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The Boole – De Morgan Correspondence 1842 – 1864

G. C. Smith

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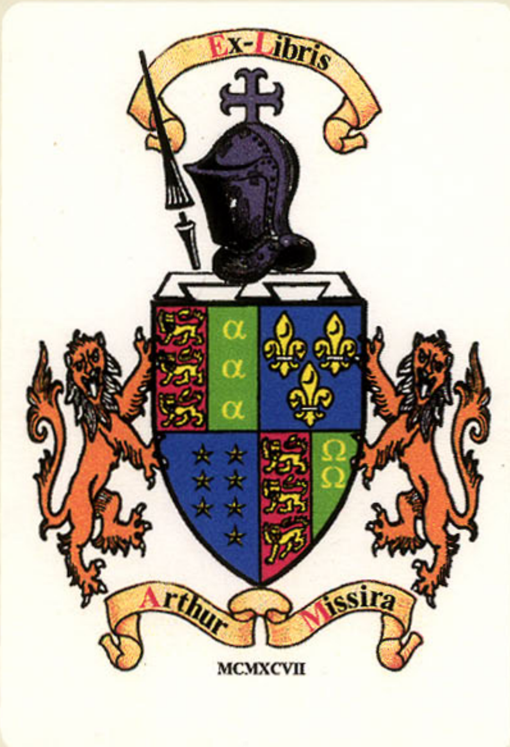
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THE BOOLE-DE MORGAN CORRESPONDENCE 1842-1864

G. C. SMITH
Monash University

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Nothing gives so just an idea of an age
as genuine letters; nay history waits for
its last seal from them.

Horace Walpole to
Sir David Dalrymple,
30 November 1761

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The first section of the bibliography includes detailed citation of the particular manuscripts in these libraries that I have quoted. I also wish to express my thanks to the staff of these libraries for facilitating the study and copying of the documents.

Many persons have helped in various ways with information and advice and to them I offer collective thanks. A few have given me a great deal of assistance and these I thank individually: Ivor Grattan-Guinness, John Crossley, and Anne-Marie Vandenberg.

G. C. S.

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INTRODUCTION

George Boole and Augustus De Morgan carried on a correspondence over a period of some 22 years. About 90 of the letters between them have survived.[1]

The major interest in the correspondence must be the exchange of ideas on logical matters, because Boole and, to a lesser extent, De Morgan were innovators in this field. But the letters also show their interest in other mathematical matters: differential equations and probability being referred to not infrequently. The letters also contain comments on a wide range of social, literary, political, and religious matters. Nor are they devoid of purely personal interest: one might instance Boole's initial unhappiness in Cork, his request to De Morgan to let him know if there was any suitable post in England, and De Morgan's letter following his recovery from pleurisy. Although in the main their correspondence was conducted for serious reasons – to make requests for information or to answer such requests – some letters are lighter in tone. De Morgan has an incisive style and, when appropriate, shows a pleasing sense of humour: while Boole's rather solemn style in his letters of the earlier years becomes increasingly more relaxed as the years go by. Of the ninety letters Boole wrote sixty-six. However, while Boole's letters are with few exceptions quite short, a few of De Morgan's extend over 8 or more sides of notepaper. In consequence the balance is not quite so heavily in Boole's favour as the numbers suggest. Nevertheless it seems to me that the correspondence gives a more balanced and comprehensive view of Boole and his work and personality than it does of De Morgan and his.

It would not be appropriate to include extended biographies of Boole and De Morgan here. It is unfortunately the case that there has never been a full-scale biography of Boole, and the biography of De Morgan written by his wife is disappointing.[2] However, it may be helpful to record the salient facts of the lives of Boole and De Morgan and to give some indication of their work, as this will help the reader to understand the correspondence.

George Boole

Boole was born in humble circumstances in Lincoln on 2 November 1815. His father, John Boole, was a cobbler who had a studious nature. John Boole's interests lay in mathematics and astronomy; he constructed optical instruments; he was also active in the Mechanics' Institute. George was the eldest child of the family. There was a sister, Mary Ann, born in 1818, and brothers William, born in 1819, and Charles, born in 1821.

George Boole attended a primary school of the National Society and, later, learned commercial subjects at, presumably, a private school of some kind. In addition he extended his education by taking lessons in Latin from a bookseller who was a friend of the family. Subsequently he taught himself Greek, French, German, and Italian.

At the age of sixteen Boole became an usher (i.e. assistant master) at a school in Doncaster – a town about 60 km from Lincoln. Within two years he returned to work as a teacher either in Lincoln or in the nearby village of Waddington until he moved to Cork in 1849. During this period Boole continued his study of languages as well as commencing the study of theology with the intention of entering the Church. He seems to have begun serious study of mathematics fairly soon after beginning to teach; in 1832 he was reading Lacroix's *Calcul Différentiel.* Later he read and learned from Poisson's *Traité de Mécanique* and Lagrange's *Calcul des Fonctions.*

His reading, and investigations based upon his reading, led to his writing papers which were published in the *Cambridge Mathematical Journal* from 1841. Thereby he became known to mathematicians in Cambridge and to a wider circle following the publication of a large-scale paper on operational methods in the *Philosophical Transactions of the Royal Society* in 1844. For this work he received a Royal Medal of the Royal Society. In 1847 his seminal work, *The Mathematical Analysis of Logic*, appeared. In 1849 he was appointed Professor of Mathematics at Queen's College, Cork, where he remained until his death in 1864. While there, Boole met Mary Everest (1832–1916) whom he married in 1856; they had five daughters.

Topics discussed in Boole's early papers included linear transformations (Boole was one of the originators of the theory of invariants); differential equations, generally solved by an operational method; the evaluation of definite and multiple integrals; and the calculus of variations.

After the publication of *The Mathematical Analysis of Logic* in 1847, Boole's papers show an increasing interest in probability. Papers on probability predominate in the years 1851–7, although Boole continued to publish on the topics mentioned in the previous paragraph. After the major paper in 1857 relating to the application of probabilities to 'the question of the combination of testimonies or judgements' (Boole 1857*b*) he published only two papers on probabilities.

The years 1859 and 1860 saw the publication of his texts on Differential Equations (1859) and Finite Differences (1860); the work involved in preparing these appears to have taken much of his time in the years 1858–60 as no papers were published in these years.

In the last years of Boole's life, 1862–4, his research activity seems to have returned to differential equations – indeed, apart from the two papers on probabilities mentioned above, all of Boole's papers in these years deal with differential equations.

Boole died at the early age of 49 from pneumonia, the result of being caught in an autumn shower. In spite of his life being spent out of the mainstream of mathematical activity, Boole made a strong impression upon his contemporaries. For example, Todhunter, writing in the introduction to his *History of Probability*, refers to the interest shown by Boole in this work:

> ... by one prematurely lost to science, whose mathematical and metaphysical genius, attested by his marvellous work on the *Laws of Thought*, led him naturally and rightfully in that direction which Pascal and Leibnitz had marked with unfailing lustre of their approbation; and who by his rare ability, his wide attainments, and his attractive character, gained the affection and the reverence of all who knew him. (Todhunter 1865, v)

Primary sources of information on Boole's life include: M.E. Boole 1878 (articles by Boole's wife); Harley 1866; Rhees 1955 (which gives comments on Boole by contemporaries and pupils). An obiturary notice by John Ryall (a colleague at Cork) appeared in the *Illustrated London News* of 21 January 1865, 59–61. The most useful modern accounts of Boole seem to be Kneale 1948; Taylor 1956 (Taylor is a grandson of Boole).

Augustus De Morgan

De Morgan was born in 1806, the son of an officer in the service of the East India Company. After attending schools of little note in the west of England, he entered Trinity College, Cambridge in 1823 and graduated B.A. in 1827. In 1828, when only 22 years old, he was appointed Professor of Mathematics at the newly-founded London University – which institution was renamed University College London in 1836. Apart from the period 1831–5, he occupied this post until his resignation in 1866. In 1837 De Morgan married Sophia Elizabeth Frend and they had three sons and two daughters. He died in 1871.

De Morgan's interests and activities were extremely wide. During his undergraduate years at Cambridge he was one of the group which became known as the 'Cambridge Analytical School', whose aim it was to free the teaching of the calculus at Cambridge from the out-dated fluxion terminology of Newton. His participation in the work of this group is the earliest indication of a continuing interest in education; indeed, this is, perhaps, the unifying feature of his widely spread interests. He wrote on many aspects of education, not only on mathematical education; examples of the diversity of this interest include works on the education of the deaf and dumb, and on the École Polytechnique. His most important contribution to mathematical education was, perhaps, the series of mathematical texts he wrote – on arithmetic, algebra, trigonometry, and a large-scale text on the calculus. Several of these appeared under the auspices of the *Society for the Diffusion of Useful Knowledge* – the activites of which was another of his interests. He contributed many articles to the Society's *Penny Cyclopaedia*.

De Morgan served on the council of the Astronomical Society for many years and took the chair at the meeting at which the London Mathematical Society was founded. He refused to be considered for a fellowship of the Royal Society and for the award of an honorary degree by Edinburgh University.

Further interests included the advocacy of decimal currency, work as an actuarial consultant, and book collecting. The last of these led to his very thorough works of bibliography.

Now I turn to what may be considered his more important contributions to scholarship. The best known of these are his contributions to the advancement of logic. They began with an elementary text, *First Notions of Logic* (De Morgan 1839*a*), but it was not until 1846 that De Morgan began what was to be a life-long series of papers and books in which he made notable advances in the ideas and the symbolic representation of logic. These advances mostly centre around generalizing the traditional theory of syllogistic reasoning. If their importance in later times seems small, it is because Boole's entirely novel ideas resulted in logic taking a quite new direction. The more substantial mathematical papers De Morgan wrote are now almost wholly unknown; certainly he made no lasting original contribution to mathematics, but his papers show an awareness of difficulties and a generally sound critical approach to the received methods. In particular, his papers on infinite series show him feeling his way towards the ideas of a theory of divergent series, while those on the Foundation of Algebra show him taking an approach which comes near to that of abstract algebra – an approach which deduces algebraic theorems from a set of axioms and which was not made explicit until 50 years later.

Primary sources of information on De Morgan's life include: S.E. De Morgan 1882 (his wife's biography of De Morgan); also the obituary notices in the *Athenaeum*, vol. 50, 25 March 1871, 369–70; and in the *Monthly Notices* of the Royal Astronomical Society, February 1872, 112–18. A useful modern account of De Morgan is given in Crowther 1968; and P. Heath's introduction to De Morgan 1966, is helpful on De Morgan's work rather than for biographical information. Rouse Ball 1915, although brief, is interesting.

Editing

Rather than presenting the matter of the letters according to the various topics their authors consider, I have chosen to stick to a chronological arrangement. This has the advantages of maintaining the integrity of each letter and of showing the developing relationship between Boole and De Morgan. The division of the ninety letters into seven Chapters created no problems as breaks in the correspondence nearly always allowed this to be done naturally. It happens that the chapters often show a predominating theme. Each chapter begins with a brief introduction which mentions some of the matter of the letters of more general interest. Most chapters are then subdivided into a number of parts. Each part is preceded by a commentary which deals with the particular matters raised in the following letters. This leaves the letters themselves largely free from intrusive comments. However on a few occasions a comment or translation has been inserted (in square brackets) in the text of a letter when this seems the most appropriate place for it. Minor individual points requiring elucidation are dealt with in footnotes. In one case (in letter 10) a theorem is announced by Boole which needs rather more extensive explanation; I have placed this in an Appendix.

The transcriptions have been only slightly edited; misspellings and failure of syntax have generally been left uncorrected and a few marks of punctuation have been supplied where wanting. I have expanded some of the contractions to improve the ease of reading; an unusual contraction used by Boole, '$=^{\text{ns}}$' for 'equations', I have rendered in full. Initial and terminal greetings have been omitted throughout. The dates have been given in a uniform fashion rather than as the writers gave them and addresses have been omitted from the transcripts.[3]

I have inserted in the text of the letters a reference to each indentifiable book or paper mentioned: these references have the form B 1848*a* or D 1849*b*, which indicate items *a*, *b* under the years 1848, 1849 of the Boole, De Morgan entries in the bibliography, respectively. For persons other than Boole or De Morgan the reference is of the form Laplace 1812*a*, or simply as 1812*a* when this shortened form is sufficiently clear.

Over one hundred persons are mentioned in the correspondence. I have included some information on their lives and activities in the biographical notes.

Parts of a few of these letters have been published before in A. De Morgan, *On the Syllogism and Other Logical Writings* (1966); the editor, P. Heath, quotes substantial portions of the letters of 16 November 1861 and 21 November 1861, together with briefer extracts from the letters of 13 July 1860 and 20 September 1862.

Notes

[1] Further information on the letters with their location may be found in the bibliography.
[2] S.E. De Morgan 1882 (see the Bibliography for the full citation of references).
[3] Up to September 1849 Boole merely indicates his address as 'Lincoln'. From November 1849 to June 1859, in general, he writes from 'Cork' or 'Queen's College, Cork'. Then from June 1859 'Blackrock' or 'Blackrock near Cork'. Exceptions occur when he is travelling, usually in the summer months; these are indicated in the notes.

De Morgan lived in London and had four addresses in the period of the correspondence. Up to February 1845 he wrote from 69 Gower St. From that date to June 1860 his address was 7 Camden St. Then he moved to 41 Chalcot Villas, Adelaide Rd., N.W. From September 1862 the Board of Works ordered that this address in future be known as 91 Adelaide Rd., N.W.

1
GETTING ACQUAINTED: 1842-1845

The years 1842–5 constituted the period when Boole's reputation was established. He had made contact with D.F. Gregory in Cambridge in 1839, and in 1840 four of his papers (Boole 1840 *a*, *b*, *c*, *d*) appeared in the *Cambridge Mathematical Journal.* In these years De Morgan had settled in the post of Professor of Mathematics at University College London which he held for most of his career.

According to De Morgan's wife, 'George Boole... had introduced himself in the year 1842 to Mr De Morgan by a letter on the *Differential and Integral Calculus*...' (S.E. De Morgan 1882, 165). The first letter of the correspondence is De Morgan's reply to a letter from Boole which, as far as I am aware, has not survived; this reply concerns difference equations, and refers to Laplace's *Théorie Analytique des Probabilités* (Laplace 1812). In De Morgan's *Differential and Integral Calculus* (1842*a*, 736–66) there is a discussion of difference equations, and in particular a reference to Laplace: 'Such equations as the preceding occur in the theory of probabilities and Laplace treated them by the method of generating functions...' (1842*a*, 748). Thus it seems likely that Letter 1 is De Morgan's reply to the letter with which Boole opened their correspondence.

Letters 2 to 8, written between June 1843 and February 1845, all concern the work that resulted in the publication of Boole's major paper *On a General Method in Analysis* (Boole 1844*a*). Boole sends De Morgan an outline of its contents in Letter 2, and asks advice on a suitable place for its publication. Although expressing some modest doubts 'if only it possesses sufficient importance', Boole is clearly concerned that the length of the paper may provide a barrier to its acceptance, but says it would 'be very injudicious to divide it'. After receiving a manuscript version of it De Morgan says he 'has read through your paper with great satisfaction' in Letter 3 and in this letter as well as in Letter 5 proceeds to give Boole detailed advice on the preparation of the manuscript, and suggests that it should be offered to the Royal Society for publication in the *Philosophical Transactions.* The Royal Society's acceptance of the paper is recorded in Letter 6 of 28 June 1844. This paper resulted in Boole's being awarded one of the two Royal Society's Royal Medals of 1844; this award is not mentioned until considerably later in the correspondence (see Letter 37 of August 1851).

Letter 9, the last letter of this chapter, is the first of many in which Boole or De Morgan acknowledge the receipt of a paper and comment on the ideas contained therein. This beginning of a continuing exchange of thoughts on their current work indicates that the relationship is changing from one in which the relatively inexperienced Boole is deferring to the better established De Morgan, to one in which the participants treat each other on a more equal basis.

The growing regard between Boole and De Morgan is indicated by the change in the style of initial and terminal greetings of the early letters. In letters 1 and 2 they address each other as 'Sir'; in letters 3 and 4 as 'Dear Sir'; in all subsequent letters they commence 'My dear Sir'. There is a similar growth in the warmth of their terminal greetings: De Morgan uses 'Yours faithfully', 'Yours truly', 'I remain, Dear Sir, yours faithfully', 'Yours very truly', in letters 1, 3, 5, and 8 respectively. While Boole, more humbly, writes 'Your obedient servant', 'Your faithful and obliged servant', 'I remain your faithful servant', in letters 2, 4, and 6 respectively, but changes to 'Believe me to remain, My dear Sir, Yours sincerely' in 7 and 'Believe me, My dear Sir, Yours faithfully', in 9.

As the correspondence proceeds both eventually terminate their letters with 'Yours very truly' or some similar phrase.

Letters 1–9

The mathematical subjects raised in the letters of this chapter are mainly concerned with differential and difference equations. These related topics were one of the main areas of Boole's early research activity. Other topics mentioned in connection with De Morgan's published papers (1844*f*, 1844*b*) concern continued fractions and triple algebra.

In Letter 1 (29 Dec. 1842) De Morgan, quoting Laplace, refers to the difference equation

$$ay_x + by_{x+1} + \ldots + x(a'y_x + b'y_{x+1} + \ldots) + x^2(a''y_x + b''y_{x+1} + \ldots) + \ldots = 0.$$

In this equation x denotes an integral variable – today one would be more likely to write such an equation in the form

$$A_n y_n + B_n y_{n+1} + C_n y_{n+2} + \ldots = 0,$$

using $A_n, B_n, C_n, \ldots$ to denote polynomials in n. On the page that De Morgan referred to Laplace commented: 'L'équation différentielle précédente n'est intégrable généralement que dans le cas où elle est du premier ordre, et alors les coefficients de l'équation aux différences finies en y_x ne renferment que la première puissance de x;. . .' (Laplace 1812, third edition, 83). Thus here we find Boole taking up a result which Laplace has stated in a general form, but which he is able to solve only for quite special cases. The technique of solving these equations described by Laplace as 'integration by generating functions' is closely allied to the (later) Laplace transform method of solving differential equations, and the work of Laplace under discussion (Laplace 1812) is one of the sources of the Laplace transform. These ideas appear in Boole's major paper *On a General Method of Analysis* (Boole 1844*a*), in Section D, pages 261–70, where we find that Boole has, as De Morgan commented in Letter 1, 'extend[ed the method] to a corresponding equation with two or more variables'.

In Letter 2 (19 June 1843) Boole gave De Morgan an outline of his ideas for *On a General Method of Analysis* (Boole 1844*a*). When this paper, a long and detailed one, finally appeared Boole had made considerable changes – he remarked in Letter 7 (15 Jan. 1845): 'the paper is so much altered and the applications so much extended that you will scarcely recognise it'. However, a comparison of the summary with the published paper shows that the overall structure of the paper was not changed; for example, the 'Inverse Applications' of the summary correspond to Section D of the published paper. One change that Boole did make was in the illustrative example given in the second section of the summary where he gave the solution of the differential equation

$$x^2\frac{\mathrm{d}^2u}{\mathrm{d}x^2}+x\frac{\mathrm{d}u}{\mathrm{d}x}+(n^2+x)u = 0.$$

In the published paper we find (Boole 1844*a*, 239) that he has changed this to

$$x^2\frac{\mathrm{d}^2u}{\mathrm{d}x^2}+x\frac{\mathrm{d}u}{\mathrm{d}x}+(n^2+x^2)u = 0.$$

However, the case of (1) in which $n = 0$ does appear in the paper (Boole 1844*a*, 237); the substitution $z = \log x$ reduces the equation to a Hill equation.[1]

In Letter 5 (11 Dec. 1843) De Morgan's remarks relate mainly to notation, but concern mathematics in so far as his notational preoccupation is here connected with the operational approach to the definition of the fundamental notions of the calculus. Although one approves of his stressing the importance of making the notation quite clear, it is difficult to make much sense of his use of the operational symbols as part of an argument supporting the case for carefully defined notations. His first remarks '$\phi' x$ is a specific [i.e. limiting?] case of

$$\psi(x,\Delta x) \quad \text{or} \quad \frac{\phi(x+\Delta x)-\phi x}{\Delta x}$$

and would be better understood [why?] by a symbol like $\mathrm{D}_{\Delta x}.\phi x$ than by $\mathrm{D}_x\phi x$' illustrate the common early 19th century British style of analysis – which was already made obsolete by Cauchy's limit-based development of the calculus. De Morgan was not unaware of this work, but could not divest himself of the older ideas.

In Letter 6 (28 June 1844) Boole expresses his interest in a paper of De Morgan's on continued fractions (1844*f*) and remarks that he had 'tried unsuccessfully' to relate them to the theory of generating functions, but thought a connection with linear functional equations was more likely.

De Morgan's paper concerns the calculation of the numerators and denominators of the approximations to an infinite continued fraction. Another paper by De Morgan on continued fractions appeared in the *Philosophical Magazine* (1844*a*). This concerns the reduction of a continued fraction

$$\frac{a}{1+}\,\frac{bx}{1+}\,\frac{cx}{1+}, \text{etc.}$$

to a power series $\Sigma_0^\infty P_i x^i$. De Morgan shows how one may conveniently find the coefficients P_i in terms of the constants $a, b, c \ldots$.

In Letter 9 (24 Feb. 1845) Boole indicated his interest in De Morgan's paper on Triple Algebra. This is the fourth of a series of four papers with the general title *On the Foundation of Algebra* (De Morgan 1839*b*, 1841*a*, 1843*c*, 1844*b*). Influenced by W.R. Hamilton's recent paper on quaternions (W.R. Hamilton 1844*a*) De Morgan investigated triples of the form $a\xi + b\eta + c\zeta$ with the object of defining operations of addition and multiplication under which they, like quaternions, will conform as nearly as possible to the usual algebraic laws, i.e. the field laws. For addition and subtraction the 'ordinary laws' were assumed. If, further, the commutative and associative laws of multiplication and the distributive law are assumed, and ξ is identified with the unit of arithmetic, viz. 1, then the structure of the system depends upon the products η^2, ζ^2, and $\eta\zeta$. De Morgan tries a number of possibilities, but recognizes that cases of the failure of the associative law arise. He also examines the possible forms of the 'modulus of multiplication' – his term for the three-dimensional analogue of $\sqrt{(a^2+b^2)}$ in respect of $a + ib$. He realizes, as did W.R. Hamilton, that $\sqrt{(a^2+b^2+c^2)}$ will not serve; with one exception De Morgan's examples yield square roots of a non-definite form in a, b, c for the modulus of multiplication. Thus even had the associative law of multiplication not failed, he could not achieve a field or even a division ring. At one point De Morgan exclaims (despairingly?) 'I am not able to present any striking geometrical interpretations' (1844*b*, 147).[2]

1. DE MORGAN TO BOOLE, 29 DEC. 1842

In reply to your's of the 23rd I have looked into Laplace (Théorie des Probab.) [1812] and cannot find that he actually integrates by generating functions any equation with variable coefficients. He shows p. 82 how to finding the generating function of y_x when

$$ay_x + by_{x+1} + \ldots + x(a'y_x + b'y_{x+1} + \ldots)$$
$$+ x^2(a''y_x + b''y_{x+1} + \ldots) + \ldots = 0$$

and his method would easily extend to a corresponding equation with two or more variables. But, as far as I know, anything like an organized mode of obtaining the generating function when the coefficients of the equation are variable, must be new.

Laplace's problems do not require any variable coefficients: that is, he does not treat any cases in which the chance of winning the xth game is a function of the chance of winning preceding games, *and of* x.

From your account of your method, I expect to be much interested by it and hope you will soon be able to complete and publish it.

2. BOOLE TO DE MORGAN, 19 JUNE 1843

Some months ago I took the liberty of troubling you for a reference to Laplace. In your reply for which it still remains to me to thank you, you were pleased to express an interest in the subject of investigation alluded to in my letter. I have now drawn up a paper embodying the principal results of the inquiry which I have had some thoughts of laying before the Royal Society. Before taking a step of this nature I am however anxious to have the opinion of a more competent judge as to its propriety. Knowing that you have written much on kindred subjects, shall I presume too far on your courtesy in applying to you a second time? The following is a brief analysis of the contents of the paper.

Direct Applications

Section 1st. Fundamental Theorem, being a general relation connecting any linear differential equation or system of linear differential equations with a corresponding equation or system of equations in finite differences.

2nd. Solution of linear and linear partial differential equations in series. Systematic theory of such solutions has I believe never before been given, the received method not providing for cases of exception & failure. Thus in the equation,

$$x^2 \frac{d^2u}{dx^2} + x\frac{du}{dx} + (n^2 + x)u = 0$$

of which the solution is

$$u = \cos(n \log x)(a_0 + a_1x + a_2x^2 + \text{etc.})$$
$$+ \sin(n \log x)(b_0 + b_1x + b_2x^2 + \text{etc.})$$

a_0, b_0 being arbitrary and in general

$$a_m = -\frac{ma_{m-1} - 2nb_{m-1}}{m(m^2 + 4n^2)}, \; b_m = \text{etc.}$$

the received theory fails unless the existence of the factors $\cos(n \log x)$, $\sin(n \log x)$ be assumed. The same method would also fail as respects equations involving a second number X not devlopable [sic] in ascending powers of x. Every difficulty which can occur is provided for in the theory which I have in this section given.

3rd. Finite solution of linear and linear partial differential equations. The theory here developed is I believe the only one in which anything approaching to a classification of the integrable forms of linear differential equations is possible – the coefficients being rational functions of the independent variables. So far as I can judge the method applies without exception to all cases in which finite integration is possible. The form under which the linear differential equation is treated throughout the paper is

$$u + \phi_1 \left(\frac{\mathrm{d}}{\mathrm{d}\theta}\right) \epsilon^{\theta} u + \phi_2 \left(\frac{\mathrm{d}}{\mathrm{d}\theta}\right) \epsilon^{2\theta} u + \text{etc.} = U.$$

It is on this equation that the reductions are effected and not on the scale as in Mr Ellis's mode – the reductions are always effected at a single step & not successively & in general by symbols of differentiation and not of integration, some few cases excepted.[3] This section also contains an extension of the theory of equations with constant coefficients to a large class of equations with variable coefficients.

Inverse Applications

1st. Theory of Series & Generating Functions
2nd. Application of the Theory of Linear Equations of Differences
3rd. Linear partial equations of Differences
To give an idea of the form of the analysis I subjoin a particular case of the fundamental theorem as applied to Sec. 2 of the Inverse Method.

Employing m as independent variable let the equation of differences be

$$u_m + \phi_1(m)u_{m-1} + \phi_2(m)u_{m-2} + \text{etc.} = 0$$

then if u be the gen. function of u_m so that $u = \Sigma u_m x^m$ and if $\epsilon^{\theta} = x$ the equation for determining u will be

$$u + \phi_1 \left(\frac{\mathrm{d}}{\mathrm{d}\theta}\right) \epsilon^{\theta} u + \phi_2 \left(\frac{\mathrm{d}}{\mathrm{d}\theta}\right) \epsilon^{2\theta} u + \text{etc.} = c_0 + c_1\epsilon^{\theta} + c_2\epsilon^{2\theta} + \text{etc.},$$

c_0, c_1, c_2 etc. being arbitrary and equal in number to the order of the degree of the original equation.

In the concluding chapter I have applied the theory of series to some important expansions and to the theory of definite multiple integrals. Some of the results to which it leads appear to be new & interesting. I may add that the investigations [B 1843*a*, *b*] published under my name in the two last Nos. of Cambridge [Mathematical] Journal arose out of this more general inquiry although treated by a different method.

What has induced me to think that the Philosophical Transactions would be a fitting medium for the publication of the paper (if only it possesses sufficient importance) is that from the intimate connexions of the different parts & from the constancy of references to the fundamental theorem it could, I apprehend, be very injudicious to divide it into separate portions. Should you have any suggestion to offer I shall receive it thankfully and with attention. If on any point I have not been sufficiently explicit it is because as a stranger I feel that I may have already taken a scarcely warranted freedom.

3. DE MORGAN TO BOOLE, 24 NOV. 1843[4]

I have read through your paper with great satisfaction and return it to you under a separate cover by this post.

With regard to the setting up, I see no remarks to be made but the following.

1. The compound symbol, $\frac{d}{d\theta}, \frac{d}{dt}$, will seriously augment the printing. Would it not be better, both mathematically and typographically, if you used D which might frequently be used by itself, with D_θ or D_t for distinction when wanted. D_θ is line and lead $\frac{d}{d\theta}$ is two lines and lead.

2. May not the examples which lead to known results be abbreviated in the work down to little more than statements of data & quaesita. I would not have them left out. The longer expositions should certainly be kept when the result is novel.

With regard to the manner of printing: I see no channel in this country except the Phil. Trans. the Cambridge Phil. Trans. or the Cambr. Journal. It is probably too long for the third & I am afraid Gregory is in no state to attend to or decide upon it. Whether the R[oyal] S[ociety] would print it or not is a question. I think they ought to do so, but in sending it to them there is the nuisance of keeping a copy or employing someone to copy it at their rooms as they are very dog-in-the-mangerish about what they call their archives and will not return a paper even when they do not print it. The Cambr. Soc. labour under want of funds and would look suspiciously I suspect, upon anything long. I think if you do not mind copying it out you should try the R.S. in the first instance. The Phil[osophical] Mag[azine] I have no doubt would print a summary but it would be decidedly too long for that periodical.

[P.S.] I have kept this by me to look at a point I wanted to see again. 1 Dec 1843.

4. BOOLE TO DE MORGAN, 8 DEC. 1843

For your kindness in examining my paper I can only express to you my most sincere thanks. Your suggestion respecting a change of notation I shall attend to should I have to print the paper on my own account or to send it to a journal after its possible rejection by the RS. As however I shall in the first instance lay the paper before the Society and as before doing this I shall have to trouble a friend here to make a copy for me, being too much engaged at present to transcribe it myself, I shall not be able to make the alterations proposed in the copy presented to the Society. Perhaps you will do me the favor[5] to give me the requisite address which I have vainly sought in the Transactions.

You do not say whether the method which I have given for the solution of differential equations by series is original *otherwise* than in form. I am anxious to ascertain this point. If the method merely enables us to do what there was an organized & general method of effecting before I should not think the paper so far as respects this particular application worth sending to the RS.

The postage on the MS having been rather heavy I beg that you will permit me to return the amount in stamps.

5. DE MORGAN TO BOOLE, 11 DEC. 1843

My opinion of your paper is that the method is an original application of the calculus of operations, and does in various instances effect, not what *could* not have been done without it (for hardly any method in mathematics does that) but what certainly *would* not have been done without it. Like all other methods it has its large classes of cases which it makes practicable and easy, and which were not practicable and easy before. On the score of newness of method, I think you need not hesitate a moment to send it to the Royal Society.

If your friend's copy be (as usually happens with copies) more legible than the original (not that there is any fault to find with that) it would perhaps be better to send the copy than the original. In which case your friend might be requested to substitute D for $d/d\theta$, d/dt and the like, and you might, in reading it over, alter D into D_θ, D_t etc. where you think the subscript symbol is wanted, which will, I think, happen seldom except in enunciatory expressions.

I have rather an objection to D_x, D_y etc. in the calculus of operations, unless x, y etc. be somewhat differently understood. If E denote the direction to change x into $x + 1$, then E^h is that of changing x into $x + h$; accordingly

$$\frac{\phi(x+h) - \phi x}{h} \quad \text{is} \quad \frac{E^h - 1}{h} \,.\, \phi x$$

the limit of which is $\log E \,.\, \phi x$, hence $D = \log(E)$. Now, trying the fundamental property of log E, we ought to have

$$\log(E^m) + \log(E^n) = \log(E^{m+n}).$$

If by mere definition we allow

$$\log(E^m) = m \log E$$

of course this equation is true: or if we use E^m at the outset instead of E, we find this true. But throughout it is requisite that $(E^h - 1)/h$ etc. should be wholly independent of x, and D_x presents an appearance of depending on x. I have some suspicion that D in the calculus of operations is not so much $d\phi x/dx$ as $d\phi(x+h)/dh$ $(h = 0)$. I am not very strong about this, but I find it rather supported by the fact of the differential calculus being rendered confused to beginners from being obliged to consider *x as varying:* now in fact, we change x into $x + \Delta x$ and *make* Δx *vary*: x itself is never the variable, but stands for a specific value.

$\phi' x$ is then a specific case of

$$\psi(x, \Delta x) \quad \text{or} \quad \frac{\phi(x + \Delta x) - \phi x}{\Delta x}$$

And would be better understood by a symbol like $D_{\Delta x} \,.\phi x$ than by $D_x \phi x$.

6. BOOLE TO DE MORGAN, 28 JUNE 1844

After all the trouble which I have given you with my paper I thought that you would be interested to learn that the council of the RS have decided to print it in the Transactions. I have adopted the notation which you suggested to me and beg once more to thank you for the advice & assistance which you so readily afforded me.

I was sorry to see so small a list of contributors to the last No. of the [Cambridge Mathematical] Journal. Not that the No. itself suffered for if in fewer it was in abler hands.[6]

Your paper (as I suppose)[7] on continued fractions [D 1844*f*] interested me much. I once tried *unsuccessfully* to discover a theory of continued fractions analogous to the theory of generating functions. I suspect that they will be found to be in reality connected with a certain class of *linear functional equations.*

7. BOOLE TO DE MORGAN, 15 JAN. 1845

Will you oblige me by accepting the accompanying paper [B 1844*a*]. You will perceive that I have adopted your suggestion relative to the notation. In other respects the paper is so much altered and the applications so much extended that you will scarcely recognize it.

8. DE MORGAN TO BOOLE, 6 FEB. 1845

I beg to thank you for your very valuable paper – which I hope to study in a little while.[8]

9. BOOLE TO DE MORGAN, 24 FEB. 1845

I ought to have sooner acknowledged your kindness in favouring me with your memoirs. The one on Triple Algebra [D 1844*b*] I have read with great interest and quite agree with your views so far as I am acquainted with them. I suppose that if there are beings who can conceive of space in more than three dimensions the subject would have to them a more than theoretical interest. The paper on Divergent Series [D 1844*c*] I hope to be able to study shortly.

You may perhaps be interested in the following expansion remarkable from its following after the first term the law of Taylor's series.

$$f\left(x + \frac{\mathrm{d}}{\mathrm{d}x}\right) = f_0(x) + f_0'(x)\frac{\mathrm{d}}{\mathrm{d}x} + \frac{1}{1.2}\, f_0''(x)\frac{\mathrm{d}^2}{\mathrm{d}x^2} + \text{etc.}$$

in which $f_0(x) = \epsilon^{1/2(\mathrm{d}^2/\mathrm{d}x^2)}\, f(x)$.[9] To determine $f_0(x)$ we must expand the exponential and in applying the theorem we may suppose both sides to operate on any function u. I may mention that the expansion was deduced from that of $f(\pi + \rho)$ in my paper [B 1845*a*] and that I have sent it with some others to the [Cambridge Mathematical] Journal though I should think too late for the Feb. No. I notice it here because you are interested in the Calculus of operations of which it appears to me to constitute one of the most singular results.

Notes

[1] For the connections of this work in analysis with Boole's logical ideas see Laita 1977.
[2] For a discussion of the development of the concept of algebra in Britain at this time see Richards 1980*a*.
[3] 'Ellis's mode' refers to Ellis 1840, 1841. Boole mentions these papers in Boole 1844*a*, 252.
[4] See the postscript.
[5] Here, and occasionally elsewhere, Boole writes '-or' where the normal English usage was, and still is, to write '-our'.
[6] Five of the twelve papers in the May 1844 part of *Cambridge Mathematical Journal* were by the editor Ellis. The other 'abler hands' included A. Cayley and De Morgan.
[7] Initially the *Cambridge Mathematical Journal* did not print the name of the author of a contribution.
[8] Presumably this is De Morgan's reply to Letter 7, and the 'paper' is the one referred to in the previous letter.
[9] De Morgan writes ϵ for the base of natural logarithms; this notation is not uncommon in British books of this period.

2
MATHEMATICAL LOGIC AND IRELAND: 1847-1850

Almost two years intervene between the last letter of Chapter 1 and Letter 10 (8 Jan. 1847), the first of Chapter 2; this letter refers to a meeting between Boole and De Morgan. S.E. De Morgan reports 'they did not often meet. . . but when his [Boole's] visits did occur they were a real enjoyment to both – I believe I may say to all, for I shared in pleasure of his conversation, ranging as it did over a wide field of thought, and touching poetry and metaphysical as well as mathematical science' (1882, 168). De Morgan rarely left London, so their meetings could only occur when Boole had occasion to go there. Letter 10 indicates that Boole wished to read in the British Museum while in London. The Museum's circular reading room was not yet built, and the accommodation for readers was rather cramped; a writer in *Bentley's Miscellany* for 1852 drew a pen-picture of the scene:

> Every class of person haunts the place, from literary lawyer's clerk to the revolutionary notorieties of Europe. There are hebdomadal humorists purloining jokes, third-rate dramatists plundering plots, girls copying heads and flowers. . . (Anon. 1852, 529)

Letters 11, 12, and 13 (Nov. 1847) are of considerable importance as they concern the influential books which Boole and De Morgan were engaged in writing and publishing: Boole's *The Mathematical Analysis of Logic* and De Morgan's *Formal Logic.* Both were published in November 1847. Letter 12 contains De Morgan's comparison of the algebraic symbolic approach to logic which Boole introduced in his book, with De Morgan's own notation, which uses symbols but cannot be properly called algebraic. In addition to the letter De Morgan sent Boole, there is a draft which he wrote but did not send: it is endorsed, in De Morgan's hand, 'De M to Boole, not sent'.

Boole's letters show the direction in which his research was tending. In Letter 15 (8 Dec. 1848) he says 'I have been quietly and steadily working at Logic. . . And I believe too that I have also reduced the general theory to a perfectly harmonious whole'. Letters 17 and 18 indicate that probabilities were entering the field of Boole's research – a topic that will recur frequently in the letters of 1851. In Letter 20 (13 Aug. 1849) Boole says he has 'been applying the Logic lately in some new fields and perceive nothing like failure or inconsistency'.

From December 1848 several letters refer to Boole's application for the professorial post at the newly-founded Queen's College, Cork. De Morgan supported Boole's application with a testimonial. S.E. De Morgan wrote: 'My husband was, I believe, in some degree instrumental in obtaining the appointment [for Boole] at Cork, where Sir Robert Kane, who had married our friend Mr Baily's niece, was Principal' (1882, 168).

Letter 10

Letter 10 (8 Jan. 1847) is the first letter which contains a piece of mathematics in detail. Boole writes for De Morgan 'a demonstration of the theorem which I showed you this morning. . .' Boole addresses this letter from London where, it appears, he was on a visit. The theorem claims that

$$\int_{-\infty}^{\infty} f\left(x - \frac{a_1}{x-\lambda_1} - \frac{a_2}{x-\lambda_2} - \ldots \frac{a_n}{x-\lambda_n}\right)\mathrm{d}x = \int_{-\infty}^{\infty} f(x)\mathrm{d}x,$$

where $a_i (1 \leqslant i \leqslant n)$ are positive. Boole published his result without proof in a brief note in *Liouville's Journal* (Boole 1848*d*), and in a somewhat longer paper in the *Cambridge and Dublin Mathematical Journal* (Boole 1849*a*). This paper contains only very sketchy indication of proof, but there are several examples and some development of the basic idea of the theorem. Thus the account of the demonstration in his letter is the most detailed given by Boole. This proof appears less than adequate in the eyes of a present-day reader. And indeed the restrictions that must be made upon the function f (and upon the more general substitution allowed in Boole 1849) are not sufficient. However the result is substantially correct. An account of the result is given in the Appendix.

10. BOOLE TO DE MORGAN, 8 JAN. 1847[1]

In accordance with my promise, I sit down to forward to you a demonstration of the theorem which I showed to you this morning, and which you appeared to take an interest.

The theorem in question is

$$\int_{-\infty}^{\infty} \mathrm{d}x\, f\left(x - \frac{a_1}{x-\lambda_1} - \frac{a_2}{x-\lambda_2} \ldots - \frac{a_n}{x-\lambda_n}\right) = \int_{-\infty}^{\infty} \mathrm{d}x\, f(x). \qquad (1)$$

Consider first the equation

$$x - \frac{a_1}{x-\lambda_1} - \frac{a_2}{x-\lambda_2} \ldots - \frac{a_n}{x-\lambda_n} = v \qquad (2)$$

in which at present we make no assumptions respecting the constants a_1, $a_2, \ldots, a_n$. We have

$$x - v - \frac{a_1}{x - \lambda_1} - \frac{a_2}{x - \lambda_2} \ldots - \frac{a_n}{x - \lambda_n} = 0$$

and multiplying by the denominators and arranging with respect to the powers of x,

$$x^{n+1} - vx^n - v'x^{n-1} - v''x^{n-2} \ldots = 0$$

in which v', v''... are functions of v and of the constants $a_1, a_2 \ldots a_n$, $\lambda_1, \lambda_2 \ldots \lambda_n$. Hence by the theory of equations

$$\begin{aligned} \Sigma x &= v \\ \Sigma \mathrm{d}x &= \mathrm{d}v \end{aligned} \tag{3}$$

Σ having reference to all values of x which correspond to a given value of v.

Now by (2)

$$f\left(x - \frac{a_1}{x - \lambda_1} - \frac{a_2}{x - \lambda_2} \ldots - \frac{a_n}{x - \lambda_n}\right) = f(v). \tag{4}$$

Multiply this equation by (3), and integrate we have

$$\Sigma \int f\left(x - \frac{a_1}{x - \lambda_1} - \frac{a_2}{x - \lambda_2} \ldots - \frac{a_n}{x - \lambda_n}\right) \mathrm{d}x = \int f(v)\,\mathrm{d}v. \tag{5}$$

It follows from (2), that if we assume p and q as the respective lower and upper limits of v, the corresponding lower limits of x, in the several integrals in the first member of (5), will be given by the equation

$$x - \frac{a_1}{x - \lambda_1} - \frac{a_2}{x - \lambda_2} \ldots - \frac{a_n}{x - \lambda_n} = p$$

and that the upper limits of x, in the same integrals, will be given by the equation

$$x - \frac{a_1}{x - \lambda_1} - \frac{a_2}{x - \lambda_2} \ldots - \frac{a_n}{x - \lambda_n} = q.$$

To this we may add the condition that to every value of v between the limits of p and q there shall correspond real values of x. Of course the upper limits of x may be permuted among themselves, and also the lower limits among themselves, in an arbitrary manner.

Let p approximate to $-\infty$ (if in such a case we may use the term, approximate) and q to ∞. First the condition that to every real value of v there shall correspond real values of x requires that v should not have a maximum or minimum value. For, if v have a maximum value, any greater value must make x impossible and if v have a minimum value, any less value of v must render x impossible. Again that v may neither have a maximum nor a minimum value for any real value of x, the equation

$$1+\frac{a_1}{(x-\lambda_1)^2}+\frac{a_2}{(x-\lambda_2)^2}\ldots+\frac{a_n}{(x-\lambda_n)^2}=0 \qquad (6)$$

which is the criterion for determining a maximum or minimum value of v, obtained by differentiating (2). Now these denominators of the several terms in (6) being squares and therefore positive, the required impossibility will be secured, by assuming $a_1, a_2 \ldots a_n$ to be all positive, and it will be obvious, that it cannot be secured in any other way. Therefore $a_1, a_2 \ldots a_n$ must be regarded as positive.

Now as v approximates to $-\infty$ the values of x approximate to $-\infty$ or exceed $\lambda_1, \lambda_2 \ldots \lambda_n$ by some indefinitely small quantity θ respectively. We do not suppose θ to be the same for each. Again as v approximates to ∞, the values of x approximate to ∞ or are less than $\lambda_1, \lambda_2 \ldots \lambda_n$ by some indefinitely small quantity θ. Hence (5) gives

$$\left(\int_{-\infty}^{\infty}+\int_{\lambda_1+\theta}^{\lambda_1-\theta}+\int_{\lambda_2+\theta}^{\lambda_2-\theta}+\ldots\right.$$

$$\left.+\int_{\lambda_n+\theta}^{\lambda_n-\theta}\right)\mathrm{d}xf\left(x-\frac{a_1}{x-\lambda_1}-\frac{a_2}{x-\lambda_2}\ldots\frac{a_n}{x-\lambda_n}\right)=\int_{-\infty}^{\infty}\mathrm{d}vf(v).$$

Now θ being indefinitely small, all the integrals in the first member, after the first, vanish when the integrated function is not infinite within the limits, and give rise, when it is so, the imaginary terms which Cauchy's rule requires us to reject. In fact, integrals which become infinite within the limits, are to be regarded as themselves the limits of more general integrals which do not become infinite under the same circumstances. Hence in all cases

$$\int_{-\infty}^{\infty}\mathrm{d}xf\left(x-\frac{a_1}{x-\lambda_1}-\frac{a_2}{x-\lambda_2}\ldots\frac{a_n}{x-\lambda_n}\right)=\int_{-\infty}^{\infty}\mathrm{d}xf(x) \qquad (7)$$

provided that $a_1, a_2 \ldots a_n$ are positive.

In applying this theorem the rule above noticed must be observed, and when either integral becomes infinite within the limits, the resulting imaginary term must be rejected.

If the function $f(x)$ be even, we have

$$\int_{0}^{\infty}\mathrm{d}xf\left(x-\frac{a_1}{x-\lambda_1}-\frac{a_2}{x-\lambda_2}\ldots\frac{a_n}{x-\lambda_n}\right)=\int_{0}^{\infty}\mathrm{d}xf(x) \qquad (8)$$

The following are particular applications

$$\text{1st} \quad \int_0^{\infty}\mathrm{d}x\,\epsilon^{-x^2}=\int_0^{\infty}\mathrm{d}x\,\epsilon^{-(x-a/x)^2}=\epsilon^{2a}\int_0^{\infty}\mathrm{d}x\,\epsilon^{-(x^2+a^2/x^2)}$$

$$\therefore \int_0^\infty dx\, \epsilon^{-(x^2+a^2/x^2)} = \epsilon^{-2a}\int_0^\infty dx\, \epsilon^{-x^2}$$

$$= \frac{\pi^{1/2}}{2}\,\epsilon^{-2a} \text{ which is known.}$$

2nd $\displaystyle\int_0^\infty dx\, \epsilon^{-x^4} = \int^\infty dx\, \epsilon^{-(x-a/x)^4}$

$$= \epsilon^{-6a^2}\int_0^\infty dx\, \epsilon^{-(x^4-4ax^2-4a^3/x^2+a^4/x^4)}$$

$$\therefore \int_0^\infty dx\, \epsilon^{-(x^4+a^4/x^4)-4a(x^2+a^2/x^2)} = \epsilon^{6a^2}\int_0^\infty dx\, \epsilon^{-x^4}$$

$$= \frac{\Gamma(\frac{1}{4})}{4}\,\epsilon^{6a^2} \tag{9}$$

and so on indefinitely.

In a similar manner were obtained the following

$$\left.\begin{aligned} \int_0^\infty \frac{dx\, x^{n-1/2}}{(a+bx+cx^2)^n} &= \frac{\Gamma(n-\frac{1}{2})\pi^{1/2}}{\Gamma(n)c^{1/2}(b+2\surd(ac))^{n-1/2}} \\ \int_0^\infty \frac{dx\, x^{n-3/2}}{(a+bx+cx^2)^n} &= \frac{\Gamma(n-\frac{1}{2})\pi^{1/2}}{\Gamma(n)a^{1/2}(b+2\surd(ac))^{n-1/2}} \end{aligned}\right\} \tag{10}$$

By a slight modification of the theorem, we can, from any definite integral with any proposed limits, deduce an infinite number of other definite integrals, having the same value. The transformations etc. may be repeated or inverted, and it is obvious that in general the forms obtained will be new.

I conceive that the most useful applications of this method will be made in connection with the theory of multiple definite integrals. The reductions of such integrals usually depends on integrations of the form

$$\int_{-\infty}^\infty dx \cos(\phi(x))$$

and it has only been for very particular forms of $\phi(x)$ that the reduction has been possible. The above theorem enables us to vary the form of $\phi(x)$ within the cosine, without affecting the dx without, with any new factor.

P.S. I wish to spend a few hours in the library of the British Museum and a friend Mr Goodacre[2] would like to see the place. I am informed that Sir H. Ellis cannot admit me without a recommendation from some official person. Can you assist me?

You are at liberty to make any use of my paper that you may please.

Letters 11–13

Letters 11 and 13 form a prologue and epilogue to the more important letter 12. In Letter 11 (31 May 1847) De Morgan comments that he 'would rather not see your investigations till my own are quite finished. . . you might have the same fancy as myself'. In a situation in which each was in the process of completing an extended work on logic, De Morgan was being meticulous in ensuring that neither could be placed in a position in which he could be accused of plagiarism. Letter 13 (29 Nov. 1847) contains De Morgan's acknowledgment of the receipt of a copy of Boole's book (1847*a*) and he suggests other persons to whom Boole might send complimentary copies.

11. DE MORGAN TO BOOLE, 31 MAY 1847

I am much obliged to you for your note.

I had no objection to let Sir W[illiam] H[amilton] communicate anything he pleased because I felt quite sure he could not look at logic in any way that could give any view to a mathematician: and so I think it will turn out. But you are another sort of person and I would much rather not see your investigations till my own are quite finished; which they are not yet for I get something new every day. When my sheets are printed, I will ask for your publication: till then please not to send it. I expect that we are more likely to have something in common than Sir W.H. and myself.

I should have sent my paper on syllogism [D 1846*c*] (the one already published) to you by this post: but I remembered that you might have the same fancy as myself – to complete your own first. Therefore when you choose to have it, let me know.

In Letter 12 (28 November 1847) De Morgan made some detailed comparative comments upon his book *Formal Logic* and Boole's *Mathematical Analysis of Logic* which were published almost simultaneously. As well as this important letter, we also have a draft letter which De Morgan wrote on the preceding day, but apparently decided against sending; the draft is marked in De Morgan's hand 'De M to Boole not sent. Nov. 27, '47'. The draft has substantial differences from Letter 12. To facilitate discussion of these differences I shall divide the draft into three parts, calling them draft a, draft b and draft c.

Draft a consists of two paragraphs containing a general comparison of their methods of writing logic; in Letter 12 these paragraphs were replaced by two much briefer paragraphs. In draft b De Morgan takes one of Boole's problems and solves it by the notation he used in *Formal Logic*; Letter 12 contains a similar but clearer account. In draft c De Morgan takes up a number of particular points concerning the techniques of Boole's symbolic notation; none of these points were discussed in Letter 12. Thus draft a and draft c contain material which add substantially to the content of Letter 12, while draft b adds little additional information.

A further problem concerning the presentation of letter 12 and the draft letter relates to the notation used by Boole and De Morgan in their newly-published books; to assist the reader to understand this now unfamiliar notation some explanation is necessary. The scheme adopted to present the transcripts and accompanying explanation is therefore:

(i) A note of notations and terminology.
(ii) Draft a.
(iii) Letter 12.
(iv) Draft c.
(v) A version of the central part of Letter 12 in a notation that will be, we trust, immediately intelligible to a reader familiar with present-day mathematical logic.

Notations and terminology

The notation used by De Morgan rapidly became obsolete and will appear quite strange. Boole's notation, however, is close to that of present-day Boolean algebra.

Boole wrote '$x + y$' to denote 'x or y', but he used 'or' in the exclusive sense of either-but-not-both. Boole wrote 'xy' for 'x and y' and '$1 - x$' to indicate the contrary to x. So much is reasonably familiar; the main notation he used that differs from present-day notation is his use of (usually) the letter v to denote 'some'. Thus

$$x = vy \quad \text{is read} \quad x \text{ is some } y.$$

This notation enabled Boole to write all relations as equations. As examples, consider

$$x = y(1 - z) + z(1 - y)$$

which might be written today as

$$x = (y \mathbin{\&} {\sim}z) \vee (z \mathbin{\&} {\sim}y).$$

Also $x = vyz$, which today could be expressed either as a statement about sets: $x \subset y \cap z$; or as a statement in the propositional calculus as $x \supset y \wedge z$.

De Morgan used different symbolic notations at different periods; we are here concerned only with the notation he used in *Formal Logic* (1847*b*). In this work he wrote x for the negation of X and

$$X)Y \quad \text{to denote} \quad \text{every } X \text{ is } Y.$$

In addition in Letter 12 he writes:

$$X, y \quad \text{or} \quad \{X, y\} \quad \text{to denote} \quad X \text{ or } y$$

$$Xy \quad \text{to denote} \quad X \text{ and } y.$$

De Morgan does not introduce quantifiers; consequently it is misleading to read into his words present-day predicate calculus. However, he seems to be aware of the difficulties that arise in the absence of quantifiers.

To illustrate De Morgan's style of writing one might observe his explanation of contradictions in *Formal Logic*:

The pair 'Every X is Y' and 'some Xs are not Ys' are called *contradictory*: and so are the pair 'No X is Y' and 'Some Xs and Ys'. Of each pair of contradictions one must be true and one must be false: so that the affirmation of either is the denial of the other, and the denial of either is the affirmation of the other. (1847*b*, 59–60).

In both Letter 12 and draft c De Morgan refers to something as 'nonexistent': in the third paragraph of Letter 12 'But zZ, yY are nonexistent', and in draft c a similar remark, 'But they are nonexistent'. In his notation z, Z are contraries so zZ is the conjunction of a proposition (or class) and its negation (or complement).

These remarks illustrate the manner in which Boole and De Morgan expressed themselves when writing about sets; others write in this way included Schröder and Husserl. They used an extensional language, considering the non-empty parts of a set; thus the empty set of modern set theory is not present in their discussion. The idea of a universal set is, however, present in Boole's work. He used the word 'a Universe' to denote 'every conceivable class of objects. . .' (Boole 1847*a*, 15).

A more recent discussion of this type of set theory was given by Leśniewski who called it 'mereology' (Leśniewski 1930).

Draft a. De Morgan to Boole, not sent

I have to thank you for your paper on logic received this evening. My book was published (publication meaning giving a copy in boards) on the 24th: but if publication mean communicating printed sheets to a reviewer to read, it was published some weeks ago. Some of our ideas run so near together, that proof of the physical impossibility of either of us seeing the other's work would be desirable to all those third parties who hold that, where plagiarism is possible $1 = a$ wherever a is > 0.

My bookseller is to send you down a copy by the first opportunity. If two individuals exchange a book on logic, is it simple conversion or by contraposition. In talking of the things which are so nearly in common I am not speaking of what I published in the Cambridge paper [D 1846*c*], but of a doctrine of compound names and syllogisms, not then evolved. I need not tell you that I am delighted with the views you have given. My working processes are not so like those to common algebra as to symbols, but more resemble the operations of our heads.

12. DE MORGAN TO BOOLE, 28 NOV. 1847

I am much obliged to you for your tract [B 1847*a*], which I have read with great admiration. I have told my publisher to send you a copy of my logic [D 1847*b*] which was published on Wednesday.

There are some remarkable similarities between us. Not that I have used the connexion of algebraical laws with those of thought, but that I have employed mechanical modes of making transitions, with a notation which represents our head work.

For instance, to the notation of my Cambridge paper [1846*c*] I add

XY name of everything which is *both* X and Y

X, Y name of everything which is either X or Y.

Take your instance of p.75

$$x = y(1-z) + z(1-y).$$

I express your data thus

$$1 \ldots X)Zy, Yz \quad Zy, Yz)X \ldots 2 \qquad [\dagger]$$

The following is all rule, helped by such perception as beginners have of the rules which will succeed in solving an equation

From 1. not $X = x$, etc.

$$\{z, Y\}\{y, Z\})x$$

$$zy, zZ, Yy, YZ)x$$

But zZ, yY are nonexistent

$$\therefore \quad zy, YZ)x$$

$$\text{or} \quad YZ)x \quad \text{or} \quad YZ)xY.$$

$$\text{But from 2} \quad yZ)X$$

$$\text{or} \quad yZ)Xy$$

$$\therefore \quad YZ, yZ)Xy, xY$$

$$\therefore \quad Z)Xy, xY \qquad [*]$$

But by 1.

$$Xy)Zy, Yzy \quad \text{or} \quad Xy)Zy$$

$XY)ZyY, Yz$ or $XY)$ [De Morgan deleted this line]

$$\text{by 2} \quad x)\{z, Y\}\{y, Z\}$$

$$\therefore \quad x)zy, ZY$$

$$xY)xyY, ZY$$

$$\text{or} \quad xY)ZY$$

$$\therefore \quad Xy, xY)Zy, ZY$$

or $Xy, xY)Z$ [*]

or Xy, xY and Z are identical. [†]

[De Morgan drew curved lines from the text lines marked [*] to this final line of the argument. [†] marks the portion of this letter for which a modern version is given below.]

This is far from having the elegance of yours; but your system is adapted to identities, in mine an identity is two propositions. Perhaps I should pass from

$$X)Zy, zY$$

to

$$Z)X, xY$$

more readily than you would. But I am not sure.

In fact there hang a multitude of points upon this question whether complex or simple forms are to come first.

Draft c. De Morgan to Boole, not sent

The solution of the elective equations will, I have no doubt, be found inexpugnable. With regard to the syllogistic process, there are unexplained difficulties about v and about division by y. Here you have recourse to verbal monitions about the meaning of v. The process of division is not *per se* allowable.

$xz = yz$ does not give $x = y$. Take page 35

$$y = vx$$

$$0 = zy$$

$$y \times 0 = vxzy \quad \text{admitted}$$

$$0 = vxzy \quad \text{do.}$$

Now you may separate

VZ.XY in my notation

No *VZ* is *XY*

But not No *VZ* is *X*

and yet *VX.ZY* give *VX.Z*

There is something to explain about the divsion by y.

I think with Mr Graves that $y = vx$ is the primitive form. But v is not a definite elective symbol, make it what you know it to be, and I think the difficulty vanishes

$$y = yx$$

$$0 = zy$$

$$y \times 0 = zxy^2$$

$$0 = zxy$$

Now some Zs are not Xs, the ZYs. But they are *nonexistent*. You may say that *nonexistents* are not Xs. A nonexistent horse is not even a horse; and, (*à fortiori*?) not a cow. This is not suggested by your paper; but appears in my system.

I see that 0 must be treated as a magnitude in form $y \times 0/y$ is 0: but $0/y$ is not capable of interpretation.

In fact, your inverse symbol is not interpretable, except where use of the direct symbol has preceded.

xy make a mark on all the Ys which are Xs

$\frac{1}{x}(xy)$ Rub them out again

$\frac{1}{x}(y)$ Rub out marks which never were made –

But I do not despair of seeing you give meaning to this new kind of negative quantity.

It may be thus

$$0 = zxy$$

on the other side as

$$(xy)z = 0$$

is an equation of condition giving in my notation $XY.Z$ or $XY)z$ or Xz or $X{:}Z$. But in the form $(yz)x$ it is an identical equation, since $yz = 0$.
In $(zx)y$ it is true also though no conclusion to a syllogism, since the middle term is not eliminated.
Observe that the conclusion of the syllogism really is
Those Xs which are Ys are not Zs.
Quaere, is there not even another process of reasoning before we arrive at the ordinary conclusion namely Those Xs which are Ys are not necessarily all Xs
∴ Xs (not necessarily all) are not Zs
Or, is not syllogistic reasoning twofold in inference, on form and on quantity.

A version of part of Letter 12

As the reader is unlikely to be familiar with De Morgan's notation, much of the contents of Letter 12 may prove difficult to follow. To aid the reader in following De Morgan's comparison of his symbolic notation with that of Boole the following remarks are appended.

To translate De Morgan's symbolism into modern symbols *that preserve his verbal expression*, one can use the following lexicon:

in place of	put	which is read
$X(Y$	$X \subset Y$	every X is Y
X, Y	$X \vee Y$	X or Y
XY	$X \& Y$	X and Y
x	$\sim X$	not-X

Note that the symbol '⊂' is used in the set theory sense (rather than that of the propositional calculus) as this preserves De Morgan's verbal expression viz. 'every X is Y'.

Translation of the part of letter 12 from [†] to [†] into this notation gives:

$$1 \ldots\ X \subset (Z \,\&\, {\sim}Y) \vee (Y \,\&\, {\sim}Z) \qquad (Z \,\&\, {\sim}Y) \vee (Y \,\&\, {\sim}Z) \subset X \ldots\ 2$$

The following is all rule, helped by such perceptions as beginners have of the rules which will succeed in solving an equation

From 1. Not $X = x$ etc.

$$({\sim}Z \vee Y) \,\&\, ({\sim}Y \vee Z) \subset {\sim}X$$

$$({\sim}Z \,\&\, {\sim}Y) \vee ({\sim}Z \,\&\, Z) \vee (Y \,\&\, {\sim}Y) \vee (Y \,\&\, Z) \subset {\sim}X.$$

But ${\sim}Z \,\&\, Z$, ${\sim}Y \,\&\, Y$ are non-existent [i.e. false]

$$\therefore \quad ({\sim}Z \,\&\, {\sim}Y) \vee (Y \,\&\, Z) \subset {\sim}X$$

or $\quad Y \,\&\, Z \subset {\sim}X \quad$ or $\quad Y \,\&\, Z \subset ({\sim}X) \,\&\, Y.$

But from 2 $\quad {\sim}Y \,\&\, Z \subset X$

or $\quad {\sim}Y \,\&\, Z \subset X \,\&\, {\sim}Y$

$$\therefore \quad (Y \,\&\, Z) \vee ({\sim}Y \,\&\, Z) \subset (X \,\&\, {\sim}Y) \vee ({\sim}X \,\&\, Y)$$

$$\therefore \quad Z \subset (X \,\&\, {\sim}Y) \vee ({\sim}X \,\&\, Y) \qquad [*]$$

But by 1

$X \,\&\, {\sim}Y \subset (Z \,\&\, {\sim}Y) \vee (Y \,\&\, {\sim}Z \,\&\, {\sim}Y) \quad$ or $\quad X \,\&\, {\sim}Y \subset Z \,\&\, {\sim}Y$

by 2 $\quad {\sim}X \subset ({\sim}Z \vee Y) \,\&\, ({\sim}Y$ or $Z)$

$$\therefore \quad {\sim}X \subset ({\sim}Z \,\&\, {\sim}Y) \vee (Z \,\&\, Y)$$

$${\sim}X \,\&\, Y \subset ({\sim}X \,\&\, {\sim}Y \,\&\, Y) \vee (Z \,\&\, Y)$$

or $\quad {\sim}X \,\&\, Y \subset Z \,\&\, Y$

$$\therefore \quad (X \,\&\, {\sim}Y) \vee ({\sim}X \,\&\, Y) \subset (Z \,\&\, {\sim}Y) \vee (Z \,\&\, Y)$$

or $(X \& {\sim}Y) \vee ({\sim}X \& Y) \subset Z$ [*]

or $(X \& {\sim}Y) \vee (X \vee Y)$ and Z are identical

[these final remarks being connected to the lines marked [*] by curved lines in De Morgan's letter].

13. DE MORGAN TO BOOLE, 29 NOV. 1847

I got your letter and the copy just now. As you know by this time I received the other. I will give my second copy to Univ. Coll. Libr.

Pray send one to Dr Whewell – who takes great interest in such things – and to Dr. Logan – St. Mary's Coll. – Oscott near Birmingham – Also to Rev. Wm. Thomson, Queen's College Oxford – And to (Mr Solly care of Mr Asher, Berlin) care of Mr Nutt Bookseller Fleet Street.

Thomson and Solly are writers on the subject and all are real readers.[3]

I find my publishers had not sent to you today – but they will forthwith.

Letters 14–16

In Letter 14 (24 Aug. 1845) Boole thanks De Morgan for a copy of a paper on partial differential equations (D 1848*a*) and comments: 'both methods are ingenious, and the second especially so...'. Both of these methods involve solving a partial differential equation by relating it to another equation or system of equations. In the first method, which concerns a differential equation of the form $\phi(x, y, p, q) = 0$, De Morgan 'contrives that this shall be the result of elimination [of v] between $A(x, y, p, q, v) = 0$ and $B(x, y, p, q, v) = 0$.' He proceeds to form a system of six partial differential equations consisting of the two latter equations and the four obtained by differentiating each of these with respect to x and y. On elimination of p, q, r, s, t – note that as usual p, q denote the partial derivatives $\partial z/\partial x$, $\partial z/\partial y$ while r, s, t denote $\partial^2 z/\partial x^2$, $\partial^2 z/\partial x \partial y = \partial^2 z/\partial y \partial x$, $\partial^2 z/\partial y^2$, respectively – from the six equations 'there will result an equation... which will be more tractable than $\phi = 0$.'

The second method concerns a partial differential equation of the form $\phi(x, y, z, p, q, r, s, t) = 0$. De Morgan forms the equation

$$\phi\left(p, q, px + qy - z, x, y, \frac{t}{rt - s^2}, \frac{-s}{rt - s^2}, \frac{r}{rt - s^2}\right) = 0.$$

He then remarks 'if either of these equations can be integrated, say by $Z = \psi(X, Y)$, then the solution of the other is obtained by eliminating X, Y from $x = \mathrm{d}Z/\mathrm{d}X$, $y = \mathrm{d}Z/\mathrm{d}Y$, $z = xX + yY - Z$.' De Morgan observed that Legendre had used a special case of this procedure.

Letter 15 (8 Dec. 1848) shows that Boole had withdrawn his application but had 'been induced to resume my application'. Boole's withdrawal may have been in part due to his father's failing health. John Boole died four days after this letter was written. This letter also contains Boole's ideas on how he might inform himself on methods of teaching at Cork should he succeed in obtaining the post – a fine illustration of the moral earnestness of the age.

In Letter 16 (3 April 1849) De Morgan makes some remarks which suggest he had come close to the idea of three-valued logic. He says he had 'considered a little the problem of – not name and contrary – . . . but any number of names – a proposition in which the alternatives are more than *X* and not-*X*. . . but never had the curiosity to investigate more than some simple cases of three alternative. . .'. As far as I am aware he never discussed three-valued logic in his books or papers; there is, however, a reference to modal propositions in 1847*a*, 232.

14. BOOLE TO DE MORGAN, 24 AUG. 1848

I am much obliged to you for your paper [D 1848*a*] on partial differential equations which I have read with great interest. Both methods are ingenious, and the second especially so: I hope to study them more fully some time but for a long time past I have been quite unable to engage in any mathematical pursuits. Nevertheless I rejoice to see that progress continues to be made.

15. BOOLE TO DE MORGAN, 8 DEC. 1848

The Irish professorships with reference to one of which you were so good as to give me a testimonial a year or two ago, are now about to be filled. I had a short time since withdrawn my name from the list of candidates, but I have been induced to resume my application. My hopes of success are not very sanguine, although in one quarter I have lately met with all the encouragement which the most generous friendship could suggest. Still I do not disguise from myself that men equal to me in more attainments, and possessed of other recommendations that I can lay any claim to may enter the field of competition. Happily for myself, I feel that I can bear a disappointment without either looking at myself as an injured man, or taking fee with those pursuits from which I have already derived far more real and solid gratification than any outward successes can afford.

However if one does resolve to enter the field it is the part of wisdom to provide for the defence of weak points. And it has accordingly occurred to me that it would do something to supply a defect in my claims and also be right in itself that I should state my intention of spending some time, (in the event of appointment), at one or more of the Universities, so as to see the practical working of different systems of instruction. If circumstances should make it convenient for me to spend a week in London, may I venture to ask you whether there would be any difficulty as to my seeing something of the state of instruction in your own college – I do not mean mathematical merely – though this is of course the most important to me. I should give you no other trouble.

You will think me by this time almost lost to original investigations in mathematics, so little have I lately done. But I have been quietly & steadily working at Logic and I wish I could some time tell you a little of the results of my inquiries. I believe that I have at length succeeded in reducing all the mathematical applications to one general method more comprehensive and yet more simple than those which I have published and including them. And I believe too that I have also reduced the general theory to a perfectly harmonious whole. These things give me the hope of making the subject interesting and of giving to it a ready practical value – ends which I conceive myself to have been very far from attaining in my published Essays. I should think it very selfish to say all this, if I did not know that you are really interested in such speculations.

Accept my apologies for thus troubling you.

16. DE MORGAN TO BOOLE, 3 APRIL 1849

The Irish question is not yet *settled* – I know.

I have considered a little the problem of – not name and contrary – X and x, – but any number of names – a proposition in which the alternatives are more than X and not-X. I looked at it enough to see the possibility of wider classes of numerically definite distributions and logical syllogisms arising therefrom – but I never had the curiosity to investigage more than some simple case of three alternatives – I hope you will go on with it.

I hope you will expand your view of probabilities – which I am not sure I understand. I look for plenty of logical symbolization from you.

Letters 17–26

In these letters we see the relationship between Boole and De Morgan deepening. Notice De Morgan's remarks in Letter 25 (8 June 1850) relating to the revised proofs of one of his papers: 'if you find anything which has your own image upon it – you must extract the evidence. . . that I may put the superscription also'. De Morgan recognizes that he has been influenced by Boole's ideas, and is anxious to acknowledge this. In the same letter 'I have two notes of yours – always on hand for answer. . .'. The correspondence had, by 1850, become a regular and fairly frequent practice.

There are two main themes in these letters: Letters 17–19 concern an essay Boole had written on probabilities and Letters 20–24 and 26 concern Boole's appointment to the professorship of Mathematics at Cork and his reactions on taking up the post.

In Letter 17 (12 April 1849) Boole writes of sending 'the paper which I hope you received', which, as the following remarks indicate, concerned probabilities. This was, Boole said, a 'sketch. . . not designed for publication – but was written mainly to register the actual state of my own knowledge. . .'. In the next letter (21 April 1849) Boole sent De Morgan an appendix to this paper. De Morgan returns the paper with some comments on 10 June 1849 (Letter 19).

Letters 17, 18 and 19 contain a number of remarks which give some indication of the contents of this paper. The date of Letter 17 suggests that the paper, which Boole refers to as a 'Sketch', was written early in 1849. The main theme is probabilities and there is some discussion of 'a first principle'. In Letter 18 we read that Boole has sent 'a short Appendix. . . which exhibits the application of the method to the syllogism'. In Letter 19 De Morgan, returning Boole's paper, mentions 'the principle' as not his but coming from Laplace and refers to an example concerning rain and thunder.

The Library of the Royal Society contains (among other Boole manuscripts) an essay entitled: 'Sketch of a Theory and Method of Probabilities founded upon the Calculus of Logic'. This essay is written in parts of two notebooks marked (by Boole) '2 Logic; and '6 Logic'. An incomplete version of this essay was printed by R. Rhees in Boole 1952, 141–66.

The essay is in two parts; the first consists of an explanation by Boole of his symbolic approach to logic. This part is clearly intermediate in date between Boole's *The Calculus of Logic* (Boole 1848*a*) – Boole refers to this paper at the beginning of the essay – and *Laws of Thought*, 1854, as parts of the essay are evidently an early version of certain topics of that work. However, Rhees suggests (Boole 1952, 141 footnote), on the basis of a remark Boole made in Boole 1851*f*, that the essay must have been written in 1848 or 1849; for in the 1851 paper cited Boole says in a discussion of probabilities in relation to the observed frequency of events: 'I shall present a solution to which that method conducted me about two years ago. . .'.

The second part of the paper and the more important, in that it contains entirely novel material, not a reworking of earlier ideas, is headed 'Of Probabilities'. An important concern here is the question of independence of events which, together, constitute a compound event. Boole quotes a 'Principle IV' from *Encyclopedia Metropolitana* – the article in question was written by De Morgan (D 1837*c*), and enunciates a principle of his own: 'The events, whether simple or compound, whose probabilities are given by observation, are to be regarded as independent of any but a logical connexion'. In the course of the discusssion Boole introduces an example concerning thunder and rain.

The essay concludes with four appendices – Rhees only gives the first two of these – the last of which has the title 'Of the probabilities of conditional events; and of the syllogism'.

Thus each of the seven points detailed above which arise in the letters can be linked with a similar piece of evidence relating to the notebook 'Sketch'. To sum up this discussion: it may not be that it was the actual 'Sketch' printed by Rhees which Boole sent to De Morgan, but it seems highly likely that it was a paper which contained substantially the same ideas as those of the second part of the 'Sketch' together with one of the appendices.

Letters 22–4 refer to a visit Boole made to London in connection with the Cork post and to a planned meeting with De Morgan which in the event did not take place.

In Letter 24 (8 Nov. 1849), written shortly after Boole had taken up the post, one notes his reactions to the sectarian divisions in Ireland – which still persist in the Northern part of that island. Yet less than a year later we see in Letter 26 (17 Oct. 1850) his disillusionment with this situation. The desire to leave Cork he expresses here recurs several times during the succeeding years.

The Queen's Colleges were founded as a result of Peel's policy of social reform in Ireland. Peel was the Chief Secretary for Ireland from 1812 to 1818, and Prime Minister of the United Kingdom from 1841 to 1846.

Although the degrees of Trinity College Dublin were open to all, in practice no Catholics attended Trinity College owing to its strongly Anglican atmosphere and the expense of the education it provided. Scholarships and fellowships could be held only by those subscribing to Anglican principles; religious tests were abolished in 1873 however.

In 1845 a Bill was introduced to incorporate Queen's Colleges at Belfast, Cork, and Galway as secular institutions, but allowed the various denominations to provide pastoral care for their adherents. The Colleges opened in 1849; fees were low and generous provision of scholarships was made. In 1850 the Queen's University in Ireland was formed to provide a body which linked the three Colleges.

Initially the secular character of the Colleges was unpopular with the Anglicans and Presbyterians as well as the Catholics. The opposition of the first two died away; that of the Catholics did not. The opposition of the Catholics was led by William McHale, the (Catholic) Archbishop of Tuam. Papal rescripts of 1847 and 1848 expressed disapproval of the Colleges and in 1850 an Episcopal Synod issued a formal condemnation of them. Thus the number of Catholic students who attended the Colleges was relatively small.

An unfortunate result of the creation of the Queen's College in Belfast is referred to in Letter 21 (14 August 1849). The new College superseded the Belfast Institution, and the appointment of J.R. Young, the professor of mathematics at the Belfast Institution, to the new College was prevented by 'the Presbyterian party who controlled the professors' nomination' (Dictionary of National Biography, J.R. Young, vol. 63, 383). De Morgan tried to obtain a post for Young (Graves 1882. 275–7, 283–4).

The later history of university education in Ireland has little bearing on the correspondence. But it may, perhaps, be of some interest to note the foundation in 1854 of the Catholic University in Dublin; J.H. Newman was the first Rector of this institution, whose degrees were not accorded recognition. In the 1860s further moves to evolve a university system acceptable to the Catholics were made, but the main attempt to this end, the Bill introduced by Gladstone's administration in 1873, was defeated; the government fell shortly afterwards. (Beckett 1966, passim.)

Another topic which will recur in later letters is De Morgan's controversy with Sir William Hamilton and his supporters, on the quantification of the predicate; and De Morgan's series of lengthy papers *On the Syllogism* which relate to this controversy. These matters are mentioned briefly in Letters 17

(12 April 1849) and 25 (8 June 1850), but I shall defer comment on them until later when they assume more substantial form.

Also in Letter 25 De Morgan refers to the use of two negatives in Greek. Both Boole and De Morgan were competent in Greek, Latin, and French, and Boole also in German; as later letters indicate, De Morgan had a slighter knowledge of German. The general rule in Greek is that a negative followed by a simple negative (i.e. *οὐ* or *μή*) denotes affirmation; but a negative followed by a compound negative (e.g. *οὐδεν*) denotes a strengthened negative. The opening line of the *Orestes* is an example of the second case; in A.S. Way's translation: 'Nothing there is so terrible to tell' (see *Euripides' Plays*, Volume 2, p. 141, Everyman Library).

In the quotation from Aristotle, the situation is rather different; indeed, as De Morgan remarks, Aristotle is 'making Greek'. For in the Loeb Classical Library edition, due to H.P. Cooke, the phrase mentioned by De Morgan is given as

οὐκ ἔστιν οὐ-δίκαιος οὐκ-ἄνθρωπος

(note the hypens), and translated:

not-man is not not-just.

Thus in this case there is a simple negative (the first οὐκ), the other negatives being 'invented' compound words.

17. BOOLE TO DE MORGAN, 12 APRIL 1849

I have been spending Easter in the country & did not get your letter till yesterday when I sent off the paper which I hope that you received. The second example in the probabilities ought to stand first. There is, owing to a peculiarity of form in all the logical equations which occur in the application to probabilities, a somewhat shorter method of solution than the one given in the Logic – but I did not think it necessary to notice it. I believe there is also a general method of reducing the final algebraic system but I have not completely examined this point.

I ought to mention that the sketch I send was not designed for publication – but was written mainly to register the actual state of my own knowledge – and to serve as a record of what I had accomplished in the event of my never accomplishing any larger design. As to the examples I suppose that far better ones might be found – but I made those hastily for a test of the method. I verified almost every result by independent considerations as it was obtained – but unfortunately I did not note down the steps of this process and what I have actually given to them in the text will I fear be meagre.

I imagine that the principle which I have assumed (the independence of the results of observation) must be regarded as a *first* principle. It is clear that in any theory a first principle is needed, and I think that the hypothesis of the independence of simple events is of this nature.

You need not return the paper until I ask for it.

P.S. I must not close my letter without thanking you for your kindness in undertaking to look over the paper – of which I am very sensible.

18. BOOLE TO DE MORGAN, 21 APRIL 1849

I just write to enclose a short Appendix which I have written out today to my paper & which exhibits the application of the method to the syllogism. I think you have yourself somewhere remarked that the theory of the probable syllogism is imperfect. If you have a difficulty in understanding any part of the paper I should be happy to endeavour to remove it; & I have a few more examples both in logic and probabilities worked out if you would like to try the method on a new case yourself.

19. DE MORGAN TO BOOLE, 10 JUNE 1849

I return you your papers with many thanks.

To say how far I agree with you would be difficult at this time, as it is my busiest time – and I read the paper two months ago – But I must urge on you to continue and publish, for your mode of viewing the subject is one which will serve those who disagree as well as those who agree. With regard to the specific reference to my *principle* (I take this word from Laplace, it is a very bad one) or rather the *principle* stated in Laplace, you are to understand that it is merely the mathematical inversion of the preceding one. It presumes, as does the preceding one, that the events are known to be independent. In your instance of rain and thunder they are known *not* to be independent. Laplace seems to have put this down to prepare for cases in which ab and a might be more easily found than b – so that ab/a would be the correct way of finding the latter – But I cannot remember that such a thing ever happened.

20. BOOLE TO DE MORGAN, 13 AUG. 1849

I received last week the official announcement of my election to the professorship of mathematics in Queen's College, Cork.

When I became a candidate for the appointment you were so good as to give me a testimonial. I feel it right therefore to inform you of my success and to say how much I am endebted to you for the assistance which you so willingly rendered me. I shall at least endeavor to justify your good opinion and kind wishes.

[P.S.] Let me take this opportunity of thanking you for looking over my paper the receipt of which I forget whether I have acknowledged. I have been applying the Logic lately in some new fields and perceive nothing like failure or inconsistency.

21. DE MORGAN TO BOOLE, 14 AUG. 1849

I am very glad to hear that the electors have had the sense to accept your offer of joining the Irish Colleges. Whether I am to congratulate you or not, I cannot tell – for Ireland is a riddle altogether – I sincerely hope, however, that by keeping out of their squabbles, you may be able to live in peace.

I believe you are better situated at Cork than you would have been in the north of Ireland – At Belfast, poor Young whose writings you know I have no doubt, is ruined by being left out – For the new Government College destroys the Belfast Institution from which his means of living came. I am assured that he kept out of the disputes of all parties – and that he had therefore all parties against him.

I suspect you are likely enough to find that an appointment in a part of the country where the Pope predominates will give you an easier berth than you could have had among the Presbyterians Church people and Papists mixed.

22. BOOLE TO DE MORGAN, 3 SEPT. 1849

I have to visit London on business connected with the Irish Colleges on Friday next & shall probably remain in town some days. It would be a great satisfaction to me to meet you & to have half an hour's conversation with you while I am there. My residence will not be very far from either Camden St or the University & if you will tell me whether you are likely to be at liberty and when & where, I will make an effort to get to see you. It is likely that I shall be engaged a good deal on Saturday & Monday but I shall I suppose be at liberty in the evenings and certainly on the Sunday. But if you will name your own time I will endeavour to make other business bend to it.

Of course I only ask you this in the event of its really being convenient. If you are engaged don't scruple to say so – as you have not a captious person to do with.

23. DE MORGAN TO BOOLE, 4 SEPT. 1849

I shall be very happy to see you on Saturday or Monday Evening – My house if full of painters etc. and my family all away so that I cannot say come to dinner – but I will have tea ready at 7 o'clock on the day on which you inform me you can come.

You should however let me have your address in town as soon as you know it – that I may be able to let you know if any thing happens to change the Evening.

[De Morgan concludes the letter with a map showing how to reach Camden St (which is near Mornington Crescent) from the northern end of Tottenham Court Rd. The house no longer exists.]

24. BOOLE TO DE MORGAN, 8 NOV. 1849

Circumstances after all prevented me from paying you my intended visit. I waited at home for a month expecting a summons to town which never came unless to be followed by a speedy postponement. I am very sorry that I did not get to see you. For many reasons I should have liked to meet you.

I find myself very comfortable here [Cork]. At present everything seems to promise harmony. I have met with nothing like intolerance among the Roman Catholics with whom I have conversed. It is understood here that the priests are favourable to our views but are withheld by the peculiar position which the forward zeal of such bigots as MacHale and O'Higgins has placed them in from manifesting their sympathies with us. I have met with but one or two of the hierarchy myself but what I saw of them confirmed this opinion which I had before heard expressed by large numbers of their church. Indeed they have good reason to be satisfied. Our statutes bind us from introducing problems on divinity into our lectures (not that a professor of Mathematics, however sound a protestant, would be likely to impugn the doctrine of transubstantiation, however likely a chemist might be) and deans of residence have been appointed for the three denominations. The bearing of the local authorities of the college has been conciliatory in the extreme, more so indeed I think than was called for. It were better to rest on the truth & justice of our principles and leave them to make their way.

Judging from the mathematical examinations which are just over elementary scientific education is in a low state here. I am desirous of starting a class for schoolmasters. You have something of the kind in connexion with your university. Could you give me any hints? or information?

Mr De Vericour and I are in the same lodgings. He takes his cigar out of his mouth to breathe out to you the kindest regards and *souvenirs.*

Following your advice and that of my friend [Charles?] Graves I intend again to enter the lists on the side of Mathesis against the logicians.

25. DE MORGAN TO BOOLE, 8 JUNE 1850

I have two notes of yours – always on hand for answer – expecting daily for many weeks past to answer by transmitting you the proofs which you are to look over to please me & take care of yourself. But these proofs[4] have been delayed till now – and they are sent one by one – the paper being I suppose longer than they like to set up at once –

Accordingly – as *revises* come – I shall send them to you *one* by *one* – and you need not return them – if you find anything which has your own image upon it – you must extract the evidence from the papers I have seen & send it to me, that I may put the superscription also –

I have no particular news about either mathematics or logic – I do not know whether you are aware that an English translation of the Port Royal Logic [Arnauld 1850] was published a few months ago by T.S. Baynes – who is, I understand, Sir W. Hamilton's locum tenens at Edinburgh – and who is preparing a work on logic [Baynes 1850] in his system. There was published two years ago at Oxford –by Mr Chretien of Oriel College – a small octavo 'On logical method' [1848] which is an interesting work – I mention these things – because I never heard of them myself till the other day – but you may have more of logical acquaintance & correspondence than I have. Do you know of anything written upon the use of the negative in Greek – which may resolve this.

It is said, and justly, that two negatives, in ordinary Greek do not make an affirmative – but a more emphatic negative. I remember no instance at this moment but the opening of the Orestes

οὐκ ἐστιν οὐδεν δεινον. . . – Nevertheless – in Aristotle – (De Interpr. cap.X) two negatives make an affirmative and *three* negatives a negative – as in *οὐκ ἐστιν οὐ δικαιος οὐκ ἀνθρωπος·* Was Aristotle here talking Greek – or making Greek.

You will find the solution of Barbara Celagrent[5] etc. in the revises – when they come. I dare say I shall send one in a day or two. When are your continuations to appear.
[PS] My kind remembrances to M. de Vericour – Tell me how you are getting on in your College.

26. BOOLE TO DE MORGAN, 17 OCT. 1850

I think that you and I are sufficiently acquainted with each other to justify me in asking you if you should hear of any situation in England that would be likely to suit me to let me know of it. I am not terrified by the storm of religious bigotry which is at this moment raging around us here. I am not dissatisfied with my duties and I may venture to say that I am on good terms with my colleagues and with my pupils. But I cannot help entertaining a feeling to which perhaps I ought not to give expression that recent events in this college have laid the foundation of a want of mutual trust and confidence among us which would be to me far more painful than any amount of outward hostility. For my own part I no longer feel as if I could make this place my home. Perhaps this is a state of feeling which I ought to endeavour to repress but it is not easy to do so. I dread that the tone of our mutual intercourse and regard may henceforth be wanting in the cordiality and trust which seemed before to prevail.

This is all that I can say to you on the subject at present but sincerely do I pray that the anticipations which I have expressed may not be realized.

Do not suppose that I have quarreled with any body here and am anxious to get away on that account. In the affair of De Vericour[6] I took his part but temperately and maintained throughout a friendly correspondence with the President [R. Kane] & Vice-President [J. Ryall]. It is what I see around me and what I cannot but anticipate in the future which causes me to think that I might consult my peace of mind and my real utility in the world by quietly withdrawing to another sphere of labour.

Now this is what I would not say to any one in whose good feeling and discretion I could not place entire confidence. What I ask of you is not to mention these circumstances but to inform me at any future period of what you suppose might suit me in England. No one else knows of my present views and feelings.[7]

Let me now turn from this subject and tell you that I am following your advice and diligently preparing a work on Logic & Probabilities for the press. Some of the most recent of my speculations in this direction would I think interest you. There is a point at which my theory of the Laws of Thought comes to bear on the question of Human Liberty – with reference to the Intellect directly – and with reference to the Will by analogy, and also be connexion with the former. My conclusion is that there is a real phenomenon in the mind whether rightly called Liberty or not which distinguishes it from the system of external Nature and which admits of being as exactly defined by its properties as any other phenomenon. When the introductory chapter is printed you shall have a copy of it and then if you care to see the others you may do so.

I hope Mrs De Morgan is well. Give my best regards to her.[8]

Notes

[1] This letter is addressed from 19 Northumberland St, Strand. The Boole papers in the library of the Royal Society contain a draft of this letter (1847 Boole other manuscripts); the draft does not differ from Letter 10 in any significant way.
[2] I have not identified Goodacre. Possibly he is either Robert or William Goodacre who resided in Nottingham and wrote arithmetic texts.
[3] T. Solly wrote two letters to De Morgan on logical matters in October and November 1847; these are in the library of University College, London, MS Add 97/5. 'Asher' is, perhaps, Adolphus Asher the Berlin bookseller who was used by Sir A. Panizzi (the Head of the Department of Printed Books at the British Museum) for the aquisition of German books for the Museum. St Mary's College was a catholic college, where H.F.C. Logan, another correspondent of De Morgan, taught.
[4] The proofs are presumably those of the part of *Transactions* of the Cambridge Philosophical Society, which contains *On the Syllogism II.*
[5] 'Celagrent' is an invention of De Morgan, analogous to Celarent. The letter g following a vowel means that the premise (or conclusion) denoted by *that* vowel takes the correlative copula: see De Morgan 1966, 54.
[6] The 'affair of De Vericour' must have soon subsided; he was still Professor at Queen's College in 1864. I do not know what the controversy was about.
[7] This letter was endorsed 'Private' by Boole.
[8] The first time Boole sends Mrs De Morgan his regards. He does this only occasionally before his marriage in 1856.

3
PROBABILITIES AND ECCENTRICITY: 1851

No letters that De Morgan wrote to Boole between June 1850 and January 1856 appear to have survived. However, the considerable number of letters that Boole wrote to De Morgan in these years testify that the exchange of letters continued; there were 16 in 1851 alone. Examination of the texts of these letters suggests that De Morgan must have written at least ten letters to Boole in this year; some of them may have been brief notes accompanying the reprints of De Morgan's papers of which Boole acknowledged receipt. The lack of De Morgan's letters is a matter for regret, but it does not seem to result in any serious lacunae in understanding of the matters raised by Boole.

There are two continuing topics in the letters of 1851. The more important concerns probability. We have seen in the previous chapter that Boole had been studying this subject. In Letter 32 (16 July) Boole gives a summing up of the result of his researches: 'I am sure that no *general* theory of probabilities can be established as any other than a preliminary general theory of Logic. . . I am sure that a perfectly general theory may be established [which] I believe to be quite beyond the scope or power of the received theory'. Clearly Boole was well on the path to the publication of *An Investigation into the Laws of Thought* in 1854. Letters 33 to 37 (July–August) are all principally about one special problem in the theory of probabilities: De Morgan's attempted solution to this problem are evidently less than adequate; Boole points out errors in letters 35 and 36 (4, 11 August). The nature of this problem will be discussed later in the chapter.

The less important continuing topic concerns John Walsh, of Cork, 1786–1847, an eccentric who published a number of brief tracts claiming discoveries which he thought superseded the usual calculus methods of treating problems relating to curves. He also claimed to have discovered the general solution to equations of the fifth degree. The references in Letters 27, 28, 30, and 39 to 41 concern Boole's memoir of Walsh (Boole 1851*h*). This memoir is still worth reading; it gives a good illustration of the 'quasi-insanity' (Boole's words) of an amateur of science who is convinced he knows better than the experts. De Morgan, not surprisingly, included Walsh in his collection of circle-squarers and others whom he dissected in *A Budget of Paradoxes* (De Morgan 1872).

By 1851 both Boole and De Morgan were near the height of their powers – Boole was 36 and De Morgan 45. Many letters refer to the exchange of their papers. But, as we shall see in the last letters of this chapter, there are also a number of literary allusions.

Letters 27–30

Letters 27 (31 March) and 29 (6 May) contain some discussion which relates to the meaning of the concept of a solution of a differential equation. In Letter 27, replying to a query from De Morgan, Boole explains his understanding of 'a primitive equation of $f(x, y, (dy/dx), (d^2y/dx^2)) = 0$' – i.e. any equation $f_1(x, y, (dy/dx)) = 0$ which yields $f(x, y, (dy/dx), (d^2y/dx^2)) = 0$ on differentiation. The last paragraph indicates that the question that De Morgan had put to Boole concerned the distinction between a primitive of a first order equation which is a general solution – which contains an arbitrary constant – and a singular solution – which does not. De Morgan had discussed this matter in his paper read in February 1851, *On some points of the Integral Calculus* (De Morgan 1851*b*), the first section of which is titled 'On the singular solution of a differential equation of the first order'. In this paper he proposed a new term, and a modified definition of a singular solution:

> The *singular solution* of a differential equation has been usually defined as the solution which is not any case of the general solution. In this paper I propose to apply the term to any solution whatsoever, be it contained in the general primitive or not, which results from any process that cannot introduce an arbitrary constant: reserving the phrase *extraneous solution* to signify any solution which is not a case of the general primitive (De Morgan 1851*b*, 109).

Boole suggests, however, that 'we are not to wantonly meddle with definitions which *semper ubique et ab omnibus* have been agreed upon.' The Latin tag here misquoted is due to Vincent de Lérins, who died about 450. In a work in which he argued against Nestorian beliefs, Vincent de Lérins defined as catholic those beliefs *'quod semper, quod ubique et quod ab omnibus'* had been agreed upon and accepted.

A minor scandal is referred to in Letter 28 (22 April) where Boole acknowledges the receipt of a 'notice of Libri'. Guglielmo Libri who came to London in 1848 under the accusation of having stolen valuable books and manuscripts from the French libraries he visited in his capacity as Inspecteur de Bibliothèques. De Morgan was one of a number of persons who thought him innocent – Lord Brougham was another of his defenders – but his guilt is now generally accepted. Libri, says S.E. De Morgan, 'became our attached and valued friend, always recognising a firm and able defender in my husband, whose articles in the *Athenaeum* and elsewhere were the means of establishing a belief in his innocence in England' (S.E. De Morgan 1882, 177). A review of Libri's defence, *Lettre à M. de Falloux, etc.*, appeared in the *Athenaeum*, vol. 22 (May 1849), 484–5. This review is unsigned, but may well be by De Morgan. There are several brief notices of this controversy in the *Athenaeum* between 1848 and 1852. De Morgan wrote an extended defence of Libri in *Bentley's Miscellany* for 1852 (De Morgan 1852*g*).

In Letter 29 (6 May) Boole again refers to De Morgan's paper (1851*b*); he considers that 'it appears to me to contain a true theory and I think an important one in some respects.' He says that parts reminded him of ideas contained in a paper of his own which 'has been completed for several months but is yet unpublished.' Boole later published a paper *On reciprocal methods in the differential calculus* – it appears in two parts – (Boole 1852, 1853) which seems to be the one he is here referring to. The published paper concerns envelopes and the elimination of constants in a set of equations.

The particular point at issue in part III of De Morgan's paper, and which Boole refers to at the end of this letter, concerns the existence of solutions of differential equations involving arbitrary functions. For example consider (with De Morgan) the differential equation $y''' = 0$; this has $y = Hx^2 + Kx + L$ for the general solution. Let f denote any differentiable function of three variables u, v, w; if u, v, w are then made functions of x,

$$\frac{\mathrm{d}}{\mathrm{d}x} f(u, v, w) = f_u \frac{\mathrm{d}u}{\mathrm{d}x} + f_v \frac{\mathrm{d}v}{\mathrm{d}x} + f_w \frac{\mathrm{d}w}{\mathrm{d}x}.$$

If, further, we substitute $u = y''$, $v = xy'' - y'$, $w = \frac{1}{2}x^2 y'' - xy' + y$ then

$$\frac{\mathrm{d}}{\mathrm{d}x} f(u, v, w) = y'''(f_u + xf_v + \tfrac{1}{2}x^2 f_w) = 0;$$

thus $f(y'', xy'' - y', \frac{1}{2}x^2 y'' - xy' + y) = 0$ is a solution of the differential equation $y''' = 0$ which, as Boole remarks, '*may* be more general than $y - Hx^2 - KX - L = 0$.' Of course, the point here is that if one takes the most obvious integrals of $y''' = 0$, viz. $y'' = c$, $y' = cx + d$, $y = \frac{1}{2}cx^2 + dx + e$, then this solution becomes $f(c, d, e) = 0$. Boole also says; 'There is need however of a good deal of additional inquiry.'

In Letter 30 (28 May) Boole expresses his regret at having offended J.J. Sylvester. In a postscript to his paper Sylvester had said 'my theorem on the subject [Linear Transformations], which is of a much more general character, and includes Mr Boole's...' (Sylvester 1850, 281). Boole had responded that Sylvester's theorem 'is original in form only' (Boole 1851*b*, 90) and had analysed the difference between their results, concluding with the remark 'Mr Sylvester has, I am assured, too much love for truth to feel offended...' (Boole 1851*b*, 92). Sylvester's theorem concerned the relations holding between the coefficients of a quadratic form in n variables and those of the form obtained when the given quadratic form is subjected to a transformation in which r of the variables which satisfy a set of r linear equations are eliminated. However, a 'Reply to Professor Boole's observations...' (Sylvester 1851*b*) shows that he was offended. Sylvester describes Boole's remarks as 'extraordinary observations... which I cannot... suffer to pass unchallenged', Boole next wrote a letter to the editor of the *Cambridge and Dublin Mathematical Journal* (Boole 1851*d*) in which he tried to conciliate Sylvester by saying that he wrote 'for the sake not of controversy but of peace...' and 'I acknowledge that in the sense stated by Mr Sylvester... his theorem is perfectly original'. He was, Boole said, 'convinced that the present misunderstanding is simply the result of hasty judgement' (Boole 1851*d*, 284–5).

In addition to these complicated mathematical questions, lesser matters also attract their attention. In Letter 30 (28 May) Boole mentions – approvingly – De Morgan's 'paper on Signs' (1851*d*). Boole says: 'The views are in essential points identical with some which I stated in a paper written many years ago and sent to the [Cambridge] Journal while Gregory was editor but of which he never acknowledged the receipt.' Gregory was the editor from its inception in 1839 until his death in 1844, and his failing health was doubtless the cause of his negligence.

De Morgan's paper, *On the Mode of Using the Signs + and − in Plane Geometry*, is a good example of his ability to take an apparently quite pedestrian elementary point of mathematics and illuminate it by explaining the various uses of the signs + and − in geometry with exemplary clarity. De Morgan summarizes these uses under ten headings which include the directions that arise in those situations where axes are given, and of projections. Examples he gives include AB + BC + CA = 0 – where AB denotes the *directed* segment AB, etc; also P°Q + Q°P = 0 – where the notation P°Q denotes the angle the line P makes with the line Q, etc. .

27. BOOLE TO DE MORGAN, 31 MARCH 1851

I have just now so much more to do than I can do well that I am unable to give to your question the careful consideration which I would otherwise most willingly do but I will just remark that it at present appears to me to be a question of definition. If I define a primitive equation of

$$f\left(x, y, \frac{dx}{dx}, \frac{d^2y}{dx^2}\right) = 0$$

to be such an equation

$$f_1\left(x, y, \frac{dy}{dx}\right) = 0$$

as that the equation $f = 0$ shall be a necessary consequence of the system $f_1 = 0$, $df_1/dx = 0$ [,] then the test of $f_1 = 0$ being a primitive of $f = 0$ is simply that the above condition shall be satisfied.

Now in the Universal Church of the Mathematicians to which you refer, the famous canon of Vincentius Lirinensis applies thus far, viz. that we are not wantonly to meddle with those definitions which *semper ubique et ab omnibus* have been agreed upon and accepted. And this I hold to be sound catholic doctrine.

If therefore you press your question I call upon you, although protesting against so unwarrantable an exercise of private judgment, to tell me what new sense you put upon the term primitive equation. You must not tell me that you mean 'ordinary or singular or both' but you must declare what is that common property of the ordinary & singular in virtue of which they are primitive.

[PS] Walsh in a few days.

28. BOOLE TO DE MORGAN, 22 APRIL 1851

I received your notice of Libri this morning and thank you for it. I will show it to De Vericour. But I now write to ask if you have received my account of Walsh which I forwarded to you about 10 days ago.

29. BOOLE TO DE MORGAN, 6 MAY 1851

I have looked at your paper [D 1851*b*] with some care & without venturing to give an absolute & final opinion upon the subject I may yet say that it appears to me to contain a true theory and I think an important one in some respects. I have met with other cases besides the general solutions of partial differential equations of which your theory reminds me. They were in connexion with the inverse problem of envelopes tangencies etc. of which I have obtained a general theory introducing in certain cases arbitrary functions of the constants of the problem. Now your functions entering under the sign ϕ are functions of the differential coefficients equivalent to constants. My paper has been completed for several months but is yet unpublished and if you would like to see it I will send it to you. I dont speak very positively about the analogy because I have not time to set about studying the question in earnest just now, but it struck me on reading & thinking over your letter. There is need however of a good deal of additional inquiry. E.g. How is it that such equations as

$$f\left(y'', xy''-y', \frac{x^2}{2}y''-xy'+y\right) = 0$$

taken in all their generality admit only of the one original primitive as their final integral an integral not involving any arbitrary functions or not made more general by them. I don't mention this as an objection for it is not one but it is a point worth looking into. Perhaps the solution

$$\chi(y-Hx^2-Kx-L) = 0$$

one of your solutions *may* be more general than

$$y-Hx^2-Kx-L = 0$$

and have some meaning with reference to singular points etc which the other has not.

All this is the mere result of impressions which the sending of your letter has produced. There is a special business in which I am engaged for our college just now that keeps me fully occupied in mind.

30. BOOLE TO DE MORGAN, 28 MAY 1851

I am now a little less busy than I was when I wrote to you last and will take the opportunity of looking again at the differential equations and if any thing new should occur to me I will write to you upon the subject – But at present I have nothing more to say than I said in my last.

Your paper on Signs [D 1851*d*] has just reached me. I had seen it before in the Cambridge Journal. I believe that the views are in essential points identical with some which I stated in a paper written many years ago and sent to the Journal while Gregory was editor but of which he never acknowledged the receipt. I have not preserved even the notes of it but remember getting in what seemed to me to be a very simple manner some of the principal formulae relative to the rotation of a solid body. It was in connexion with that problem that I had felt the necessity of fixing with accuracy the sense of positive & negative with reference to rotations. I think that I could recover some part of the matter if it were worth while which as you have taken up the question I do not think that it is. Of course I only mention this as a coincidence and not with the slightest view to any personal claim. And indeed I should now feel precluded from naming it any one but yourself. I would however advise you just to look into the mechanical application. I think it led to theorems of transformation for rotations similar to the

$$x = ax' + by' + cz'$$
$$\text{etc.} \qquad \text{etc.} \quad \text{etc.}$$

where $a = \cos xx'$ $b = \cos xy'$ etc. and to some useful applications of them dependent upon fixing the sense of rotation.* Something of this sort may have been done since but I am not well read in these matters.

When you write again say what you mean to do with Walsh. Perhaps you would think that I replied coldly to your proposal to publish it. But I thought that you might have written under the immediate impression of his strange story and that upon reflection your opinion might change. The question is whether the publication would do good or whether it would be interesting in a psychological point of view or not. Should you ever think that there are sufficient grounds for its publication I shall willingly consent to your doing so and shall not object to bear half the expense but I would not have you proceed to publication unless you are tolerably clear about the matter. A thing may be worth preserving which is not worth separate publication. I am very sorry to have given such offence to Mr Sylvester. I thought that I had not said one word that was not strictly true and even called for by the mode in which he announced his theorem and I really endeavoured to speak the truth in the manner least likely to wound him.

Letters 31–6

The mathematics contained in the letters of this section is primarily concerned with probability and differential equations. Without question the most

*E.g. the expression of pdt, qdt, rdt in term of $d\phi$, $d\psi$, $d\theta$ Poisson Mécanique vol. II, p.134. Poisson's reduction is very complex. [Boole's footnote] interesting matter is the discussion of Boole's general problem on probability in Letters 33–7 (July–August).

Another point about probability arises in Letter 31 (24 June), viz. 'the general doctrine among mathematicians concerning the probabilities of causes'. Boole refers to two notes he contributed to the *Philosophical Magazine* in June and August 1851 (Boole 1851*f,g*). These notes relate to Mitchell's problem of the distribution of the fixed stars (Mitchell 1767). Also he refers to 'a passage in the *Edinburgh Review*' – a review by J. Herschel of Quetelet 1846 which appeared in volume 92 (1850), 1–57, of that journal. Mitchell's problem concerns the question whether the observed distribution of stars is consistent with the hypothesis of their being randomly distributed. The 'general doctrine' is explained as follows: let p be the probability of the statement:

if the condition A has been satisfied, the event B has not happened. (1)

Now consider the statement:

if B has happened, the condition A has not been satisfied. (2)

The general doctrine, says Boole, asserts that the probability of (2) is p: not so, claims Boole, in fact the probability of (2) is

$$\frac{c(1-a)}{c(1-a)+a(1-p)},$$

where a is the probability of A being satisfied, and c the probability of B happening when A is not satisfied. The incorrect 'doctrine is explicitly maintained in a passage in the *Edinburgh Review*' where it appears in the context of mineralogy (page 32). In Letter 31 Boole also says the doctrine is 'I think strongly implied by Laplace'. He gives detailed reference to Laplace's implied use of the doctrine in Boole 1851*g*, where he quotes certain words appearing in the introduction of Laplace's 'Great work on Probabilities', presumably Laplace 1812.

In present-day notation the problem is an easy one:

$$\Pr(A'|B) = \Pr(A' \cap B)/\Pr B = (\Pr(B|A')/\Pr B$$

and $\Pr B = \Pr(B|A')\Pr A' + \Pr(B|A)\Pr A$.
Using p, c, a as defined above, $\Pr(A'|B) = c(1-a)/[c(1-a)+a(1-p)]$, as Boole says.

In Letter 31 (24 June) Boole is also at pains to make amends to De Morgan in that he thought that the latter, too, had subscribed to the erroneous general doctrine: 'I thought. . . it had your sanction but I find upon reconsidering the passage. . . that it has not.' Boole exonerated De Morgan from error on this matter in Boole 1851*g* also. In Letter 35 (4 August) Boole again refers to this paper, asking De Morgan to 'tell me whether you think that I have in any way misunderstood you. . . and it is now surprising to me how I could ever have mistaken your meaning.'

We turn now to the general problem which Boole stated in Letter 33 (24 July). The problem concerns n events A_i, $1 \leqslant i \leqslant n$, which occur with probability c_i, $1 \leqslant i \leqslant n$; each of these events may be the cause of an event E, with probability p_i, $1 \leqslant i \leqslant n$; required is the total probability of the event E. Boole asks De Morgan: (i) is any solution known to him? (ii) does he 'think it can be solved by... known methods or methods deducible from known science?' Finally Boole remarks that 'when you answer this I will tell you more particularly why I put the question to you.'

The reader conversant with modern probability may be asking whether the events E_i, $1 \leqslant i \leqslant n$, are to be regarded as independent or not. Evidently De Morgan made such a query in his reply for in the next letter (29 July) Boole insists: 'I mean that there should be no restrictions but what are explicitly stated... I apprehend that having given you the data it is not my business to give you hypotheses.' The *absence* of any explicit statement concerning the independence or dependence of the events E_i, $1 \leqslant i \leqslant n$, is crucial to understanding of Boole's thoughts on this problem. Boole remarks in the Letter 35 (4 August). 'The grand difficulty in the common theory is to know what hypotheses you may lawfully make and what you cannot' – i.e. precisely what relationship may hold between E_i, $1 \leqslant i \leqslant n$. In this letter Boole answers a letter from De Morgan containing an attempted solution; Boole wrote: 'I am also obliged to you for your solution... which however I think erroneous.' Its incorrectness is shown by Boole by a counterexample which indicates that when $n = 2, c_1 = c_2 = p, \; = p_2 = 1$ the resulting probability of E is 4/3!

It seems that De Morgan made a second attempt – but this gave Boole no more satisfaction than the previous one. In Letter 36 (11 August) Boole again writes out a statement of the problem with the comments: 'I think I should have done better not to have endeavored to answer your questions... but simply to have restated in clearer language the data and left you to analyse them yourself. The case is simply this... You must I think admit that the data are *clear* and *intelligible*. Don't you?' Again Boole is able to point out that De Morgan's attempted solution is wrong, and he concludes with a strong statement that if De Morgan cannot solve the problem either the 'ordinary principles' are insufficient or if they do suffice 'ordinary methods fail to direct us in their applications.' The final stage of these exchanges is reported in Letter 37 (25 August) where Boole says he 'has decided to send it to one of the journals as it appears to me to afford the most practicable and *fair* test which I know of the sufficiency of the received methods in probability. When it has appeared you may wish to try it again. If you do not I shall have no objection to communicate to you privately and in confidence (which I would not do to every body) the solution.' The surviving letters do not contain any indication of whether Boole did this, however.

Boole sent his problem to the *Cambridge and Dublin Mathematical Journal* where it appeared in November 1851 (Boole 1851*e*). He did not succeed in obtaining a solution, however. In a paper that appeared in the *Philosophical Magazine* of January 1854 (Boole 1854*b*), he stated: 'Several attempts at its solution have been forwarded to me, all of them by mathematicians of great eminence, all of them admitting of particular verification, yet differing from each other and from the truth.' This paper is Boole's answer to 'the only published [solution] I have seen' – by Arthur Cayley (Cayley 1853).

Boole and Cayley corresponded on the problem. The substance of their exchange of ideas is contained in Cayley 1889, vol.2, 594–6. Apart from references to Boole's and Cayley's papers, Cayley also, interestingly, refers to a paper by Dedekind (1855). There are other references to the problem in Cayley 1862*a*,*b* (= Cayley 1889, vol.5, 80–85).

Boole presented his own solution to the problem he had unavailingly put to his contemporaries in *Laws of Thought.* The problem with its solution appears as Problem VI of Chapter XX (Boole 1854*a*). Boole mentions that the publication of the problem failed to elicit a solution in *Laws of Thought*, although it 'led to some interesting correspondence' (336, footnote). The attentive reader may have noticed by now that Boole preferred to write 'the theory of probabilities' rather than 'the theory of probability'.

31. BOOLE TO DE MORGAN, 24 JUNE 1851

I have been travelling about lately or I should have written to you before.[1] I send under another cover a paper [B 1851*f*] of mine which you have probably seen before this time. I have sent to the same Journal a second paper [1851*g*] in which I have corrected a misstatement made in the first as to the general doctrine among mathematicians concerning the probabilities of causes. The doctrine I spoke of is explicitly maintained in a passage in the Edinburgh Review & is I think strongly implied by Laplace. I thought too though I made no special references that it had your sanction but I find upon reconsidering the passage at which I had looked that it has not. I have therefore in my new paper stated what the views of writers are on the subject so far as they are known to me and also what is in my opinion a true summary of the theory which you will perceive should the paper be printed to be partly taken from my former paper & partly from your treatise.

On my return to Lincoln about the 28th or 30th inst. I shall send you Walsh.

I hope you will look at my next paper [1851*g*?] & tell me if you agree with me. I hope & believe that you will do so. I have discussed one or two other points upon which from a former correspondence I think that we differ but in speaking of these I have not alluded to our correspondence at all.

32. BOOLE TO DE MORGAN, 16 JULY 1851

I should before now have answered your last letter if I had had anything more than *impressions* to send you about the mathematics of your last letter. But I have not. I think your views are just but I do not find that I can at present so far disengage my mind from other things as to enable me to study your demonstration with the care which would be necessary in order to make my opinion of the least value.

What has occupied me lately has been the theory of probabilities at which I have been working hard. The result is that I feel myself occupying a ground upon which it is not presumptuous to say these two things: 1st that I am sure that no *general* theory of probabilities can be established upon any other than a preliminary general theory of Logic, 2ndly that upon the principles stated in the little paper which I sent you in MS about two years ago I am sure that a perfectly general theory may be established.[2] I now include in this the theory of *observations*, least squares, etc. etc. and a great deal more which upon the most careful attention I believe to be quite beyond the scope or power of the received theory. Of course I wish you to retain the right of making every deduction for the infinite self-deceptions of authorship but I have examined with care very case that I have either been able to meet with or to think of and this is the conclusion to which I am irresistibly led.

I am glad that you have seen the bishop of Cork. I think his a very estimable person which is more than can be said for every body's bishop. I am glad too that you have seen Sir R. Kane but I was previously aware that you were acquainted.

[P.S.] If Taylor makes the least demur about Walsh do not urge him at all. For he must be supposed to know best what will suit his readers & he may say that he does not keep a psychological journal for chronicling the delusions of a quasi-insanity.

33. BOOLE TO DE MORGAN, 24 JULY 1851

Are you too busy to give half an hour's attention to the following question? If not you will confer on me an obligation.

You know well the solution of the problem in which are given the probabilities of certain exclusive & exhaustive causes $A_1\ A_2 \ldots A_n$ and the respective probabilities that an effect E will follow them taken singly, and in which is required the total probability of the effect E. If c_i is the probability of the cause A_i and p_i the probability that if that cause exists the event E will follow then on the assumptions

$$\text{Prob E} = c_1 p_1 + c_2 p_2 \ldots + c_n p_n.$$

Now *first*; Is there any solution of the problem when the causes are not assumed to be exclusive of each other? For example one observer attends only to the cause A_1 and the effect E, neglecting all account of the other causes and he finds that there is a probability c_1 that the cause A_1 will take place and a prob p_1 that the event E will follow that cause. Another observer attends similarly to the cause A_2 and consequent event and so on. But nothing is known as to the connexion of the causes. All which is known is that the event E cannot happen except from the causes singly or conjoined. And what is required is the total probability of the event E as a function of $c_1, c_2, \ldots, c_n, p_1, p_2, \ldots, p_n$.

Secondly supposing that the problem has not been solved (I *cannot here* refer for myself) do you think it can be solved by known methods or methods deducible from known science? I don't want you to spend much time over it but I am very much interested to know what your impression would be. I don't ask you this until I have anxiously considered the subject myself. The result is that I cannot solve it except by my own calculus. But I am not skilful in *combinations*, etc. I ought to add that the result does not look as if it could be got in that way.

When you answer this I will tell you more particularly why I put the question to you. You will see that I have a good reason for doing so.

P.S. I should have no objection to your asking any one else the question if you know any one who has been working in this direction. I have asked Mr Donkin.

34. BOOLE TO DE MORGAN, 19 JULY 1851

I mean that there should be no restrictions but what are explicitly stated. It might be better to use the word circumstances or events instead of causes. Thus there are n events $A_1\ A_2 \ldots A_n$ whose separate probabilities are $c_1\ c_2 \ldots c_n$ also p_i is the prob that if the event A_i happens the event E will happen, whether by any causal efficacy in the event A_i or not. In fact p_i is simply got by dividing the whole number of cases in which E has been observed to be connected with A_i by the whole number of times in which the event A_i has occurred. Further it is known that E cannot happen if all the events $A_1\ A_2 \ldots A_n$ fail. What is the prob of E.

I apprehend that having given you the data it is not my business to give you hypotheses. It is implied in the above statement that the events $A_1\ A_2 \ldots A_n$ are not mutually exclusive. There is no restriction on the mode of their happening which is to be the most general possible consistent with the values of $c_1\ c_2 \ldots c_n$. The event E I regard not as quantitative but as a simple phenomenon which either happens or does not happen.

I shall be greatly interested to know whether the above question is really amenable to ordinary treatment. My own impression would be that it is not.

P.S. You ask if one circumstance is regarded as hindering another when simultaneous with it. I suppose the data do not imply any such hindrance but the question is one which in applying my method I had no occasion to consider.

35. BOOLE TO DE MORGAN, 4 AUG. 1851

I am much obliged to you for your paper which contains other matters I perceive than were touched upon in our late correspondence. I am also obliged to you for your solution of the question in probabilities which however I think erroneous. For leaving v_1 & v_2 unrestricted as you say at the close of your letter we get

$$\text{Prob} = \frac{\int_0^1\int_0^{1-v_1} dv_1\,dv_2(c_1p_1 + c_2p_2 - c_1c_2(v_1+v_2))}{\int_0^1\int_0^{1-v_1} dv_1\,dv_2}$$

whence

$$\text{Prob} = c_1p_1 + c_2p_2 - \tfrac{2}{3}c_1c_2.$$

Now suppose $c_1 = c_2 = 1$, $p_1 = p_2 = 1$, it is evident that the solution to be 1 but your solution gives $1\frac{1}{3}$ which is unmeaning.

The grand difficulty in the common theory is to know what hypotheses you may lawfully make and what you cannot. And it is a difficulty which in 99 cases of 100 is quite insuperable. I may just mention that Mr Donkin gets

$$c_1p_1 + c_2p_2 - \alpha\beta$$

where α is the prob that both causes exist and β the prob that E exists if both causes exist. But he remarks that there are difficulties in framing the possible hypotheses as to the values of α and β which he does not see how to overcome. So far as he goes his solution appears to be consistent with yours. To your process my objections would be 1st that you assume that E whenever it occurs is distinctly referrible [sic] to *one* of the causes 2ndly that you assume v_1 & v_2 to be equally susceptible of all values within the limits of your integration.

I have thought of proposing the general problem which I regard as a fundamental one to the consideration of mathematicians not at all as a trial of personal skill but as a means of ascertaining the real power and limitations of the received theory of probabilities. Of course I take upon myself the responsibility for the correctness of my own views & of my own solution.

I am anxious that you should read my paper [B 1851*g*] in the Phil. Mag. for this month & tell me whether you think that I have in any way misunderstood you – for if I have I will endeavour to set all right. I really thought when I wrote about the syllogism that you had maintained the probability of the conclusion to be simply *pq* and omitted to notice the *possibility* of its truth on other grounds; and it is now surprising to me how I could ever have so mistaken your meaning. However I don't think that this mistake appears in my paper although certainly if I had discovered it sooner I should have omitted the passage altogether.

P.S. There is a copy of the Théorie Analytique des Probabilités of Laplace advertised in Lumley's Catalogue for 12/0. It is the edition of 1820 avec les trois supplements. Does it contain as much as the last edition. What is the price of Poisson's work?

36. BOOLE TO DE MORGAN, 11 AUG. 1851

I think I should have done better not to have endeavoured to answer your questions about the connexion of the elements in my problem but simply to have restated in clearer language the data and left you to analyse them yourself. The case you have to consider is simply this. The probability of an event A_1 is c_1 that of an event A_2 is c_2. The prob that if A_1 happens an event E happens is p_1 and the prob. that if A_2 happens E happens is p_2. Finally E cannot happen unless one or both of the events A_1 A_2 happen. Required the prob of E. You must I think admit that the data are *clear* and *intelligible.* Don't you? Now I have nothing to add to them, – no hypothesis to give you. You are from these data alone to solve this problem *on the principles of the theory of probabilities.*

With reference to your first solution I have to remark that the limits of integration which I employed were those given by yourself. With respect to the second solution (received on Saturday) although I do not quite understand the principle upon which it is obtained I can show that the result is erroneous. E.g. Try $p_2 = 0$ the others are not vanishing.[3] But it is sufficient to ask you this question viz. whether the hypothesis which you adopt and which you think that I in some of my explanations have sanctioned is a legitimate consequence of the data stated above *on the principles of the mathematical theory of probabilities.*

I have nowhere seen the fundamental positions of the theory better stated than in your little book on probabilities [D 1838*a*] (Lardner's C.C.)[4] into which I looked for the first time the other day, and in a paper [1851] by Mr Donkin in the May no. of the Phil. Mag. Whenever the data of a question are the probabilities of simple events there is no difficulty in applying those principles. But when the data have reference to compound events it is generally all but impossible to apply them without the aid of a logical calculus. There are certainly no hypotheses involved in my method if by hypotheses we mean something not necessitated by the principles of the theory of probabilities and the fundamental laws of reasoning.

If you do not solve the problem, as it stands, you must I conceive be brought to one of these two alternatives: 1st that the problem in insoluble on ordinary principles without inventing *new* hypotheses, in which case I shall be at issue with you 2nd that if ordinary principles do suffice ordinary methods fail to direct us in their application.

P.S. My best compliments to Mrs De Morgan.

Letters 37–41

The letters of the last months of 1851 are less mathematical in content, but abound with references to literature. In Letter 37 (25 August) there is a mention of the award of a Royal Medal of the Royal Society to Boole and in Letter 40 (17 November) of the publication of a lecture by him, *The Claims of Science* (Boole 1851*a*).

In the nine years since their correspondence began Boole has developed a freer and somewhat lighter epistolary style. This is particularly evident in Letter 38 (9 September), where Boole says: 'Mr Dickens has described the body of his hero Quilp as finding its last resting place. . .'. Daniel Quilp, who is a villian rather than a hero, appears in Dicken's *The Old Curiosity Shop* and Quilp's death occurs in Chapter 67. Also Boole quotes the phrase 'fool'd by words' but gives no further indication of the source of these words. This is perhaps a recollection of Wordsworth's line

> Slaves, vile as ever were befooled by words,

from no. XXIV of the *Poems dedicated to National Independence & Liberty,* 1837.

These literary allusions arise as a result of a curious misapprehension on Boole's part. It appears that he did not know of the Kent coastal resort of

Broadstairs to which De Morgan had referred. The story of this mistake of Boole's was told by De Morgan in a letter to W. Heald, 11 Sept., 1852:

> 'They [Mrs De Morgan and their children] have gone this year to Herne Bay – not so far from London as last year, when they were at Broadstairs. By the way a scientific friend of mine directed to me at Broadstairs, near London, when near Ramsgate would have been nearer the mark. On my asking him what he meant, he said he remembered some very broad stairs down to the river just below London Bridge, and he had a vague idea that they were the Broad stairs.
>
> (S.E. De Morgan 1882, 218)

In letter 41 (28 November) Boole thanks De Morgan for 'the very curious paper about Newton and Leibnitz'. De Morgan wrote several papers on the controversy concerning the question of priority between Newton and Leibniz on the invention of the calculus. Indeed, De Morgan was the first British scholar to point out the injustice done to Leibniz both by Newton's contemporaries and later historians. To which paper Boole here refers is not clear; it may be either *On a point connected with the dispute between Keill and Leibnitz about the invention of fluxions* (1846*b*), or *On the Additions made to the Second Edition of the Commercium Epistolicum* (1848*d*). The latter is the more likely, except that Boole acknowledges receipt of this explicitly in Letter 42 on 28 June 1852, i.e. six months later. However, there is another possibility, as De Morgan's contribution to the *Companion to the Almanac* for 1852 (1852*f*) entitled *A short account of some recent discoveries in England and Germany relative to the controversy on the invention of fluxions* would also be in print by this time. We may note that Boole's first publication was an address to the Lincoln Mechanics Institute: *An address on the Genius and Discoveries of Sir Isaac Newton* (Boole 1835).

37. BOOLE TO DE MORGAN, 25 AUG. 1851

> That you may be saved without knowing or believing any thing about the distribution of the Royal Society's medals is a truth as indisputable as it is consolatory.[5] And therefore whatever popes and councils may say you may relieve yourself of any apprehension upon that head. However to gratify your curiosity (though I cannot but condemn it as a useless prying into nonessentials) I have no objection to tell you that the RS did give me a medal and further that Dr M[arshall] H[all][5] did once when I had occasion to consult him tell me something of the kind which you mention in your letter and offer to give me a full history of the whole affair either then or at any future time with names etc. I declined the information with thanks and should do so again were the offer repeated for although I was grateful to the doctor for his good intentions towards me I thought that the knowledge which I might get might make me think ill of some one of whom I either thought well or thought not at all and so the gain would be a loss. To poor Mr Davies I am grateful for the part he is said to have taken.[5]

As to the question, I have decided upon sending it to one of the Journals [B 1851*e*] as it appears to me to afford the most practicable and *fair* test which I know of the sufficiency of the received methods in probabilities. When it has appeared you may wish to try it again. If you do not I shall have no objection to communicate to you privately and in confidence (which I would not do to every body) the solution.

Hoping that you may enjoy your aquatic musings in which I am also thinking of indulging myself.

38. BOOLE TO DE MORGAN, 9 SEPT. 1851

I have sent the proof [of Boole 1851*h*] corrected to Taylor this day & have taken upon myself to order 50 separate copies, 25 for myself & the rest for you. If you do not care to have any will you write to the Editor & say so. The cost for 50 is only 1/6 more than for 25.

I really did not know the locality of Broadstairs now so famous as 'our watering place'. To tell you the truth I connected it in imagination with some of those innumerable flights of *broad stairs* which one sees from the steamers coming down to the Thames, and if I had any more particular conception of its whereabouts it was that it was not far of from that tidewash'd isle upon which Mr. Dickens has described the body of his hero Quilp as finding its last resting place, somewhere between Thames a river and Thames an estuary. Thus are we 'fool'd by words', in the first instance, & left to the play of a vagrant imagination afterwards spite of all the nonsense that is talked about Mathematics curbing the licence of fancy etc. etc. etc.

39. BOOLE TO DE MORGAN, 10 SEPT. 1851

One of Walsh's titles of publications quoted in the memoir of him is 'Trinity College Notes of a Mathematical controversy between the Rev. F. Sadleir Provost the Rev. Mr Luby and the Rev. Dr O'Brien Fellows of the College and John Walsh Author of the Geometric Base' [Walsh.]. It has struck me since sending off the proof yesterday that it might be disagreeable to some of the above to have their names publicly mentioned. There can no doubt of my perfect right to publish the title in full but I am not equally clear whether it would precisely be doing as one would be done by. And as I am connected with a University which is the rival of theirs a more than ordinary carefulness to avoid giving pain is incumbent upon me. Very likely I am only creating a shadow and I would therefore ask you who know more of these matters to consider what would be best. If an alteration is made it should be the substitution of an 'etc' for all that follows 'Controversy'.

Will you if you think that there is ground for the omission forward this note or its substance to Taylor immediately.

40. BOOLE TO DE MORGAN, 17 NOV. 1851

Will you oblige me by asking Messrs Taylor and Walton if they will print and publish for me a lecture [The Claims of Science, B 1851*a*] which I delivered at the opening of the College Session here and which some of those who heard it including my colleagues are desirous of seeing in print. I publish the lecture at my own expense and fees here being few and small, I should prefer that the lecture should be printed in a not very expensive form. I took considerable pains over the *matter* of the lecture thinking while I wrote it that it was possible I might be called upon to publish it, beside which I felt as I have always done unwilling to labor [sic] for a merely temporary object. This I mention that you may think that it is solely on account of a few compliments that I design to publish the lecture though at the same time it is possible that I may have misjudged the fitness of the matter for publication.

I have been looking with interest at your paper [D 1851*e*] on evolutes in space.

[P.S.] I directed the Ed. of the Phil. Mag. to send to me the account for Walsh.

41. BOOLE TO DE MORGAN, 28 NOV. 1851

I have desired Taylor to send you the account for Walsh together with a former 6/6 of mine due for another set. Will you settle both and then let me know how much is due to you viz. the moity of Walsh with the 6/6. Thank you for the very curious paper [D 1848*d*] about Newton and Leibnitz. I have sent my lecture to Taylor and Walton.

Notes

[1] This letter is addressed from: Mrs Knight's, 13 Surrey St., Strand. Perhaps Boole had visited the Great Exhibition – which opened on 1 May.
[2] See Letter 17.
[3] Boole inserted the sentence as a footnote.
[4] 'Lardner's C.C.' refers to Lardner's *Cabinet Cyclopaedia*, the 133 volumes of which appeared between 1829 and 1849. De Morgan's *Essay on Probabilities*, 1838, was volume 107 of this work.
[5] Boole is referring to the Royal medal he received for Boole 1844*a*. Boole first wrote Marshall Hall, then crossed through all but the initial letters; however, the name is still legible. Davies is probably T.S. Davies, 1795–1851.

4
THE LAWS OF THOUGHT AND MARRIAGE: 1852-1856

In this chapter all the letters but two are from Boole; only in 1856 do we have letters from De Morgan again. One wonders why his letters from the earlier and later periods should have survived, but none between June 1850 and January 1856.

Boole's activity in this period was centred around the preparation, and publication in 1854, of his book *An Investigation of the Laws of Thought* (Boole 1854*a*). In this work Boole recapitulates the ideas on the algebraic formulation of logic put forward in his earlier book; also there is much on the basic principles of the theory of probabilities. In Letter 42 (28 June 1852) we read: 'I am now about to prepare for the press my long talked-of papers on Logic and Probabilities'. And in Letter 44 (23 July 1852): 'I have something like 500 pages in MS which however I am going to recast before publication'. Letters 45, 46, and 47 (September–December 1852) of Boole refer briefly to making arrangements for the publication of the book. Letter 49 (15 February 1854) contains a correction that Boole discovered was necessary just before publication; he managed to get the correction printed in the book following the table of contents. Letter 50 (23 February 1854) is evidently Boole's reply to a congratulatory letter from De Morgan. De Morgan appears to have had some doubts on certain points; but Boole's reply expresses notable confidence in the correctness of his work: 'Satisfy yourself on this point – whether the solutions my principle gives are ever false. If you find one instance in which they are I give it up . . . I don't think any man's mind ever was imbued with a more earnest desire to find out the truth and say it and nothing else . . .'. Although we do not have any letters from De Morgan to Boole at this time, De Morgan, writing to W.R. Hamilton on 5 October 1852 referring to Boole, said: 'I shall be very glad to see his work out, for he has, I think, got hold of the true connexion of algebra and logic.' (Graves 1882, vol. 3, 421–2.)

Again we find several letters in which Boole shows some dissatisfaction with his position in Cork and his hopes of moving to a place which he may find more remunerative and congenial. In Letter 51 (30 May 1854) he refers to financial difficulties, but also says of Cork that he has 'become attached to the place and to some of the people.' Nevertheless he considered the possibility of going to

Australia to take a foundation chair in the University of Melbourne. In Letter 54 (3 February 1855) he says: 'I am so out of the way here that all chance of making any further advance is cut off unless I take some opportunity like this of letting it be known that I should be glad to do more than I am doing'.

However, a change in his bachelor status was in the offing: in September 1855 Boole married Mary Everest. He met his future wife when she was visiting her uncle, John Ryall, the Vice-president of Queen's College Cork. Another uncle was Colonel Everest of the Indian Survey, after whom the mountain was named.

Several letters throw light upon the personality and views of Boole. In Letter 42 (28 June 1852) we note his humanity in asking if De Morgan could help 'a widow, struggling to bring up her children in London'. Letter 47 (8 December 1852) with his reference to the Hymn of Hildebert shows his breadth of knowledge – the sacred poetry of the eleventh and twelfth centuries is surely not reading matter that one would expect a mathematician and logician who was self-educated to indulge in. Boole also refers to the Hymn of Hildebert in a footnote of the last chapter of *Laws of Thought,* page 415. The footnote indicates that the book in which he read this hymn was Trench 1849.

Letters 42–50

The mathematical topics raised in these letters are again mainly concerned with probability. However, parts of Letters 42 and 48 indicate De Morgan's historical interests.

In Letter 42 (28 June 1852) Boole thanks De Morgan for his paper on the *Commercium Epistolicum* – the report of the committee set up by the Royal Society to investigate the priority dispute regarding Newton and Leibniz and the discovery of the calculus. The members of this committee were predominantly Newton's friends and the report itself is strongly biassed in favour of Newton. The first edition was published in 1712, and a second edition in 1722; the latter was reprinted in 1725. The second edition purports to be a reprint of the first, prefaced by some new matter. The substance of De Morgan's paper was that that part which was ostensibly a reprint of the first edition had in fact been altered in various places – De Morgan gives full details of the additions or changes – in such a way as to make the anti-Leibniz bias more explicit and condemnatory. De Morgan concludes: 'The more the whole matter is looked into from its beginning to its end, the more will the evidence of reckless injustice thicken about the enquirer.' Boole's comments show he had 'read a letter of his [Newton's] some time ago giving advice to a young friend about to travel and I thought it full of the spirit of a cold and calculating prudence.' No doubt Boole here refers to Newton's letter of 18 May, 1669, probably to Aston; Boole would have read this in Rigaud 1841, vol. 2, 292–5. It also appears in Newton 1959, 9–13. His judgement 'cold and calculating' of this letter is most apt.

Probability is discussed by Boole in Letters 43 (12 July 1852) and 44 (23 July 1852). In the first of these Boole answers a question raised by De Morgan by referring him to 'the paper which I sent to you three years ago and which contains all this part of the theory and a good deal more'. This paper must, I think, be that referred to in Letters 17–19 and discussed in Chapter 2. But, as Boole indicates in the last paragraph, he has improved upon the methods of that paper: 'The modes of solution which I now employ are also considerably different from those of the paper . . .'. The major part of this letter is an account of 'an example of a solution got by me the other day'. This method was to form a part of Chapter 19 of *Laws of Thought* (see in particular pages 316–18). In brief, Boole's method was the following: there is a collection of events, $x_1, x_2, \ldots, x_n$. Concerning these events Boole takes separately (i) the data – a collection of statements of the probabilities of certain combinations of the events; (ii) the *quaesitum* – the probability of a certain event which is expressed in terms of $x_1, x_2, \ldots, x_n$. From (i) Boole derives what he calls 'the fundamental central equation', and he shows that there is a unique root, λ, that satisfies limitations imposed by the given data. From (ii) Boole derives a function ψ; the solution to the problem is then given by $\psi(\lambda)$.

As an example Boole takes as data the statements: p_i is the probability that x_i occurs, or all the events $x_1, x_2, \ldots, x_n$ fail for $1 \leqslant i \leqslant n$. As *quaesitum*, the probability of the event x_1. He then shows (in summary in the letter, in more detail in Chapter 19 of *Laws of Thought*) that the 'fundamental central equation' is

$$1 - \lambda = (1 - p_1\lambda) \ldots (1 - p_n\lambda) \qquad [3]$$

which he shows has only one (positive) root, λ, satisfying $0 \leqslant \lambda \leqslant 1/p_1$ (he assumes $p_1 \geqslant p_2 \geqslant \ldots \geqslant p_n$). The function ψ is given by

$$\psi(\lambda) = p_1 - (1-c)p_1 p_2 \ldots p_n \lambda^{n-1}, \qquad [2]$$

where c is an arbitrary number satisfying $0 \leqslant c \leqslant 1$ – but, Boole adds, 'The method gives the interpretation of c and informs us what new observations are necessary to determine it'.

The type of problem considered by Boole – in particular the absence of any explicit statement regarding dependence or independence of the events – has a solution which generally contains one or more unknown parameters. Such parameters may have to satisfy certain inequalities. Consequently, a modern approach to such problems can usefully adopt the techniques of linear programming. In recent work T. Halperin has examined Boole's logic and probability in this light (Halperin 1976).

The last eight lines of the first paragraph of letter 44 (23 July 1852) concern what Boole was to call 'a perfect method' in *Laws of Thought* (Boole 1854*a*, Ch. X). The substance of this method is contained in Proposition 1 of Chapter X (1854*a*, 151): 'To reduce any equation among logical symbols to the form $V = 0$, in which V satisfy the law of duality $V(1 - V) = 0$.' In the letter Boole observes that any equations satisfying this law may be added together yielding an equation which still satisfies the law. If the separate equations are $v = 0$, $v' = 0$, $v'' = 0$, etc. and v, v', v'' etc., satisfy the conditions $v(1 - v) = 0$, $v'(1 - v') = 0$, etc., then $V = v + v' + v''$ etc., also satisfies $V(1 - V) = 0$; for we can write

$$V = v + (1 - v)v' + (1 - v)(1 - v')v'' + \text{etc.}, \qquad (*)$$

and it is easy to see this satisfies $V(1 - V) = 0$.

This technique used in writing V in the form (*) is exactly that used in set theory when one has a denumerable collection of sets $\{A_i\}_1^\infty$, and wishes to express their union $\cup_1^\infty A_i$ as a union of pairwise disjoint sets: one writes

$$\cup_1^\infty A_i = A_1 \cup (A_2 \backslash A_1) \cup (A_3 \backslash \cup_1^2 A_i) \cup (A_4 \backslash (\cup_1^3 A_i) \cup \ldots$$

The concluding paragraph of this letter contains an interesting remark: 'I have long been trying to get at the principle of a suspected connexion between the results of my methods and those of integration', which suggests that Boole may have had some inkling of the common ground of probability and integration which later became explicit in measure theory.

Letter 48 (7 February 1854) contains Boole's answer to an enquiry from De Morgan about 'young Murphy'. Robert Murphy, 1806–43, was the son of a shoemaker in Mallow, who showed great ability in mathematics as a boy. A Mr Mulcahy, a tutor of Cork, heard of his ability and money was raised to enable him to go to Cambridge. He was 3rd Wrangler in 1829, and became Dean of his college (Caius) in 1831. He fell into dissipated habits, left Cambridge in debt in 1832, afterwards living in London until his death in 1843. De Morgan wrote a biographical notice of him in Volume 2 of the (first) supplement to the *Penny Cyclopaedia*, 337–8.

42. BOOLE TO DE MORGAN, 28 JUNE 1852

I must thank you for your paper on the Commercium Epistolicum [D 1852*d*] which I have read. I am afraid that it is only upon an intellectual throne that Newton must sit. I read a letter of his some time ago giving advice to a young friend about to travel & I thought it full of the spirit of a cold & calculating prudence.

I am now about to prepare for the press my long talked of papers on Logic & Probabilities. The medium I have not fully determined upon. As an application of Mathematics the probabilities will I think appear to you the most remarkable that I have made. I was told a few days ago that a work on the laws of thought (mathematical) has been presented to the French Academy by a M. Courtois (or some such name). Do you know anything about it![1]

I enclose a circular from a very deserving lady whom I have known many years and who has recently been left a widow, struggling to bring up her children in London. Should you think my recommendation sufficient a word from you to any one residing in her neighbourhood might be of service to her, & would oblige me.

43. BOOLE TO DE MORGAN, 12 JULY 1852

The particular cases you discuss come under this general theorom stated in my papers on probabilities viz. If $\phi(x\ y\ z) = 1$ be the logical equation expressing the occurrence of some particular combination of the events $x, y, z \ldots$ and if p, q, r be the respective probabilities of these events $\phi(p\ q\ r)$ is the prob of the compound event above described.

I enclose you again the paper which I sent to you three years ago and which contains all this part of the theory and a good deal more. You will see from it some thing of the spirit of my method. It occurred to me when you returned the paper that you had not had time to read it *carefully*. Mind I don't want you to read it now, unless you care to do so but I think it better at once to send you the paper which I do not want than to give a necessarily more imperfect account of the theory as applied to your examples by letter.

Here is an example of a solution got by me the other day. The following particulars are known respecting n events $x_1, x_2 \ldots x_n$

1st The probability that either x_1 occurs or all the events fail is p_1

2nd The prob that x_2 occurs or all fail is p_2 & so on

What is the prob that any particular combination $\phi(x_1\ x_2 \ldots x_n)$ of the events $x_1\ x_2 \ldots x_n$ will occur.

The solution is in all cases of the form

$$\psi(\lambda)$$

ψ being a functional symbol dependent upon $\phi(x_1\ x_2 \ldots x_n)$ and known when $\phi(x_1\ x_2 \ldots x_n)$ is given in form; λ is a root of the equation

$$1-\lambda = (1-p_1\lambda)(1-p_2\lambda)\ldots(1-p_n\lambda)$$

Here you will ask how I know what root. The method itself tells me that it must be that root of the above equation which is less than each of the quantities $\frac{1}{p_1}, \frac{1}{p_2} \ldots \frac{1}{p_n}$, it further informs me that $p_1\ p_2 \ldots p_n$ must be subject to the condition

$$p_1 + p_2 \ldots p_n \;\overline{\overline{>}}\; 1$$

in order that the problem may be a real one.

If the combination whose prob is required is the following viz. the occurrence of some one and only one event of the series, the function $\psi(\lambda)$ reduces to the form

$$\frac{p_1\lambda(1-p_2\lambda)\ldots(1-p_n\lambda)\ldots+p_n\lambda(1-p_1\lambda)\ldots(1-p_{n-1}\lambda)}{\lambda} \qquad [1]^2$$

If the quaesitum is the prob of the event x_1, $\psi(\lambda)$ becomes

$$\frac{p_1\lambda-(1-c)p_1p_2[\ldots]p_n\lambda^n}{\lambda} \qquad [2]$$

c being an arbitrary constant admitting of any value from 0 to 1. The method gives the interpretation of c and informs us what new observations are necessary to determine it.

I have proved that the equation

$$1-\lambda = (1-p_1\lambda)\dots(1-p_n\lambda) \qquad [3]$$

has but one root which satisfies the conditions required when $p_1\ p_2 \dots p_n$ are fractions satisfying the condition

$$p_1 + p_2 \dots + p_n \overline{\overline{>}}\ 1.$$

This will give you an idea of what the method will do. There is always a central fundamental equation [i.e. [3]] depending on the *data* alone & *independent of the quaesitum.* The method gives this equation, & the general form of the function of its root expressing the quaesitum, which form becomes determinate when the nature of the quaesitum is known. The method gives the conditions for limiting the root & it assigns the requisite conditions among the constants of the data so that if probabilities were given which experience could not furnish it would detect the imposition. Finally it determines the nature of the experience necessary for fixing the values of the arbitrary constants if there are any. It is applicable to all sorts of problems.

By far the greatest difficulties I have had, have been in proving that the algebraic equations for λ have had one root and only one within the limits assigned by the method. But I have always found that such is the case.

Now I don't way [sic] to trouble you in any egotistical way but this seems the best answer to your letter. You will not find all that I have said in the paper which I enclose but you will find enough to satisfy you of the possibility of the higher matters which I have mentioned. The modes of solution which I now employ are also considerably different from those in the paper chiefly in this respect that I am able to dispense with arbitrary constants $c_1\ c_2$ & in the *logical* part of the solution & that the whole is more symmetrical.

P.S. In apology for writing so late I must mention that I have been absent on unavoidable business.

44. BOOLE TO DE MORGAN, 23 JULY 1852

Your letter followed me here[3] where I purpose to stay about a week longer. I am glad that you propose to keep the paper, which I shall not want again I believe, certainly not at present, and I would ask you to look particularly at the rule of elimination which it contains for logical symbols as well as at the probabilities, with this view that you may be able to certify should it be required, that I was in possession of the rule some three years ago. For it is really the turning point upon which all the higher applications of the method depend. As to systems of propositions I will only just say that my present practice is to reduce all the equations in a system separately to the form $V = 0$ in which V satisfies the law

$$V(1-V) = 0$$

and then any of the equations may be added together without arbitrary multipliers & the result will be equivalent to the equations thus added.

I agree with you fully that the laws of the symbols are independent in some degree of the psychological views of different minds. Nevertheless I feel convinced that my use of *time* is the right one. However I may get up to London before long & if so I should like to talk to you about any of these things. I have something like 500 pages in MS which however I am going to recast before publication.

What you say about the origin of the [illegible] from the principle of means is very curious. Something of the kind has occurred to me and I have long been trying to get at the principle of a suspected connexion between the results of my method & those in integration.

45. BOOLE TO DE MORGAN, 27 SEPT. 1852

I am going up to London tomorrow to make arrangements for the publication of my book on Logic and Probabilities [B 1854*a*]. It has occurred to me that you may be able to give me some information which may be useful and accordingly I shall in a day or two probably write to you again.[4]

46. BOOLE TO DE MORGAN, 8 OCT. 1852

I do not remember leaving anything at my landlady's,[5] but think that the book you mention may have been sent there for my sister by a friend of hers without my knowledge. Perhaps you will be so good as to send it & I will remit the cost. If you find it inconvenient to do so let it remain for the present.

I set out for Ireland in a day or two. I feel doubtful whether I shall be able to remain there long as I am never well when in Cork – the damp is so excessive.

As to my book I shall in a week or two get estimates for it. There would be some advantages in employing Gill I think.[6]

Kant's argument from the *Prolegomena* is quite inapplicable. It is certainly as impossible to prove the purely objective character of Space. It is just like the old dispute about the reality of an external world. I do think that when we know all the scientific laws of the mind we shall be in a better position for a judgment on its metaphysical questions – of which Kant's is one.

I will write soon after getting to Ireland & will not forget Mrs De Morgan.[7]

Let me thank you for the trouble you have taken.

47. BOOLE TO DE MORGAN, 8 DEC 1852

I am sorry that I have not been able conveniently to send Mrs De Morgan before this time the Latin Hymn of Hildebert which I enclose – so much of it at least (for it is very long) as is likely to interest her. It contains a very good summary of the scholastic notions about the Deity in the first portion, & the conclusion is really very beautiful. When I returned to Cork I found that I had lent the book containing it to a friend and I did not like immediately to ask for it. I ought perhaps to have written to say this but

Mrs De Morgan is I hope lenient to the failings of mathematicians – not that I mean to imply that in the circle of domestic life she has need to exercise this particular form of the great duty of charity.

I have agreed with Gill to print my book and hope to get a good deal of the MS to press before the end of the year. I have chosen a tinted paper some thing like yours and hope that our joint example may do something to reform the public taste in this matter.

De Vericour tells me that he saw six months ago a notice of a report to the Institute by Cournot on some paper on Mathematical logic. I mentioned this to you[8] but did not remember then the name Cournot.

48. BOOLE TO DE MORGAN, 7 FEB. 1854

I cannot directly learn what you wish but believe it probable that Dr Mulcahy's father who was a very able teacher of mathematics here discovered the genius of young Murphy. I have however heard that one of the fellows of T[rinity] C[ollege] D[ublin] dining with the rector of Mallow examined him and gave him a college paper one of the more difficult problems in which Murphy asked leave to take home but found out the solution on his way and returned in haste and solved it. I can I think get the name of the Fellow.

[P.S.] I think you pervert Shakspeare[9] who had certainly a soul above proofsheets. And it is very well that he had for otherwise where would be the glory of the critics who restore readings that never existed in his text & of the philosophers who search out hidden meanings in his very mistakes. I must further remark that one of your verses is defective in its number which is not creditable to a professor of mathematics.

49. BOOLE TO DE MORGAN, 15 FEB. 1854

If you are reading my theory of probabilities [*Laws of Thought*, B 1854*a*] I would wish you to interpret 'absolute probabilities' in Prop II p. 261 as the probabilities which the events x, y, z ought to have in order that if regarded as independent and as furnishing our only data the probabilities of the same events under the condition assigned should be p, q, r and interpret the problem of the urn accordingly. The solution of that problem *as it stands* would be $p' = cp$ $q' = cq$, c being the arbitrary probability of the condition of a white marble, or white not-marble, or marble not-white ball being drawn. The solution

$$p' = \frac{p+q-1}{q} \qquad q' = \frac{p+q-1}{p}$$

gives what the probabilities of a white and a marble ball *ought to be in order that regarded as independent and as our only data the probabilities of the same event under the conditions should be p, q.*

This does not affect the principle of the general demonstration in Prop IV which is the following. By the logical reduction the solution of all questions is reduced to a form in which the data are the probabilities of simple events $s, t \ldots$ under a given condition V and the quaesitum the prob. of a definite combination of those events *under the same condition.*

It is then affirmed that probability must be calculated as if the events s t were independent and possessed of such probabilities as would cause the probabilities of the same events under the condition V to be such as they are assigned to be in the data.

This principle is certainly correct. I have seen its ramification in hundreds of instances though it occurred to me in the first instance as axiomatic and I hold it be so.

E.g. If the probabilities of a white ball w and a marble ball m under the condition $wm + w + m + \overline{1 - wm}$ are p, q what is the probability of wm under the same condition.

Here Prob wm under the condition $\left.\right\} = \dfrac{p'q'}{p'q' + p'\overline{1-q'} + q'\overline{1-p'}}$

p' and q' being determined by

$$\frac{p'}{p'q' + p'\overline{1-q'} + q'\overline{1-p'}} = p$$

$$\frac{q'}{p'q' + p'\overline{1-q'} + q'\overline{1-p'}} = q$$

whence $\quad p' = \dfrac{p+q-1}{q} \qquad q' = \dfrac{p+q-1}{p}$

and substituting

Prob wm under the condition $= p + q - 1$

as is easily verified.

P.S. I have had a very kind letter of thanks from Sir J. Herschel.

50. BOOLE TO DE MORGAN, 23 FEB. 1854

I am much gratified with your letter and not surprised at the difficulty you mention about the probabilities. But the principle to which you object whether axiomatic or not is certainly *true*. You will be convinced of this when you have read further on. You will also see that I *have* given the general theory of quantification Chap XIX and further that it is connected with the principle above mentioned – serving to determine the limits of the roots of the equations furnished by that principle. I will write a short tract on the laws of *expectation* & send you and I think remove your objections. But at any rate satisfy your self on this point – whether the solutions my principle gives are ever false. If you find one instance in which they are I give it up. Are you satisfied with this declaration? I am sure if there is any quality that I think you have in preeminence it is integrity in the pursuit of truth – but that is a quality in which I should be sorry to think myself your inferior. I don't think any man's mind ever was imbued with a more earnest desire to find out the truth and say it and nothing else, than mine was while writing that book. And the very consciousness of this would make it *not* painful to me to give up half my book if it were proved to be unfounded. However what I now ask of you both as a friend of truth & of me is to examine the questions fully – to settle it in your mind to make out whether I am right or wrong.

Do you now admit the validity of my theory of Secondary Propositions and their connexion with *Time*? With sincerest thanks.

Letters 51–9

The matters raised in these letters are predominantly personal – concerning Boole's thoughts of leaving Cork and his marriage. There are passing references to mathematical topics including differential equations, the terminology relating to the theory of invariants and a minor point of spherical trigonometry.

Letters 51 and 52 (May 1854) indicate that Boole had thoughts of leaving Ireland to go to Australia. Melbourne University was set up by an act of the Victorian Parliament of 1853. In January 1854 the Chancellor of the University, Redmond Barry, who was the Puisne Judge in Victoria, wrote to a committee of five persons, putting in their hands the task of selecting four foundation professors. The committee included John Herschel, G.B. Airy, and R. Lowe. Barry's letter included the following observations to aid the committee in their task of selecting professors:

> It is considered expedient that the persons whom you may deem eligible for the office be men not in holy orders, of approved worth and moral standing, and of such stability of character as to command respect. . . . A devotedness on the part of those selected to the cause of literature and the interests of the University is deemed to be of great moment . . . And these general suggestions being submitted, it will be considered desirable that the Professors should be men under the middle age, of approved diligence in literary pursuits, graduates of one of the Universities of Oxford, Cambridge, London, Dublin, Edinburgh, or Glasgow, and designated by some particular excellence in their collegiate career, accustomed, if possible, to the inculcation of knowledge (with clearness and readiness) in the department to which they propose to apply themselves, and, more especially, of such habits and manners as to stamp on their future pupils the character of the level, well-bred, English gentleman. (Melbourne 1854, 8, 9)

The committee received applications from 90 persons for the four chairs (Scott 1936, 23), and 'on August 14th we agreed on Wilson, Rowe, McCoy and Hearn' (Airy 1896, 220). The successful applicant for the chair of mathematics, W.R. Wilson, was professor of mathematics at the Queen's College, Belfast. Two other of the four foundation professors also came from the Queen's Colleges – McCoy from Belfast and Hearn from Galway (Scott 1936, 23).

In Letter 52 we find that Boole had 'given up all thought of Melbourne'. It is unfortunate that Boole forgot to put the date on this letter. However, a comparison of the types of notepaper he used in 1854 and 1855 suggests that the letter was written soon after the previous one. So no change of locale came about and he himself seems to have recognized that a move was unlikely. In Letter 51 (30 May 1854) he says: 'I begin to feel that this is a wish of which it is not the design of Providence that I should attain the fulfilment'.

In Letters 53 (3 January 1855) and 55 (21 February 1855) Boole refers to some alternatives for the word 'determinant'. The word 'determinans' was introduced by Gauss for the discriminant of a quadratic form in *Disquisitiones Arithmaticae* (Gauss 1863, Bd. 1, 121). Boole 'had employed the term "final derivative" for what has since been called the determinant' – in Boole 1843*c* in fact. Boole mentions some terms introduced by Cayley in his work on invariants. Cayley used 'hyperdeterminant' to denote an invariant in 1845, and 'hyperdeterminant derivative' in 1846. 'Quantic' was introduced by Cayley in 1854 to denote a homogeneous algebraic form.

This discussion of terminology is a reminder that Boole was one of the originators of the theory of invariants. He wrote several papers on linear transformations in which the idea of invariants arises (see Boole 1841, 1844*c*, 1851*b*). However, this early interest was one that he did not follow up later in his career.

In Letter 55 (21 February 1855) Boole mentions that he has 'used the word eliminant for determinant' – as De Morgan had proposed in his paper (1854*a*). However, Boole thinks 'there ought to be a better word from the Greek and I will try to find one . . . Would not something having to "eliminate" or to some equivalent verb the relation of *ποιημα* [a construction, act or deed] to *ποιεω* [to construct, make] be what we want.'

In Letter 53 (3 January 1855) we find one of the few references to public affairs in the correspondence; although in Ireland in the aftermath of the famine caused by the potato blight Boole never refers to it. In this letter, however, Boole expresses his disquiet on the mismanagement of the British Army's part in the campaign in the Crimea: at the time of writing this letter, January 1855, Sebastopol had been under siege for several weeks. This war was the first in which telegraphic communication made reporting of events on a day-to-day basis possible, and so marks the beginning of the newspaper correspondent sending regular reports from the battlefield.

In Letters 53 and 55 Boole refers briefly to a paper of De Morgan on differential equations (De Morgan 1854*a*). In Letter 53 (3 January 1855) Boole thanks De Morgan for the paper 'part of which I have read and of which I look forward with interest to the further purusal'. However, in Letter 55 (21 February 1855) Boole says: 'I have been compelled for the present to stop in the reading of your paper'. De Morgan's paper is, as he says himself at its beginning, 'of a miscellaneous character' (1854*a*, 513), and is 40 pages long. Among other matters De Morgan takes up again the points regarding primitives, singular, and extraneous solutions which he discussed in his earlier paper (1851*b*), and which were raised in letters 17–19 of Chapter 3.

In Letter 55 Boole also remarks: 'Just now I am busy at analytical dynamics'. He wrote few papers on applied mathematical topics and these could equally be characterised as being on differential equations. In 1847 and 1856 he published papers relating to Laplace's equation; the former (1847*c*) concerned the attraction of a solid of revolution, the latter (1856) was about the equation of continuity of an incompressible fluid. In this paper he refers to a letter he had written to Charles Graves on this subject; the main idea is the use of quaternions to obtain solutions to $\nabla^2 u = 0$. Boole finds an appropriate form of Maclaurin's expansion for a quaternion-valued function. Then he uses the factorization

$$\frac{\partial^2 u}{\partial x^2}+\frac{\partial^2 u}{\partial y^2}+\frac{\partial^2 u}{\partial z^2} = \left(\frac{\partial}{\partial x}+j\frac{\partial}{\partial y}+k\frac{\partial}{\partial z}\right)\left(\frac{\partial}{\partial x}-j\frac{\partial}{\partial y}-k\frac{\partial}{\partial z}\right)u,$$

where j, k are two of the non-real quaternion units i, j, k, to obtain a solution in the form

$$u = \cos(x\Delta^{1/2})\phi_1 + \sin(x\Delta^{1/2})\Delta^{-1/2}\left\{j\frac{\partial\phi_2}{\partial y}+k\frac{\partial\phi_1}{\partial z}\right\}$$

where $\Delta = \dfrac{\partial^2}{\partial x^2}+\dfrac{\partial^2}{\partial y^2}$ and ϕ_1, ϕ_2 are arbitrary functions of y and z.

Some years later Boole wrote a more substantial paper which he called 'On the differential equations of dynamics' (1863*a*; abstract 1862*e*). This paper arose out of an earlier paper (1862*c*) about simultaneous differential equations and in its dynamical aspect relates to W.R. Hamilton's major paper which appeared in two parts in the *Philosophical Transactions* in 1834–5 (Hamilton 1834).

Letters 56 to 59 (January–February 1856) form an exchange of correspondence containing De Morgan's congratulations to the Booles' on their marriage, and Boole's response. These letters show both writing in a humorous vein, De Morgan making puns on mathematical terms and Boole in Letter 57 (8 January 1856) making 'one of such peculiar atrocity' that he has to explain it!

51. BOOLE TO DE MORGAN, 30 MAY 1854

I thought I might venture to ask you if you could tell me anything (more than is contained in the pamphlet) about the Melbourne professorships. I am in some doubt as to whether I should apply for one or not. To speak candidly my income from the college has averaged scarcely more than 300 £ per annum and as I have a mother & sister wholly dependent upon me in England I see no prospect of making even the most moderate provision for old age. Do you know if there are many applicants – if it is likely that I should suit etc. I believe that I am pretty successful as a lecturer & I have always been on the best of terms with the students.

I have now lived long enough in Cork to become attached to the place & strongly so to some of the people. But I feel that I should not like to spend the decline of life anywhere but in England. And I begin to fear that this is a wish of which it is not the design of Providence that I should attain the fulfilment.

I will not ask you now to say whether you have made up your mind . . .[10] . . . that I have succeeded in verifying I think every point that was left in the treatise conjectural or doubtful – the process leading in one instance to a valuable extension (as I think it) of the theory of simultaneous algebraic operations.

I hope Mrs De Morgan is well.

52. BOOLE TO DE MORGAN, NO DATE

I just write to say that I have given up all thought of Melbourne and to thank you for your letter. If you should ever hear of any thing likely to suit me in England I should be glad if you would let me know of it. My objections to Ireland are however growing less and less and I have really very little to complain of beside the smallness of the remuneration which I receive. I incline to think that there are few places in Ireland so desirable for residence as Cork and its environs. That the climate however diffuses a kind of soft languor indisposing for exertion I feel sure.

53. BOOLE TO DE MORGAN, 3 JAN. 1855

I am very much obliged to you for your paper [D 1854*a*] a part of which I have read & of which I look forward with interest to the further perusal. I do not in general think your papers easy to read but I think that they repay the trouble of reading them. My eye fell upon a note in which you speak of the word 'determinant'. I agree with you in your remarks. I think the word was introduced by Mr Cayley when he took up the subject of linear transformations. I had employed the term 'final derivative' for what has since been called the determinant in that theory. But I quite think that the word 'eliminant' which you propose would be better than either. It may be doubtful whether the term hyperdeterminant could so easily be replaced. However I think that eliminants covariants & invariants would (with a proper liberty in the use of adjectives) answer every purpose. The term Quantic recently introduced by Mr Cayley is in my opinion a very bad one. It adds a Greek termination to a Latin adjective & expresses nothing.

If you are still interested in the theory of probabilities you would I think find advantage in looking at a paper of mine in the August No. of the Phil. Mag. 'On the Conditions by which solutions are limited' etc. [B 1854*d*] and at some others in the same journal. I have however one to send which will in my opinion put it beyond all dispute that the method which I have published is a mathematically consistent one & that it is the only such. The conditions of possible experience are identically the conditions of success of the method viewed as an analytical instrument. I all along felt that something of this kind must be the case but I have only lately proved it after an investigation of extreme difficulty, which has added some new theorems to the most difficult part of Algebra.

I don't write this to invite you to look at the paper unless it lies quite within your plans to do so, and I shall not expect you to say anything about it.

I have determined on applying for an examinership in Mathematics under the commissioners for the affairs of India – not expecting to succeed but feeling that I ought to make the effort to better my condition here. Can you tell me when the elections are to be made & whether the examinerships are to be tenable with other offices?

How melancholy is the intelligence from the east not disaster merely but national humiliation & disgrace mourning that refuses to be comforted in every household – sorrows in every heart! Want of patriotism seems to me the radical evil. Members of parliament will not serve their country without the bribe of patronage. Hence official incapacity, heads of an army commissariat who do not know how to keep their charge from starvation, registrars of colleges who cannot spell the English language and graver abuses still. I have heard it said & partly believe it that a total abandonment of all moral principles gives a man a power in official & political life which nothing else will. Surely the end of these things must come, and let it come. Order is not the first of the heavenly virtues.

I think this looks rather strong for a mathematician & yet I beg you to think that I am not of a revolutionary spirit or a lover of change for its own sake. But I have been profoundly impressed for a long time back with the immorality of our official system in every department which I have had the opportunity of examining. The loss of our whole army may perhaps initiate a change.[11]

54. BOOLE TO DE MORGAN, 3 FEB. 1855

When I was an applicant for my present chair you were so good as to give me a testimonial. I feel upon consideration a little scruple at using it for another and different object without your permission. I therefore enclose it wishing to say 1st that if you have any thoughts of becoming a candidate yourself (in which case I should heartily *rejoice* to hear of your success) I of course would not use it, and 2ndly if you have any objection to my using it. If neither case should happen and you should desire to make any alteration in the testimonial you are at liberty to do so.

As I said I do not expect to succeed but I think it a duty to offer myself. I am so out of the way here that all chance of making any further advance is cut off unless I take some opportunity like this of letting it be known that I should be glad to do more than I am doing.

55. BOOLE TO DE MORGAN, 21 FEB. 1855

I have been so occupied lately that I forget whether I wrote to thank you for the kind attention which you showed to my request & for the addition which you further made to the testimonial. However if I did not I do it now most heartily.

I have been compelled for the present to stop in the reading of your paper [D 1854*a*] – only however for a *time* – as there is as [sic] a good deal in it that I should wish to master. Just now I am busy at analytical dynamics.

In a paper of mine which will appear in the Phil. Magazine for March [B 1855*b*] I have used the word eliminant for determinant, but it has since occurred to me that there ought to be a better word from the Greek & I will try to find one. The objection as it strikes me to eliminant is that what it is meant to express is the *result* of elimination not a something by which we eliminate. It is perfectly true that the function $ab' - a'b$ may be looked upon as a canonical form by the aid of which we can eliminate x & y from any equations of the form

$$ax + by = 0$$
$$a'x + b'y = 0$$

so that in a certain sense it might be said that $ab' - a'b$ is an eliminant *by* which we can determine the result of elimination from any *particular* given set of the requisite form as

$$x + py = 0$$
$$qx + y = 0.$$

But it may be replied that the relation of $1 - pq$ in the above instance to $ab' - a'b$ is that of species to genus much rather than that of effect to cause or work to instrument. Would not something having to 'eliminate' or to some equivalent verb the relation of ποιημα to ποιεω be what we want. The misfortune is that our verbs most of them come from Latin and that is so miserably stunted a language in its verbal substantives that we cannot get what we want.

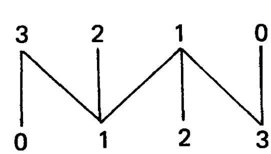
The word 'determinant' is not liable to the same *kind* of objection in one sense because it usually expresses a determining condition and in this sense is *active* but it is subject to fatal objections in that 1st determining conditions often are not expressed by determinants, 2ndly that the mathematical origin of determinants is quite lost sight of. Don't think it necessary to answer these hasty remarks. I will write again when I have read your paper. [D 1854*a*]

56. DE MORGAN TO BOOLE, 4 JAN. 1856

I happened to meet Mr & Mrs Stevens the other night, and the latter informed me that you have been a married man some little time. If it had been Mr S. I would not have given full credence, at once: but ladies are always accurate on such points. I therefore confidently let fly a congratulation, and beg you to present mine and Mrs De M's compliments to Mrs Boole, and we hope that we shall have some opportunity of making her acquaintance.

Of course mathematics and logic and probability have suffered for a time – but no doubt they will raise their heads again.

I have been propounding a puzzle with nothing in it – You are to guess an exceedingly elementary theorem of spherical trigonometry – not thought worth insertion in any book – by the following enunciation



A beginner ought to know it – but where he is to get it from I do not know.[12]

57. BOOLE TO DE MORGAN, 8 JAN. 1856

My wife and I are both much obliged to you for your kind congratulations. You see the information was perfectly correct. I have been a married man now nearly four months. If wedding cards & the usual ceremonies on such occasions had been observed you would have known of the event at the time. You have heard doubtless of that division of our sex into 'happy men' and 'lucky dogs' which some wit proposed to substitute for that of 'married men' and 'bachelors'. Well I have long felt that the distinction was a real one and that to be a 'lucky dog' was *not* to be a 'happy man'. And this will in some degree explain my migration from the one category to the other.

You sent me a little tract some time ago which I did not acknowledge – but I did what was better. I read it through & liked it.

I don't at once see the way to your theorem in S[pherical] T[rigonometry] and I am afraid that I must leave it for the present as I am very fully occupied. I only guess from an inspection of the figure that the theorem is one of *reciprocity* in some way. Was this intended as anything personal?

Thank you for your good wishes about the 'logic' and 'probability' in connexion with my new state. I have only to say in reply that so far as I can judge it is *certain* that the 'logic', and *probable* that the 'probability' will not permanently suffer. Of course a man must as the old song advises be 'off with the old loves – before he is on with the new'.[13] However I don't see but that we may in this case continue to live together after a while as a very 'happy family' and the more especially as my wife had some previous acquaintance with her rivals.

We both reciprocate the kind expressions of Mrs De Morgan & hope that her wish for a meeting may some day be realized. Present our best regards to her.

P.S. Looking over my letter I see that I have described my migration from the family of the 'lucky dogs' to that of the happy men as a removal from one *cat*egory to the other. You may know the pun was unintentional. It has but just dawned upon me that it is a pun. But it is one of such peculiar atrocity – it so closely resembles the murderous attempts of the country correspondent of a Lincolnshire newspaper, that I protest against the mere suspicion of having committed it by 'malice aforethought'.

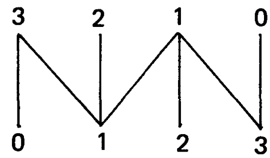
58. DE MORGAN TO BOOLE, 13 JAN. 1856

If *supplemental* triangles had been called *conjugate*, you might have made out a case of personal allusion. As it is, you cannot.

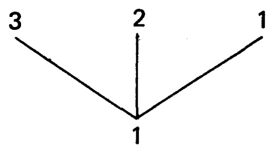
I imagine how various persons will smile when they receive the solution of my riddle – and will say, *is that all*? It is as follows.

Nobody has, so far as I can find, taken the trouble to state the way in which acuteness of sides and angles is connected with obtuseness. I can not find such a thing in Cagnoli, Delambre, Puissant, Legendre or T.S. Davies.[14] I shall be glad if you can tell me where the following is found.

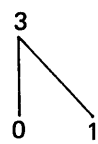
In any spherical triangle (excluding the intermediate case where there is a right angle or quadrant) – either such side is of the same name (acute or obtuse) with its opposite angle, or – some *odd* number of *acute* sides is joined with some *odd* number of *obtuse* angles and conversely, every case just described exists, except *three* acute sides with *three* obtuse angles. Now all this is seen in the diagram I gave



where the lower numbers mean numbers of *obtuse angles* the upper ones mean numbers of *acute sides*. Then



means that one obtuse angle (meaning *only* one) may coexist with 3, 2 or 1 acute sides. But



means that all sides acute cannot coexist with more than one obtuse angle. That *reciprocity* which you interpret into personal reflexion upon your present condition – is that of the supplemental triangles – the $\begin{matrix}3\\|\\0\end{matrix}$ and $\begin{matrix}0\\|\\3\end{matrix}$ at the beginning and end being supplemental etc.

I suppose if I had spoken of differential equations you would have interpreted the general solution as the husband, and the singular solution as the wife, and the contact of the latter with all cases of the former as a reflexion upon the constancy of woman & variableness of man, or as a hint that the former is up to every dodge of the latter – or something like that.

You know the derivation of the words husband and wife? They are from the Sanscrit which compresses a good deal into a few letters. The word wife originally means a demanding of money – and the word *husband* means a person who deceives himself and the truth is not in him if he imagine that by any possible method he will avoid forking out.
I dare not send compliments to Mrs Boole after the last fling.

59. BOOLE TO DE MORGAN, 23 FEB. 1856

I am your debtor both for a pretty little theorem in Spherical trigonometry and for a paper of a more important character. And I have nothing to send you in return but thanks. The paper I have about half read and with great pleasure and I should have read the whole if I had not been obliged by conscience and [illegible] to stick to a paper of my own which is long behind its time and which had been promised by almost every thing short of an oath to be ready at Xmas last.[15] For the same reason I will be short now. My wife who has forgiven the Sanscrit roots sends her kind regards to you and Mrs De Morgan.

Notes

[1] The person Boole named as Courtois was in fact Cournot; see Letter 47.

[2] The numbers on the right of the formulae are editorial insertions. In equations [1] and [2] Boole in error wrote $1 - \lambda$ in place of λ in the denominator. I have corrected this slip in the text.

[3] Boole addresses this letter from: Wickwar Rectory, Near Wotton Underedge, Gloucestershire. The rector, the Rev. T.R. Everest, was Boole's father-in-law. The village's name is more often written Wotton-under-Edge.

[4] This letter does not make very good sense as it stands. Perhaps Boole intended to write 'tomorrow week' or 'next week' in place of 'tomorrow'.

[5] As the previous letter indicates, Boole had been in London in the last few days. This letter is addressed from Lincoln.

[6] M.H. Gill was the printer in charge of the University Press in Dublin at this time.

[7] See Letter 47.

[8] See Letter 41.

[9] This spelling of Shakespeare's name is quite common in the nineteenth century. De Morgan also omitted the first e (De Morgan 1864*a*, 201).

[10] Part of the lower fold of this letter has been cut away: hence the omission in the transcript is unfillable.

[11] This letter is written on black-edged notepaper indicative of mourning. The notepaper is larger than that Boole habitually used. Boole's mother died on 18 August, 1854.

[12] I have not identified Stevens. This puzzle is explained in Letter 58.

[13] The quotation 'off with the old loves – before he is on with the new' is part of a verse from a ballad; in *Songs of England and Scotland,* 1835, the verse reads:

> It is good to be merry and wise
> It is good to be honest and true
> But it is best to be off with the old love
> Before you are on with the new.

The last two lines appear to have become well known in mid-nineteenth century England; they are quoted by Scott, Dickens, and Trollope.

[14] Cagnoli, Delambre, Puissant, Legendre, and T.S. Davies all wrote works on trigonometry or mathematical astronomy in which De Morgan's puzzle might have been expected to appear. He published the puzzle – as a mnemonic – in a note appended to 1857*b*, 269–70.

[15] The paper which Boole was working on seems likely to have been one of two major papers he published in 1857: Boole (1857*b*) or Boole (1857*a*). Boole was awarded the Keith prize for Boole (1857*b*).

5
BOOKS OLD AND NEW; AND HOMOEOPATHY: MARCH 1859-MAY 1861

No letters written in 1857 or 1858 appear to have survived; thus the first letter in this chapter, of 21 March 1859, is over three years subsequent to the last letter of Chapter 4.

The letters of this chapter contain a good deal on matters of general interest but relatively little on mathematics or logic. Nevertheless, the years 1857 to 1860 were very productive ones for Boole, who published two long papers one applying probability theory to the combination of testimonies or judgements (Boole 1857*b*), the other on the comparison of transcendents with applications to definite integrals (Boole 1857*a*). The first of these papers resulted in Boole being awarded the Keith medal by the Royal Society of Edinburgh (Boole 1857*b*). In addition Boole must have been working on his books on differential equations (Boole 1859) and finite differences (Boole 1860). In Letter 60 21 March 1859) Boole gives De Morgan a correction in the differential equation book; in Letter 63 De Morgan acknowledging a copy of the finite difference book says: 'the book is capital in itself, capitaller as a successor to your Differential Equations.' Another book, of which Boole says in Letter 60 it 'was announced for me. The announcement was premature', was never completed. There are manuscripts in the library of the Royal Society which appear to be the work referred to in this letter. For an account of some aspects of these manuscripts see Hesse 1952.

Some of De Morgan's most important publications on logic appeared in the years 1857–60. *On the Syllogism III* and *IV* (De Morgan 1858*a* and 1860*b*) belong to this period. The fourth paper of the *On the Syllogism* series was described by C.S. Peirce as 'a brilliant and astonishing illumination of every corner and every vista of logic' (Pierce 1931, vol.1, 301). These years also see the publication of two works which gave summaries of De Morgan's view of logic: *Syllabus of a Proposed System of Logic* (De Morgan 1860*a*), and the contribution *Logic* to the *English Cyclopaedia* (the volume containing this was published in July 1860).

Letters 60–65

These letters mention the illnesses suffered by Mrs Boole and De Morgan as well as the birth of Boole's third child. There are frequent references to the books that Boole and De Morgan had been reading and, in the case of De Morgan, reviewing.

In the first letter of this chapter, Letter 60 (21 March 1859) Boole says: 'I have not gone to the diggings yet.' Taken at its face value, this paragraph might suggest that Boole was still considering leaving Cork for Australia; however, I think that Boole is here indulging in a piece of jocularity.

One book mentioned in Letter 60 (21 March 1859) was Hamilton's *Lectures on Metaphysics and Logic* (Hamilton 1859). Hamilton died in 1856 and never wrote a full account of his teaching on logic. This work, based upon his lectures and edited by his former pupils H.L. Mansel and J. Veitch, constitutes the most complete account of Hamilton's logic. Another book mentioned in this letter is H.L. Mansel's *The Limits of Religious Thought* (Mansel 1858).

In this letter also Boole asks De Morgan's advice on the value of an early edition of Euclid's *Elements*. Billingsley's translation of *Elements* was the first published in English and appeared in 1570. The preface by John Dee is a historically important statement of the usefulness and relevance of mathematics. The title page, as Boole says, gives this preface as by 'M.I. Dee'; M.I. perhaps stands for Master Iohn. The book is both rare and sought after, and now sells for more than £3000. De Morgan, a bibliophile, owned a copy which is now in the library of the University of London.

In Letter 61 (9 June 1859) Boole mentions that 'Mrs Boole has been very ill... but is better'. The Booles' third child, Alice, was born in June 1860; in Letter 65 (17 July 1860) Boole reports that Alice 'is to be made a Christian of next Sunday'. According to Sir Geoffrey Taylor, a grandson of Boole, 'Alice, the third daughter, like her father, began, without any mathematical training, to make mathematical discoveries.' (Taylor 1964a, 51).

In 1860 De Morgan suffered an attack of pleurisy: 'I have had the honour of a mortal illness for the first time in my life – which few people of the age of 54 can say.' In Letter 64 (13 July 1860) he tells Boole of his recovery following homoeopathic treatment. Boole's reply, Letter 65 (17 July 1860) indicates some scepticism about homoeopathy in contrast to De Morgan's faith in it: 'if... homoeopathy does not produce decided effects *soon*, do not sacrifice your life to an opinion... but call in some accredited priest of Esculapius'.

De Morgan's *ἀριθμος* paper, mentioned in Letter 62 (15 September 1859) is philological rather than mathematical in character. De Morgan distinguished three senses of this Greek word usually translated simply as 'number'. There is the general sense of many – in his words 'the notion of many prior to enumeration' – as well as the familiar concepts of cardinal and ordinal number. He then discusses the questions: which of these senses was the original one; and 'what is the idea presented to the Greek mind throughout the best period of philosophical writing?'. He concludes that in Aristotle's writing '*ἀριθμος* hovers between the senses I [ordinal] and II [cardinal]'.

In Letter 63 (10 June 1860) De Morgan writes some formulae on finite differences; it might aid the reader to recall that

$$\Delta U_x = U_{x+1} - U_x$$

and that $\Delta^n 0^m$ denotes $\Delta^n x^m$ evaluated at $x = 0$. De Morgan says that 'Herschel, I think, calculated $[\Delta^n 0^m = n(\Delta^{n-1} 0^{m-1} + \Delta^n 0^{m-1})]$ by real differencing. . .'. Herschel wrote a text on finite differences (Herschel 1820) and this is probably what De Morgan has in mind here.

In Letter 64 (13 July 1860) De Morgan mentions a number of books apparently in answer to a request by Boole; the titles suggest that Boole was asking for books on the demonstrative aspect of mathematics suitable for the instruction of teachers. Most of the books elliptically referred to can be identified with some certainty, and these are included in the bibliography.

Two remain uncertain: 'Barrow' may be Isaac Barrow – his *Mathematical Works*, edited by W. Whewell, was published in 1860; 'Kelland' may refer to P. Kelland who wrote *Lectures on the principles of demonstrative mathematics*, Edinburgh 1843.

Letter 64 also contains some remarks which indicate the opening of the controversy that De Morgan carried on with Mansel and other successors of Hamilton. This controversy receives a good deal of attention in the letters of the next chapter. In this letter De Morgan says: 'I have fired some more shot in the July number of the English Cyclopaedia 'Logic'.' And later he says in the same paragraph, 'I cannot imagine what keeps Mansel so long about the logic, unless it be that he finds very serious difficulties about the novel parts. Hamilton left them very rough, and he has to defend as well as explain them.'

The English Cyclopaedia was published by Charles Knight, between 1854 and 1862. It was a revised and augmented version of *The Penny Cyclopaedia*, which was originally published in parts, then in volume form (27 volumes, plus three supplemental volumes) between 1832 and 1848; De Morgan is said to have been responsible for about one-sixth of the articles of this work, which was one of the publications of the Society for the Diffusion of Useful Knowledge. A shortened form of De Morgan's article on Logic in *The English Cyclopaedia* appears in De Morgan 1966, 247–70.

Letter 64 contains one of the few mathematical or logical topics mentioned in this chaper. The logical notation used by De Morgan in this letter differs from that which he used earlier (e.g. in letter 12). Now he uses the notation of *On the Syllogism II* and writes $X)$ or $(X$ to indicate that X enters universally, while $X($ or $)X$ indicates that X enters particularly. Thus in a formula he builds up one observes an even number of brackets; in addition he uses a dot to denote negation (as well as the previous convention where x denotes the negation of the attribute denoted by X). Examples taken from *On the Syllogism II* (De Morgan 1966, 31) are:

$X))Y$	all X are Y
$X(\cdot(Y$	some X are not Y.

Note that $Y((X$ also means all X are Y. De Morgan combines two such statements:

$$X))Y(\cdot(Z$$

i.e. $X))Y$ and $Y(\cdot(Z$. He is then able to derive the inference of a syllogism by deleting the middle term (letter) and its accompanying brackets and (possibly) dots. Thus the inference from $X))Y(\cdot(Z$ is $X)\cdot(Z$ or every X is not Z.

60. BOOLE TO DE MORGAN, 21 MARCH 1859

I have not gone to the diggings yet. If any one shall tell you that I have believe him not. I hereby enter into a solemn engagement not to transport myself thither without consulting you on the subject. How could you imagine that I should expatriate myself without at least bidding you good bye?

You refer to the book on Logic which I have announced or which was announced for me. The announcement was premature. I have written at different times as much as would make two or three books but when returning to a subject I can seldom make much use of old materials. They have lost their freshness & I can only begin again *de novo*. And that is what I am now doing – but – with a modest plan before me, having certain things to say & only desiring to say them. I am not going to set aside anything in the Laws of Thought – but only to interpret within the province of pure Logic what is done there. When this is done I shall quit the subject for ever.

An edition of Euclid in small folio (not absolutely small but about two thirds the size of the old folios of the fathers) was shown to me today. It was the property of a widow who wished to sell it & I was asked my opinion of its value. It is Billingsley's translation (1570) with a very fruitful preface by M.I. Dee. No doubt you know all about it. The work is complete, with pictorial frontispiece etc., but the old binding gone & replaced by a shabby one of the last century I suppose. I presume the work not to be of much value – but think it as well to ask you – as I am writing already.

If you should be looking into my book on Differential Equations [B 1859] I would wish to tell you that there is an oversight on p.347 where I say (lines 2–5) that it suffices to get $n - 1$ integrals such as will suffice to determine $p_1, p_2, \ldots, p_n$ in terms of the original variables etc. It is not proved that any such system of $n - 1$ integrals will do – though every such system which rules dx is included in the integrals of the auxiliary differential equation derived from (45). I intend to add an appendix in which I shall supply an omission in another part of the work and correct this error. I now refer to it because I should like to get the best form of the conditions which ought to be fulfilled – and I am not inclined to think that Cauchy has got the best. I have an impression that I intended after writing the passage to reexamine the subject – but forgot it till too late.

I have not seen Hamilton's Metaphysics [1859] but I have read Mansel's Bampton lectures [1858] which are closely connected with Hamilton's views & also a review of Mansel in the National [Review] which I think full of genius.[1]

61. BOOLE TO DE MORGAN, 9 JUNE 1859

After I had written the enclosed letter to Mr Heaviside I thought on reflection that I might in the first instance apply to you which I now do asking you to send it only in case you cannot find a *probable* answer to the question yourself.[2]

I hope Mrs De Morgan & your sons and daughters are well. Mrs Boole has been very ill since her return home but is better. She sends her kind regards.

62. BOOLE TO DE MORGAN, 15 SEPT. 1859

Your paper reached me safely but I take it very ill that you did not send me the one on the word *αριθμος* [D 1859*b*]. I hope if you have not got one left you will beg one from some one of the people who will not read and send it to me who will.

The paper you have sent I have as yet only looked into but intend to read it through.

63. DE MORGAN TO BOOLE, 10 JUNE 1860

I never got your book [B 1860] till yesterday, for McMillan sent it to my old address, whence it was returned to him. I am very much obliged to you: the book is capital in itself, capitaller as a successor of your Differential Equations. This I say at once. I hope the Cambridge writers will study these models a little.

By the way (p.19, 20) I did not calculate $\Delta^n 0^m$ from the troublesome (5) of yours [B 1860], but from

$$\Delta^n 0^m = n(\Delta^{n-1} 0^{m-1} + \Delta^n 0^{m-1})$$

(p. 255) [D 1842*a*]. Or rather, Herschel, I think, calculated, by real differencing, I suspect, and I verified. But I calculated

$$\frac{\Delta^n 0^m}{2.3\ldots n} \text{ from } \frac{\Delta^{n-1} 0^{m-1}}{2.3\ldots n-1} + n\frac{\Delta^n 0^{m-1}}{2.3\ldots n}.$$

I forget whether I discovered these theorems, and saw them afterwards, in some other book, or whether I got them from another book. I could not find them again when I looked for them in various likely places.

Poor Bertrand was charged in some foreign journal (Libri told me) with pillaging me. Now it so happened that he had given every possible proof of fairness. Independently of his putting my name very prominently forward, he gave the correct date of my publication, which he had to take, not from the title page of the book, but by looking at the list I gave of the dates of publication of the numbers, and dividing the pages by 32. He might have missed this refinement without any suspicion of fraud. And he sent me his paper immediately. Not to speak of his method bearing the impress of another view as clearly as it could do. So it seems that a man cannot escape, let him do what he will.[3]

I hope Mrs Boole and the children are well. With my wife's kind remembrances.

64. DE MORGAN TO BOOLE, 13 JULY 1860

I have been busy convalescing after an attack of illness. I have had the honour of a mortal disorder for the first time in my life – which few people of the age of 54 can say. The name of the beast was pleurisy: but it did not even put me in danger, though of course a medical looks grave at danger of danger, and even at $(\text{danger})^3$. My symptoms were cut up root and branch by infinitesimal doses in three days – not rising to a maximum and then diminishing *during* the application of remedies: but having their maximum at the moment of first application. And this I have observed to be the character of homoeopathic medicines whenever they are to succeed at all.

I agree with you that the explanation of (·) and)(is not down at the bottom. Quantification is an incident – not the fundamental basis of explanation.

You must remember that all the rules of validity in p.19 are *one*.[4] The contained of the contained is contained. Thus

$X(\cdot(Y(\cdot)Z$

is $X(\ )y))Z$

or part of X in y

All y in Z

∴ that part of X in — in Z [*]

or in Z [*]

[Note that when he expresses this symbolism in words De Morgan uses the language of sets. Thus $X(\cdot(Y(\cdot)Z$ is read

part of X is not in Y and part of Y is not part of Z,

This is the same as $X(\ )y))Z$ or

part of X is in not-Y and all not-Y is in Z,

hence $X(\ )Z$, i.e. part of X is in Z.

The lines marked [*] are transcribed as De Morgan wrote them; their meaning is not very clear. The explanation given above contains the gist of what I think De Morgan intended.]

The splitting and straggling is only the application of this to the varieties of entry of contraries etc.

I have fired some more shot in the July number of the English Cyclopaedia 'Logic' [D 1861*b*]. I agree with you about Hamilton. He is a *monster* of capability; a monster because so unequally balanced that some parts are of gigantic development and others only rudimentary. I cannot imagine what keeps Mansel so long about the logic, unless it be that he finds very serious difficulties about the novel parts. Hamilton left then very rough, and he has to defend as well as to explain.

I should think Maynard likely to have Barrow.[5] As you mention Barrow and Kelland together, I should think that in the same list might come Gregory (O.) hints to teachers, Young's *lectures* (Belfast) Beddoes on Demonstrative Evidence (not much worth) Newman's Geometry – Walker's Philosophy of Arithmetic – H. Wedgewood on Geometry – and many others. I have no list of such books.

Our kind regards to Mrs Boole – children well I hope.

65. BOOLE TO DE MORGAN, 17 JULY 1860

I am sincerely glad that you have so completely and so satisfactorily (as to the manner) recovered from an illness which if not of a mortal character is at least of a very dangerous one. I have witnessed pleurisy and its former mode of treatment more than once in my father. One would say beforehand that homoeopathy could have no effect on such a disease. I remember hearing of another form of inflammation some years ago treated by homoeopathy unsuccessfully, and when the patient was in extremity by the vigorous measures of ordinary practice. This was in London – the patient a literary man – my informant a clergyman in Lincolnshire who went up to see his friend, found him getting no better but worse and insisted on the lancet. My wife's father died of an inflammation of the stomach under homoeopathic treatment. The moral is – if you are ever attacked with inflammation and homoeopathy does not produce decided effects *soon*, do not sacrifice your life to an opinion, or to the opinion of any one else, or to a notion of going through with a thing when you have once begun with it but call in some accredited priest of Esculapius with all his weapons of war and do as your ancestors did – submit to be killed or cured according to rule.

I have not seen and fear that I shall not see here the English Cyclopaedia except the biographical portion – which did not impress me very favourably. The publication of lives of living men is a bad feature I think. If ever I do get access to the work I will not fail to turn first to your paper.

I am inclined to think that I sent you an Appendix to my Differential Equations containing corrections chiefly supplied by Mr Todhunter. If not send me a line at any convenient time and I will forward it to you by post.

Mrs Boole and her children (three) the youngest of whom is to be made a Christian of next Sunday are quite well. The little neophyte will then be about three weeks and a half old.

Letters 66–9

Apart from a brief query of Boole concerning Jacobi's work on maxima and minima, these letters are devoid of mathematics. There are interesting comments on contemporary literary and military affairs. Also Letter 69 indicates Boole's friendly relationship with one of his pupils.

In Letter 66 (18 Oct. 1860) Boole points out 'a most audacious instance of misquotation' in Boase's *The Philosophy of Nature* (Boase 1860). Boole, claimed Boase, 'had not clearly apprehended and formulated the principles with which he commences'. De Morgan's review appeared in the *Athenaeum* for 18 August 1860 (vol. 33, 222–3). Boole writes with some heat to De Morgan: 'it ought not to be passed over without public notice. If you review Dr Boase's book in the

Athenaeum. . . the lash ought to be yours'; he concludes by imputing 'gross ignorance' to Boase.

In Letter 66 Boole refers to De Morgan's contributions to *Notes and Queries.* Some of these were reprinted in the *Mathematical Gazette* under the heading *Some Incidental Writings by De Morgan*: see vol. 9, 78–83, 114–22, 126–278; vol. 10, 69–74, 146–9; vol. 11, 157–63, 200–203.

In Letter 67 (13 November 1860) Boole asks De Morgan whether 'you have particularly examined Jacobi's theory of the criteria of *max.* and *min.* in the Calculus of Variations?'

In Jacobi 1837 were given for the first time sufficient conditions for an extremal curve to be a maximum or minimum. Previous criteria – due primarily to Euler and Legendre – were necessary conditions only. This paper had stated results without proof, so Boole's attempt to discover whether an essay presented to the Academie des Sciences had been printed shows his interest in the details of the work. Boole had been interested in the calculus of variations from the beginning of his research career; one of his earliest published papers was on the subject, viz. Boole 1840*b*.

In Letter 67 there is mention of the book *Essays and Reviews* 'which was to raise the greatest religious storm of the century' (Faber 1958, 245). The idea of publishing the collection is due to H.B. Wilson; it appeared in February 1860. The book contained seven essays which 'covered almost the whole ground of the then existing controversies between Anglican churchmen, and between religion and science' (Faber 1958, 233–4). The most distinguished of the contributors was Benjamin Jowett. Two of the contributors, Williams and Wilson, were condemned for denying the inspiration of Holy Scripture in the (ecclesiastical) Court of Arches – but this was reversed on appeal to the Privy Council. An attempt to arraign Jowett before the Vice-Chancellor at Oxford failed.

Boole considered Jowett's essay – titled *The Interpretation of Scripture* – the best, while the mathematician Powell's essay – titled *On the study and the Evidences of Christianity* – he considered the worst. De Morgan's review of this work appeared in *The Athenaeum*, 27 October 1860, vol. 33, 546–9. Also in Letter 67 Boole paraphrases some remarks that Thomas Carlyle made in a letter of 4 June 1835 to John Sterling:

> . . . assure yourself that I am neither Pagan nor Turk, nor circumcised Jew; but an unfortunate Christian individual resident at Chelsea in *this* year of grace, neither Pantheist nor Pot-theist, nor any Theist or Ist whatsoever, having the most decided contempt for all such manner of system-builders or sect-founders – as far as contempt may be compatible with so mild a nature – feeling well beforehand (taught by long experience) that all such are and ever must be *wrong*.

This letter was published in Froude 1884. The substance of the remark must have been known by 1860, but I have not found any earlier source for the letter.

In Letter 69 (27 May 1861) Boole acknowledges the receipt of De Morgan's 'very learned paper on Tables', presumably his article on mathematical Tables which appeared in the English Cyclopaedia (De Morgan 1861*b*).

De Morgan's paper is basically a historical and bibliographical report on Tables. He makes a partial disclaimer, however: 'The list which we mean to give does not profess to be a bibliography of tables, but will nevertheless give information on the subject to all who are not particularly given to mathematical bibliography' (De Morgan 1861*c*, col. 978).

De Morgan begins with some remarks about modes of arrangement and typography to secure a high level of legibility. The main part of the article consists of eight sections in which he discusses tables of the following kinds: 1. multiplication; 2. division and prime number; 3. squares, cubes, and other powers and roots; 4. pure decimal tables; 5. pure trigonometrical; 6. logarithms; 7. higher mathematical; 8. commercial. Within each section the discussion is chronological. In his conclusion De Morgan remarks: 'In the present article we have given about 457 tables, of which 332 are from actual inspection' (De Morgan 1861*c*, col. 1015). One wonders to what lengths he would have gone had he professed to write a bibliography!

Euclid's *Elements* is mentioned again in letter 69 (27 May 1861) where Boole refers to Peyrard's Greek and Latin edition of Euclid. Peyrard's was the best edition of Euclid's *Elements* at this time. The Αἰτηματα are postulates, the κοἰναι ἐννοιας are the axioms (literally: common notions). In most nineteenth century editions the axioms included '(α) all right angles are equal', and '(β) that if a straight line falling on two straight lines make the interior angles on the same side less than two right angles, the two straight lines, if produced indefinitely, meet on that side on which are the angles less than the two right angles' (i.e. Euclid's parallel axiom). However, in certain manuscripts of the *Elements* these statements are given among the postulates (usually numbered (4) and (5)). The latter is now regarded as the better place for these assumptions.

Letter 69 contains a reference to one of Boole's pupils R.A. Jamieson, who 'has been a very distinguished student here... also a great favorite of Mrs Boole's.' Jamieson was an applicant for an interpretership post in the Far East; he obtained this post, and made his career in the Far East.

After Boole died Jamieson, writing from Shanghai, recorded his impressions of Boole as a teacher: 'The secret of his success, I think, with senior classes, and to a limited extent with the junior, was that he never seemed to be repeating or reproducing what he had himself once learned – he always appeared to be discovering the results he educed, and his students were generally carried along with him and, as it were, shared in the honour of the discovery.' (Rhees 1955, 76).

66. BOOLE TO DE MORGAN, 18 OCT. 1860

Thank you for the Notes & Queries. I have read all your contributions.

I send what appeared to me at first a most audacious instance of misquotation but what upon reflection I think may be more charitably interpreted. Whatever the cause of the misquotation may be the instance is remarkable that in the interests of literary honesty it ought not I think to be passed over without a public notice. If you reviewed Dr [H.S.] Boase's book [1860] in the Athenaeum it occurs to me that the lash should be yours & therefore I send the example. It is not the only one but it is the worst.

The charitable interpretation is that Dr Boase read the passage in my book without understanding it, made a wrong guess at the meaning then altered the passage so as to give it that meaning & made use of it in order to display his critical powers – but that having altered the passage so as to have destroyed almost all verbal resemblance to the original he should have printed it without quotation marks as if it were a *bona fide* extract is a thing not easy to account for charitably in any supposition. The gross ignorance which it implies is what most men would shrink from the imputation of even more than from that of wilful falsehood.

I hope you are all well.

67. BOOLE TO DE MORGAN, 13 NOV. 1860

I think you are the author of an article on Essays & Reviews which appeared in the Athenaeum a week or two ago, & which I read last night. I judge from internal evidence only. Now after saying that I agree with you in nearly all your observations. I want to ask you what you mean by describing the last Essay in the volume as 'sabbatical'. You are too much a lover of truth to do this merely to complete a parallel or to make a climax & I have no doubt you attach a meaning & one you can justify to the term as applied. If you are not busy I wish you would tell me what that meaning is. I ask you because Jowett's Essay, the one referred to, was to me by far the most interesting in the volume. I think it the *best* of the essays & for the sake of mathematics I am sorry to add Baden Powell's nearly the worst.

Have you ever particularly examined Jacobi's theory of the criteria of *max.* & *min.* in the Calculus of Variations? [1837] Do you know whether an Essay on this subject which was crowned (I don't know the meaning of that) some years ago by the French Academy was ever printed?[6]

Having written this it occurs to me though I did not think of it before, that the transition from the Church of England to the Calculus of Variations is not a very violent one. I suppose the *formal* difference to be that in the variations of opinion in matters of theology there is no absolute *max* or *min* though there may be a relative one to the individual i.e. a man may advance to a certain degree in opinions of one particular kind & thus gradually recede. If you tell me that Pantheism is an absolute maximum in one direction of thought I must remind you that Mr Carlyle has discovered a region beyond these, 'Pantheism Sir! What matters if it were pot theism so long as it is true?'

I know that you are a hard worker & I feel almost ashamed of sending this. Don't answer it if you feel it would be trouble.

68. BOOLE TO DE MORGAN, 7 FEB. 1861

I have had most of the papers you sent me arranged and bound – but I cannot find among them the one you refer to – so if you will be so good as to send it to me I shall be much obliged and if you have no second copy I will return it. I have no means of referring to C[ambridge] P[hilosophical] S[ociety] Transactions here after the 8th volume.

69. BOOLE TO DE MORGAN, 27 MAY 1861

A young friend of mine is going up as the nominee from our college for one of the Chinese & Japanese interpreterships. He called last night and said that it would be a great pleasure to him if he could get to see Prof. De Morgan while in London. I asked him why & he replied Why Sir I have read a great deal of his Formal Logic & also of his Calculus & *I should like to see him.* I told him where you were likely to be found (Gower St) & promised to write to you. I hope you will not take it ill that you have to bear one of the penalties of fame. My young friend's name is Jamieson. He has been a very distinguished student here & I am personally very fond of him. He is also a great favorite of Mrs Boole's. I hope therefore for her sake as well as mine you will be gracious to him.

And you may tell him for me whether in all the Greek texts of Euclid as in Peyrard's [1814] the properly geometrical axioms of the editions in use fall among the Αἰτηματα & not the Κοιναι ἐννοιας.

I have to thank you for your very learned paper on Tables [D 1861*b*], a great deal of which I have read.

Notes

[1] The review is in the *National Review,* March 1859, 209–27.
[2] In the absence of 'the enclosed letter' it is impossible to know what the question was about. However, the emphasis given to the word *probable* suggests that it concerned probabilities.
[3] De Morgan gave another – and clearer – account of the accusation against Bertrand in a letter to Sir John Herschel of 9 August 1862 (see S.E. De Morgan, 312).
[4] De Morgan is, I think, here referring to 1860.
[5] Maynard was a second-hand mathematical bookseller (see Graves 1882, vol. 3, 313).
[6] An essay was said to be 'crowned' by the Academie des Sciences when it was awarded a prize medal. It is not clear whether Boole here means that the essay was written by Jacobi or by another; I think probably the latter.

6
THE CONTROVERSY WITH HAMILTON'S SUCCESSORS; AND THE JEWS: OCT. 1861 - NOV. 1862

In Chapter 6 De Morgan's letters are primarily concerned with his controversy with the successors of Hamilton, and with the fifth paper *On the Syllogism* (De Morgan 1863*a*). The central issue of the controversy relates to the quantification of the predicate, and I shall begin by giving as much background to the logical ideas and to the controversy as shall be necessary to understand the letters that follow. For a resumé of the controversy see P. Heath's introduction to De Morgan 1966.

In his lectures Hamilton had introduced the idea of the quantification of the predicate into syllogistic reasoning. In addition to syllogisms of the form 'all Ps and Qs' and 'some Ps are Qs', Hamilton introduced statements such as 'some Ps are some Qs' and 'all Ps are some Qs'. The background in logic to this idea of quantification of the predicate is discussed in Prior 1955, 146–56; Prior described Hamilton as advocating the quantification of the predicate 'with a quite fantastic incompetence' (Prior 1955, 148).

The controversy arose from a correspondence between De Morgan and Hamilton in 1846, which led Hamilton to accuse De Morgan of plagiarism. This seems to have been based on a misunderstanding, in which Hamilton confused De Morgan's idea of numerically definite syllogisms with syllogisms containing a quantified predicate; Hamilton made a partial retractment of his accusation, but it is generally agreed that De Morgan had the better of the argument.[1]

In Letter 11, of 1847, De Morgan had remarked to Boole that 'I felt quite sure he [Hamilton] could not look at logic in any way that could give a view to a mathematician'. Nevertheless De Morgan maintained an interest in Hamilton's logic over the next fifteen years. Hamilton had published no definitive account of his logic when he died in May 1856, his views having become known through his lectures, and from books published by his former pupils. In Letter 64, of 1860, De Morgan had addressed Hamilton thus: 'He is a monster of capability because so unequally balanced that some parts are of gigantic development and others only rudimentary'.

The later phase of the controversy began in 1861 when De Morgan made four contributions to *The Athenaeum* entitled *Hamiltonian Logic*. These appeared in

July, August, November, and December, on pages 51, 222, 582 and 883–4 of volume 34. At this time he was in the process of writing *On the Syllogism V* which was read on 4 May 1862. This paper gives a comprehensive analysis of Hamilton's logical ideas and was characterized by C.S. Peirce as 'final and unanswerable' (Peirce 1931, Vol. 2, 324).

Letters 70–74

These letters are predominantly concerned with the controversy and particularly with De Morgan's paper *On the Syllogism V* (De Morgan 1863*a*). In Letters 70 (16 October 1861) and 74 (1 February 1862) De Morgan rehearses (but not briefly) ideas of his paper. Letters 71–3 are replies by Boole which, among other matters, contain his reactions to De Morgan's ideas.

Letter 70 is one of the longest letters in the correspondence and is full of meat. De Morgan says, in Letter 70, 'In this same number five now [16 October 1861] fermenting, I have some points that can be shortly enunciated'. The letter continues for 10 sides on these points. He begins with some historic remarks relating to the singular and plural mode of expressing universals (compare 'each man is an animal' with 'all men are animals'). He continues: 'I hold that *all* and *some* emerge *à posteriori*' and justifies this by a postulate: 'A term is that which *divides* the universe'. This classificatory approach leads to a consideration of quantification.

Letter 70 shows the extent of De Morgan's familiarity with the older literature relating to logic. He refers to ten authors from Boethius to Wallis, mainly in reference to their exposition of Aristotelian logic. In *On the Syllogism V* he makes similar remarks, but these gave slight indications of the particular works of these authors that he had earlier consulted. (De Morgan 1966, 292.) Several of these authors are little known.

Paulus Venetus (Paulus Nicolletius Venetus, or Paul of Venice) died about 1429. The Cologne Regents (or Masters) refers to a fifteenth or sixteenth century edition of Aristotle which I have not traced. Jodochus Isenach (Justus Judocus of Eisenach) was professor of theology and philosophy at Erfurt; he wrote *Summa Totius Logicae*, Erfurt, 1501. The last four all wrote works at the end of the sixteenth century, or in the first two decades of the seventeenth century: J. Pacius, or G. Pace, 1550–1633; Burgersdicius, or F.P. Burgersdijck, 1590–1635; B. Keckermann, 1573–1609; Richard Crackanthorpe, 1567–1624. J. Wallis, 1616–1703, was Savilian Professor of Geometry at Oxford, 1649–1703. His *Institutio Logicae*, Oxford, 1687, is the work that Morgan obliquely refers to.

At the end of this long and detailed letter there is an item of a very different kind, De Morgan describes it as 'a bit of correspondence between physiology and sociology' in De Morgan's Letter 70 (16 October 1861). This exercise in digital dexterity concludes with a reference to Sir Creswell Creswell; he was the first judge appointed to the Divorce Court on its establishment in 1858.

A feature of the correspondence of this chapter is the interest shown by both correspondents in the Jews and in Jewish culture. De Morgan begins with Letter 70 by expressing his thanks to Boole for 'the extract about Jews' (in a letter that has not survived) and continues with some remarks that show he read Jewish newspapers. Letter 71 (4 November 1861) indicates that Boole read religious poetry of the Spanish Jews. Boole makes further references to his Jewish studies in Letter 72 (21 November 1861). Boole was still troubled by financial worries at this time. In the same letter we find him asking De Morgan's advice regarding advertising for private pupils – presumably a project intended to increase his income.

The length and detailed arguments of Letter 70 made a considered reply by Boole a matter of some difficulty. Not surprisingly Boole's Letter 72 begins with: 'I don't think I shall be able to write to you in reply to the 'Logic' portion of your letter for a fortnight or three weeks'. However, De Morgan is an understanding correspondent; he begins Letter 74: 'I shall write you no more logic if you pester yourself with the duty of answering', and proceeds to expound further ideas from *On the Syllogism V*. Boole's reply (Letter 75, 2 February 1862) commences: ' I have received your second logical epistle and have put it aside with its predecessor to be studied in due time'.

In Letter 73 (7 January 1862) Boole gives expressions of support for De Morgan in the controversy: 'you seem to me to be quite in the right – at any rate substantially so'.

In Letter 74 (1 February 1862) De Morgan is concerned with Hamilton's use of the term 'some'. De Morgan distinguishes three senses – which he claims Hamilton had confused. There is a non-partitive sense; in this sense some indicates only not-none. There is the singly partitive sense in which some indicates some-not-all, with no assertion about the remainder. And there is the doubly partitive sense which indicates some-at-most, with the remainder not possessing the particular attribute (De Morgan 1966, 276–7). This leads to De Morgan's analysis of Hamilton's 'poor syllogisms'. Seventeen of the 36 are adequate, 4 are 'purely Aristotelian'. But 'There remain 15 . . . and *all are vicious*'.

70. DE MORGAN TO BOOLE, 16 OCT. 1861

I am much obliged to you for the extract about Jews. They were a well trounced community. I have no doubt they exhibited a counter feeling when they dared: and so increased the hatred against them. Shylock must have been a picture of things which had happened, even if exaggerated.

The Jews now dare to publish their own newspapers, in which they treat Christianity as blasphemy. I get hold of one now and then. In one there was a very moderate article, which spoke of the Rabbi Joshua of Nazareth [Jesus Christ] and the Rabbi Saul of Tarsus as teachers of considerable merit, regretting that their injudicious followers had interlarded their teaching with mythical biography and forged miracles. My! how the age advances. Whether the writers, or some of them, in Essays and Reviews, mean the same thing, is a matter of curious speculation.

Being always grubbing at logic in some state or other, I have been at Sir W.H. – my best friend, whom I treat with prepense ingratitude – and you may see by the Athenaeum [vol. 34,51,222] that I convict him of actually forging invalid syllogisms. His friends are quite silent. Can you do anything for him? I mean in the way of suggesting a possible meaning for his quantifications which will make his syllogisms valid. Dr Mansel, who rushed in so boldly to the defence of his mathematical blunders, and left me the field at last, was wise enough, when I wrote my *first* letter in the Athenaeum to write me a private note, saying that he had intended to write to the Athenaeum – but thought it was really so simple a thing to answer that he must have mistaken my meaning. *He* thought that it was etc. etc. I gave him a spicilegium[2] of what I should think – and he abandoned his intention. That I should be allowed to advance, without contradiction, that the great reformer – who blew his own trumpet so very loud – has committed actual paralogisms, is so strange, that I really want an opponent for the sake of the case and the subject. Mr (I mean Dr – *meum cuique*) Mansfield Ingleby – has written to say that he cannot *allow* me to assume etc. etc. without contradiction. I have answered that he must publish, if he wants me to notice. He is the man who signalised himself by a very curious blunder a year or two ago. If you correspond with anybody who is inclined to contradict me in private, I wish you would put him up to firing a shot in public. It is nothing at the time that no answer appears: but if I be allowed to recapitulate in my Cambridge no V [D. 1863*a*], which is on the stocks, and to say that there was no reply, it will have a great deal of meaning 20 years hence. Besides I am personally in want of a row: as the Irishman said, I am dry-moulded for want of a bating.

In this same number five, now fermenting, I have some points which can be shortly enunciated. Two of them are as follows.

1. On coming to examine Aristotle etc. independently, I find that all the old logicians from A. downwards are singular, monadic, exemplar, – as you please – in their enunciations. Their universal is always *each one*, omnis, πας, – singular: their particular is always *some one*, Quidam, aliquis, τις. When I made Hamilton's system *exemplar*, in 1850, I had not thought of this in the comparatively little enunciation I had made then. The logicians now all read plurally or rather collectively – by lumping the individuals into the *extent* of a term. So that with them *Omnis homo est animal* means all (the extent of the term) *man* is in (the extent of the term) *animal*. But the ancients meant no such thing – by *omnis homo* they mean every man. At this moment I remember Aristotle, Boethius, Paulus Venetus, the Cologne Regents, Isenach, Pacius, Burgersdicius, Keckermann, Crackanthorpe, who are all unmistakeable. The *pluralisers* are such moderns as Wallis, the Port Royal, etc. – and most of them are rather mathematical. If you want the latest collection of the purest Aristotelian, Crackanthorpe is the man. He collects all the quantifiers; 12 in number, all singular! So I believe that I hit upon the extension of the old systems when I cut down Hamilton's plurals into singulars. What do you make of Hamilton's form for (B) 'No *X* is *Y*' i.e. Any *X* is not any *Y* (A).

He affirms that 'any' is exclusively limited to negation: I say any one knows better than that. But I want to know whether, in English (A) and

(B) are of identical meaning. Speaking of English merely, of course I see that *any* man is not *any* stone. But should I not be obliged to say this of *stone man* if there were one. This man is not *any* stone: there is one bit of stone *he is*, and no other. That 'no fish is a fish' is rendered by Hamilton 'Any fish is not any fish'. But to my idea this seems *true*. *Any* fish is not *any* fish – any fish is but itself and not any other fish. Turbot is not salmon. There is a prime ambiguity, it strikes one, about the meaning of any: is-not-any fish and is-not any-fish mean to differ in meaning.

2. I have been overhauling the sources of enunciation, and I conclude that the primary introduction of quantification is an error and a misfortune: I hold that *all* and *some* emerge *à posteriori*. I have touched this point in my third paper [D 1858*a*], but not to the extent of showing the basis of my eight forms independently of explicit introduction of contrary terms. I now get it as follows. I prefix all necessary matters.

Postulate. The *universe* – any assigned extent of thinking ground.

Postulate. A *term* is that which *divides* the universe. A name which embraces the universe cannot be the means of affirming or denying – that is of affirming/denying that which might have been denied/affirmed in that universe. Hence the lowest term is *singular*, an individual: its contrary, the highest term, is *penultimate*, containing all but an individual.

Postulate. Any individuals whatever in the universe, which are not *all*, may be the contents of *a term*. The *name* may be wanting in language, but the power of separating these individuals exists in thought. And one need not look for a *differentia* for there must have been one before or in separation. For instance, I separated as a species of 'material object' – 'All the men who have killed their brothers; the hundred largest inkstands that ever were made; and the first Gaul who set eyes on Caesar'. What is the *differentia* of this species? – It is 'selected by the fancy of a logical radical to illustrate the unlimited power of division of the universe'. The old logicians would say that these objects are not a species because the *differentia* is not *of their essence*: but I defy them to show this. If all things be predestined from all eternity – it was as much of the οὐσια of the inkstands that they should be thus associated with the fratricides as that they should be capable of holding ink: there are no degrees of necessity. So much for the essence.

Pragmatic enunciation is comparison of terms as to contained or not contained. It is of two kinds:–

1. In which subject and predicate are compared by *difference of relation* to *an indefinite third term* (Inconvertibles).

2. In which subject and predicate are compared by *sameness of relation* to *an indefinite third term*.

N.B. This indefinite third term avoids the necessity of bringing in other comparing relations, as *excluded* etc.

Let	*X*)	be *X* is in	do (*X*
–	*X*(·	be *X* is not in	do ·)*X*
Let	*X*(	be *X* takes in	do)*X*
–	*X*)·	be *X* does not take in	do (·*X*

1. X))Y	X is in, Y takes in, – some *third* term (N.B. the third term may be X or Y itself)
X(·(Y	X is not in, Y does not take in, *any* third term (i.e.) not *both*
X((Y	X takes in, Y is some third term.
X)·)Y	X does not take in, or Y is not in, any third term.
2. X)·(Y	Either X does not take in or Y does not take in any third term.
X()Y	X takes in, and Y takes in, some third term.
X(·)Y	Either X is not in, or Y is not in, any third term (i.e. Everything is either X or Y: *the universe is not a term*)
X)(Y	X is in, and Y is in, some third term.

The affirmations are *conjunctive* and the third term is particular – *some one*. The negatives are *disjunctive*, and the third term is *universal* – *any one*.

My usual reading of (·) and)·(has very much bothered several who have spoken to me about it. Here (·) is as negative as the rest, etc.

Here the spiculae [i.e. (·),) etc.] are not directly quantitative – and 'all' emerges from 'is in' and 'does not take in' – while 'some' emerges from 'takes in' and 'is not in'.

The other modes of reading turn out ineffective. For example

X is in, *Y* takes in, any one third term. Not true of *X* in any sense, nor of *Y*, unless it can be the universe. And so on.

X is not in, and *Y* is not in *some* third term. True of every possible pair, unless the universe have but two individuals, one *X* and one *Y*.

And so I could write on through a plusquam legible number of sheets. I will only add that when one comes to quantify there are two modes of reading – by *part* and by *whole*: a *whole* of *X* meaning any thing which contains *X*, from *X* itself upwards. And

Any part requires *some* whole
Some part requires *any* whole.

Thus

X)) Y

any part of *X* is in some part of *Y*
any part of *X* is in any whole of *Y*
Some part of *X* is in some part of *Y*
Some whole of *X* is in any whole of *Y*
etc. etc. etc.

This is the ground work of the change of quantities, in passing from extension to intension.

But it will take a life to get hold of the wider aspects of onymatic relations which have given rise to forms of language – But no one form [is] *completely octagonal*. It reminds one of the Indo-Germanic philology, here a form essential to a bit of system is utterly absent in one language, and turns up in another.

Here is a bit of correspondence between physiology and sociology. The two hands are to be joined together in the following manner [Fig. 1]

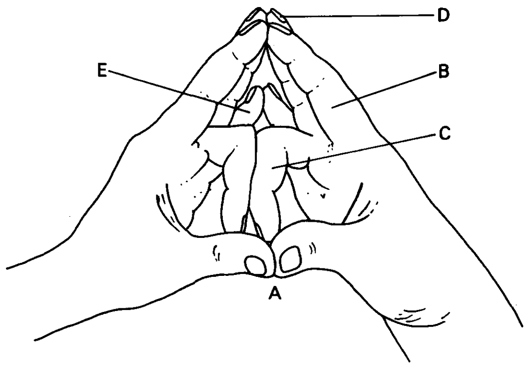


Fig. 1.

A	meant for thumbs.	Father and son
B	——— forefingers	Mother & daughter
C	——— second fingers	Presently explained
D	——— third	Husband & wife
E	——— fourth	Brother & sister

It will be found that father & son can separate – do, mother & daughter – do Brother & sister – but that husband and wife *cannot* – But let the Cs rise up and join – This is Sir Creswell Creswell – then husband & wife *can* separate.

With our kind regards to Mrs Boole & the etc. etc.

71. BOOLE TO DE MORGAN, 4 NOV. 1861

As you are great in all sorts of Arithmetical antiquities I send you an extract from a very remarkable book which I have lately read, 'Die Synagogale Poesie des Mittelalters', Dr [Leopold] Zunz, Berlin 1855. The book contains two terrible chapters on the sufferings of the Jews. In the second of these and referring to the seventeenth century occur these words, p. 345:

'Sogar in den Rechenbüchern wurden die Juden verfolgt, wo die Kinder die Zinsen ausrechneten, die Joseph der Wucherer einem mitteleidenden Christen abnahm u.dgl. Es gab damals weder einen Unterricht und ein Buch, noch ein Gesetz und eine Sitte aus denen nicht, von frühenter Jugend an, der Judenhass genährt und so zur zweiten Natur geworden wäre.'

[Translation: Even in books on arithmetic the Jews were persecuted, when children calculated the rate of interest which Joseph the Usurer charged to a compassionate Christian, etc. At that time there was no instruction or book, nor a law or custom which did not, from earliest youth onwards, nourish the hatred of the Jews, which thus became second nature.]

72. BOOLE TO DE MORGAN, 21 NOV. 1861

I don't think I shall be able to write to you in reply to the 'Logic' portion of your letter for a fortnight or three weeks. And I very much fear that though I am in the land in which a disposition to fight is supposed most to bloom I shall not be able to find you an antagonist. You could not contrive to bring in the Pope or Garibaldi or the Queen's Colleges – and without something of the kind I fear I can do nothing for you here. If I entered the lists myself it would be on your side and that would only make your case worse. Shade of Duns Scotus arise!

As to the Jews, the fullest information which I have seen about their present state & movement is contained in Vols I & X of 'Die Gegenwart' – a sort of supplement to the Conversations-Lexikon. Vol. X gives all the history, up to the time of its publication, of movements in Germany for getting rid of the ceremonial law, which some wish to do *wholly*, of renouncing all national distinction, etc. etc. – including even all veneration for and hope of ultimate return to the Promised Land. A member of this party in Frankfurt refusing to have his child circumcised, the hadebonary party actually went the length of calling upon the magistrates to deprive him of the civil privileges accorded to the other Jews.[3]

I fancy from what I have read that the literary portion of the Jews have been more deeply influenced by modern destructive criticism than the corresponding class among Christians – perhaps because the Jewish ceremonial must be felt now as it was in the days of Paul to be a grievous burden too heavy to be borne.

There is a curious fact about the religious poetry of the Jews of the Middle Ages the quantity of which as you probably know is very great. It is that the only really good poetry was written by the Jews of Spain especially under the Mohammedan princes. I noticed this myself in the book I mentioned compared with another, 'Die religiöse Poesie der Juden in Spanien' [Sachs 1845] – before I saw that it had been observed before. I believe the cause to have been that in Spain in the 10th, 11th & 12th centuries the Jews were less oppressed than in other countries and at subsequent periods. Shelley's lines

> Men
> Are cradled into poetry by wrong
> They learn in suffering what they teach
> in song

contains with some truth a great error.[4] Freedom for the development of faculty is essential to all intellectual products that are of any value. When sorrow falls upon a nature that has before been cultivated under favourable influences it may call out poetry – but a state of *continued* oppression and misery dwarfs all the faculties alike. Accordingly the poetry of the Synagogue of the middle ages – is for the most part one long wail.

This is quite enough about the people of Israel. I now want to ask you for some advice. I have got my house enlarged and want to take into it a few pupils. I should greatly prefer to get a few from England. Communication is now so easy that this ought not be difficult. My plan would be for my pupils to attend the classes in the College here and for me to keep a general supervision over *all* their studies and help them in their difficulties in *most*. Now is there any objection to my advertising in some such form (I *have* obtained the requisite sanction for receiving pupils) as thus 'Professor Boole wishes to receive a few pupils into his house. Terms & conditions may be known on application' – or is there any better way. The mention of my name would perhaps be my best mode of getting pupils here – for I fear the vague generality of 'A Professor in a College' etc. would not do much for me. I have done nothing yet.

With kind regards from Mrs Boole as well as myself to Mrs De Morgan etc. etc.

P.S. I should feel obliged if you would send me a Jewish newspaper some time or other – which I would return if you wished.

73. BOOLE TO DE MORGAN, 7 JAN. 1862

I ought to have written before to thank you for your answer to my question on your criticism of Jowett[5] – the answer is quite satisfactory – and also to say that in your controversy with the defenders of Hamilton's reputation you seem to me to be quite in the right – at any rate substantially so. The delay has this good in it – that it gives me the opportunity of sending you and yours our united good wishes for the coming year – I should rather say for that part of the new year which is yet to come. Perhaps I may have the pleasure of seeing you again before long. I think it not unlikely that Mrs Boole will accompany me on a visit to London at Easter but nothing is settled yet.

I have nothing to tell you except that very great changes are talked of a new college – beneficial ones I think in every way. I have had a good deal of work lately most of it voluntary and not I think and hope quite without result. With best regards to Mrs De Morgan in which Mrs Boole joins with me.

74. DE MORGAN TO BOOLE, 1 FEB. 1862

I shall write no more logic if you pester yourself with the duty of answering. I write as I am moved – by what spirit I cannot say till I have talked the matter over with Hamilton in the next world. In the meanwhile I have completed my examination of him in this, and the results are amusing.

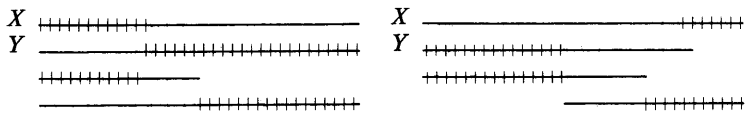
Bear in mind that what he says of *some* he means to deny of *all* the rest. Now first, he has actually forgotten to carry his system through. In his

Some X is not some Y

he leaves his some *singly* partitive. If he had carried his system into this proposition, queerly enough, his 'Some X is *not* some Y' would have been the equivalent of the old Aristotelian 'Some X *is* some Y' – the simple denial of 'No X is Y'. As follows

Some X is not some Y =
Some Y is not some X (by hyp.)
Therefore all other X is Y
all other Y is X.

This accords with



The |||||| being subject and predicate.

Every relation is here except X ———— Y ————

Now as to his poor syllogisms. They are 36 in number. 15 are saved by containing the identiy 'All X is all Y'. No other premise, with this will make a false syllogism, or a difficult one. For there is nothing to do but to insert X, equivalent of Y, in the other premise. In these 15 no peculiarity of H's system does work.

2 are saved by having *his* 'Some X is not some Y' as a premise, and conclusion also. But *his* $X(.)Y$, as he says 'quadrates with all the rest'. But these two would be good if $X(.)Y$ had been systematically treated.

4 are purely Aristotelian – H's system does not work any change of meaning in their premises or conclusion. There remain 15, in which his system works its will: and *all are vicious.*

11 are absolutely false, and turn men into stones, or anything else you please.

4 are only incomplete – but would be complete and valid if (.) were treated by the system.

I have written to Mansel to know what he took Hamilton's system to be in 1851, when he spoke in such high terms of it. From some of his expressions he clearly acknowledges some departure from the old meaning of 'some'. But whether by single or double partition I cannot make out. He has not yet answered. If he meant double partition, he is in an ugly fix: but I suspect he really only thought of the single.

I hope all is well with you. My wife unites in kind regard.

Letters 75–82

In these letters the controversy about Hamilton's logic rumbles on. In Letter 77 (20 September 1862) he mentions his intention to make a 'last appeal to Hamilton's successors' in order to find out from them exactly what Hamilton meant by 'some'. This appeal was contained in a letter in *The Athenaeum*, 18 October 1862, which led to a correspondence in the columns of that journal with Baynes. De Morgan summed up the results of this correspondence in an *Addition* to *On the Syllogism V*, dated 26 December 1862 (De Morgan 1966, 337–45).

De Morgan begins this letter with some unusually astringent remarks' 'I am much obliged to you for your last on probability. Having nothing at all on hand except a valuation, an introductory lecture, an examination, a paper for the Cambridge Trans., a last appeal to Hamilton's successors. . . with some odds and ends not worth recounting – I shall be able to give it a full reading in less than six months'.

An item of general interest in this letter concerns one of De Morgan's introductory lectures at University College London. The lecture was on 'the method of examining at Cambridge'. This lecture was never printed, however (S.E. De Morgan 1882 278–9.

Letters 79 (6 November 1862) and 80 (7 November 1862) contain a number of interesting remarks on logic and other matters. Perhaps the most interesting remark is made in Letter 79 (6 November 1862) when Boole asks De Morgan for his thoughts on 'Renan's declaration that to the Semitic mind expressing itself in the Semitic forms of language no logic was possible'. As well as his well-known *La Vie de Jesus*, Renan wrote on linguistics. This remark (which I have not traced) may have occurred in *L'Histoire des Langues Sémitiques*, 1847 or in *Histoire générale et système comparé des langues sémitiques,* 1855. De Morgan's reply – in Letter 80 (7 November 1862) – is an anecdote on the Persians; a curious error on De Morgan's part, as the Persians are not Semites.

In Letter 79 Boole remarks: 'I have been studying a bundle of letters of yours on Logic . . . [I] feel I have nothing to say about them but I have been interested in them very much. But it has been like reviving an interest that had died . . . However I . . . look forward to the time when I shall study Logic again'. Near the end of the same letter: 'I will look at the syllogisms of Hamilton but I shall do it for your sake not his'.

In Letter 70 (16 October 1861) De Morgan had proposed the notion of a terms as 'that which divides'. Boole was not convinced and expressed his doubts in Letter 80.

Boole seems still to have felt isolated in Cork. In Letter 79 Boole writes plaintively: 'There is absolutely no person in this country except myself with whom I ever speak on subjects like this [i.e. logic]. I feel this as one of the many drawbacks in living in this country. . . '. But De Morgan replies trenchantly in Letter 80: 'I have not *one* person to whom I can speak on logic . . . I go from one month to another without any conversation on my studies with a person whom I cannot claim to teach . . . therefore I warn you against the notion that you are a mental Robinson Crusoe'.

But there are also matters unconnected with the Hamilton controversy. In Letter 78 (4 November 1862) Boole sends De Morgan a quotation from Leibniz on logic. The extract is from a letter Leibniz wrote to Wagner and may be found in Leibniz 1875, Band 7, 514–27. An English translation appears in Loemker 1956, (see third edition 1969, 462–71).

Letter 75 contains a detailed account of some research on differential equations; near the end he remarks: 'I fear that I may have said enough to tire you. Pray do not feel any scruple about treating my letters in the way you ask me to treat yours'. We may feel surprised that people in those times found the time to indulge in extensive letter writing; that they did shows the value they placed on correspondence. But the longer letters that passed between De Morgan and Boole were not easily answered. Boole is concerned with eliminating a number of differentials from a system of partial differential equations. As the process is one of some complexity and Boole's notation is rather cumbersome, I shall give an account here in present-day style but adhering to Boole's notation as far as is practicable. Roman numerals are used to distinguish equations I have numbered from those numbered by Boole.

Boole begins with an equation

$$\sum_{j=1}^{n} \phi_{ij}(x_1, x_2, \ldots, x_n)\,\mathrm{d}x_j = 0, \qquad \text{(I)}$$

where $1 \leqslant i \leqslant n-r$, and seeks a function $P(x_1, x_2, \ldots, x_n)$ such that the equation

$$\sum_{j=1}^{n} \partial_j P\,\mathrm{d}x_j = 0 \qquad \text{(II)}$$

is compatible with (I). Any such P is an integral of (I). Solving (II) for $\mathrm{d}x_n$ and substituting the resulting expression for $\mathrm{d}x_n$ in (I) yields:

$$\sum_{j=1}^{n-1} \{\phi_{in}\,\partial_j P - \phi_{ij}\,\partial_n P\}\,\mathrm{d}x_j = 0 \qquad \text{(III)}$$

where $1 \leqslant i \leqslant n-r$. Using these $n-r$ equations we may eliminate $\mathrm{d}x_{n-1}$, $\mathrm{d}x_{n-2}, \ldots, \mathrm{d}x_{n-r+1}$ finally obtaining the single equation

$$\sum_{j=1}^{r} \psi_j\,\mathrm{d}x_j = 0. \qquad \text{(IV)}$$

The coefficients ψ_j, $1 \leqslant j \leqslant r$ are linear in $\partial_j P$. Now set these $\psi_j = 0$; in the case $r = 2$ these equations yield Boole's equations (1) and (2). From this point, following Boole, I shall assume $r = 2$. Thus we now have Boole's equations

$$\Delta_1 P \equiv \sum_{j=1}^{n} A_j \partial_j P = 0 \qquad (1)$$

$$\Delta_2 P \equiv \sum_{j=1}^{n} B_j \partial_j P = 0. \qquad (2)$$

Form now $(\Delta_1\Delta_2 - \Delta_2\Delta_1)P = 0$. The second order partial derivatives cancel out in pairs and we obtain

$$\Delta_3 P \equiv (\Delta_1\Delta_2 - \Delta_2\Delta_1)P = \sum_{j=1}^{n} K_j \partial_j P = 0 \qquad \text{(V)}$$

where

$$K_j = \sum_{i=1}^{n} (A_i B_{ji} - A_{ji} B_i),$$

(here B_{ji} means $\partial B_j/\partial x_i$ etc.). Using (1), (2) to eliminate $\partial_n P$, $\partial_{n-1} P$ from (V) we obtain

$$\Delta_3 P \equiv \sum_{j=1}^{n-2} C_j \partial_j P = 0. \qquad (3)$$

Boole envisages repeating this argument, forming $\Delta_2\Delta_3 - \Delta_3\Delta_2$ and hence $\Delta_4 P$ etc.. Now it may be that $\Delta_3 P$ is identically zero; in any case the construction of successive $\Delta_k P$ will ultimately become identically zero and the process terminates. Let $\Delta_m P$ be the last non-identically zero expression and consider

$$\Delta_1 P = \Delta_2 P = \ldots = \Delta_m P = 0.$$

The final part of the argument consists of forming

$$\sum_{j=1}^{m} \lambda_j \Delta_j P = 0,$$

which may be written

$$\lambda_1 \sum_{j=1}^{m} A_j \partial_j P + \lambda_2 \sum_{j=1}^{m} B_j \partial_j P + \ldots = 0.$$

a differential equation which yields

$$\frac{dx_1}{\lambda_1 A_1 + \lambda_2 B_1 + \ldots} = \frac{dx_2}{\lambda_1 A_2 + \lambda_2 B_2 + \ldots} = \ldots = \frac{dx_n}{\lambda_1 A_n + \lambda_2 B_n + \ldots}.$$

In conclusion Boole envisages 'eliminating the λs [giving] a system . . . which will be capable of reduction to *exact differentials*'.

Two points need to be made in conclusion. First: in Letter 75 Boole made one or two minor slips writing $n + r$ for n or n for $n - r$. I have corrected these slips. Second: Boole had intended including a discussion of the ideas of this work in the second edition of his *Treatise on Differential Equations*. The supplementary volume, edited by I. Todhunter (Boole 1856) included related material in Chapters 25–7.

Letter 76 (21 April 1862) mentions a correspondence carried on at this time between Boole and Cayley. 'Mr Cayley', writes Boole, 'wrote to me about an old subject of dispute between us . . .'. The subject was the question on probabilites which Boole put to De Morgan in Letter 33 (24 July 1851) and which was referred to in Letters 34–7 (July–August 1851). Cayley wrote a paper giving his answer to the question (Cayley 1853); Boole disputed Cayley's interpretation of the question in Boole 1854*b*. Cayley summed up this phase of the controversy in a note in volume 2 of his *Mathematical Papers* (Cayley 1889, vol. 2, 594–5). The disagreement hinged upon the matter of what should be assumed relating to the independence or otherwise of events.

In 1862 Cayley again tried to settle this question and sent Boole his new thoughts; Boole again did not wholly agree with Cayley and wrote him a letter setting out eight observations. Cayley included these as part of his published solution (Cayley 1862*a*). Perhaps the most important outcome of the renewal of this correspondence was that it led Boole to a general determinantal criterion for 'the conditions of analytical validity of the method . . .', which he published in the *Philosophical Transactions of the Royal Society* (Boole 1862*a*).

Boole's query in Letter 76, 'Are you ever disposed to see Ireland (I have seen enough of it) travelling is easy now . . .' indicates the effect the development of the railways had on travel in Britain in the period of the correspondence. By 1850 the railway route from London to Holyhead and thence by boat to Dublin was open; also Dublin and Cork were connected by rail by this time. However, this suggestion must have fallen on barren ground. De Morgan rarely left London and writing to W.R. Hamilton in 1853 he siad: 'I never got further north than Cambridge, and never while at Cambridge penetrated to the northern extremity of the town. So much for me as a sight-seer and traveller. And yet I have been in three-quarters of the globe – in *arms* – not as a combatant but as an infant' (Graves 1882, vol. 3, 462).

75. BOOLE TO DE MORGAN, 12 FEB. 1862

I have received your second logical epistle & have put it aside with its predecessor to be studied in due time. The subject at which I have been working is not logical though it contains a good deal of applied Logic & I was once obliged even to have recourse to my own Calculus of Logic in order to guide me through the maze. The results connect themselves a good deal with some of your own speculations & I will give you a brief account of them as I think they will interest you.

The subject is the theory of the solution of simultaneous differential equations in which the number of variables exceeds by more than one the number of equations so that we cannot say beforehand even that an integral in the ordinary sense of the term exists. I wished to discover a process for determining how many integrals such a system admits & how they may be found i.e. how their determination may be made to depend on the always theoretically possible solution of differential equations in which the number of variables exceeds by one the number of equations. I have succeeded in this and the method is as follows.

1st A system of $n - r$ simultaneous differential equations of first order and degee between $n + r$ variables can always be reduced to a system of r partial differential equations linear and of first order (Let $P = c$ be an integral of the system, eliminate $dx_1\, dx_2\, dx_n$ between the system and $dP = 0$ and equate to 0 the coefficients of the other differentials).

In what follows I will suppose $r = 2$ as the theory of other cases is involved in this.

2ndly In the supposed case of $r = 2$ we have two partial differential equations of the form

$$A_1\frac{dP}{dx_1} + A_2\frac{dP}{dx_2} \ldots + A_n\frac{dP}{dx_n} = 0 \qquad (1)$$

$$B_1\frac{dP}{dx_1} + B_2\frac{dP}{dx_2} \ldots + B_n\frac{dP}{dx_n} = 0 \qquad (2)$$

If we multiply the second by λ, add to it the first, form the auxiliary system of common differential equations by Lagrange's method and then eliminate λ we fall back on the *original* system of differential equations. Instead of this proceed as follows.

3rd Let

$$\Delta_1 = A_1\frac{d}{dx_1} + A_2\frac{d}{dx_2} \ldots A_n\frac{d}{dx_n}$$

$$\Delta_2 = B_1\frac{d}{dx_1} + B_2\frac{d}{dx_2} \ldots B_n\frac{d}{dx_n}$$

then the partial differential equations are

$$\Delta_1 P = 0, \qquad \Delta_2 P = 0.$$

Form now the partial differential equation

$$\Delta_1\Delta_2 P - \Delta_2\Delta_1 P = 0$$

or for simplicity

$$(\Delta_1\Delta_2 - \Delta_2\Delta_1)P = 0.$$

This will be *linear* like (1) & (2) and if we eliminate $\frac{dP}{dx_{n-1}}$ $\frac{dP}{dx_n}$ will be of the form

$$C_1\frac{dP}{dx_1} + C_2\frac{dP}{dx_2} \ldots + C_{n-2}\frac{dP}{dx_{n-2}} = 0 \qquad (3)$$

The equations $\Delta_1 P = 0$ $\Delta_2 P = 0$ may be so prepared that this equation shall be obtained directly without the final elimination.

Represent (3) by $\Delta_3 P = 0$ and repeat the process between it & either or in succession both of the former equations we shall thus get two new equations

$$\Delta_4 P = 0 \qquad \Delta_5 P = 0$$

one of which shall have one, the other two, differential coefficients of P fewer than $\Delta_3 P = 0$.

This process must be continued always in the form of *diminishing* the number of differential coefficients till it stops i.e. till no new partial differential equations arise.

Suppose in this process m partial differential equations have been obtained then the original system of common differential equations will have just so many integrals less than it has equations as there have been partial equations *obtained* i.e. additional to the two by which the given system was replaced.

To find the integrals let

$$\Delta_1 P = 0 \quad \Delta_2 P = 0 \quad \ldots \quad \Delta_m P = 0$$

be the whole system of partial differential equations. Then forming the equation

$$\Delta_1 P + \lambda_1\Delta_2 P \ldots + \lambda_{m-1}\Delta_m P = 0$$

and by Lagrange's method its auxiliary system, & lastly eliminating the λs we have a system of final equations which will be capable of reduction to *exact differentials* & will give the integrals sought.

I will not go into the Logic of this further than to note that from the form of the symbolical equation

$$(\Delta_1\Delta_2 - \Delta_2\Delta_1)P = 0 \qquad \text{(A)}$$

and from the nature of the symbols Δ_1, Δ_2 any simultaneous solutions of $\Delta_1 P = 0$ $\Delta_2 P = 0$ will satisfy (A) – secondly that (A) expresses the condition which must be satisfied in order than an integral of the given system obtained by making a superfluous variable constant may by variation of parameters become an integral of the system *un*reduced.

One of the most important applications of the method is to the determination of the conditions under which

$$Rr + Ss + Tt + s^2 - rt = V$$

admits of different forms of integration. Here is one result. The necessary & sufficient conditions for the above admitting an integral expressed as the envelope of

$$z = \phi(x, y, a, b, c)$$

a, *b*, *c* being subject to two arbitrary connecting conditions are the following viz. $S^2 = 4(RT - V)$

(1) $m^2 - Sm + RT - V = 0$. [Crossed out by Boole]

(2) $\Delta_1 R + \Delta_2 m = 0$

(3) $\Delta_1 m + \Delta_2 T = 0$

in which *m* is one of the equal roots of

$$m^2 - Sm + RT - V = 0$$

and

$$\Delta_1 = \frac{d}{dx} - m\frac{d}{dq} + T\frac{d}{dp} + p\frac{d}{dz}$$

$$\Delta_2 = \frac{d}{dy} + R\frac{d}{dq} - m\frac{d}{dp} + q\frac{d}{dz}$$

The first condition was given by Ampère and yourself. I have never seen the others.

I fear that I may have said enough to tire you. Pray do not feel any scruple about treating my letters in the way in which you ask me to treat yours.

We are all very well & give our kind united regards to Mrs De Morgan.

P.S. If you happen to know of anything of this kind having been done before I need not say that I should be obliged by your letting me know. Please to keep my letter.

76. BOOLE TO DE MORGAN, 21 APRIL 1862

I was very glad to receive your tract this morning because not having heard from you for some time and also not having recognised your hand in the Athenaeum we began to think you were ill. I dare say you were busy.

I have been myself hard at work on the Theory of Probabilities. I had just finished my original on differential equations which I sent you a short account of – had not and have not yet drawn up a paper on the subject, when Mr Cayley wrote to me about an old subject of dispute between us adding however that he was strongly inclined to believe in my theory of Probabilities developed in the Laws of Thought but could not understand the metaphysics of it. I do not know whether I have made him do so now but the correspondence has led me to resume the analytical discussion of my method which I had vainly attempted to complete before – this time with success. I have proved that in all cases the conditions of analytical validity of the method are simply the conditions of consistency in the data – what I have elsewhere termed the conditions of possible experience.

I do not think I have ever engaged in as difficult a mathematical investigation. The most important part of it consists in proving that a certain functional determinant is always positive whatever the number of the variables n. When I tried to do this before I could not get beyond $n = 4$ and the difficulty of getting thus far you may imagine when I mention that while for $n = 2$ the number of terms of the determinant is 4 – for $n = 3$ it is 52. That is a tolerably rapid rate of progression. For $n = 4$ it is some monstrous number which I never took the trouble to investigate.

I was going to get about Logic including your letters when Cayley's letter came and when I once got fairly started in the inquiry I could not stop. Now I must finish both papers before taking up the Logic.

We hope you are all well. Are you ever disposed to see Ireland (I have seen enough of it) travelling is easy now and you might be the better for a change. If so it would be the greatest pleasure to see you here and Mrs De Morgan with you if she could come. We are all pretty well.

77. DE MORGAN TO BOOLE, 20 SEPT. 1862

I am much obliged to you for your last on probability. Having nothing at all on hand except a valuation, an introductory lecture, an examination, a paper for the Cambridge Trans. a last appeal to Hamilton's successors to know what his system was, and an approaching session, – with some odds and ends not worth recounting – I shall be able to give it a full reading in less than six months.

I am going to try whether by any sarcasm I can get a pupil of Hamilton to say what he taught as the meaning of 'some'. Spencer Baynes, the pupil, the prizeman, the substitute, and the accredited expounder will *not* answer a letter on the subject – I wrote to him months ago, and he will not even acknowledge receipt. Mansel does not know: Fraser neither knows nor can find out. But my letter to the Athenaeum – which I intend for the latter end of October, will announce that registered copies of the Athenaeum containing it are to be sent to Mansel, Veitch, Sp. Baynes, and Fraser, editors, substitute, and successor. And I expect that even then there will be no answer.

I hope Mrs Boole and the little ones are well.

78. BOOLE TO DE MORGAN, 4 NOV. 1862

I congratulate you on having got an antagonist at last. I shall look for the next forthcoming numbers of the Athenaeum with interest and I will read over also your letters to me which I had taken out of the repository in which they were kept, in order to examine them again, just before I saw the letter of Mr Baynes.

Here are a couple of lines which show what Leibnitz though about the Logic of the past and that of the future.

– 'so muss ich zwar bekennen dass alle unsere bisherigen Logiken kaum ein Schatten dessen sein *so* ich wünsche und *so* ich gleichsam von ferne sehe'.
Schreiben an Wagner.

[Translation: 'Indeed I must admit that all our logics up to now are a mere shadow of what I would desire and what I see from afar'. Written to Wagner.]

As you once mentioned that you are not much in the habit of reading German I add that *so* in the German of Leibnitz's time is often used for the relative pronoun. The whole piece is interesting.

79. BOOLE TO DE MORGAN, 6 NOV. 1862

I have been studying a bundle of letters of yours on Logic, and now that I sit down to write a few lines to you feel that I have nothing to say about them but that I have been interested in them very much. But it has been like the reviving of an interest that had died. There is absolutely no person in this country except my wife with whom I ever speak on subjects like this. I feel this as one of the many drawbacks in living in this country and as not the least of them. However I do continually look forward to a time when I shall study Logic again, and begin to hope that it is not far off. I do not so much care about the mere forms of Logic as about the philosophy of the connexion between thought and speech. What do you think for instance of E. Renan's declaration that to the Semitic mind expressing itself in the Semitic forms of language no Logic was possible. I wish I knew Hebrew or still better Arabic.

One of your postulates I have always felt doubtful of. It is that a term *divides*[6] the Universe that therefore its local extension is the individual its highest the penultimate one – all but one. I don't deny that it is possible to devise a scientific scheme of logical forms on this basis. I don't deny that common language often seems to favour the idea of such a postulate as its ground. But this is not uniformly the case. I think the nature of the discourse, the state of mind of the speaker determine generally not only the extent of the Universe of discourse but also the extension of terms. But very often misunderstandings about these things arise and must be settled by distinct question. In what sense do you speak of All, What do you mean by Some? I think that limiting conditions ought in general not to be introduced in the scientific treatment of a subject. Conceptions and terms ought to be as general as possible and the limitations introduced afterwards when wanted. Indeed I have been led to think in pure logic the existence of the objects ≢ designated by the terms ought not to be assumed, even the categorical proposition partaking of the hypothetical character or being connected with a tacit hypothetical

All men (if men exist) are mortal.

Mathematically I would say that limiting cases ought to be included not excluded.

I will look at the syllogisms of Hamilton but I shall do it for your sake not his. He and all his followers appear to me to have been trifling when writing about Logic. The notion that they have mapped out the whole kingdom of formal thought is a delusion that can only exist through ignorance – a kind of ignorance which prevails in no other subject.

80. DE MORGAN TO BOOLE, 7 NOV. 1862

I have not *one* person to whom I can speak on logic – nor, except pupils, on mathematics. I go from one month to another without any conversation on my studies with a person whom I cannot claim to teach. And you might live in London and do the same: therefore I warn you against the notion that you are a mental Robinson Crusoe. How many are there who can talk or think of the first principles of anything? Even those whom one would surfeit, by reason of the depth of their knowledge for applications make a terrible hash of any attempt to probe anything to the foundation.

As to Semitics not being capable of logic, the first thing that arises in my mind is the account given me by a relation who was diplomatically employed in Persia. He says the Persian is a stickler for the precise sense of words in all his dealings. If he can entrap you into a phrase which is literally explicable in his own favour, he claims to hold you to it. He would, if it suited him – for he knows better than to let the net catch himself – bind the Spartans to keep their laws for ever, when Lycurgus had made them promise that they would do so till he came back – he having given them to understand he was going away for a while. Now all this – though very crafty – is logical craft: these shufflers must have logical power – for shuffling is of its own nature perverted logic.

I think I shall persuade you at last that the term divides the universe in fact – and ought to do in reason.[7] I admit a chapter on the *omniternal* name – and one on the *vacuous* name. Would you under *some*, include the limiting case *none*? If the x of logic may = 0, then 'Some A is B' is *always* true. With the *omniternal* & the *vacuous* terms I think you can do nothing in comparison of *relations*. If, the universe being *animal*, I have occasion to affirm *sentience* of a species, I think I am – for the moment – recognising something external to my *universe*; that is, thinking in another and larger universe. I am satisfied that A is always in thought *compared* with not-A except in this one proposition: 'let us dismiss not-A' – that is, let A be our universe; let our propositions refer to distinction between one thing and another in A.

Is it the *rule* of mathematics that the limiting case is included?

To my system it matters nothing but this – when you will introduce the idea of your *universe*, you introduce it as distinguished from your *non-universe*: for you cannot bring in your universe without bringing in the idea of that which is out of it. A system which admits and systematises contrary notions brings in non-U or u, whenever it brings in U at all. Thus

Every X is U

Every Y is U

has reference to extents out of U, or is unmeaning. And the conclusion

Some things are neither *X*s nor *Y*s – refers to those things out of *U*.

As to the Hamiltonian – or any system – mapping out thought – they might as well say that Columbus mapped out America. I am sure that the forms of thought are of a development as yet unconceived – and will be more so as we get higher.

I have my paper no. V [D 1863*a*] before me in MSS (from Cambridge) for some last corrections. Mr Baynes, you will see, has not satisfied me. I would bet even that he will not say *yes* or *no* to my question in today's Athenaeum, without qualification. If he do, it will be the first time I have got a decided answer about quantity from an ordinary logician, in print or out of print. I defy you to give me a writer who is clear on the meaning his 'some'.

Baynes's not seeing my letters is all gammon. He tells me that he saw the *first* – I may think it possible that he thought it best *not to see* the others. Mansel who bridled up when he thought he had some defence for H's mathematical blunders – tells me – by way of excuse for not seeing that I had challenged any one to say that his editor did not propound paralogisms – that he 'does not always see' the Athenaeum.

Our kind regards to Mrs Boole – the small ones well, I hope.

81. BOOLE TO DE MORGAN, 10 NOV. 1862

Do you happen to have preserved and can you give me the date of a letter in which I communicated to you the method of solving simultaneous differential equations.

82. DE MORGAN TO BOOLE, 12 NOV. 1862

I consider a letter such as this a limit to append the answer: a thing sometimes useful in priority matters.[8]

The only letter having reference to differential equations which I can lay my hand bears date February 12, 1862 and refers to

'1st A system of $n-r$ simultaneous differential equations between n variables . . .'

'One of the most important applications . . . is to the . . . conditions under which

$$Rr + Ss + Tt + s^2 - rt = V$$

admits of . . . integration

'Please keep this letter'

That is, I suppose, the letter you refer to.

Notes

[1]A discussion of the influence of this controversy on Boole's *Mathematical Analysis of Logic*, Boole 1847*a* may be found in Laita 1979.

[2]A spicilegium is 'a gleaning, a collection or anthology' according to the Shorter Oxford English Dictionary.

[3]See *Die religiöse und culturhistorische Bewegung im Judenthum*, Gegenwart 1848, vol. 1, 253–406, vol. 10, 526–603.

[4]The lines of Shelley are from *Julian and Maddalo*, written 1818–19 after Shelley's first visit to Venice, where he met Byron (who is identified with Count Maddalo of the poem) and first published in *Posthumous Poems*, 1824. Shelley wrote 'Most wretched men/Are cradled . . .'.

[5]The name of the person criticised by De Morgan is not easy to read, but I think Jowett is the correct reading.

[6]See Letter 70.

[7]See Letters 70 and 79.

[8]De Morgan wrote this answer to Letter 81 upon the blank portion of that letter.

7
FROM DIFFERENTIAL EQUATIONS TO SPIRITUALISM: 1863-1864

The final group of letters span the seventeen months from January 1863 to May 1864; Boole died in December 1864. The letters contain the usual mixture of serious mathematical and logical matters and light-hearted general remarks.

Both Boole and De Morgan mention their being busy. Boole in Letter 83 (3 January 1863) says: 'Your letters on Logic are not forgotten and will be taken up again in due time. I want to get clear of mathematics . . .'. De Morgan in Letter 86 says: 'I have escaped at last from Session work . . . I have put differential equations by until I come on the subject again, in Heaven's good time . . .'.

Among the variety of minor points we find Boole asking if De Morgan has any spare copies of the Life of Walsh (Boole 1851*h*) 'for Mrs Boole who has not read it'. Boole apparently had no difficulty in disposing of his 50 reprints (see Letter 38). De Morgan indicates his dislike for the German language in Letter 86 (5 July 1863) – 'I am not in love with it . . . I impute to that unfortunate language seven deadly sins . . .'. Commenting on some of Boole's logical remarks and a paper on probabilities, De Morgan says: 'We are, I see, on different rails, but we may come to a junction. I feel sure of there being many separate routes . . .'. Another form of transport crops up in Letter 85 (7 February 1863) when De Morgan relates a 'remark of an old gentleman in an omnibus today'. De Morgan's wish for an 'inverse method of elimination' in Letter 85 (7 February 1863) seems to have remained unfulfilled.

Letters 83–86

By far the most important topic discussed in these letters is contained in Letter 84 (7 January 1863). The major part of this letter is concerned with the simplification of a differential form. The problem is to express $\Sigma_{i=1}^{2n} x_i \mathrm{d}x_i$ as $\Sigma_{i=1}^{n} U_i \mathrm{d}V_i$. Boole states (but gives no proof of) a number of relations which X_i, U_i, V_i and their partial derivatives must satisfy. These are more intelligible in modern notation. Writing

$$U_{kj} = \frac{\partial U_k}{\partial x_j}$$

etc., and using matrix notation so that

$$B_{ij} = X_{ij} - X_{ji} \qquad 1 \leqslant i, j \leqslant 2n$$

and A_{ij} are the elements of the inverse of the matrix $[B_{ij}]$ the relations become

$$\sum A_{ij} X_i U_{kj} = U_k \tag{I}$$

$$\sum A_{ij} V_{kj} = 0 \tag{II}$$

$$\sum A_{ij} U_{ki} U_{lj} = 0 \tag{III}$$

$$\sum A_{ij} V_{ki} V_{lj} = 0 \tag{IV}$$

$$\sum A_{ij} U_{ki} V_{lj} = \delta_{kl} \tag{V}$$

where the summations are over i and j which range from 1 to $2n$. This work arises from Clebsch's discussion of Pfaff's problem; this problem concerns the solution of a total differential equation in which not all the integrability conditions are satisfied. Pfaff wrote on this in 1814 (Pfaff 1815). Clebsch's discussion appeared in Crelle (Clebsch 1862).

De Morgan replies in Letter 85 (7 February 1863): 'Your method will, I see, simplify the matter, so far as I understand it from you . . . You will of course shape it and publish it'. Boole began to write a paper in German on this work but death intervened before it was completed; the fragment was included in the second edition of Boole's *Differential Equations* (Boole 1865, 715–7).

A mention of Isaac Todhunter in Letter 83 (3 January 1863) reminds us of another piece of work that Boole did not live to complete – the second edition of his *Differential Equations.* He had planned extensive changes; after his death I. Todhunter took the responsibility for the second edition; he made only minor changes in the existing text, but put the major additions that Boole had been writing in a *Supplementary Volume* which appeared, as did the revised second edition of the original work, in 1865. The chapters of the *Supplementary Volume* are numbered consecutively with those of the original work, viz. 19 to 33.

De Morgan, in Letter 86 (5 July 1863), writes on matters of logical terminology. This letter also refers to a possible sixth paper *On the Syllogism*; such a paper was never written, although he did sketch an outline of it, which was printed by Heath in De Morgan 1966, 346–7.

Also in this letter De Morgan mentions 'a paper I have long threatened myself with – on *infinity*'. The main object of the paper, which is in two parts, is to rescue from oblivion a concept of the actual infinite – as opposed to infinity as a description of a kind of limiting process.

The early pages of the first part contain an interesting recapitulation by De Morgan of the views of various philosophers and mathematicians upon infinity. He starts with Aristotle, and proceeds with Leibniz, Locke, D'Alembert, Hobbes, John Mill, Kant, Berkeley. Later he connects his ideas on infinity with related ideas on infinitesimals and introduces ideas of orders of infinity. The latter is the primary concern of the second part of the paper where what is perhaps the most interesting idea of the paper appears. De Morgan introduces the idea of infinite quantities of various integer orders writing A_n, B_n to denote two infinites of the nth order. He then defines the notation $=^m$ to indicate that the symbols on either side of the sign $=^m$ denote infinities which differ by an infinite of order less than m. This idea is used also in respect of infinitesimals. Thus

$$\mathrm{d}x =^0 \mathrm{d}x + (\mathrm{d}x)^2,$$

(one of De Morgan's examples) indicates that we are able to discard higher order infinitesimals – without disregarding the proper meaning of =.

De Morgan's wife's book on spiritual (i.e. psychic) experiences (S.E. De Morgan 1863) is mentioned lightly as 'a funny production' in Letter 86. De Morgan wrote the preface under the pseudonym A.B. His wife took C.D. as pseudonym. De Morgan 'proposed that the title page should have the legend "Jack Sprat could eat no fat/His wife could eat no lean". But this was judged *infra dig*'.

De Morgan's account of a remark of Samuel Johnson's in this letter lacks the force of the original. On being asked by a Scot what he thought of Scotland, Johnson replied: 'That it is a very vile country to be sure, Sir'. 'Well, Sir', replied the other, 'God made it'. 'Certainly he did', (answered Johnson), 'but we must always remember that he made it for Scotchmen, and comparisons are odious, Sir, but God made hell'; (Hill 1897, vol.1, 265).

83. BOOLE TO DE MORGAN, 3 JAN. 1863

I send you a paper in another cover. Do you happen to have *one* or *two* copies of the Life of Walsh [1851*h*] to spare. If *one* please send it to me for Mrs Boole who has not read it – if *two* please send one to me and one to Mr Todhunter of St John's College Cambridge if you do not think it too much trouble.

I wish you all, and Mrs Boole joins me in this, a happy new year. Your letters on Logic are not forgotten & will be taken up again in due time. I want to get clear of mathematics but this will not be till a new edition of the Differential Equations is out, and certain portions of this subject have been quite changed in the year or two which have elapsed since the first edition was published.

84. BOOLE TO DE MORGAN, 7 JAN. 1863

What I now write will interest you a good deal or not at all according to whether you have ever been working in the same direction or not.

Clebsch in Crelle vol.60 Heft 3 [Clebsch 1862] has made a great step in the theory of Pfaff's problem of reducing

$$X_1 \mathrm{d}x_1 + X_2 \mathrm{d}x_2 \ldots + X_{2n} \mathrm{d}x_{2n}$$

to the form

$$U_1 \mathrm{d}V_1 + U_2 \mathrm{d}V_2 \ldots + U_n \mathrm{d}V_n$$

He has found the partial differential equations which $V_1 \ldots V_n$ satisfy.

On reading his paper it occurred to me that a method founded on the Calculus of variations, which I had applied to some other problems for instance to the proof of Jacobi's principle of the Ultimate Multiplier ought to give the general solution of Pfaff's problem. I tried & succeeded deducing the partial differential equations for $U_1, \ldots U_n$ as well as for $V_1, \ldots V_n$; as well as also those connecting the two sets of quantities.

They are as follows

1st Let $$\frac{\mathrm{d}X_h}{\mathrm{d}x_k} - \frac{\mathrm{d}X_k}{\mathrm{d}x_h} = (h, k)$$

Let the linear algebraic equations[1]

$$(1, 1)s_1 + (2, 1)s_2 \ldots + (2n, 1)s_{2n} = t_1$$
$$(1, 2)s_1 + (2, 2)s_2 \ldots + (2n, 2)s_{2n} = t_2$$
$$(1, 2n)s_1 + (2, 2n)s_2 \ldots + (2n, 2n)\, s_{2n} = t_{2n}$$

be solved determining $s_1, s_2 \ldots s_{2n}$ and let

$$s_r = A_{r,1}t_1 + A_{r,2}t_2 \ldots + A_{r,2n}t_{2n}$$

be the type of the solutions. The whole series of quantities $A_{h,k}$ is thus determined as functions of $x_1, x_2 \ldots x_{2n}$.

Then 1st any quantity U_i satisfies the partial differential equations

$$\sum_h \sum_k A_{hk} X_h \frac{\mathrm{d}U_i}{\mathrm{d}x_k} = U_i \qquad \text{(I)}$$

h & k each admitting all values from 1 to $2n$ inclusive

2nd Any quantity V_i satisfies

$$\sum_h \sum_k A_{hk} \frac{\mathrm{d}V_i}{\mathrm{d}x_k} = 0 \qquad \text{(II)}$$

3rd Between any two U_i, U_j exists the relation

$$\sum_h \sum_k A_{hk} \frac{\mathrm{d}U_i}{\mathrm{d}x_h} \frac{\mathrm{d}U_j}{\mathrm{d}x_k} = 0 \qquad \text{(III)}$$

4th Between any two V_i, V_j the relation

$$\sum_h \sum_k A_{hk} \frac{\mathrm{d}V_i}{\mathrm{d}x_h} \frac{\mathrm{d}V_j}{\mathrm{d}x_k} = 0 \qquad \text{(IV)}$$

5th Between any two U_i, V_j the relations

$$\sum_h \sum_k A_{h,k} \frac{dU_i}{dx_h} \frac{dV_j}{dx_k} = \begin{matrix} 1 \text{ if } i = j \\ 0 \text{ if } i \text{ not} = j \end{matrix} \qquad \text{(V)}$$

(II) & (IV) are what Clebsch gets. His analysis is of the most extraordinary complexity. What I send this to you for is chiefly to illustrate by a result the remarkable separating power of the calculus of variations in other modes of *integration* giving in an orderly series the results of complex transformations practically all but unmanageable by other modes.

A happy new year to you all from me & mine.

85. DE MORGAN TO BOOLE, 7 FEB. 1863

I thank you for yours of 7 Jan. Your method will, I see simplify the matter, so far as I understand it from you. But it is not simple in itself. You will of course shape and publish it.

I have not thought about differential equations for some time. I wish somebody would study the inverse method of elimination: that is, the *anti*-reduction of one equation to two or more with introduced letters.

For example

$$\phi(x, y, y' \ldots y^{(n)}) = 0$$

that this shall result from

$$F(x, y, \ldots y^{(n)}, v) = 0$$
$$f(x, y, \ldots y^{(n)}, v) = 0$$

differentiate these k times giving $2k + 2$ equations containing $x, y, \ldots y^{(n+k)}$, $v, v', \ldots v^{(k)}$. Let $2k + 2 = n + k + 2$ or $k = n$[;] eliminate $y\, y' \ldots y^{(2n)}$ from the $2n + 2$ equations, and there remains an equation between $x\, v\, v' \ldots v^{(n)}$.

If this can be integrated, substitute for v in $F = 0, f = 0$, and $2n - 2$ of the differentiations, which will then be between $x\, y\, y' \ldots y^{(2n-1)}$.

Eliminate $y', y'', \ldots y^{(2n-1)}$ from $2 + 2n - 2$ equations or $2n$ equations, and y is found in terms of x.

[5 lines are crossed out here.]

I meant to work out an example, but I find I must stop. I have a pile of letters to answer. With our kind regards to Mrs Boole.

[P.S.] Remark of an old gentleman in an omnibus today

'These omnibus conductors must be the happiest fellows in the world. They never say anything but 'full inside' and 'all right'.

86. DE MORGAN TO BOOLE, 5 JULY 1863

I have escaped at last from Session work and several adjuncts, and remember that I have to thank you for letters and print. Your German paper [B 1863*b*] I can just manage. As to that language I am not in love with it enough to learn more of it. I am tired even of the introductions

from it into English. I am much inclined to copy Samuel Johnson's sarcasm on the Scotch 'I cannot imagine, Dr. J. why God Almighty made Scotland!' Why, Sir! you are to remember that he made it for Scotchmen." Very well, then German was made for Germans. I impute to that unfortunate language seven deadly sins, which are as follows –
1. Too many volumes in the language
2. Too many sentences in a volume
3. Too many words in a sentence
4. Too many syllables in a word
5. Too many letters in a syllable
6. Too many strokes in a letter
7. Too much black in a stroke.

I have put the differential equations by until I come on the subject again, in Heaven's good time. I don't know whether you find what I do, namely, that my subjects of thought are not self-selected: they come because they must. There is '*whatever is, is*,' which settles the present, and *che sarà sarà* which provides for the future, and *c'est égal* which prevents any wish to alter the arrangement.

As to the logic I had hoped that no. V [D 1863*a*] would end it: but no. VI begins to loom in the distance. Interposed, however, is a paper I have long threatened myself with, – on *infinity* [D 1864*a*]. I hope to succeed in differing from every body most completely.

Your last logical remarks I must omit till I have read your paper on probabilities more fully [B 1862*a*,*d*?]. We are, I see, on different rails, but we may come to a junction. I feel sure of there being many separate routes, or which appear separate, at present. But when a paper is just finished, I find a certain *inertia* about me as to the whole subject. I suppose I am like a gorged boa constrictor.

I will add one little simplification which shows that common words may be made very useful.

When two universals come together or two particulars, let us say we have a *level.* When a universal and a particular, let us say we have a *slope.*

Universal followed by particular, *descent*
Particular __________ Universal *ascent*

Thus (( )) is a balanced level
((((is an unbalanced level
(.( )) is a balanced ascent
(.(((is an unbalanced ascent

The law of the secondary relations is as follows

1. ( )).(may connect
Any universal
Any unbalanced slope

2.) ((.) may connect
Any particulars
Any unbalanced slope

3.)) (.(may connect
Any balanced level
Any descent

4. (( ).) Any balanced level
Any ascent

1.).( ) (.) ((( ) ( ) etc. are valid
)(( ) (.)

2. ( ) (.)).))) (.))(are valid

3. ( ))) (.( ).( )) ( ) are valid
)))) ((

4. ( ) (((.( ).(((( )
)) ((((

I am not sure whether in some of your remarks you are quite aware of the change which there is in my use of the word mathematical, from and after my third paper. Formerly, I should have called the numerical syllogism mathematical, as opposed to the ordinary one; but latterly all the logic of extension is mathematical – as opposed to the forms of intension, which are metaphysical. Thus *man* and *brute* are parts of animal, mathematically: animal and reason are parts of man, metaphysically. But this distinction is *postponed* in the latter part of my last paper.

I have been lately engaged in quite a different kind of job. My wife has collected all the spiritual experiences of her own and others, and has made thereof a book and an argument, by C.D. I have written a preface, as A.B. contending that though facts are true, the spiritual hypothesis is too hasty, and assailing the philosophers for their omniscient mode of decrying facts by the light of nature. The two will appear together in a few weeks, under the title of 'From Matter to Spirit'. [S.E. De Morgan 1863] I proposed that the title page should have the legend

Jack Sprat could eat no fat
His wife could eat no lean

But this was judged *infra dig.*

Letters 87–90

The last letters contain little of substance. Nevertheless the letters show how much each valued the exchange of ideas provided by their correspondence. Thus Boole in Letter 89 (3 May 1864) says: 'I was glad to see the well-known handwriting again'.

Letter 87 (8 August 1863) is an answer by De Morgan to a query raised in letters that have not survived. On this occasion the loss makes the remark barely intelligible, but the query appears to relate to an actuarial problem.

In letter 87 the mention of his wife and children being at the Welsh coastal resort of Port Madoc brings to his mind a line of S.T. Coleridge:

And delights in the things of earth, water and skies;

this line comes from *Metrical Feet, Lesson for a boy,* which was written for Hartley Coleridge about 1806, and first published in 1834.

In Letter 88 (25 April 1864) De Morgan announces a theorem on divergent series – a subject which had interested him for 20 years; in 1844 he published a thoughtful paper (1844*c*) on it. He wrote a paper (1864*b*) giving this theorem as well as other discussion about divergent series and subsequently two other papers containing related ideas (1865*a*, 1868*c*).

The substance of the theorem is this: if a series $\Sigma(-1)^z a_z$, where each a_z is a non-negative function of some variable, is convergent for $x < x_0$ (or $> x_0$ perhaps), $a_z \to 1$ as $x \to x_0$ and

$$\lim \frac{a_{z+1}}{a_z} = 1 \text{ as } z \to \infty \text{ and } x \to x_0$$

then

$$\lim_{x \to x_0} \left(\sum (-1)^z a_z \right) = \lim_{\substack{x \to x_0 \\ z \to \infty}} \left(\frac{a_z - a_{z+1}}{a_z - a_{z+2}} \right) = \tfrac{1}{2}.$$

The genesis of this idea is presumably the series $\Sigma(-1)^z x^z$, $0 < x < 1$. De Morgan's objective in stating this theorem is the problem of the meaning of the divergent series $1 - 1 + 1 - 1 \ldots$ The series $\Sigma(-1)^z a_z$ has (in De Morgan's phrase) the 'limiting form $1 - 1 + 1 - 1 \ldots$' (De Morgan 1864*b*, 191).

The proof offered by De Morgan is hardly satisfactory. By appeal to Taylor's theorem applied to a_z, a_{z+2} as functions of z he obtains

$$a_{z+1} = a_z + a_z' + \tfrac{1}{2}a_{z+\nu}'' \qquad 0 < \nu < 1$$

$$a_{z+2} = a_z + 2a_z' + 2a_{z+\mu}'' \qquad 0 < \mu < 2$$

and hence

$$\frac{a_z - a_{z+1}}{a_z - a_{z+2}} = \frac{1}{2} \frac{a_z' + \frac{1}{2}a_{z+\nu}''}{a_z' + a_{z+\mu}''}.$$

But, De Morgan argues, $a_{z+\nu}''$, $a_{z+\mu}''$ are infinitesimal compared with a_z' when z is sufficiently great, so 'at the limit of summation'

$$\frac{a_z + a_{z+1}}{a_z - a_{z+2}} = \frac{1}{2}.$$

(There are two or three misprints in the argument as printed in the paper, so I have here not quoted De Morgan *verbatim*.) Following this argument De Morgan comments: 'The above paragraph will, I hope, be narrowly scrutinized . . .' (1864*b*, 193). Also he gives a geometrical form of this argument – which indicates the requirement that a_z must be meaningful for all positive z, and that a_z tends monotonically to zero as $z \to \infty$.

The weakness of this argument will be apparent. And it is significant that his later paper, 'Note on 'A Theorem relative to Neutral Series' in Vol. XI, Part II', begins with the words 'The detection of an inaccuracy in my paper on neutral series led me to think again on the subject' (1868*c*, 447). But these later thoughts included no more convincing results than had appeared in the earlier paper.

Letter 90 bears no indication of either date or addressee: it is in De Morgan's hand, and it is included in the packet of letters from De Morgan to Boole in the collection MS Add 97. One can only infer that it was possibly addressed to Boole. It shows De Morgan in typical vein, combining a mathematical enquiry with an experimental use of a patent writing instrument.

87. DE MORGAN TO BOOLE, 8 AUG. 1863

I think your plan is right. I thought the best way to try it was to do it my own way first – and I find we agree, with little differences of method. If we suppose the number kept up to 139 by substitution of new living for dead as fast as they die, we certainly are in a ± state of ignorance as to what would be the effect of assuming 167/(365 × 139) as the fraction of daily mortality.

My wife etc. are at Port Madoc in Carnarvonshire – very happy in the

'things of earth, water, and skies'

as the rector of Opium-cum-metaphysics said. And I am here as usual, routing in my book like a pig in a potatoe garden, who does not need much care where his snout goes, as he is sure of finding something.

C.D. and A.B. (praefator) are printed off and will appear in October. It is a funny production. The preface writer makes cock-shies of the philosophers – and is rational, so far as his gravity will allow towards the people who know what is and is not *a priori*.

I hope Mrs Boole and the children are well.

88. DE MORGAN TO BOOLE, 25 APRIL 1864

I send you a theorem which throws some light upon a difficult point.

Let $a_0 - a_1 + a_2 - \ldots$

be the *limit of summation* of a *converging series* which continues convergent up to a *limit of variation* of the terms, in which it is lost to calculation in the form $1 - 1 + 1 - 1 + \ldots$

First, let the law of the terms be, or finally become, permanent: that is, so far as this, that a_{z+1}/a_z approaches at last permanently to a fixed limit (of course not >1 but 1 at the limit of variation, if not before)

The the limit of variation of $a_0 - a_1 + a_2 -$ is the limit of $(a_z - a_{z+1})/(a_z - a_{z+2})$

And this is always $\frac{1}{2}$.

Secondly, let the law of the series proceed by cycles of *even* number of terms, so that

$$a_{2nz} - a_{2nz+1} [,] a_{2nz+1} - a_{2nz+2} [,]$$

$$\ldots\ldots a_{2nz+2n-1} - a_{2nz+2n}$$

approach in ratio to $k_0, k_1 \ldots k_{2n-1}$.

Then the limits of variation of

$$\left.\begin{array}{l} a_0 - a_1 + a_2 - \ldots \text{ and} \\ a_1 - a_2 + a_3 - \ldots \end{array}\right\} \text{where sum is 1 at last}$$

have the ratio of

$$k_0 + k_2 + k_4 + \ldots + k_{2n-2}$$

to

$$k_1 + k_3 + k_5 + \ldots + k_{2n-1}$$

But if the number of terms in a cycle be *odd*, both series have $\frac{1}{2}$ for the limit.

This theorem explains every case I ever met with in which $1 - 1 + 1 - 1 + \ldots$ was alleged to mean other than $\frac{1}{2}$.

I hope you are all well. Our kind regards to Mrs Boole.

89. BOOLE TO DE MORGAN, 3 MAY 1864

Thank you for your letter. I was glad to see the well-known handwriting again. I have nothing to say about the results – nothing more than that I am glad to see you still working and that I have no doubt they are of value. But I have no critical observations to make.

I was lecturing the other day on Spherical Trigonometry and was struck with the cumbrous character of the proofs of Napier's Analogies. Thinking the matter over yesterday evening I was led to the following proof.

1st Since[2] $\tan \frac{1}{2}A = \sqrt{\dfrac{\sin(s-b)\sin(s-c)}{\sin s \sin(s-a)}}$

we have if $m = \sqrt{\dfrac{\sin(s-a)\sin(s-b)\sin(s-c)}{\sin s}}$

$$\tan \tfrac{1}{2}A = \frac{m}{\sin(s-a)}, \quad \tan \tfrac{1}{2}B = \frac{m}{\sin(s-b)} \quad \text{etc.}$$

Hence by substitution

$$\frac{\tan \frac{1}{2}A + \tan \frac{1}{2}B}{\tan \frac{1}{2}A - \tan \frac{1}{2}B} = \frac{\sin(s-b) + \sin(s-a)}{\sin(s-b) - \sin(s-a)}$$

or

$$\frac{\sin \frac{1}{2}(A+B)}{\sin \frac{1}{2}(A-B)} = \frac{\tan \frac{1}{2}c}{\tan \frac{1}{2}(a-b)}. \qquad \text{(I)}$$

2nd By the same substitution

$$\frac{1 - \tan \frac{1}{2}A \tan \frac{1}{2}B}{1 + \tan \frac{1}{2}A \tan \frac{1}{2}B} = \frac{1 - \dfrac{m^2}{\sin(s-a)\sin(s-b)}}{1 + \dfrac{m^2}{\sin(s-a)\sin(s-b)}}$$

$$= \frac{1 - \dfrac{\sin(s-c)}{\sin s}}{1 + \dfrac{\sin(s-c)}{\sin s}}$$

$$= \frac{\sin s - \sin(s-c)}{\sin s + \sin(s-c)}$$

$$\therefore \frac{\cos\frac{1}{2}(A+B)}{\cos\frac{1}{2}(A-B)} = \frac{\tan\frac{1}{2}c}{\tan\frac{1}{2}(a+b)}. \qquad \text{(II)}$$

Is this new?

My kind regards to AB or CD I forget which Mrs De Morgan is. I read the preface and the book – *through*, which will show you that you had not failed to produce interest. But I was not convinced. I do not at all understand why the *reality* of the phaenomenon of a table rising up from the ground and remaining suspended in the air without any hand touching it or material communication of any kind (which I have heard positively asserted by a person who said he saw it with his own eyes) should not be investigated by scientific men as any admitted or presumed natural phaenomenon would be. I confess my opinion to be that the exhibitors of such phaenomena dare not submit them to such a test. One of three suppositions must be taken to represent the real state of the case. Either such phaenomena are done by juggling i.e. by unseen mechanical appliances dependent upon *known* laws, or by the application of unknown laws of nature, or by agencies which are not in the ordinary sense of the term natural. As to the second supposition which would involve that Mr Home knows more of natural laws than all the world of scientific men, it may I suppose be put aside. I know of no admitted phaenomenon having any kind of analogy to the suspended table but that of the suspended needle in a helix through which a galvanic circuit passes – but that is an experiment of the most refined and difficult kind. We are I think then shut up to juggling or to influences not natural in the ordinary sense of the term. I say in the ordinary sense of the term because there may be properties of the bodies & souls of *living* men that are so different from anything in admitted physiology or psychology that they must appear at first to be out of the range of natural things. I don't say there are. I suspend my judgment. But to return to what I first said why do not the spiritualists set themselves clean before the world by inviting scientific men to examine the *physical* phaenomena in their own way?

P.S. My wife expressly sends her compliments to AB *and* CD.

90. DE MORGAN TO [BOOLE?], UNDATED

Herewith a copy of the paper which you need not return. Have you any reference to any good writing on symmetrical functions of the root of unity? If

$$a, b, c, \ldots \text{ be } \sqrt[m]{1}'s$$

$$a_1, b_1, c_1, \ldots \text{ be } \sqrt[m_1]{1}'s$$

etc.

I want an easy way of finding a symmetrical function of the form

$$a^{\alpha} b^{\beta} c^{\gamma} \ldots a_1^{\alpha_1} b_1^{\beta_1} c_1^{\gamma_1} \ldots$$

I have a faint remembrance of having once had a rule to distinguish which are zeros and which are integers. But if so, I have forgotten it.

What I am writing with purports to be not pencil but solidified ink. It is said not to rub out, after the first hour. It is called 'Melvilles Patent'.

[Two large crosses appear here in the letter.]

The one on the right has had 25 hard rubs with a bit of Indian rubber as soon as written.

Notes

[1] The notation $(2, 1)s_2$ stands for a coefficient multiplied by s_2 : today we should write $a_{21}s_2$, perhaps.

[2] In the following formula Boole wrote 'sin a' where 'sin $(s - a)$' should appear. I have corrected this minor slip.

8
EPILOGUE

Boole died in December 1864 at the early age of 49. He left a widow and five daughters and De Morgan was active in canvassing support for an application to the Government for a pension for them. A draft letter to this end survives (De Morgan 1864, other manuscripts). In addition to throwing light on the parlous financial position that Boole's early death left his wife and family in, this draft letter contains an assessment by De Morgan of Boole's work.

De Morgan begins by saying that Boole worked for 15 years as a public teacher, the first thirteen of these with small remuneration, so no provision for his family was possible. Boole was, De Morgan said, a success as a teacher, was held in high regard by students and colleagues for his character and intellect *generally* (De Morgan's emphasis). He continued:

> We submit that one who has done so much, and has worked through the period of comparative failure and discouragement, may without presumption, be presented to H.M. Government with good hope of favourable consideration.

In addition to Boole's record of public service De Morgan placed on record his intellectual achievements:

> He is one of those men who have devoted rare genius with great success to parts of science which have no reward except what comes directly from the public purse, or else to men of academical education from the endowments of their Universities.

De Morgan gave some brief biographical notes: Boole was self-educated, a schoolmaster; then he became known through the papers contributed to the *Cambridge Mathematical Journal* – papers which contained

> some very remarkable speculations which can here be described only in general terms, as extensions of the power of algebraic language. These papers helped to give that remarkable impulse which algebraic language has received in the interval from that time to the present. Various papers followed, one of which received the gold medal of the Royal Society.

After mentioning the value of the texts that Boole had written De Morgan referred to his work in logic:

That peculiar turn for increasing the power of mathematical language which is the most characteristic point of Dr Boole's genius, was shown in a singularly remarkable way in his writings on logic. Of late years the two great branches of exact science, mathematics and logic, which had long been completely separated, have found a few common cultivators. Of these Dr Boole has produced far the most striking results. In alluding to these we do not say that the time is come in which they can ever be generally appreciated, far less extensively used. But if the public acknowledgement of progress and of genius be delayed until the whole world feels the results, the last century, which had the benefit of the lunar method of finding longitude, ought to have sought for the descendents of Apollonius, to reward them for his work on the conic sections.

There followed a deleted portion which attempted to explain in simple terms the significance of Boole's

system of logic [which] shows that the symbols of algebra . . . are competent to express all the transformations and deductions which take place in inference.

De Morgan also recorded Boole's honorary degrees (from Oxford and Dublin) and mentioned that there was:

a prospect of admission into the French Institute, cut short by his death.

He concluded the letter with the hope that the grounds he has put forward 'are good and our request worthy of favourable consideration'.

After Boole's death his wife presented his manuscripts which related to mathematics and logic to the Royal Society. In 1867 De Morgan examined these to ascertain whether there were any that merited posthumous publication. Apart from Boole 1868 De Morgan found nothing complete enough to warrant publication. He reported:

After much consideration I am satisfied with two things. First, the author himself, would have objected to their publication as they stand. He would have introduced much change of expression and allusion to his higher views, or rather, preparations without allusion.

Secondly, a false impression would be produced: a posthumous work by George Boole on logic would be taken for his latest and highest view. Those who would know better when they came to open the book would not find out how the matter stood, would really believe they were in possession of all Boole's intentions. And as a hundred copies would sell for one of the laws of thought, a very wide misapprehension of the contents of the 'Laws of Thought' would get about. [De Morgan 1867, other manuscripts].

De Morgan was 58 years old when Boole died; he had a little more than six years left to him, but the fires were burning low.

In October 1864 De Morgan's second son, George Campbell De Morgan, together with a friend, A.C. Ranyard, conceived the idea of a mathematical society at University College. This idea developed into the London Mathematical Society which first met in January 1865. De Morgan was the first President and delivered an inaugural address (De Morgan 1866*g*).[1]

From this time professional and personal matters were a source of unhappiness for De Morgan. In September 1865 two friends of long standing died: W.R. Hamilton (1805–65) and W.H. Smyth (1788–1865). Also in 1865 the dispute at University College concerning the College's failure to appoint the Rev. James Martineau to the Chair of Mental Philosophy and Logic erupted – he was clearly the best candidate but was a notable Unitarian. De Morgan concluded that the real reason for Martineau's rejection was his religious beliefs and this he considered to be contrary to the strictly non-sectarian constitution of the College; he resigned in protest. According to his wife:

> My husband told me that during the session in which he worked after his resignation was sent in he met his colleagues as before in the Professors' room. Not one of them ever spoke on the subject of his retirement, and he left the place without one word of acknowledgement for all he had done for it [S.E. De Morgan 1882, 358.]

De Morgan's earlier resignation – in 1831 – had been as a protest over the action of the Council in the dismissal of G.S. Pattison, the Professor of Anatomy. Two other professors had resigned with De Morgan; F.A. Rosen, the Professor of Oriental Languages and G. Long, the Professor of Greek.

In October 1867 George Campbell De Morgan died, followed in August 1870 by his sister Helena Christiana, both victims of tuberculosis. De Morgan himself was ill in 1868.

De Morgan's papers in his last years, i.e. 1865–71, were often short notes of little importance. In these years he contributed a number of notes to the *Assurance Journal* and the *Journal of the Institute of Actuaries*. His longer papers continued to appear in the *Transactions of the Cambridge Philosophical Society* – in particular De Morgan 1866*c*, 1868*c* – but these papers were only a reworking of earlier ideas. He also contributed two brief notes to the early *Proceedings* of the *London Mathematical Society* (De Morgan 1868*a*, *b*), but of some historical interest is the report in volume 1 of the *Proceedings* of his speech at the first meeting of the Society (1866*g*).

The letters have shown De Morgan as a writer with a humorous manner. One feels that he must have been a valued acquaintance. A person who recorded his appreciation of De Morgan's friendship was Henry Crabb Robinson, who said of De Morgan:

He is the only man whose calls, even when interruptions, are always acceptable. He has such luminous qualities even in his small talk. [Robinson 1872, vol. 2, 385.]

An autobiographical note and his will reveal De Morgan's view of himself: first, professionally:

Mr De Morgan is one of the few mathematicians who hold mathematics to be no sufficient substitute for the study of logic . . . He has been a voluminous writer on many branches of mathematics [De Morgan 1860, other manuscripts.]

Second, he gave in his will the reason why he had consistently refused to speak about his religious beliefs:

I commend my future with hope and confidence to Almighty God; to God the Father of our Lord Jesus Christ, whom I believe in my heart to be the Son of God, but whom I have not confessed with my lips, because in my time such confession has always been the way up in the world. [S.E. De Morgan, 1882, 368.]

Any comparison of the manner in which Boole and De Morgan tackled the mathematical and logical problems they chose to attack appears to show that Boole had the finer mind. He chose interesting and important topics, had new ideas to express about them, and communicated these incisively. De Morgan, on the other hand, often wrote at great length without quite reaching the heart of the matter, although what he had to say contained interesting – often vividly expressed – remarks. All of De Morgan's texts had short lives with the exception of the *Elements of Arithmetic* (1830*a*), which was regularly reprinted for more than 40 years. Only his *Budget of Paradoxes* (1872), a compilation posthumously issued by his wife, has been reprinted in recent times. At least three of Boole's books have survived. *Laws of Thought* (1854*a*) and *The Mathematical Analysis of Logic* (1847*a*) have been reprinted in recent years. So have his texts on *Differential Equations* (1859) and *Finite Differences* (1860); the former seems to be in steady demand at the author's university's library.

What does the correspondence add to our knowledge of Boole and De Morgan as persons and as scholars? The wide range of literary interests, and their language skills, are a salutary reminder of changes that have taken place in education and educational achievements over the past century. Of course Boole and De Morgan were not average persons; by different routes they became exceptionally well educated. But making comparisons between well-educated persons of today and Boole and De Morgan, one cannot but observe that the latter had a better general education than comparable persons of the 1970s. One would have to search very hard among present-day university professors to find one like Boole who, beyond the level of an elementary school instruction, had largely taught himself.

At this point we might usefully sum up and compare the contributions of Boole and De Morgan in the field where, in all probability, their major contributions to knowledge lie – i.e. logic.

De Morgan's early work (i.e. before 1859) on logic can be characterised as (usually) notable extensions of the theory of the syllogism. De Morgan introduces new forms of the syllogism but his ideas, though new, can still be seen as belonging to the style of logical reasoning that began in ancient Greece. The symbols he introduced to express the old and new types of syllogism were in their conception not algebraic – he used literary symbols such as brackets (,) to denote the relationship between the statements that formed the syllogisms. He found that his symbols could be manipulated in a way that resembled the familiar method of manipulation of algebraic formulae (see, in particular, Letter 70). His later work is more original and is perhaps his most lasting contribution to logic. In the paper *On the Syllogism IV* (De Morgan 1859*b*) he moves beyond the syllogism to investigate the theory of relations.[2] Although he was not the first to study relations in logic, he was probably the first to give this subject concentrated attention (Kneale 1962, 427). Later Peirce was to take up and develop the theory of relations (for the beginning of his work on the subject see Peirce 1870).

Boole's work on logic is thoroughly algebraic in character from the beginning. His boldness in the adaptation of ordinary algebra to a form suitable for the expression of logic is remarkable. Almost at the beginning novel algebraic laws arise, e.g. $x^2 = x$. In the formulae by which he expands elective functions (e.g. $\phi(x,y) = xy\phi(0,0) + x(1-y)\phi(1,0) + (1-x)y\phi(0,1) + (1-x)(1-y)\phi(0,0)$) he finds it necessary to use the symbols 0/1, 1/1, 1/0, 0/0. The latter two of these are inadmissible in ordinary algebra. But these novelties are accepted and woven into the system by Boole. The use of algebraic formulae to express logical problems allows Boole to enunciate and solve some that are of such complexity that their treatment in the traditional language is decidedly difficult – see for instance his analysis of the definition of annelida (Boole 1854*a*, 144–6). Although it is not possible to find Boolean algebra, the theory of lattices or the present-day mathematical development of propositional logic in Boole's work, the germs of ideas which were to lead to these topics are clearly present. It was, however, left to later workers to recognise and develop these ideas.

In a recent paper van Evra has made a reassessment of Boole's contribution to the theory of logic. He describes Boole's work as 'one of the first important attempts to bring mathematical methods to bear on logic while retaining the basic independence of logic from mathematics' (van Evra 1977, 374). While acknowledging that Boole's logic is imperfect, van Evra notes that 'he displays a greater awareness of logic as an independent discipline, as well as more sophistication in the use of mathematics as an aid to logic while avoiding the conflation of the two areas, than did his successors in algebraic logic'. [van Evra 1977, 365.]

The long-lasting exchange of ideas in the letters leads one to consider the question of the influence each might have had upon the thoughts of the other. In the earlier years of the correspondence the relatively greater maturity and experience of De Morgan compared with Boole naturally suggests that the influence flowed from De Morgan to Boole. Examination of the letters, however, suggest that Boole's ideas were almost entirely his own. Nearly always we observe De Morgan reacting to Boole's ideas, rather than *vice versa*. Constantly Boole is raising ideas and putting them to De Morgan for comment; further De Morgan's reactions are not of a kind that suggest that De Morgan is capable of directing Boole's ideas into a significantly different course. It appears, then, that any influence that De Morgan had on Boole was either slight or delayed, so that no immediate indication of it shows in the letters. I would go further and claim that after the early letters the evidence of the letters shows that it was Boole who had the most original ideas and most vigorous intellect. Consequently if there is any question of one influencing the other in the period of Boole's intellectual maturity it is Boole who is influencing De Morgan. However, what might have been important was not so much the way that each directly influenced the other as the stimulation of a steady interaction of ideas – and this stimulation perhaps acted on a level below that of immediate overt influence.[3]

It is fitting to conclude with words used by De Morgan which expressed in a vivid manner his view of Boole's work:

> When the ideas thrown out by Mr Boole shall have borne their full fruit, algebra, though only founded on ideas of number in the first instance, will appear like a sectional model of the whole form of thought. [1861*b*, 346.]

Notes

[1] For further information of the De Morgan's activities in the foundation of the London Mathematical Society see Collingwood 1966.
[2] For a recent discussion of De Morgan's theory of relations see Hawkins 1979.
[3] The various influences – including De Morgan's – on the genesis of Boole's ideas of logic are discussed in Laita 1977, 1979, and 1980.

APPENDIX:
BOOLE'S THEOREM ON DEFINITE INTEGRATION

The theorem that Boole discussed in Letter 10 (8 Jan 1847) is given in more generality in his two published notes (Boole 1848*d*, 1849*a*). In these he claims that

$$\int_{-\infty}^{\infty} f(x - R)\,\mathrm{d}x = \int_{-\infty}^{\infty} f(v)\,\mathrm{d}v \qquad (1)$$

where R is a rational function of x such that the roots of the equation $x - R = v$ are real for all values of v for which $f(x)$ does not vanish. Although the result appears, at first sight, unlikely the 1849 paper contains a number of verifiable examples which suggest (at least) a substratum of truth. In fact subject to an additional condition upon R and assuming, as Boole did, that there are no problems with the convergence of the integrals, the result is substantially correct. The additional condition is this: that $R(x) = p(x)/q(x)$ where the degree of $p(x)$ is less than the degree of $q(x)$ and that $x - R(x)$ is piecewise increasing i.e. increasing in each of the intervals into which the x-axis is divided by the zeros of $q(x)$. That Boole was aware to some extent of this added condition is clear in that in both the papers after stating the result in terms of a general rational function R he gives also a 'particular form'

$$\int_{-\infty}^{\infty} f\left(x - \sum_{1}^{n} \frac{a_i}{x - \lambda_i}\right)\mathrm{d}x = \int_{-\infty}^{\infty} f(v)\,\mathrm{d}v,$$

where λ_i, $1 \leqslant i \leqslant n$, are any real constants and a_i, $1 \leqslant i \leqslant n$, are positive. It is easy to see that this particular form does satisfy the condition imposed above.

The simplest version of the theorem is capable of an elementary proof: this version asserts that if $a > 0$ and $v = x - \dfrac{a}{x}$, then

$$\int_{-\infty}^{\infty} f(v)\,\mathrm{d}v = \int_{-\infty}^{\infty} f\left(x - \frac{a}{x}\right)\mathrm{d}x.$$

Following Boole I shall assume that the function is such that the infinite integrals are convergent. First assume, further, that f is an even function. The substitution $v = x - \dfrac{a}{x}$ yields

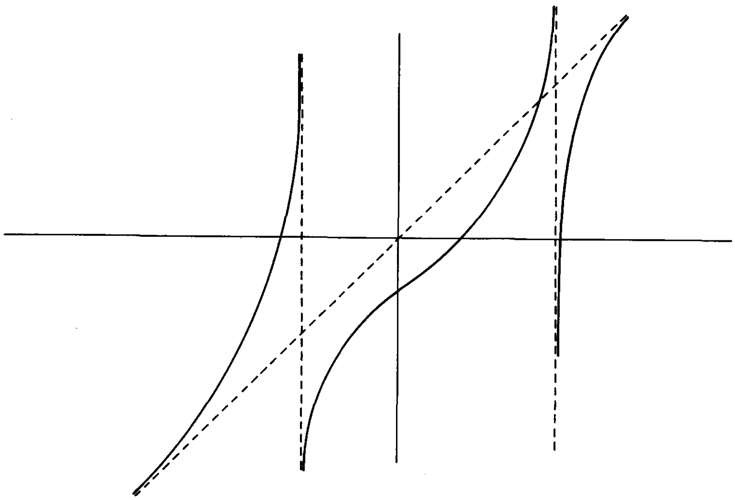


Fig. 2

$$\int_{-\infty}^{\infty} f(v)\,dv = \int_{-\infty}^{0} f\left(x-\frac{a}{x}\right)\left(1+\frac{a}{x^2}\right)dx = \int_{0}^{\infty} f\left(x-\frac{a}{x}\right)\left(1+\frac{a}{x^2}\right)dx.$$

Thus

$$2\int_{-\infty}^{\infty} f(v)\,dv = \int_{-\infty}^{\infty} f\left(x-\frac{a}{x}\right)\left(1+\frac{a}{x^2}\right)dx. \tag{2}$$

Put $y = a/x$ in $\int_{-\infty}^{\infty} f\left(x-\frac{a}{x}\right)dx$; one obtains

$$\int_{-\infty}^{\infty} f\left(\frac{a}{y}-y\right)\frac{a}{y^2}\,dy = \int_{-\infty}^{\infty} f\left(y-\frac{a}{y}\right)\frac{a}{y^2}\,dy \tag{3}$$

as f is an even function. The result now follows from (2) and (3). But the result is certainly true if f is an odd function (it becomes $0 = 0$); so writing $f(x) = \frac{1}{2}(f(x)+f(-x)) + \frac{1}{2}(f(x)-f(-x))$, i.e. expressing f as the sum of an even and an odd function, the result now follows.

I turn now to the general result, viz.

$$\int_{-\infty}^{\infty} f(v)\,dv = \int_{-\infty}^{\infty} f\left(x-\sum_{1}^{n}\frac{a_i}{x-\lambda_i}\right)dx, \tag{4}$$

where $a_i > 0$ $(1 \leqslant i \leqslant n)$. The elementary approach used previously does not seem to be susceptible of generalization. However a proof can be given on the following lines; to simplify the details I shall take the case $n = 2$, but the method is quite general.

Note first that $v = x - a_1/(x-\lambda_1) - a_2/(x-\lambda_2)$ has a graph of the form shown in Fig. 2. Now consider the special case

$$f(x) = \chi_{[X, X+\delta]} \qquad (\delta > 0)$$

where as usual χ denotes a characteristic function. Then $\int_{-\infty}^{\infty} f(v)\,dv = \delta$. Also, because of the chosen form of f

$$\int_{-\infty}^{\infty} f\left(x - \frac{a_1}{x-\lambda_1} - \frac{a_2}{x-\lambda_2}\right) dx = \sum_1^3 l_i$$

where l_i $(1 \leqslant i \leqslant 3)$ denote the lengths of the three intervals in which $x - a_1/(x-\lambda_1) - a_2/(x-\lambda_2)$ takes values between X and $X+\delta$. If $X = x - a_1/(x-\lambda_1) - a_2/(x-\lambda_2)$, then on clearing fractions and simplifying one obtains a cubic equation

$$x^3 - x^2(\lambda_1 + \lambda_2 + X) - X(-a_1 - a_2 + \lambda_1\lambda_2 + \lambda_1 X + \lambda_2 X)$$
$$+ a_1\lambda_2 + a_2\lambda_1 - \lambda_1\lambda_2 X = 0.$$

The left-hand end of the three intervals are the three roots of this cubic equation. Similarly the right-hand ends are the three roots of the corresponding cubic equation in which $X + \delta$ replaces X. Thus $l_1 + l_2 + l_3$ is found from the difference of the x^2 coefficients i.e. $l_1 + l_2 + l_3 = \delta$. So

$$\int_{-\infty}^{\infty} f\left(x - \frac{a_1}{x-\lambda_1} - \frac{a_2}{x-\lambda_2}\right) dx = \delta = \int_{-\infty}^{\infty} f(v)\,dv,$$

and the result is proved. It is now clear in what circumstances the general case may be proved: we require that f be a function that can be suitably approximated by a linear combination of characteristic functions $\Sigma_1^N Y_i \chi_{[X_i, X_i+\delta_i]}$. 'Suitably' implying that a sequence of such approximations can be found so that their integrals $\Sigma_1^N Y_i \delta_i$ shall have $\int_{-\infty}^{\infty} f(v)\,dv$ as limit.

As noted above the elementary approach which succeeds for $v(x) = x - \dfrac{a}{x}$ does not appear to generalise. One obtains without difficulty

$$3\int_{-\infty}^{\infty} f(v)\,dv = \int_{-\infty}^{\infty} f\left(x - \frac{a_1}{x-\lambda_1} - \frac{a_2}{x-\lambda_2}\right)\left(1 + \frac{a_1}{(x-\lambda_1)^2} + \frac{a_2}{(x-\lambda_2)^2}\right) dx$$

but I have found no substitution that will reduce

$$\int_{-\infty}^{\infty} f\left(x - \frac{a_1}{x-\lambda_1} - \frac{a_2}{x-\lambda_2}\right)\left(\frac{a_1}{(x-\lambda_1)^2} + \frac{a_2}{(x-\lambda_2)^2}\right) dx \qquad (5)$$

to $2\int_{-\infty}^{\infty} f(v)\,dv$. However the above proof indicates that (5) must indeed be $2\int_{-\infty}^{\infty} f(v)\,dv$ – and indeed in general that

$$(n-1)\int_{-\infty}^{\infty} f(v)\,dv = \int_{-\infty}^{\infty} f\left(x - \sum_1^n \frac{a_i}{(x-\lambda_i)^2}\right) dx.$$

In the most general form of the theorem Boole states that

$$\int_{-\infty}^{\infty} f(v)\,dv = \int_{-\infty}^{\infty} f(x - R)\,dx,$$

where 'R[is] any rational fraction, a function of x ... and if the values of x given by the equation $x - R = v$ are real for all values of v included within the actual limits of integration'. (Boole 1849 14–15).

It is rather difficult to see what Boole means by this condition. As mentioned above the actual requirement is that $v = x - R(x)$ should be piecewise increasing. Thus R may be a sum of terms of the form $a_i/(x-\lambda_i)^{\tau_i}$, where τ_i is an odd (positive) integer and $a_i > 0$.

I turn now to Boole's proofs of this result. In the announcement in Liouville's *Journal* there is no indication of proof. The proof in Boole 1849 is slight to the point of unintelligibility:

Suppose that $f(v)$ is discontinuous, let it be imagined to vanish for all values of v which do not lie within the limits p and q. These are then the actual limits of integration. According to this definition of the character of the function f, it is evident that $f(x-R)$ will vanish whenever $x - R$ transcends the limits p and q.

Let $p_1 p_2 \ldots p_n$ be the roots of the equation

$$x - R = p,$$

and $q_1 q_2 \ldots q_n$ those of the equation

$$x - R = q \tag{6}$$

and suppose $p_1 p_2 \ldots p_n$ and $q_1 q_2 \ldots q_n$ arranged in the same order of magnitude, we have then

$$\int_{p_1}^{q_1} \mathrm{d}x f(x-R) + \int_{p_2}^{q_2} \mathrm{d}x f(x-R) \ldots + \int_{p_n}^{q_n} \mathrm{d}x f(x-R) = \int_p^q \mathrm{d}v f(v) \ldots \tag{7}$$

and this may be applied to the determination of the sums of an infinite variety of transcendental integrals.

The proof – perhaps more accurately described as a discussion indicative of a proof – in Letter 10 is likely to seem obscure to a present-day reader. Note the 'deduction' of $\Sigma\, \mathrm{d}x = \mathrm{d}v$ from $\Sigma\, x = v$. With hindsight, and a good deal of charity, one can perceive some resemblance between Boole's argument and the measure-theoretic one given above e.g. may not Boole's p and q be analogous to my $X + \delta$ and X? Although the style of argument he used seems strange now it was common enough in the nineteenth century and one presumes was an expression of a reasoned mathematical insight which was then generally intelligible. We, today, may find this style strange and imprecise; but that is no good reason for dismissing it out of hand.

Boole gives a number of examples in his paper (Boole 1849). The first – and one supposed the formula that led him to the general result – is that which arises when one takes $f(v) = \mathrm{e}^{-v^2}$. Then Boole's theorem yields

$$\int_{-\infty}^{\infty} \mathrm{e}^{-v^2}\,\mathrm{d}v = \int_{-\infty}^{\infty} \mathrm{e}^{-\left(x-\frac{a}{x}\right)^2}\mathrm{d}x$$

$$= \mathrm{e}^{2a}\int_{-\infty}^{\infty} \mathrm{e}^{-\left(x^2+\frac{a^2}{x^2}\right)}\mathrm{d}x \tag{9}$$

(The numbers identifying the formulae are those given in Boole 1849.) He also gives the analogous result that arises from taking $f(v) = \mathrm{e}^{-v^n}$, 'for n-even'. At this point, at least, he has realised the need for f to be an even function if the result is to be non-trivial.

Boole claims that 'in an exactly similar manner we may deduce

$$\int_0^\infty dx \cos\left(x^2+\frac{a^2}{x^2}\right) \quad \int_0^\infty dx \sin\left(x^2+\frac{a^2}{x^2}\right),$$

but gives no further explanation. Allowing the use of complex-valued functions, these integrals can be obtained on taking $f(v) = e^{iv^2}$; their values are, respectively;

$$\tfrac{1}{2}\sqrt{\frac{\pi}{2}}(\cos 2a - \sin 2a), \quad \tfrac{1}{2}\sqrt{\frac{\pi}{2}}(\cos 2a + \sin 2a).$$

Another example arises on taking $f(v) = \dfrac{\cos av}{1+v^2}$. Boole deduces that

$$\int_{-\infty}^{\infty} \frac{\cos\left(a\left(x-\frac{r}{x}\right)\right)}{1+\left(x-\frac{r}{x}\right)^2} dx = \pi e^{-a}.$$

It is a tedious exercise in contour integration to verify this odd-looking formula.

In the middle of these results (1849, 17) Boole claims 'from the known integral $\int_0^\infty \dfrac{d\theta\,\theta^{-\frac{1}{2}}}{(1+\theta)^n}$, I in like manner deduced

$$\int_0^\infty \frac{dx\,x^{n-3/2}}{(a+bx+cx^2)^n} = \frac{\Gamma(n-\frac{1}{2})\sqrt{\pi}}{\Gamma(n)a^{\frac{1}{2}}}\frac{1}{(b+2\sqrt{ac})^{n-\frac{1}{2}}}$$

$$\int_0^\infty \frac{dx\,x^{n-\frac{1}{2}}}{(a+bx+cx^2)^n} = \frac{\Gamma(n-\frac{1}{2})\sqrt{\pi}}{\Gamma(n)c^{\frac{1}{2}}}\frac{1}{(b+2\sqrt{ac})^{n-\frac{1}{2}}} \qquad (12)'$$

He does not state the conditions on a, b, and c, but from the right side of (his) equation (12) one infers that $a > 0$, $c > 0$ and $b + 2\sqrt{ac} > 0$. The integral he starts from, $\int_0^\infty \dfrac{\theta^{-\frac{1}{2}}}{(1+\theta)^n} d\theta$, is a form of the beta integral. The formula (12) cannot be deduced from this integral without some preliminary transformation; it seems better to prove them by adaption of the ideas of the proof of the general result. On making the substitution

$$\theta = \frac{\sqrt{ac}}{b+2\sqrt{ac}}\left(\sqrt{\frac{c}{a}}x + \sqrt{\frac{a}{c}}\frac{1}{x} - 2\right)$$

in the beta integral, after some tedious manipulation one obtains

$$\int_0^\infty \frac{\theta^{-\frac{1}{2}}}{(1+\theta)^n} d\theta = (b+2\sqrt{ac})^{n-\frac{1}{2}}(\sqrt{a}I + \sqrt{c}J)$$

where

$$I = \int_{\sqrt{\frac{a}{c}}}^{\infty} \frac{x^{n-3/2}}{(a+bx+cx^2)^n}\,\mathrm{d}x, \qquad J = \int_{\sqrt{\frac{a}{c}}}^{\infty} \frac{x^{n-1/2}}{(a+bx+cx^2)^n}\,\mathrm{d}x.$$

On making the substitution $y = \frac{a}{c}x$ in these, one finds

$$I = \sqrt{\frac{c}{a}} \int_0^{\sqrt{\frac{a}{c}}} \frac{x^{n-\frac{1}{2}}}{(a+bx+cx^2)^n}\,\mathrm{d}x, \qquad J = \sqrt{\frac{a}{c}} \int_0^{\sqrt{\frac{a}{c}}} \frac{x^{n-3/2}}{(a+bx+cx^2)}\,\mathrm{d}x.$$

Hence

$$\int_0^{\infty} \frac{\theta^{-\frac{1}{2}}}{(1+\theta)^n}\,\mathrm{d}\theta = (b+2\sqrt{ac})^{n-\frac{1}{2}} \left(\sqrt{a}I + \sqrt{c}\sqrt{\frac{a}{c}} \int_0^{\sqrt{\frac{a}{c}}} \frac{x^{n-3/2}}{(a+bx+cx^2)^n}\,\mathrm{d}x\right)$$

$$= (b+2\sqrt{ac})^{n-\frac{1}{2}}\sqrt{a} \int_0^{\infty} \frac{x^{n-3/2}}{(a+bx+cx^2)^n}\,\mathrm{d}x,$$

which yields the first of Boole's formulae in (12). The other is obtained similarly.

In his Cambridge paper Boole remarked of his theorem: 'There is a fair promise of interesting if not of important consequences, but I have no intention now of persuing the enquiry' (1849, 14). However some years later he did take up these ideas again. The Boole papers in the Library of the Royal Society contains a note which discusses this result. Although it is only an incomplete draft it is interesting to observe in it the more mature manner in which Boole deals with the theorem. The relevant portion reads:

One of the most general theorems to which I have been conducted may be thus stated. It is always possible to reduce the definite integral

$$\int_{-\infty}^{\infty} Rf\left(x - \frac{a_1}{x-\lambda_1} \ldots - \frac{a_n}{x-\lambda_n}\right)\mathrm{d}x$$

to the form $\int_{-\infty}^{\infty} R'f(x)\,\mathrm{d}x$ where R' is a rational function of x similar in form to R but with different constants.

A particular case of this theorem was given by me several years ago in the Cambridge M[athematical] J[ournal]. It was the following:

$$\int_{-\infty}^{\infty} f\left(x - \frac{a_1}{x-\lambda_1} \ldots - \frac{a_n}{x-\lambda_n}\right)[\mathrm{d}x] = \int_{-\infty}^{\infty} f(x)\,\mathrm{d}x.$$

Another particular case was described by Cauchy and has been made the subject of a memoir. His result amounts to this: that

$$\int x^{2n} f\left(x^2 + \frac{a^2}{x^2}\right)[\mathrm{d}x]$$

may be reduced to depend on the integrals

$$\int x^{2n} f(x^2+2a)\,\mathrm{d}x, \quad \int x^{2n-2} f(x^2+2a)\,\mathrm{d}x, \text{ etc.} \qquad [*]$$

I have deduced Cauchy's theorem as a special result from my own.

The most general theorem is, however, the following:

$$\int_{-\infty}^{\infty} \mathrm{d}x R(x) f(x-R) = \int_{-\infty}^{\infty} \mathrm{d}x R'(x) f(x)$$

provided that R is such a function x that the roots of the equation

$$x - R = v$$

are rational for all real values of v.

There are some trigonometrical functions which satisfy this condition and it is very remarkable that such conditions have been also discussed by Cauchy. He has, if I may be allowed the expression, discussed the *condition* of a problem which he had not solved.

A great many known definite integrals and sums which occur in the theory of electricity fall under the above general results. They add largely also to our power over multiple integrals but the new forms to which they lead are not apparently important ones.

The symbol θ with the interpretation which I have given to it agrees with Cauchy's symbol of residues with this difference that it involves one additional element of meaning. I have no manner of doubt that that element is of essential importance. In many cases in which it is written as in the writings of Cauchy it requires to be supplied in a less convenient manner. (Boole, Roy. Soc.)

The reference to Cauchy suggests that it was in one of his memoirs that Boole may have got the initial idea behind his result. It is not possible to be sure which memoir of Cauchy Boole was referring to. One possibility is that it was to Cauchy's important paper of 1823 on partial differential equations (Cauchy 1823) where some similar formulae appear in Section 11. Another paper of Cauchy's which relates the integrals

$$A_{2n} = \int_0^{\infty} x^{2n} f(x^2)\,\mathrm{d}x$$

$$B_{2n} = \int_0^{\infty} x^{2n} f\left[\left(x - \frac{1}{x}\right)^2\right] \mathrm{d}x$$

appears in *Exercises de Mathematiques Annèe 1826* (Cauchy 1826). In this paper Cauchy makes use of complex numbers to deduce such formulae as

$$\int_0^{\infty} x^{2n} \mathrm{e}^{-s\left(x-\frac{1}{x}\right)^2} \cos t\left(x - \frac{1}{x}\right) \mathrm{d}x.$$

The final paragraph seems to refer back to an earlier part of Boole's note – which is possibly lost or misplaced. The specific formulae that Boole deduced from his theorem as examples (Boole 1849, 116–17) have an appearance which suggests to a modern reader that contour integration might provide a suitable means of proving them. It is possible, but not particularly convenient to prove them by such means.

Boole's theorem is the kind of result which is discovered and forgotton more than once. I have not seen any version as general as Boole's, or of any earlier date. Simpler versions appear in Todhunter's *Integral Calculus*, (Todhunter 1857, 233) and J.W.L. Glaisher also published a note containing a simple form of it (Glaisher 1875, 186–7).

BIOGRAPHICAL NOTES

These notes contain concise biographical information on all persons (including fictional ones) mentioned in the letters. In the case of M. E. Boole and S. E. De Morgan, only references other than those in terminal greetings are given.

	Letter
A.B. Pseudonym of A. De Morgan.	87, 89
Ampère, A.M. (1775–1836). Ampère is best known for his investigations on electricity, began his career with papers on partial differential equations.	75
Aristotle (384–322 BC). Greek philosopher.	70
Barrow, Isaac (1630–77). Mathematician and divine; sometime Lucasian professor and Master of Trinity College, Cambridge.	64
Baynes, T.S. (1823–87). Baynes studied under Sir W. Hamilton and later was professor of Logic, Metaphysics and English Literature at St Andrews University.	25, 77
Beddoes, Thomas (1760–1808). Physician and writer.	64
Bertrand, J.L.F. (1822–2900). A French author who wrote many books on mathematics and physics.	63
Billingsley, Sir Henry (d. 1606). Merchant, sometime Lord Mayor of London.	60
Boase, H.S. (1799–1883). Boase wrote on geology and chemistry. He was elected FRS in 1837.	66
Boethius (*c.* 475–526). Roman nobleman and philosopher.	70
Boole, Mary Everest (1832–1916). Wife of George Boole and writer on psychological and educational topics.	56, 57, 59, 61 65, 69, 79, 83
Burgersdicius (i.e. F.P. Burgersdijck) (1590–1635). Dutch philosopher.	70
Caesar, G. Julius (*c.* 100–44 BC). Roman general and dictator.	70
Cagnoli, A. (1743–1816). Italian astronomer and scientific writer.	58
Carlyle, Thomas (1795–1881). Historian and essayist.	67
Cauchy, A.L. (1789–1867). Mathematician, one of the creators of modern analysis.	60
Cayley, Arthur (1821–95). A prolific mathematician who contributed to the theory of invariants, matrices, *n*-dimensional geometry, and non-Euclidean Geometry.	53, 76
C.D. Pseudonym of S.E. De Morgan.	87, 89
Chretien, C.P. (1820–75). Dean and Tutor of Oriel College, Oxford, 1843–64.	25

Hall, Marshall (1790–1857). A physician who was elected to the Royal Society in 1832. 37

Hamilton, Sir William (1788–1856). Philosopher and logician. 11, 25, 64, 70, 74, 79

Heaviside, J.W.L. (1808–97). Senior professor of Mathematics at the East India College, Haileybury, 1838–57, and examiner in mathematics at the University of London. 61

Herschel, Sir John (1792–1871). Astronomer and the son of the astronomer William Herschel. 49, 63

Hildebert (*c.* 1056–1133). Sometime Archbishop of Tours. 47

Home, D.D. (1833–86). Home held private spiritualist seances in London, Europe, and America. 89

Ingelby, C.M. (1823–86). Critic and essayist. 70

Isenach, J. (i.e. Justis Joducus of Eisenach) (*fl.* 1500). Teacher of theology and philosophy at Erfurt. 70

Jacobi, C.G.J. (1804–51). Jacobi was one of the discoverers of multiply-periodic functions. 67, 84

Jamieson, R.A. (*fl.* 1860). Pupil of George Boole. 69

Johnson, Samuel (1709–84). Lexicographer and essayist. 86

Joshua of Nazareth. Jesus Christ. 70

Jowett, Benjamin (1817–93). Professor of Greek and sometime master of Balliol College, Oxford. 67

Kane, Sir Robert (1809–90). President of Queen's College, Cork, from 1845 to 1873, and an editor of the Philosophical Magazine. He was elected a Fellow of the Royal Society in 1849. 21, 26, 32

Kant, I. (1724–1804). Philosopher. 46

Keckermann, B. (1573–1609). Divine and author. 70

Kelland, P. (1808–79). Professor of Mathematics at Edinburgh, 1838–79. 64

Laplace, P.S. (1749–1827). Mathematician. 1, 2, 19, 31, 35

Lardner, Dionysius (1793–1859). First professor of natural philosophy at London University (i.e. University College London), a Fellow of the Royal Society and many other learned bodies. 36

Leibniz, G.W. (1646–1716). Philosopher and mathematician. 41, 68

Legendre, A.M. (1752–1823). Mathematician. 58

Libri, Guglielmo (1803–69). Libri was born in Florence, moved to France where he became naturalized in 1833. He was Inspecteur des bibliothèques de France. 28, 63

Lloyd, B.C. (1808–72). Provost of Trinity College, Dublin, 1831–7. 34

Logan, H.F.C. (*fl.* 1830–50). Professor at the Catholic College at Oscott, and a friend of De Morgan. 13

Luby, Thomas (1800–70). Luby occupied various posts at Trinity College, Dublin, and wrote astronomical texts. 34

Lumley, Edward (*fl.* 1830–40). A bookseller who had a shop in Chancery Lane. 35

Lycurgus (9th century BC). The traditional law-giver of Sparta. 80
MacHale, J. (1791–1881). Archbishop of Tuam from 1834 to 1881. 24
MacMillan, Daniel (1813–57) and Alexander (1818–96). The founders of the firm of publishers. 63
Mansel, H.L. (1820–71). Philosopher, sometime Reader in Theology, Oxford. 60, 64, 74, 77
Maynard, S. (1740–1862). A mathematical bookseller who wrote and edited mathematical texts. 64
Mulcahy, John (d. 1853). Professor of Mathematics, Queen's College, Galway, 1849–53. 48
Murphy, Robert (1806–43). Mathematician, sometime Fellow and Dean of Caius College, Cambridge. 48
Napier, John (1550–1617). Inventor of logarithms. 89
Newman, F.W. (1805–97). Professor of classics at Manchester University and of Roman Literature at University College London. 64
Newton, Isaac (1642–1727). Physicist and mathematician. 41, 42
O'Brien, J.T. (1792–1874). Fellow of Trinity College, Dublin 1820–36. 39
O'Higgins, William (d. 1853). Bishop of Ardagh 1829–53. 24
Pacius (G. Pace) (1550–1633). Pacius wrote on philosophy. 70
Paul of Tarsus (First century). The Apostle Paul. 70, 72
Peyrard, Francis (1760–1822). A scholar who edited Euclid's and Archimedes works. 69
Pfaff, J.F. (1765–1825). Writer on differential equations; he was a close friend of Gauss. 84
Poisson, S.D. (1781–1840). Poisson worked at the École Polytechnique on mathematical physics. 30, 35
Powell, Baden (1796–1860). Savilian Professor of Geometry at Oxford. 67
Puissant, Louis (1769–1843). Author of books on geodesy and astronomy. 58
Quilp, Daniel. A character in Dicken's *The Old Curiosity Shop*. 38
Renan, Ernest (1823–62). Renan wrote on biblical, Jewish, and linguistic subjects as well as the well-known *La Vie de Jesus*. 79
Ryall, Dr John (d. 1875). Professor of Greek at Queen's College, Cork. 26
Sadleir, F. (1774–1861). Fellow and Professor of Mathematics at Trinity College, Dublin. 39
Saul of Tarsus (First century). The Apostle Paul. 70
Shakespeare, William (1564–1616). Dramatist and poet. 48
Shylock. Character in Shakespeare's *The Merchant of Venice*. 70
Solly, T. (1813–75). Professor of English at Berlin University from 1843 to 1875. 13
Stevens. Not identified (possibly Henry Stevens 1819–86, bibliophile, bibliographer, and bookseller, an American who settled in London in 1845). 56

Sylvester, James, J. (1814–97). Algebraist who contributed to the theory of quadratic forms and invariants. 30

Taylor, Richard (1781–1858). Editor of the *Philosophical Magazine* from 1822; a fellow of the Linnean, Astronomical, and other learned societies. 32, 38, 39, 41

Thomson, William (1819–90). Sometime Tutor, The Queen's College, Oxford; Archbishop of York, 1862–90. 13

Todhunter, Isaac (1820–84). Fellow of St John's College, Cambridge, and the author of many textbooks. 65, 83

Veitch, John (1829–94). Professor of Logic at St Andrews and Glasgow Universities. 77

Venetus, Paulus (Niccoletti, Paolo) (d. 1429). Philosophical writer. 70

Wagner, Gabriel (*fl.* 1700). A controversialist who opposed late seventeenth century scholasticism; he wrote under the pseudonym Realis de Vienna. 78

Walker, John (*fl.* 1790–1830). Fellow of Trinity College, Dublin. 64

Wallis, John (1616–1703). Mathematician, Savilian Professor of Geometry, Oxford, 1649–1703. 70

Walsh, John (1786–1847). An eccentric who lived in Cork. 27–31, 39–41 83

Wedgwood, Hensleigh (1803–91). A barrister and magistrate who who wrote on philology. 64

Whewell, William (1794–1866). Master of Trinity College, Cambridge, 1841–66 and Knightbridge Professor of Moral Philosophy, 1838–55. 13

Young, J.R. (1799–1885). Professor of Mathematics in the Belfast Institution, 1833 to 1849, and author of many textbooks. 21, 64

Zunz, Leopold (1794–1886). Author. 71

BIBLIOGRAPHY

The bibliography is arranged in two main parts, first *manuscripts*, second *printed works.*

Manuscripts

This part of the bibliography is arranged in two sections:
(a) The letters of Boole and De Morgan
(b) Other manuscripts.

(a) *The letters of Boole and De Morgan*

With five exceptions – Letters 10, 20, 73, 78, 79 – all the letters are in the library of University College, London. They are catalogued under the number MS Add 97.

The Letters 10, 20, 73, 78, and 79 are to be found in the library of London University. Letter 20 is pasted in a bound collection of pamphlets (once part of De Morgan's library), and is catalogued under G. Boole, On a General Method in Analysis, 1844, L° [B.P. 21]. Letters 10, 73, 78, and 79, all from Boole to De Morgan, are designated MS 775/370/5, items 1 to 4. MS 775/370 is a collection of letters to and from De Morgan.

No.	Writer	Date	No.	Writer	Date
1	De M	29 Dec. 1842	22	B	3 Sept.
2	B	19 June 1843	23	De M	4 Sept.
3	De M	24 Nov.	24	B	8 Nov.
4	B	8 Dec.	25	De M	8 June 1850
5	De M	11 Dec.	26	B	17 Oct.
6	B	28 June 1844	27	B	31 Mar. 1851
7	B	15 Jan. 1845	28	B	22 Apr.
8	De M	6 Feb.	29	B	6 May
9	B	24 Feb.	30	B	25 May
10	B	8 Jan. 1847	31	B	24 June
11	De M	31 May	32	B	16 July
12	De M	28 Nov.	33	B	24 July
13	De M	29 Nov.	34	B	29 July
14	B	24 Aug. 1848	35	B	4 Aug.
15	B	8 Dec.	36	B	11 Aug.
16	De M	3 Apr. 1849	37	B	25 Aug.
17	B	12 Apr.	38	B	9 Sept.
18	B	21 Apr.	39	B	10 Sept.
19	De M	10 June	40	B	17 Nov.
20	B	13 Aug.	41	B	28 Nov.
21	De M	14 Aug.	42	B	28 June 1852

No.	Writer	Date	No.	Writer	Date
43	B	12 July	67	B	13 Nov.
44	B	23 July	68	B	7 Feb. 1861
45	B	27 Sept.	69	B	17 May
46	B	8 Oct.	70	De M	16 Oct.
47	B	8 Dec.	71	B	4 Nov.
48	B	7 Feb. 1854	72	B	21 Nov.
49	B	15 Feb.	73	B	7 Jan. 1862
50	B	23 Feb.	74	De M	1 Feb.
51	B	30 May	75	B	12 Feb.
52	B	n.d.	76	B	21 Apr.
53	B	3 Jan. 1855	77	De M	20 Sept.
54	B	3 Feb.	78	B	4 Nov.
55	B	21 Feb.	79	B	6 Nov.
56	De M	4 Jan. 1856	80	De M	7 Nov.
57	B	8 Jan.	81	B	10 Nov.
58	De M	13 Jan.	82	De M	12 Nov
59	B	23 Feb.	83	B	3 Jan. 1863
60	B	21 Mar. 1859	84	B	7 Jan.
61	B	9 June	85	De M	7 Feb.
62	B	15 Sept.	86	De M	5 July
63	De M	10 June 1860	87	De M	8 Aug.
64	De M	13 July	88	De M	25 Apr. 1864
65	B	17 July	89	B	3 May
66	B	18 Oct.	90	De M	n.d.

(b) *Other manuscripts*

De Morgan, A. (1860). [Autobiographical Note] British Library Add MS 28, 509 f.421.

——— (1864). Draft letter to H.M. Government. University College London Library. MS Add 97/1.

——— (1867). Note on Professor Boole's Papers. Library of the Royal Society. MS M.M. 16.34.

Boole, G. (1847). Draft letter to A. De Morgan, 8 Jan. 1847. Royal Society Library, Boole papers W8.

Roy. Soc. [part of A97 of Boole papers]. Library of the Royal Society.

Printed works

This part of the bibliography is arranged in three sections:

(a) Boole
(b) De Morgan
(c) Other authors.

For authors *other than G. Boole and A. De Morgan* titles of periodicals, etc. are given in full. For G. Boole and A. De Morgan titles of periodicals have been indicated by initials: a list of these abbreviations is given on page 138.

Collective works (encyclopaedias, etc.) with no identifying author's name are assigned a brief title in place of an author: thus *The English Cyclopaedia* is designed Eng. Cyc. 1854 and will be found in the listing under 'Eng. Cyc.'.

[R] preceding an entry should be read as 'Review of'. [Rs] following an entry means 'Review of books'. 'SDUK' indicates that a book was published under the auspices of the *Society for the Diffusion of Useful Knowledge.*

The parts of the *Cambridge Mathematical Journal* and the *Transactions of the*

Cambridge Philosophical Society were issued earlier than the volumes; I have assigned papers in these periodicals to the date of the part (which is given after the page reference) rather than to that of the volume.

Abbreviations of titles of periodicals used in the G. Boole and A. De Morgan bibliographies

AM	Assurance Magazine	
ASM	Astronomical Society, Memoirs	(*)
ASMN	Astronomical Society, Monthly Notices	(*)
Ath	Athenaeum	(*)
BA	British Association for the Advancement of Science, Report	
BM	Bentley's Miscellany	(*)
CA	Companion to the Almanac	
CDMJ	Cambridge and Dublin Mathematical Journal	
CJM	Crelle's Journal der Mathematik	
CMJ	Cambridge Mathematical Journal	
CPST	Cambridge Philosophical Society, Transactions	
CSEP	Central Society of Education, Publications	
DR	Dublin Review	
GM	Gentlemen's Magazine	(*)
IAJ	Institute of Actuaries, Journal	
LJM	Liouville's Journal de Mathematique	
LMSP	London Mathematical Society, Proceedings	
Ma	Mathematician, The	
MM	Mechanics Magazine	
NBR	North British Review	
NR	National Review	
NQ	Notes and Queries	(*)
PM	Philosophical Magazine	
PSP	Philological Society, Proceedings	(*)
PST	Philological Society, Transactions	(*)
QJE	Quarterly Journal of Education	(*)
QJM	Quarterly Journal of Pure and Applied Mathematics	
RIAP	Royal Irish Academy, Proceedings	
RIAT	Royal Irish Academy, Transactions	
RSET	Royal Society of Edinburgh, Transactions	
RSP	Royal Society, Proceedings	
RSPT	Royal Society, Philosophical Transactions	
SPAB	St Petersburg Academy, Bulletin.	

* May contain other items by De Morgan.

(a) *G. Boole*

I have tried to make the bibliography of Boole's work complete; I should add that substantial portions of letters of Boole are included in Cayley (1862*a*) (= Cayley 1889, vol. 5, 80–84) and in Jourdain (1913).

1835 An address on the Genius and Discoveries of Sir Isaac Newton. Lincoln 1835.

1840*a* Researches on the theory of analytical transformations. *CMJ* 2 (1841) 64–73 (Feb. 1840).

b On certain theorems in the calculus of variations. *CMJ* 2 (1841) 97–102 (May 1840).

c On the integration of linear differential equations with constant coefficients. *CMJ* 2 (1841) 114–19 (May 1840).

d Analytical Geometry. *CMJ* **2** (1841) 178–88 (Nov. 1849).
1841 Exposition of a general theory of linear transformations. *CMJ* **3** (1842) 1–20 (Nov. 1841), 100–119 (May 1842).
1843*a* On the transformation of definite integrals. *CMJ* **3** (1843) 216–24 (Feb. 1843).
b Remarks on a theorem of M. Catalan. *CMJ* **3** (1843) 277–83 (May 1843).
c On the transformation of multiple integrals. *CMJ* **4** (1845) 20–28 (Nov. 1843).
1844*a* On a general method in analysis. *RSPT* **134** (1844) 225–82.
b On the inverse calculus of definite integrals. *CMJ* **4** (1845) 82–7 (Feb. 1844).
c Notes on linear transformations. *CMJ* **4** (1845) 167–71 (Nov. 1844).
1845*a* On the theory of developments, Part 1. *CMJ* **4** (1845) 214–23 (Feb. 1845).
b On the equation of Laplace's functions. *BA* (1845) Part 2, 2.
1846 On the equation of Laplace's functions. *CDMJ* **1** (1846) 10–22.
1847*a* *The Mathematical Analysis of Logic.* Cambridge 1847 (reprinted Oxford 1948, 1951).
b *The Right Use of Leisure.* London 1847.
c On the attraction of a solid of revolution on an external point. *CDMJ* **2** (1847) 1–7.
d On a certain symbolical equation. *CDMJ* **2** (1847) 7–12.
e Remarks on the Rev. B. Bronwin's method for differential equations. *PM* (3) **30** (1847) 6–8.
f Note on a class of differential equations. *PM*(3) **30** (1847) 96–7.
1848*a* Remarks on a paper by the Rev. B. Bronwin. On the solution of a particular differential equation. *PM*(3) **32** (1848) 413–15.
b Remarks on a paper by the Rev. B. Bronwin. On the solution of a particular differential equation. *PM*(3) **33** (1848) 211.
c Note on quaternions. *PM*(3) **33** (1848) 278–80.
d Théorème général concernant l'intégration définie. *LJM* **13** (1848) 111–12.
e On the analysis of discontinuous functions. *RIAT* **21** (1848) 124–39.
f On a certain multiple definite integral. *RIAT* **21** (1848) 140–49.
g On a general transformation of any quantitative function. *CDMJ* **3** (1848) 112–16.
h On the calculus of logic. *CDMJ* **3** (1848) 183–98.
i Mr Boole's theory of the mathematical basis of logic. *MM* **49** (1848) 254–5.
1849 On a general theorem of definite integration. *CDMJ* **4** (1849) 14–20.
1851*a* *The Claims of Science.* London 1851.
b On the theory of linear transformations. *CDMJ* **6** (1851) 87–106.
c On the reduction of the general equation of the *n*th degree. *CDMJ* **6** (1851) 106–13.
d Letter to the Editor. *CDMJ* **6** (1851) 284–5.
e Proposed question on the theory of probabilities. *CDMJ* **6** (1851) 286.
f On the theory of probabilities and in particular on Mitchell's Problem of the distribution of the fixed stars. *PM*(4) **1** (1851) 521–30.
g Further observations on the theory of probabilities. *PM*(4) **2** (1851) 96–101.
h An account of the late John Walsh of Cork. *PM*(4) **2** (1851) 348–58.

1852 On reciprocal methods in the differential calculus. *CDMJ* 7 (1852) 156–66.

1853 On reciprocal methods in the differential calculus, continued. *CDMJ* **8** (1853) 1–24.

1854*a* *An Investigation into the Laws of Thought.* London, 1854; for another edition (reprinted 1958 New York) see 1916.

b Solution of a question in the theory of probabilities. *PM*(4) **7** (1854) 29–32.

c Reply to some observation of Mr. Wilbraham on the theory of chances developed in Prof. Boole's Laws of Thought. *PM*(4) **8** (1854) 87–91.

d On the conditions by which the solutions of questions in the theory of probabilities are limited. *PM*(4) **8** (1854) 91–8.

e Further observations relating to the theory of probabilities in reply to Mr. Wilbraham. *PM*(4) **8** (1854) 175–6.

f On a general method in the theory of probabilities. *PM*(4) **8** (1854) 431–44.

1855*a* *The Social Aspects of Intellectual Culture.* Cork, 1855.

b On certain propositions in algebra connected with the theory of probabilities. *PM*(4) **9** (1855) 165–79.

1856 On the solution of the equation of continuity of an Incompressible Fluid [letter to C. Graves, 5 May 1856]. *RIAP* **6** (1853–57) 375–85.

1857*a* On the comparison of transcendents with certain applications to the theory of definite integrals. *RSPT* **147** (1857) 754–804.

b On the application of the theory of probabilities to the question of the combination of testimonies or judgements. *RSET* **21** (1857) 597–652.

1859 *Treatise on Differential Equations.* London, 1859. 2nd edition 1865, 3rd 1872, 4th 1877, and a number of later reprints.

1860 *Treatise on the Calculus of Finite Differences.* London, 1869, 2nd edition 1872, 3rd 1880, and a number of later reprints.

1862*a* On the theory of probabilities. *RSPT* **152** (1862) 225–52.

b On simultaneous differential equations of the first order in which the number of variables exceeds by more than one the number of the equations. *RSPT* **152** (1862) 437–54.

c On the integration of simultaneous differential equations. *RSP* **12** (1862–3) 13–16.

d On the theory of probabilities. *RSP* **12** (1862–3) 179–84.

e On the differential equations of dynamics. *RSP* **12** (1862–3) 420–24

f On a question in the theory of probabilities. *PM*(4) **24** (1862) 80.

g Considerations sur la recherche des integrales premieres des equations differentielles partielles du seconde ordre. *SPAB* **IV** (1862) col. 198–215.

h On simultaneous differential equations in which the number of variables exceeds by more than unity the number of equations. *RSP* **12** (1862–3) 184.

i Supplement to a paper 'On the differential equations of dynamics'. *RSP* **12** (1862–3) 481.

1863*a* On the differential equations of dynamics. A sequel to a paper on simultaneous differential equations. *RSPT* **153** (1863) 485–501.

b Uber die partielle Differentialgleichungen zweiter Ordnung $Rr + Ss + Tt + U(s^2 - rt) = V$. *CJM* **61** (1863) 309–33.

1864*a* On the differential equations which determine the form of the roots of an algebraic equation. *RSPT* **154** (1864) 733–55.

b On the differential equations which determine the form of the roots of an algebraic equation. *RSP* **13** (1864) 245–6.

1865 *Treatise on Differential Equations.* Supplementary Volume, editor I. Todhunder. Cambridge 1865.

1868 On propositions numerically definite (read posthumously by A. De Morgan, March 1868). *CPST* **11** (1871) 396–411.

1916 *Collected Logical Works*, vol. II. Chicago and London 1916. Edited by P.E.B. Jourdain. (Note that no vol. I was issued: vol. II was reprinted in 1940 and 1952. The bulk of the volume is a reprint of 1854*a*.)

1952 *Studies in Logic and Probability.* London and La Salle 1952. Edited by R. Rhees. (This volume fills the gap caused by the non-appearance of vol. I of *Collected Logical Works.* Some editions are titled: *Collected Logical Works Vol. I, Studies, etc.* It prints for the first time some Boole manuscripts now in the Library of the Royal Society, together with the following items: 1847*a*, 1848*h*, 1868, 1851*a*, 1851*f*, 1851*g*, 1854*b*, 1845*c*, 1854*d*, 1854*e*, 1845*f*, 1857*b*, 1862*a*.)

(b) *A. De Morgan*

De Morgan wrote prolifically for a wide range of periodicals; the periodicals marked (*) in the list on page 138 may contain other items by De Morgan – this is certainly the case for *Notes and Queries* and the *Athenaeum.* For the periodicals not so marked, I hope the bibliography is complete. The only items I have deliberately omitted are certain reviews of elementary textbooks in the *Quarterly Journal of Education* and the brief reports of the papers read before the Cambridge Philosophical Society in the *Proceedings*: these merely summarize the papers in the *Transactions.* In addition I have not attempted to include a number of prefaces, introductions, and indexes he wrote in books by other authors: see S.E. De Morgan 1882, 415, for further information on such items.

1828 *The Elements of Algebra.* Translated by A. De Morgan from the first three chapters of the Algebra of M. Bourdon. London 1828.

1830*a* *The Elements of Arithmetic.* London 1830. 2nd edition 1832, 3rd 1835, 4th 1840, 5th 1846, 6th 1876, and many reprints).

b *Remarks on Elementary Education in Science.* London 1830.

c On the general equations of curves of the second degree. *CPST* **4** (1833) 71–8 (Nov. 1830).

1831*a* *The Study and Difficulties of Mathematics.* SDUK. London 1831 (reprinted 1832, 1836, 1840, 1847; Chicago edition 1898, 1902, 1910; La Salle edition 1943).

b On life assurance. *CA* 1831 86–105.

c On mathematical instruction. *QJE* **1** (1831) 264–79.

d Polytechnic School of Paris. *QJE* **1** (1831) 57–86.

1832*a* *Elementary Illustrations of the Differential and Integral Calculus.* SDUK. London 1832 (2nd edition 1842; Chicago editions 1899, 1909; La Salle edition 1943).

b On the general equation of the surfaces of the second degree. *CPST* **5** (1835) 77–94 (Nov. 1832).

c On eclipses. *CA* 1832 5–12.

d Study of natural philosophy. *QJE* **3** (1832) 60–73.

e On some methods employed for the instruction of the deaf and dumb. *QJE* **3** (1832) 203–19.

f State of mathematical and physical sciences in the University of Oxford. *QJE* **4** (1832) 191–208.

1833*a* A new method of reducing the apparent distance of the moon. *ASM* **5** (1833) 245–52.
b On comets. *CA* 1833 5–15.
c On teaching arithmetic. *QJE* **5** (1833) 1–16.
d On the method of teaching fractional arithmetic. *QJE* **5** (1833) 210–22.
e On the method of teaching the elements of geometry. Pt. 1. *QJE* **6** (1833) 35–49.
f On the method of teaching geometry. Pt. 2. *QJE* **6** (1833) 237–51.
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c On the notation of the differential calculus adopted in some works lately published at Cambridge. *QJE* **8** (1834) 100–110.
d [R] *Airy's Gravitation. QJE* **8** (1834) 316–25.
1835*a* *The Elements of Algebra preliminary to the Differential Calculus.* London 1835 (2nd edition 1837).
b *Examples of the Processes of Arithmetic and Algebra.* SDUK. London 1835.
c On Taylor's theorem. *PM*(3) **7** (1835) 188–92.
d Halley's comet. *CA* 1835 5–15.
e [R] Peacock's Treatise on Algebra. *QJE* **9** (1835) 91–110, 293–311.
f Ecole polytechnique. *QJE* **10** (1835) 330–40.
1836*a* *The Connexion of Number and Magnitude.* London 1836.
b *Examples of the processes of arithmetic and algebra.* London 1836 (reprinted 1847).
c *An Explanation of the Gnomic projection of the sphere.* SDUK. London 1836.
d A Treatise on the Calculus of Functions [contribution to *Encyclopaedia Metropolitana*] London 1836, 305–92.
e Old arguments against the motion of the Earth. *CA* 1836 5–19.
f On the relative signs of coordinates. *PM*(3) **9** (1836) 249–54.
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h [Contributions: Bradley, Delambre, Descartes, Dollond, Euler, Halley, Harrison, W. Herschel, Lagrange, Laplace, Leibniz and Maskelyne in *The Gallery of Portraits: with Memoirs*, London 1836.]
1837*a* *The Elements of Trigonometry and Trigonometrical Analysis preliminary to the Differential Calculus.* London 1837.
b *Thoughts suggested by the Establishment of the University of London.* London 1837.
c Theory of Probabilities [contribution to *Encyclopaedia Metropolitana*]. London 1837.
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