As we saw above, if we assume that no factor but pure chance affected the outcome, that probability came to less than $\frac{1}{10{,}000{,}000}$, or ten million to 1 against. De Morgan then calculated the combined probability of both (independent) events happening together, which would be about $\frac{1}{2048} \times \frac{1}{10!}\left(\frac{92}{100}\right)^{10} \approx \cdot 000000000058$, but which he gives as 'more than 20,000,000,000 to 1 against it' (*De Morgan 1838*, p. 27).

He deduces that 'It is consequently of the same degree of probability that there has been something at work which is not chance, in the formation of the solar system' (*De*

15 Neptune was not discovered until 1846, and Pluto not until 1930.

16 Following Laplace he gives their angular measurement in French metric degrees, in which system a right angle would be 100°.

Morgan 1838, pp. 27–28). And thus: 'it is a necessary conclusion that the existence of something which combines together different and independent arrangements to produce an end which could not, *ceteris manentibus*, be produced without them, must be added to the notion of a Providence, intelligent or not, which is required in the first principles' (*De Morgan 1838*, p. 28). De Morgan thus (wrongly) concludes that it is certain that there exists (or existed) some design or cause that led to the present planetary configuration of the solar system.

In reaching this conclusion, De Morgan had actually committed *two* errors, although one was far less serious than the other. The first, more minor, inaccuracy came in his opening premises: 'Let it be granted, to fix our ideas, that we admit as proved, a proposition which has a hundred million to one in its favour' (*De Morgan 1838*, p. 26). In other words, given a proposition X, if its probability is $\frac{100{,}000{,}000}{100{,}000{,}001}$ or more, De Morgan would consider $P(X) = 1$. This, of course, has the consequence that a proposition Y with probability $\frac{1}{100{,}000{,}001}$ or less would be considered as $P(Y) = 0$. But this minor inaccuracy can, perhaps, be explained by the fact that he was writing for a lay audience. His second error, however, was much more serious.

As we saw, De Morgan begins by assuming that, given that a cause or design exists, the probability that the planets' orbits are aligned as they are would be 1. So,

$$P(\text{planets are as they are} \mid \text{design exists}) = 1.$$

He then deduces that the probability of this alignment given that no such design existed would be more than 20,000,000,000 to 1 against (or 0). So, by his opening premise,

$$P(\text{planets are as they are} \mid \text{design does not exist}) = 0.$$

Therefore, he concludes,

$$P(\text{design does not exist} \mid \text{planets are as they are}) = 0$$

and so

$$P(\text{design exists} \mid \text{planets are as they are}) = 1.$$

Thus, he has essentially committed the same logical blunder as both Laplace and Poisson by unintentionally assuming that $P(X \mid Y) = P(Y \mid X)$.[17] His logic is, in effect, that 'If a design or necessary cause existed, the planets' orbits would be aligned as they are; but the planets' orbits are aligned as they are, therefore a design or necessary cause exists.' This was De Morgan's 'thundering error'.

6. Conclusion

The publication of his *Formal Logic* in 1847 effectively marked the beginning of a new phase in De Morgan's career as a logician. In particular, the various responses to it, both positive (e.g. Boole) and negative (e.g. Hamilton) fueled his research on the subject for the next quarter of a century. The negative reaction to his first paper (*De*

17 *Hailperin 1986* (pp. 355–9) contains a discussion of a similar error.

Morgan 1846) and *Formal Logic* prompted De Morgan to adjust and develop his system, and much of his subsequent logical research (*De Morgan 1850*, *1858*, *1860a*, *1860b*, *1861a*, *1863*) was driven by the need to reply to various criticisms leveled at it by Hamilton and prominent adherents to his philosophy such as H. L. Mansel and T. S. Baynes. In fact, of his later publications on logic only one (*De Morgan 1860a*) bore no reference to the long-standing dispute with the Hamiltonian school.

Among these later works, only his paper of 1850 included any consideration of probability theory,[18] and even in this case these observations were contained merely in a final independent section. (Entitled 'Application of the theory of probabilities to some points connected with testimony' (*De Morgan 1850*, pp. 116–125), this part of the paper had nothing to do with the rest of the discussion and had in fact been written several months earlier, being originally intended as an entirely separate article (*De Morgan 1850*, 116n).) This was the last time De Morgan treated mathematical probability in a paper on logic. From this point, probability is almost entirely absent from De Morgan's work on the subject.

Perhaps because probability was missing from the majority of his logical works, and almost entirely absent from his post-1850 publications on the subject, De Morgan's use of probability in logic and his philosophy of the logical foundations of probability had little or no impact on subsequent logicians. For example, the work of his former student W. S. Jevons on logic entirely ignored the contributions of his former professor, concentrating almost exclusively on Boole's approach (*Grattan-Guinness 2000*, pp. 56–60, 62n). Indeed, only John Venn, in his *Logic of chance* (*1866*), made any detailed reference to the intersection of De Morgan's logic and probability, and here his motivation was to undermine the subjective view of probability that De Morgan had advocated (*Venn 1962*, p. ix, 119n, 122–123). With the swift eclipse of his logical system by those of Boole and his successors, De Morgan's work in this area was quickly forgotten.[19]

Why then did De Morgan make no further use of probability in his logical research? This is an interesting, but difficult, question to answer. As Maria Panteki has observed when considering the evolution of his logical system, there is very little evidence to explain what she describes as 'the slow and peculiar evolution of his reasoning' (*Panteki 1992*, p. 428). The most obvious rationalisation for the absence of probability in his later logical work seems to be that his attention was simply diverted to other issues. Most noticeably, since the majority of his work after 1847 was motivated by responses to critiques by Hamilton, Mansel and others, his treatment of the subject was pushed in a far more abstract and philosophical direction than his initial, more mathematical, forays. As a result, during the 1850s, De Morgan's logic became less arithmetical in character, evincing far more concern with abstract general propositions than specific calculations.

In fact, having devoted so much effort in his early work to dealing with syllogisms involving probabilities and numerically definite propositions, it might seem ironic that De Morgan rarely treated them again after 1850. But in his *Syllabus of a Proposed System of Logic* (1860), his second (and final) book on logic, he offered what was, perhaps, a reason for this apparent disregard of an issue that had previously been so central to his work, saying that 'Syllogisms with numerically definite quantity rarely

18 *De Morgan 1860b* (pp. 67–72) did contain some paragraphs on probability towards the end. However, since it was intended merely as an overview of his work on the subject, no new material is presented.

19 The most recent work I have found which refers to De Morgan as a probabilist is *Keynes 1921*.

occur, if ever, in common thought' (*De Morgan 1860b*, p. 29).[20] Thus, although he still considered the examples he had given of numerical and probabilistic reasoning to be perfectly valid in theory, he came to the pragmatic realization that they were never likely to be useful in practice.

Another possible reason why he abandoned the use of probability in his logic lies in the inherent difficulty and complexity of the subject, of which he was well aware and, as we have seen, to which he was not immune. Indeed, the above-mentioned error in his probabilistic reasoning was not a unique occurrence.[21] In the early 1850s, he was engaged in a lively correspondence with Boole on the subject, and Boole posed him a complex question involving the deduction of the probability of an event E from given information about the probabilities of n independent or dependent events E_1, $E_2,\ldots,E_n$. Although De Morgan's letters from this period have not survived, it is obvious from Boole's replies that his attempted solutions were incorrect. As Boole said in one letter from August 1851: 'I am also obliged to you for your solution of the question in probabilities which however I think erroneous' (*Smith 1982*, p. 50). Boole goes on to give a counterexample to show that De Morgan's solution would give a meaningless probability of $1\frac{1}{3}$ (*Smith 1982*, p. 51). Perhaps his recognition of the practical difficulties involved in using probability in logical reasoning, combined with the comparative ease of making mistakes, resulted in De Morgan's abandonment of the subject in his later work.

Boole's own interest in probability also stemmed from his work in logic (*Hailperin 1986*), and seems to have arisen in the late 1840s presumably, in part, from a perusal of De Morgan's work on the subject. In a letter of 1851, he even complimented De Morgan that 'I have nowhere seen the fundamental positions of the theory better stated than in your little book on probabilities [*De Morgan 1838*] into which I looked for the first time the other day, and in a paper by Mr Donkin [*Donkin 1851*]' (*Smith 1982*, p. 52). Although Boole's initial work on logic contained very little probability, a substantial proportion (no less than 150 pages) of his monumental 1854 work, *An Investigation of the Laws of Thought* (*Boole 1854*, pp. 243–398), is devoted to the subject, and his interest continued throughout the 1850s and early 1860s.

In contrast, however, De Morgan's interest in probability seemed to wane throughout the 1850s. We see from the published Boole–De Morgan correspondence that, although the two men were working in similar areas, their precise interests rarely coincided exactly, and it is clear that by 1851 Boole was more focused on using probability in logic than De Morgan. It may even have been the case that, as illustrated by his inability to satisfactorily answer Boole's question, De Morgan may have come to believe the superiority of Boole's work to his own on this matter. After all, he expressed his admiration of the quality of Boole's logical work on several occasions.[22]

20 *De Morgan 1858* also contained the following: 'There is no use in the arithmetically definite syllogism: to this there is almost unanimous agreement. ... [Q]uantification of the predicate is a superfluity, an excrescence which disappears in the elaboration of rules of inference...' (*De Morgan 1858*, p. 206)

21 Another error is pointed out by Isaac Todhunter in his *History of the Mathematical Theory of Probability from the Time of Pascal to that of Laplace* (*Todhunter 1865*, p. 557).

22 In an 1861 encyclopedia article on logic, De Morgan expressed the opinion that 'Mr Boole's generalization of the forms of logic is by far the boldest and most original of those of which we have to treat' (*De Morgan 1966*, p. 255). Writing shortly after Boole's death in 1864, he commented that of the few 'common cultivators' of logic and mathematics, 'Dr Boole has produced by far the most striking results' (*Smith 1982*, p. 119).

In contrast, he believed his own methods to 'have nothing in common with that of Professor Boole, whose mode of treating the forms of logic is most worthy the attention of all who can study that science mathematically, and is sure to occupy a prominent place in its ultimate system' (*De Morgan 1850*, p. 79) In her analysis of De Morgan's research on the links between logic and algebra, Maria Panteki may just as well have been describing his work on logic and probability when she remarked: 'Apparently, De Morgan had perceived a further proximity between the two sciences than that discussed in [*De Morgan 1850*], but he must have felt unable to cope with it due to the complexity of the task and his poor technical machinery' (*Panteki 1992*, p. 490). It is plausible therefore that, being aware of the limitations of his own abilities, De Morgan chose to concentrate on those aspects of logic with which he felt comfortable, rather than those in which he was more likely to encounter difficulties and error, such as probability.

Perhaps the most succinct answer to the question we have raised can be gleaned from a simple remark made by De Morgan himself in 1853. Contained in a letter to Hamilton, it encapsulates his attitude to probability at the time and, read in the light of the above discussion, seems to give a reasonable explanation for his later neglect of the subject: 'Everybody makes errors in probabilities, at times, and big ones' (*Graves 1889*, p. 459).

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References

Boole, G. 1847. *The Mathematical Analysis of Logic*, Cambridge: Macmillan.
Boole, G. 1854. *An Investigation of the Laws of Thought*, London: Walton and Maberly.
Boole, G. 1868. 'On propositions numerically definite', *Transactions of the Cambridge Philosophical Society* **11**, 396–411.
De Morgan, A. 1837a. 'Theory of probabilities', *Encyclopedia Metropolitana*, London: Baldwin and Cradock, vol. 2, pp. 393–490.
De Morgan, A. 1837b. 'On a question in the theory of probabilities', *Transactions of the Cambridge Philosophical Society* **6**, 423–30.
De Morgan, A. 1837c. Anonymous review of *Théorie analytique des probabilités, par M. Le Marquis de Laplace*, 3ème edn, 1820, *Dublin Review* **3**, 237–48, 338–54.
De Morgan, A. 1838. *An Essay on Probabilities, and on their Application to Life Contingencies and Insurance Offices*, London: Longman, Orme, Brown, Green & Longmans, and John Taylor.
De Morgan, A. 1839. *First Notions of Logic*, London: Taylor and Walton.
De Morgan, A. 1846. 'On the structure of the syllogism, and on the application of the theory of probabilities to questions of argument and authority', *Transactions of the Cambridge Philosophical Society* **8**, 379–408.
De Morgan, A. 1847. *Formal Logic: or, the Calculus of Inference, Necessary and Probable*, London: Taylor and Walton.
De Morgan, A. 1850. 'On the symbols of logic, the theory of the syllogism, and in particular of the copula, and the application of the theory of probabilities to some questions of evidence', *Transactions of the Cambridge Philosophical Society* **9**, 79–127.
De Morgan, A. 1858. 'On the syllogism III, and on logic in general', *Transactions of the Cambridge Philosophical Society* **10**, 173–230.
De Morgan, A. 1860a. 'On the syllogism IV, and on the logic of relations', *Transactions of the Cambridge Philosophical Society* **10**, 331–58.

De Morgan, A. 1860b. *Syllabus of a Proposed System of Logic*, London: Walton and Maberly.
De Morgan, A. 1861a. 'Logic', *English Cyclopedia* vol. 5: cols 340 – 54.
De Morgan, A. 1861b. 'Probability, Theory of', *English Cyclopedia* vol. 6: cols 769 – 80.
De Morgan, A. 1861c. 'On the theory of errors of observation', *Transactions of the Cambridge Philosophical Society* **10**, 409 – 27.
De Morgan, A. 1863. 'On the syllogism V, and on various points of the onymatic system', *Transactions of the Cambridge Philosophical Society* **10**, 428 – 488.
De Morgan, A. 1868. Review of J. M. Wilson's *Elementary Geometry*, *The Athenæum*, No. 2125, (18 July 1868), pp. 71 – 3.
De Morgan, A. 1966. *On the Syllogism, and Other Logical Writings*, ed. Peter Heath, London: Routledge and Kegan Paul.
De Morgan, S. E. 1882. *Memoir of Augustus De Morgan*, London: Longmans, Green, and Co.
Donkin, W. F. 1851. 'On certain questions relating to the theory of probabilities', *Philosophical Magazine* (4) **1**, 353 – 68, 458 – 66.
Grattan-Guinness, I. 2000. *The Search for Mathematical Roots, 1870 – 1940: Logics, Set Theories and the Foundations of Mathematics from Cantor through Russell to Gödel*, Princeton: Princeton University Press.
Graves, R. P. 1889. *Life of Sir William Rowan Hamilton*, Vol. 3, Dublin: Hodges, Figgis, & Co.
Hailperin, T. 1986. *Boole's Logic and Probability*, 2nd edn, Amsterdam: North Holland.
Hailperin, T. 1996. *Sentential Probability Logic*, Bethlehem: Lehigh University Press.
Hawkins, B. S. 1979. 'A reassessment of Augustus De Morgan's logic of relations: a documentary reconstruction', *International Logic Review* **19 – 20**, 32 – 61.
Heath, P. 1966. 'Introduction', in De Morgan 1966, pp. vii – xxxi.
Keynes, J. M. 1921. *A Treatise on Probability*, London: Macmillan & Co.
Kneale, W. and Kneale, M. 1962. *The Development of Logic*, Oxford: Oxford University Press.
Laita, L. M. 1979. 'Influences on Boole's logic: the controversy between William Hamilton and Augustus De Morgan', *Annals of science* **36**, 45 – 65.
Laplace, P. S. 1812. *Théorie analytique des probabilités*, Paris: Mme Ve. Courcier.
Lewis, C. I. 1918. *A Survey of Symbolic Logic*, Berkeley: University of California Press.
Merrill, D. D. 1990. *Augustus De Morgan and the Logic of Relations*, Dordrecht: Kluwer Academic.
Panteki, M. 1992. 'Relationships between algebra, differential equations and logic in England: 1800— 1860', PhD thesis, Middlesex University.
Peirce, C. S. 1931. *Collected papers of Charles Sanders Peirce*. *Vol. 1: Principles of philosophy*, ed. Charles Hartshorne and Paul Weiss, Cambridge, MA: Harvard University Press.
Poisson, S. D. 1837. *Recherches sur la probabilité des jugements en matière criminelle et en matière civile: précédées des règles générales du calcul des probabilités*, Paris: Bachelier.
Smith, G. C. 1982. *The Boole – De Morgan Correspondence 1842 – 1864*, Oxford: Clarendon Press.
Stigler, S. M. 1999. *Statistics on the Table*, Cambridge, MA: Harvard University Press.
Todhunter, I. 1865. *A History of the Mathematical Theory of Probability from the Time of Pascal to that of Laplace*, Cambridge and London: Macmillan.
Venn, J. 1962. *The Logic of Chance*, 4th edn, New York: Chelsea.