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# Augustus De Morgan, the History of Mathematics, and the Foundations of Algebra

By Joan L. Richards\*

It is astonishing how strangely mathematicians talk of the Mathematics, because they do not know the history of their subject. By asserting what they conceive to be facts they distort its history. . . . There is in the idea of every one some particular sequence of propositions, which he has in his own mind, and he imagines that that sequence exists in history; that his own order is the historical order in which the propositions have been successively evolved. . . . If he be to have his own [mathematical] researches guided in the way which will best lead him to success, he must have seen the curious ways in which the lower proposition has constantly been evolved from the higher.<sup>1</sup>

THE ABOVE QUOTATION from Augustus De Morgan's presidential address to the first meeting of the London Mathematical Society, on 16 January 1865, sounds a rather odd note. De Morgan is emphasizing that the historical development of mathematical ideas is often very different from the "particular series of propositions" that are neatly ranged before each conclusion in the practicing mathematician's mind. He is emphasizing the idiosyncrasies of the historical processes of mathematical discovery as opposed to the intellectual clarity of *ex post facto* mathematical demonstration.

In this, De Morgan's statement is not particularly startling. That the history of any subject is full of curious developments is almost a truism. De Morgan is saying more, however. Standing at the apex of a long and prolific mathematical career, he is exhorting the members of a fledgling mathematical society to explore those curious byways of mathematical history *in order to know how to conduct their mathematical researches*. He is specific in his admonitions against allowing the order of well-mastered mathematics color and order mathematicians' perceptions of the nature of its development. Instead, as this article will attempt to show, De Morgan is insisting that the twists and turns of mathematical history should be openly examined because they provide crucial clues to understanding mathematics.

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This paper was initially suggested by Lynn Joy, who proposed that I contribute to a session on the influences of the history of science on the practice of science at the History of Science Meetings, Norwalk, Conn., October 1983.

<sup>1</sup> "Speech of Professor De Morgan," *Proceedings of the London Mathematical Society*, 1865-1866, 1:1-9, p. 6.



*Augustus De Morgan. From Sophie De Morgan, Memoir of Augustus De Morgan (London, 1882). Courtesy of the American Philosophical Society.*

De Morgan's respect for the vagaries of mathematical history stands in marked contrast to the tendencies often exhibited in modern mathematicians' treatments of their history. These are often characterized by constructions of clear connections that show the past as a logical stage on the road to the present. Accordingly, past records are combed for glimmerings of future discoveries, and ideas obscure in their nativity are polished for modern comprehension. The unclear conceptions, peculiar interpretations, and strange motivations that might be found in an old mathematical document are ignored in favor of, or at least subordinated to, the logical structures that can be distilled from the argument. This is recognizably the historical approach against which De Morgan directed his address in 1865.

The difference in attitude between a mathematical history constructed as a logically ordered development and De Morgan's more protean approach is not just an issue of historiography or of personal taste. The difference reflects a disagreement about the nature of mathematics itself. De Morgan resisted logical reconstructions of mathematical history because he did not believe that mathematics was essentially logical. He carefully differentiated the conceptual subject matter of mathematics from the formal expression of its results. The history of mathematics traced the essentially undefinable organic process of conceptual growth, rather than the clearly definable process of formal development. For De Morgan the historical evolution of mathematical ideas provided important insights into the essential nature of the mathematics, which could not be counted on to fit neatly into logical or formal frameworks.

The difference between a formal view of mathematics and De Morgan's conceptual one is difficult to describe or demonstrate precisely. This is true even though De Morgan was passionately concerned with mathematical rigor and wrote explicitly and extensively about mathematical foundations. The difficulty lies in interpreting and understanding what he said. This problem arises largely because the difference between the conceptual and logical views of mathematical foundations is often masked for modern readers by apparently familiar words and phrases whose meanings and implications have changed slightly. Words like *logical*, *rigorous*, and *exact* are precisely those in which subtle differences in meaning can have powerful repercussions on interpretation.

Many historians of mathematics have interpreted De Morgan as the opposite of a conceptualist. He is often cast as a major representative of an early nineteenth-century school of British algebraists—including George Peacock, Duncan Gregory, and, according to some, Sir William Rowan Hamilton—that laid the groundwork for a formal, abstract view of algebra.<sup>2</sup> His views on the subject have routinely been read as some of the earliest statements defining and defending a formalist view of the nature of mathematical truth.

Recent scholars have noted, however, that the role of abstract algebraist sits rather uncomfortably with De Morgan, as well as with his mathematical compatriots. Interpreted this way, their works are riddled with ambiguities. Their foundational writings are difficult to understand precisely and sometimes even seem internally contradictory. In addition, their research practices often seem to belie their foundational outlook. A closer look is now revealing that the work of Brit-

<sup>2</sup> See, e.g., Eric Temple Bell, *The Development of Mathematics* (New York: McGraw Hill, 1954); and Luboš Nový, *Origins of Modern Algebra*, trans. Jaroslav Tauer (Leyden: Nordhoff, 1973).

ain's great algebraists rests on a precarious middle ground suspended between conceptual and formal views.<sup>3</sup>

This article attempts to recapture De Morgan's views of the nature of mathematics by looking to the wider context of his intellectual world for guidance in interpreting what he meant when he considered the foundations of the subject. In a letter to the Irish mathematician Hamilton, De Morgan stated: "In reading an old mathematician you will not read his riddle unless you plough with his heifer; you must see with his light if you want to know how much he saw."<sup>4</sup> The opening quotation suggests that one might find clues for reading De Morgan's riddle by looking at his view of mathematical history. This paper attempts to apply De Morgan's metaphor to himself: to plough with his heifer through the adjacent and often overlapping fields of mathematics and the history of mathematics in order to clarify how and what he saw.

De Morgan's path will be joined in the field of algebra, a field in which his work has long been well known. Here the pattern of furrows is often strangely tangled. These twists lead into an exploration of De Morgan's views of history in the second section. His historical musings provide an illuminating perspective from which to view his algebraic arguments. This is so because De Morgan's historical attitudes shaped his mathematical work and integrated it with his more broadly held intellectual and cultural convictions.

The peculiar stamp of De Morgan's historical view of mathematics is particularly clear in his reactions to contemporaneous French work in the calculus. This clarity results from the clash between widely held French and British attitudes that forced explicit negotiation. Despite apparent agreement with his French contemporaries that the calculus is based on the limit, De Morgan defended this conviction for reasons radically different from theirs. Following De Morgan's heifer, then, provides us with a glimmering of understanding that might in a larger study shed light on the texture of mathematics in Britain and in France and clarify the intellectual relations between the two mathematical communities. For the purposes of this paper, though, it is enough that his ideas both grew from and influenced the culture in which he lived. De Morgan's views of mathematics and its history illuminate many of the attitudes that colored British mathematical work throughout the nineteenth century.

#### THE FOUNDATIONS OF ALGEBRA

Augustus De Morgan was born in 1806 to a lieutenant-colonel in the British army. He passed through a variety of schools, taught by both Anglican and dissenting clergy, and in 1823 entered Trinity College, Cambridge. Here he showed a marked interest in mathematics and finished as fourth wrangler in 1827. De Morgan always viewed this as a poor showing, and he prepared for a career in law. On 13 February 1828 he was rescued from a life at the Bar by being elected to the chair of mathematics at the fledgling London University (later University

<sup>3</sup> This literature is growing fast. See, e.g., Joan L. Richards, "The Art and Science of British Algebra: A Study in the Perception of Mathematical Truth," *Historia Mathematica*, 1980, 7:343–365; Helena Pycior, "George Peacock and the British Origins of Symbolical Algebra," *Hist. Math.*, 1981, 8:23–45; and Pycior, "Early Criticisms of the Symbolical Approach to Algebra," *Hist. Math.*, 1982, 9:413–440.

<sup>4</sup> Quoted in Robert Perceval Graves, *Life of Sir William Rowan Hamilton*, 3 vols. (New York: Arno, 1975), Vol. III, p. 438.

College, London). He remained and thrived in this position for virtually all of his adult life.

While at Cambridge De Morgan had studied under a variety of tutors and teachers, notably George Biddle Airy, George Peacock, and William Whewell. These men were holdovers from the Analytical Society, and they played a part in the introduction of Continental mathematics into the Cambridge curriculum in the decade before De Morgan matriculated.<sup>5</sup> This was not the end of their impact, however. Long after the Analytical Society had dissolved, its members pursued careers self-consciously centering on the promotion and development of science in Britain. Airy became the Astronomer Royal. Peacock worked actively in mathematics through the 1830s. Whewell pursued an eclectic career as mathematician, philosopher, educator, and historian. Along with other Analytical Society members, like John Herschel and Charles Babbage, they were active in attempts to change the complacent outlook of the Royal Society, and they played important roles in the formation of the British Association for the Advancement of Science. In the first half of the nineteenth century hardly any aspect of British scientific theory or practice was untouched by the ideas of one or another member of this group.<sup>6</sup>

De Morgan can be regarded as a satellite of the Analytical Society. Slightly younger and fiercely independent, he nonetheless corresponded regularly with many of these men and tried to promote the study of mathematics from the somewhat academically provincial outpost of University College. He was an active member of the London scientific scene: his interests spanned astronomy, history, logic, probability, law, and often politics. At heart, however, De Morgan was a mathematician. The diversity of his activities attests to the breadth of his conception of his subject, the wide range of implications attendant on his perception of his role as mathematical researcher and educator.

De Morgan was deeply involved in education. University College was a young and often struggling institution, and De Morgan was totally devoted to his duties there. In addition, he was an active member of the Society for the Diffusion of Useful Knowledge. The *Penny Cyclopaedia*, published by this group, bristles with more than six hundred articles by De Morgan on mathematical and related subjects. Spreading his influence still further, he published several textbooks, as well as less elaborate guides to the study of mathematics.

De Morgan did not confine himself to elementary issues in these works. He felt that the problems that posed the major difficulties for the student learning mathematics were just the problems that lay at the foundations of the subject itself. His attempts to clarify mathematics for beginners parallel his struggles to formulate a view of it for himself. A contemporary map of the British mathematical landscape is clearly laid out in De Morgan's popular tracts.

<sup>5</sup> For a fuller discussion of the Analytical Society per se see Philip C. Enros, "The Analytical Society (1812–1813): Precursor of the Renewal of Cambridge Mathematics," *Hist. Math.*, 1983, 10:24–47.

<sup>6</sup> Recently considerable scholarship has focused on the activities of nineteenth-century British scientific promoters, including many of these figures. See, e.g., Susan Faye Cannon, *Science in Culture: The Early Victorian Period* (New York: Dawson/Science History Publications, 1978); Jack Morrell and Arnold Thackray, *Gentlemen of Science: Early Years of the British Association for the Advancement of Science* (Oxford: Oxford Univ. Press, 1981); and S. S. Schweber, "Scientists as Intellectuals: The Early Victorians," in *Victorian Science and Victorian Values: Literary Perspectives*, ed. James Paradis and Thomas Postlewait (New York: New York Academy of Sciences, 1981), pp. 1–38.

Part of what De Morgan did in his works was to examine and defend his image of mathematical knowledge and its role in intellectual culture. He was deeply concerned with understanding and explaining the essential nature of his subject and its relationship to other forms of knowledge. In this he reveals a temperamental similarity to his Analytical Society friends, who characteristically were fascinated by the philosophy of science. Herschel and Whewell were the major scientific philosophers of their age. Within mathematics, this philosophical focus translated into an interest in foundational questions—particularly in the foundations of what was called “symbolical algebra.”

The term *symbolical algebra* described a peculiar approach to algebra that is evident in much of the work of Analytical Society members. The society’s original focus was largely on mathematical symbols: they advocated the replacement of Newton’s symbology, wherein  $\dot{y}$  represented the first derivative of an equation (if  $y = x^2$ ,  $\dot{y} = 2x$ ), with the Leibnizian symbology, where the same derivative would be expressed as  $dy/dx$ . Several members of the society campaigned vigorously to change the symbology used at Cambridge, and by 1819, when Peacock became examiner, the symbology on the Tripos was changed.

This effort at changing the symbols was seen as important not because it would allow easier transmission of Continental results to England, but because it promised to strengthen British mathematical research. Algebraic manipulations of the Continental symbology of  $dy/dx$  and its inverse  $\int y dx$  had long proved suggestive of new results. Although it was widely acknowledged that such practices as reaching results by “multiplying both sides by  $dx$ ” could not be rigorously justified, they were remarkably effective. Much of the fertility of eighteenth-century Continental mathematics sprang from the suggestive powers of the Leibnizian symbology. A large part of the Promethean program of the Analytical Society involved making this symbolical power accessible to their compatriots.<sup>7</sup>

The members of the Analytical Society did not see the power of well-chosen symbologies to be merely pragmatic. In fact, they emphasized that the very foundations of the calculus rested on this symbolic power. In the period when the foundations of calculus were still in dispute, they unambiguously cast their lot with J. L. Lagrange, who had hoped to avoid the foundational difficulties of the calculus by arguing that the derivative was an epiphenomenon of the symbolism of the Taylor series. He postulated that any function could be expanded into a series of the form

$$f(x) = f(a) + f'(a)/1!(x - a) + f''(a)/2!(x - a)^2 \dots f^n(a)/n!(x - a)^n + R(x).$$

He then defined the first derivative of a function as the coefficient of the second term in the series, the second derivative as that of the third, and so forth. Using this approach Lagrange hoped to avoid the abstruse metaphysical questions raised by the concepts of the calculus.

The Analytical Society followed Lagrange’s lead. The group chose to translate S. F. LaCroix’s text as part of their campaign to bring Continental mathematics to Britain. LaCroix had based his text on d’Alembert’s notion of the limit, but the Analytical Society rejected this aspect of the work. In several “Notes,” Babbage carefully showed how to ground LaCroix’s results in Lagrange’s algebraic

<sup>7</sup> For a more complete and detailed treatment of the various symbologies developed for the calculus see Florian Cajori, “The History of the Notations of the Calculus,” *Annals of Mathematics*, 2nd Ser., 1924, 25:1–46.

approach.<sup>8</sup> In this the young analysts, like Lagrange himself, were hoping to bypass the metaphysical difficulties of the calculus completely. Making its results fall out of the manipulation of symbology revealed the calculus to be no more than a complicated algebra.

De Morgan arrived on the scene in the 1820s, after the initial battle over the use of Continental symbology had been won. However, he was not too late for the second phase of the struggle, the attempt to define clearly the foundations on which the obvious power of mathematical symbologies rested. Understanding the foundations of symbolical algebras was a problem that challenged De Morgan throughout his adult life.

De Morgan's first published work was a translation, in 1828, of L. P. M. Bourdon's *Elements of Algebra*, which, he claimed, was "particularly well adapted for elementary instruction, on account of the care which is taken to deduce every rule from first principles, and to distinguish between the results of convention and those of demonstration." Thus *Elements of Algebra* contains a clear statement of the foundational problems De Morgan and his compatriots found in the study of symbolical algebra. "Algebra," the book begins, "is the part of mathematics in which symbols are employed to abridge and generalize the reasonings which occur in questions relating to numbers": to paraphrase slightly, arithmetic is the science of number, and algebra is generalized or universal arithmetic.<sup>9</sup> This was not a vague definition for De Morgan. The process of generalization followed specific rules, which required that the terms of any general statement have specific numerical referents. Thus, if one generalized the equation

$$(4^2 - 2^2) = (4 + 2)(4 - 2)$$

to get

$$(a^2 - b^2) = (a + b)(a - b),$$

one had to specify that  $a > b$ . The restriction was necessary because algebra was a generalization of reasonings relating to numbers. The boundaries of legitimate interpretation and hence of legitimate algebra were circumscribed by the idea of number.

Actually defining *number* was impossible for De Morgan, because the term referred to a complex and open-ended mathematical concept. Although he nowhere offered a simple definition of it, he did occasionally try to describe it, as in the article entitled "Number" in the *Penny Cyclopaedia*:

The notion of number is suggested by repetition or succession; . . .

If we never numbered any things capable of division into parts like themselves, our notion of number would rest in what is now called *whole* number. If the intellect were taught to count by the beating of the clock, and never came in contact with any other magnitude except that of the intervals between the beats, it is difficult to see how the idea of fractions would be obtained. But when we come to put together continuous magnitudes, . . . such as lines, surfaces, etc., we then begin to see that the unit is

<sup>8</sup> S. F. Lacroix, *An Elementary Treatise on the Differential and Integral Calculus*. Translated from the French [by Charles Babbage, George Peacock and John Herschel] (Cambridge: Deighton, 1816); see esp. "Note B," pp. 595–620; also "Note G," pp. 654–660, and "Note L," pp. 679–681.

<sup>9</sup> Louis Pierre Marie Bourdon, *The Elements of Algebra: Translated from the Three First Chapters of the Algebra of M. Bourdon, and Designed for the Use of Students in and Preparing for the University of London*, trans. Augustus De Morgan (London: J. Taylor, 1828), quoting from "Advertisement," and from p. 1.

purely arbitrary, . . . so that the consideration of smaller or larger units, and the reduction of processes from one unit to another, become necessary. Hence the doctrine of fractions, and finally that of Incommensurables.<sup>10</sup>

For De Morgan, the difficulty encountered when defining number in no way diminished its centrality as the foundation of arithmetic. All arithmetic truths described specific properties of the idea, which, in turn, defined the subject. The open-endedness of the concept did not imply that it had no limits. So, for example, succession may suggest fractional and incommensurable values in geometry, but all succession involves positive numbers. Negative numbers are nowhere to be found and hence have no obvious place in arithmetic.

De Morgan's treatment of negative numbers in his translation of Bourdon's *Elements of Algebra* is a powerful illustration of the limitations imposed by interpreting algebra as generalized arithmetic. There the status of negative numbers forces itself to center stage because negative numbers, which often appear in the solution of algebraic problems, cannot be given an arithmetic meaning. So, for example, Bourdon considered the problem, "To find a number which, added to a number, gives for their sum the number  $a$ ." This can be translated into the formulas  $b + x = a$  or  $x = a - b$ . Then, if  $a = 24$  and  $b = 31$ ,  $x$  is equal to  $-7$ , which is numerically incomprehensible. Bourdon interpreted this as an indication of the impossibility of solving the problem as stated. "But," he continued, "if we consider the solution independently of its sign, that is,  $x = 7$ , we may say that it is the solution of the following problem, 'To find a number which, subtracted from 31, gives 24'; which only differs from the first, viz. 'To find a number which, added to 31, gives 24,' in this, that the words 'added to' are supplied by the words 'subtracted from.'" <sup>11</sup> This kind of argument occurred and recurred with copious explanatory notes in De Morgan's translation. It represents a valiant effort to explain how symbolic manipulations that were nonsensical within arithmetic, the science of number, invariably yielded correct results in algebra.

De Morgan abandoned his struggle to understand the validity of negative and impossible numbers within the strict numerical confines of generalized arithmetic in the wake of a new interpretation of algebra suggested by one of his Cambridge mentors. In his 1830 text, *A Treatise on Algebra*, Peacock proposed freeing algebra from the limits of arithmetic. Instead of defining it as a universal arithmetic, Peacock regarded it as its own science, symbolical algebra, which did not rely on arithmetic interpretation for its validity.

In the absence of the interpretative relationship, arithmetic served merely as the "science of suggestion" for Peacock's symbolical algebra. Arithmetic equations like

$$(4^2 - 2^2) = (4 - 2)(4 + 2)$$

pointed the way to symbolic forms like

$$(a^2 - b^2) = (a - b)(a + b).$$

Beyond this suggestive function, Peacock argued, arithmetic had no necessary foundational relation to symbolical algebra. Algebra was a separate study that

<sup>10</sup> *Penny Cyclopaedia*, s.v. "Number," pp. 363–364.

<sup>11</sup> Bourdon, *Elements of Algebra* (cit. n. 8), p. 77.

focused on the symbolical forms themselves and on their interrelationships, looking for interpretations wherever they could be found.

Peacock's approach allowed the relevance of all manner of interpretations to understanding the arithmetically generated but numerically meaningless symbolic forms of algebra. Within the old view of algebra as universal arithmetic, even legitimating negative numbers by drawing analogies to debits and credits was technically inadmissible because it moved from arithmetic into accounting. Peacock's new approach recognized the validity of this kind of reasoning. It opened the door, for example, to sophisticated models like the geometrical one developed in France by Adrien Quentin Buée and introduced into England by John Warren, which embraced not only negative numbers but imaginary ones as well.<sup>12</sup>

Peacock's new view was clearly very effective in resolving the conflicts inherent in regarding algebra as universal arithmetic. At the same time, though, it was profoundly disturbing. De Morgan's review of it was five years in the writing because, in his words, of "the very great difficulty of forming fixed opinions upon views so new and so extensive. At first sight it seemed to us something like symbols bewitched, and running about the world in search of meaning."<sup>13</sup> In freeing symbolical algebra from its arithmetic roots, Peacock risked turning the manipulation of algebraic symbols into an empty game with no essential ties to the mathematical ideas that had spawned them.

Peacock attempted to show a tie between algebra and mathematical ideas by formulating what he called "the principle of equivalent forms." He had trouble encapsulating the principle in a single consistent phrase, but a representative statement of it reads: "Whatever equivalent form is discoverable in arithmetical algebra considered as the science of suggestion, when the symbols are general in their form, though specific in their value, will continue to be an equivalent form when the symbols are general in their nature as well as in their form."<sup>14</sup> This principle allowed Peacock to make the leap from recognizing that

$$(a^2 - b^2) = (a + b)(a - b) \quad (\text{a "general" equivalent form})$$

was true in arithmetical algebra for all "specific values" of  $a$  and  $b$  where  $a > b$ , to stating that

$$(a^2 - b^2) = (a + b)(a - b) \quad (\text{an equivalent form for which "the symbols are general in their nature as well as in their form"})$$

is universally true without restricting the values of  $a$  and  $b$ . The principle served to guarantee the validity of the algebraic system even when its terms were uninterpretable.

When an equivalent form results from the performance of definable operations, its existence is *necessary*, as a consequence of them: but if an equivalent form exists, or

<sup>12</sup> The works generally cited were John Warren, *A Treatise on the Geometrical Representation of the Square Roots of Negative Quantities* (Cambridge, 1828); and Adrien Quentin Buée, "Mémoire sur les quantités imaginaires," *Philosophical Transactions of the Royal Society of London*, 1806, pt. 1, pp. 23–88.

<sup>13</sup> Augustus De Morgan, review of George Peacock, *A Treatise on Algebra*, in *Quarterly Journal of Education*, 1835, 9:91–110; 293–311, on p. 311.

<sup>14</sup> George Peacock, "Report on the Recent Progress and Present State of Certain Branches of Analysis," *Report of the Third Meeting of the British Association for the Advancement of Science*, 1833, pp. 185–352, on p. 199.

is supposed to exist, when the operations which produce it are not definable, its existence is no longer *necessary*, . . . thus if  $n$  be a whole number, the existence of the equivalent series for  $(1 + x)^n$  is *necessary* inasmuch as the operation which produces it may be completely defined; but if  $n$  be a general symbol, we are unable to define the operation by which we pass from  $(1 + x)^n$  to its equivalent series, which exists therefore under such circumstances, only in virtue of the permanence of equivalent forms: the connection between one and the other therefore only becomes necessary, when its existence is assumed: in other words, if such an equivalent series *does* exist, it *must* be the series in question and no other.<sup>15</sup>

In this passage Peacock uses the term *necessary* to refer to cases where the truth of a symbolical equation is attested to by its meaning as well as its form. Even when no such meaning could be assigned, however, the “permanence of equivalent forms” served to guarantee its validity.

Peacock’s principle is fundamentally historical in that it defines the validity of a mathematical form by its genesis. However, he did not interpret this principle as temporally contingent; he presented it as the timeless conceptual foundation stone of the science of algebra.

The attempt to fix the principle atemporally, to establish its absolute validity, is explicitly elaborated in the philosophical work of Peacock’s friend William Whewell. In his two-volume *Philosophy of the Inductive Sciences*, Whewell focused on the problem of how one could find truth by induction from a series of discrete particulars. His solution was one in which scientific truth was not contingent, culled from the recognition of patterns or relations inherent in a set of facts. Rather, truth was attained when the observer applied the appropriate idea, generated from within his mind, to order the facts presented in a particular inquiry. At the basis of each successful scientific subject were a very few subjective fundamental ideas, on which the truth and comprehensibility of the entire science rested. For geometry the fundamental idea was space; for Newtonian dynamics it was force; for symbolical algebra it was Peacock’s principle of equivalent forms. The truth of algebra rested on this principle as firmly and timelessly as that of arithmetic rested on number.<sup>16</sup>

In his initial response to Peacock’s work, his 1835 review, De Morgan apparently embraced Peacock’s view of algebraic foundations, complete with the principle. “The work of Mr Peacock is difficult,” he noted, “but logical.”<sup>17</sup> In later writings, however, De Morgan modified his position. He recognized more clearly than Peacock that arithmetic suggestion was not necessary to provide equivalent forms for internally consistent algebraic systems; a set of well-constructed axioms could be relied upon equally well. A typical statement attesting to the possibility of axiomatically generating algebras is found in an 1840 article, “Negative and Impossible Quantities,” which De Morgan wrote for the *Penny Cyclopaedia*:

When we wish to give the idea of symbolical algebra, . . . we ask, firstly, what symbols shall be used (without any reference to meaning); next, what shall be the laws under which such symbols are to be operated upon; the deduction of all subsequent

<sup>15</sup> George Peacock, *A Treatise on Algebra* (Cambridge: J. & J. J. Deighton, 1830), pp. xviii–xix.

<sup>16</sup> William Whewell, *The Philosophy of the Inductive Sciences Founded upon Their History*, 2 vols. (2nd ed., rev., London: John W. Parker, 1847; rpt. London: Frank Cass, 1967).

<sup>17</sup> De Morgan, review of Peacock, *Treatise on Algebra* (cit. n. 13), p. 309; for a fuller treatment of the development of De Morgan’s ideas of algebra see Helena M. Pycior, “The Three Stages of Augustus De Morgan’s Algebraic Work,” *Isis*, 1983, 74:211–226.

consequences is again an application of common logic. Lastly, we explain the meanings which must be attached to the symbols, in order that they may have prototypes of which the assigned laws of operation are true.<sup>18</sup>

This formulation reverses the order Peacock's principle established in algebra. Rather than moving from meaning to abstract forms, the development is from abstract forms to meaning. More important, though, it does not refer to historical questions about their genesis in judging the validity of algebraic forms. Rather, it grounds their truth on the atemporal internal logic of the axiomatic system.

Not only did De Morgan state, here and elsewhere, that all algebraic consequences could be deduced from a series of basic axioms; he was the first to specify the axioms that would do the trick. In his paper "On the Foundation of Algebra, No. II," he set out the basic axioms of field theory, omitting only associativity, an accomplishment for which he is often remembered in the history of mathematics. De Morgan's actual presentation of the field axioms is rather anticlimactic, however. He prefaced his list with the casual remark: "As far as I can see (and I believe no writer has professed to throw together in one place every thing that is essential to algebraical process) the laws of operation are as follow." Having presented the axioms he did nothing with them, merely commenting, "I believe the preceding rules to be neither insufficient nor redundant, though I should be noways surprised to see them proved both the one and the other; least of all if it were the latter."<sup>19</sup>

De Morgan's offhand presentation of this material is not false modesty. It accurately reflects his assessment of the intrinsic interest of actually laying down the axioms of a given algebraic system. For De Morgan, such systems are only incidentally logical. They are essentially historical. For the true mathematician they are suggested by mathematical ideas, and, if they are to be significant, they must end in mathematical ideas. In the same paper De Morgan tried to explain his view using the metaphor of a puzzle.

By itself, this method of operation, this algebra of rules without meaning, is no more of a science than the use of the well-known toy called the Chinese puzzle, in which a prescribed number of forms are given, and a large number of different arrangements, of which the outlines only are drawn, are to be produced. Perhaps a dissected map or picture would be a still better illustration: a person who puts one of these together by the backs of the pieces, and therefore is guided only by their forms, and not by their meanings, may be compared to one who makes the transformations of algebra by the defined laws of operation only: while one who looks at the fronts, and converts his general knowledge of the countries painted on them into one of a more particular kind by help of the forms of the pieces, more resembles the investigator and the mathematician.<sup>20</sup>

The importance of laying out the axiomatic structure of an algebraic system paled in comparison to the final task of interpretation, which gave the system meaning and significance.

De Morgan's insistence that understanding meaning was the ultimate goal of

<sup>18</sup> *Penny Cyclopaedia*, s.v. "Negative and Impossible Quantities," pp. 133–134.

<sup>19</sup> Augustus De Morgan, "On the Foundations of Algebra, No. II," *Transactions of the Cambridge Philosophical Society*, 1842, 7:287–300, on pp. 287, 288.

<sup>20</sup> *Ibid.*, pp. 289–290.

the mathematical researcher complicates the atemporal thrust of his axiomatic developments. This is because the process of interpretation took place outside of the logical framework of the algebraic system. De Morgan recognized, more clearly than had Peacock, that this process could not be defined or fixed by timeless principles. For him it was a historically conditioned rather than a permanently defined search. As his 1865 speech suggests, De Morgan's perception of the work of the mathematician was intimately bound up with his interpretation of mathematical history. Thus understanding his view of the mathematical enterprise requires plodding behind his ploughing heifer into the field of the history of mathematics.

#### THE HISTORY OF ALGEBRA

In the first half of the nineteenth century, when De Morgan was at the height of his powers, the study of history was coming into its own in Britain. In the social sciences, like political economy, historical arguments provided a satisfying compromise between the stale dullness of immutable principles and the anarchy of ungrounded change. De Morgan lived in the age of the great political historians, who justified and extolled Britain's present by reference to her past.<sup>21</sup> In his view of mathematical history De Morgan embodied the same progressive spirit.

As a mathematical historian De Morgan left several distinct legacies. One was bibliographic. He had a passion for mathematical books and amassed an impressive library, now unfortunately scattered indiscriminately through the University College system. His bibliographical work, *Arithmetical Books from the Invention of Printing to the Present Time*, stands as a testament to his passion for bibliographic accuracy and remains a standard resource work. Another interest was moral. He tried to understand his mathematical predecessors not merely as intellectual forefathers but as human beings. To this end, he devoted considerable effort to the study of Newton and his circle, attempting to understand and evaluate the characters of various people involved in this flowering of British science.

Yet a third strand of De Morgan's historical perspective links him to contemporary historians and essentially informed his mathematical practice: his exploration of mathematical history as part of a larger attempt to understand and explain the nature of mathematics itself. De Morgan's interest in history as a clue to successful mathematical practice is not readily apparent in his bibliographical lists or in his moralistic musings. However, his popular works are rich in historical illusions and interpretations, attesting to his conviction that mathematics is often best understood through its history.

De Morgan himself never wrote a synthetic history of mathematics, although he actively responded to those who did—offering encouragement and criticism and sharing bits of information. When in need of comprehensive historical argument about arithmetic or analysis, he consistently referred to Peacock's historical summaries: either to the article "Arithmetic" in the *Encyclopedia Metropolitana* or to the "Report on Analysis" in the *Report of the Third Meeting of the British Association for the Advancement of Science*. De Morgan viewed these works by his mentor as definitive treatments that obviated the need to write his own.

<sup>21</sup> For a treatment of these historians see J. W. Burrow, *A Liberal Descent: Victorian Historians and the English Past* (New York: Cambridge Univ. Press, 1981).

In these works Peacock had espoused a conceptual view of mathematical development. His unit of historical analysis was mathematical ideas rather than mathematical forms or techniques. Thus in "Arithmetic" Peacock defined arithmetic as "the science of numbers and their notation, and of the different operations to which they are subject." He went on to specify that the "idea of number is one of those which are first presented to the mind, and which indeed may be considered as nearly coexistent with the exercise of our natural faculties," and emphasized the independence of this idea from the number systems, relations, and so forth that were used to describe it.<sup>22</sup>

The historical development of arithmetic, as Peacock presented it, involved ever-increasing sophistication in understanding the concept of number through the development of new symbology and more sophisticated numerical operations. It was a history of the progressive understanding of fundamental mathematical concepts through the manipulation and development of the symbolical systems that described them.

Peacock's interpretation of mathematical history as the progressive unfolding and clarification of mathematical concepts is similar to the historical interpretation William Whewell developed in his three-volume *History of the Inductive Sciences*, published in 1837. There Whewell had found all of scientific history to be a saga of the identification and clarification of a few basic scientific concepts. Peacock's histories reflect the same view in the area of the mathematical sciences.

The similarity between Peacock's—and by implication De Morgan's—view of mathematical history and Whewell's view of scientific history is the product of a shared attitude toward history, an attitude that is more explicit in Whewell than in Peacock. This is the conviction that the history of science is itself a science. Accordingly, historians also must be concerned with the identification and clarification of a few basic concepts that can be expected to provide essential material for an understanding of the process of scientific discovery and the nature of science itself. In the introduction to *History of the Inductive Sciences* Whewell proclaimed:

The examination of the steps by which our ancestry acquired our intellectual estate, may . . . afford us some indication of the most promising mode of directing our future efforts to add to its extent and completeness.

To deduce such lessons from the past history of human knowledge, was the intention which originally gave rise to the present work.<sup>23</sup>

Whewell maintained this orientation in the two-volume *Philosophy of the Inductive Sciences Founded upon Their History* published three years later. In the introduction to this work he emphasized:

We can point out a very important peculiarity by which this work is, in its design, distinguished from preceding essays on like subjects; . . . it is this: that we purpose to collect our doctrines concerning the nature of knowledge, and the best mode of acquiring it, from a contemplation of the Structure and History of those Sciences (the Material Sciences), which are universally recognized as the clearest and surest exam-

<sup>22</sup> *Encyclopedia Metropolitana*, s.v. "Arithmetic," by George Peacock, p. 369.

<sup>23</sup> William Whewell, *History of the Inductive Sciences*, 3 vols. (3rd ed., rev., London: John W. Parker, 1857; rpt. London: Frank Cass, 1967), Vol. I, p. 4.

ples of knowledge and of discovery. It is by surveying and studying the whole mass of such Sciences, and the various steps of their progress, that we now hope to approach to the true Philosophy of Science.<sup>24</sup>

Whewell pursued a dual goal in his attempt to develop a philosophy of science based on history. In part he aimed at illuminating the process by which true knowledge was discovered: "By examining the steps by which such acquisitions have been made, we may discover the conditions under which truth is to be obtained." Implicit in this statement is the conviction that ontogeny recapitulates phylogeny, that the individual searcher after truth would do well to be guided by the processes of history in his attempts to attain true understanding. In addition, Whewell was hoping that history would enable him to understand the defining characteristics of true knowledge: "By considering what is the real import of our acquisitions where they are certain and definite, we may learn something respecting the difference between true knowledge and its precarious or illusory semblances."<sup>25</sup> He believed that intellectual genealogy could provide important demarcation criteria for scientific veracity.

De Morgan was a friend and correspondent of Whewell's who also reviewed his works. His review of *The Philosophy of Discovery*, a reedited version of part of the larger *Philosophy of the Inductive Sciences*, reveals his essential agreement with Whewell's view of mathematical and scientific history as a process of conceptual unfolding:

When persons in general think of the course of discovery, they imagine, if not a chapter of accidents, a book of accidents, of which the chapters might possibly have been written in a different order. They have not arrived at any idea upon a point on which Dr. Whewell has arrived not only at a clear idea, but at a clear mode of teaching. . . . He has been the first to urge, and to demonstrate from history, that the several great steps of discovery have arisen out of the acquisition of distinctness about one and another idea.<sup>26</sup>

This quotation is also evidence that De Morgan accepted Whewell's natural historical approach to understanding the philosophy of science: he hails Whewell as the first "to demonstrate from history" the nature of scientific advance.

An important aspect of Whewell's theory of history was that it was essentially organic; it followed an Aristotelian model of growth. The progress of science was a movement from poorly understood potential knowledge to clearly grasped actual knowledge. As in the growth of an Aristotelian tree, the goal of the development—a fully developed tree, or, in science, a clearly grasped fundamental idea—was fixed. The route to this outcome was contingent and unpredictable, however. One could analyze scientific development in retrospect but could not legislate the process that would produce future results. To encourage progress one could but provide optimum conditions and wait to see what happened.

This point is one that Whewell acknowledged but De Morgan insisted upon. When Whewell published an excerpt from the larger *Philosophy of the Inductive Sciences* entitled *Novum organon renovatum* he solicited a review from De Mor-

<sup>24</sup> Whewell, *Philosophy of the Inductive Sciences* (cit. n. 16), Vol. I, p. 6.

<sup>25</sup> *Ibid.*, p. 3.

<sup>26</sup> Augustus De Morgan, review of William Whewell, *The Philosophy of Discovery*, *Athenaeum* 14 Apr. 1860, no. 1694, p. 502.

gan. "I have there talked," he wrote in December 1858, "about a certain kind of Logic, the Logic of Induction, and as all logic belongs to you I have some hope of seeing some criticism of what I have said written by you in the *Athenaeum* or elsewhere."<sup>27</sup>

The response was immediate. De Morgan reviewed the work for the 8 January 1859 issue of the *Athenaeum* and focused directly on the question of whether one could reduce scientific discovery to a logic, a fixed "organon." He firmly emphasized that one could not and carefully excluded the process of discovery from the purview of any fixed method:

Dr. Whewell holds that the practical results of the philosophy of science must be rather classification and analysis of what has been done, than precept and method for future doing. Here again we entirely agree. Even in geometry and algebra, . . . the rule is, Imitate those who have succeeded by patiently thinking out, as they did, the method of succeeding. You may be aided by observation of your predecessors: they may give useful hints, but not digested and infallible rules.<sup>28</sup>

Here, anticipating his 1865 address, De Morgan sees knowledge of historical development as a guide to the individual researcher. Its message is not prescriptive, however; it gives "hints, but not digested and infallible rules."

De Morgan continued to articulate this organic emphasis, widening his point to include not just the private practices of the individual but the entire form of scientific advance. Just as one could not talk of a logic of discovery or create an organon, he argued, one could not speak of scientific method as "inductive." *Induction* was too limited a term to describe the development of the sciences as Whewell's history was discovering them to be. "Let induction mean, as it always has done, the generalization by collection of particulars: let the act of the discoverer, by which he divines the general notion under which the particulars can be brought, receive its own proper name."<sup>29</sup>

For De Morgan, Whewell's historically based philosophy suggested a radically new model for the sciences that was neither deductive nor inductive. The Cambridge man's major contribution to philosophy was his recognition that scientific formalisms were logically structured descriptions of an underlying conceptual reality that was being slowly revealed through time. Whewell had gleaned this insight from studying the historical development of the sciences. At the same time, it had important implications for the form that development would take in the future. Conceptual progress was not linear in the way logical progress would be. Since formalizations were logical, but the conceptual reality they described was only slowly revealed, science developed in unpredictable ways. The scientific researcher had to be ready to accept confusion and ambiguity on the path to truth.

Whewell's historically based conceptual philosophy was focused primarily on the sciences. He treated mathematics less historically, as an essentially static field. Not surprisingly, this was not adequate for De Morgan, who saw himself as actively involved with mathematical development and progress. In his myriad

<sup>27</sup> William Whewell to Augustus De Morgan, 9 Dec. 1858, Whewell Archives, Trinity College Library, Cambridge, England, MS 0.15.47<sup>24</sup>.

<sup>28</sup> [Augustus De Morgan], review of William Whewell, *Novum organum renovatum*, *Athenaeum*, 8 Jan. 1859, no. 1628, p. 43.

<sup>29</sup> *Ibid.*, p. 44.

writings about various aspects of mathematics he extended Whewell's historically based interpretation of the conceptual nature of science to cover mathematical development.

The history to which De Morgan turned his attention for information about the nature of mathematics and its development was that of algebra, specifically the theory of negative and impossible (what are now called imaginary) numbers. The origin and development of these algebraic anomalies was a major interest to which he devoted several scholarly articles.<sup>30</sup> As usual with De Morgan's histories, however, these are dry and technical bibliographic discussions. His theoretical interest in thus piling up factual information is revealed more clearly and simply in his more popular writings, particularly those appearing in the *Penny Cyclopaedia*.

In "Negative and Impossible Quantities" De Morgan traced the recognition that such quantities are a problem in the Hindu *Viga Ganita* (to which he devoted a long article in its proper alphabetical place). In this twelfth-century work "there is mention of a modification of quantity unknown in arithmetic. . . ." Rules are provided for their use, as in the following example: "Patna is fifteen yojanas east, and Allahabad eight yojanas west, of a place called Varanasi; 'the interval or difference is twenty-three yojanas, and is not obtained but by addition of the numbers. Therefore, if the *difference* between two *contrary* quantities be required their *sum* must be taken.'" "Surely," De Morgan continued, "it will be said that algebra began in a strange confusion of ideas; but yet the fault is rather in expression than in conception. An art was in existence presenting undoubted means of discovering truth, commencing with a generalization of which the use was obvious, but not the meaning."<sup>31</sup>

The "strange confusion of ideas" here traced by De Morgan to the *Viga Ganita* bears a striking resemblance to the confusions he had encountered a decade before when translating Bourdon's *Elements of Algebra*. De Morgan recognized the universality of the position and interrupted his narrative to analyze it:

We must here pause to remind the reader that errors, however palpable and admitted, are not necessarily productive of error. *True* reasoning, on *true* principles, *must* lead to truth; but if for true we write false, and for truth falsehood, we have no longer any right to say *must*, but only *most probably will*. If then we can show of a particular class of errors that, used in a certain way, the results agree with those of true reasoning on true principles, we may demand the use of those errors as demonstrated means of finding truth. The mind of man would never stop at such a point; but for all that, we have the conclusion, as a logical consequence of the rules of arithmetic, that the mistake of the impossible subtraction . . . will produce no falsehood in the result.

De Morgan here describes what he sees as having happened to the author of the *Viga Ganita*. Later in the article he offers a clearly stated generalization of the historical process of discovery:

The order of discovery is as follows: —We first ask what sort of magnitude [e.g., distance] is to be reasoned upon; next, what are the obvious relations existing between such magnitudes; lastly, what is a convenient mode of representing the

<sup>30</sup> Augustus De Morgan, "On the Invention of the Signs + and -; and on the Sense in Which the Former Was Used by Leonardo da Vinci," *Philosophical Magazine*, 1842, 20:135-137; and De Morgan, "On the Early History of the Signs + and -," *Trans. Cambridge Phil. Soc.*, 1871, 11:203-212.

<sup>31</sup> *Penny Cyclopaedia*, s.v. "Negative and Impossible Quantities," p. 130.

magnitudes in question; all that follows is an application of the logic common to all branches of reasoning.”<sup>32</sup>

The possibility of generating meaningless but truth-producing forms from the relationships originally suggested by interpretable magnitudes occurs in the final step of symbolic development. This is where the author of the *Viga Ganita* and the young De Morgan were faced with negative numbers. From this step arose the need to extend the original meanings to encompass new entities, a separate process that De Morgan labeled “interpretation.”

The process of interpretation was so important that De Morgan devoted a separate *Penny Cyclopaedia* article to it. There he asserted that interpretation is needed when a “definition has been laid down, leading to results which cannot be explained by it [e.g.:  $a \cdot a = a^2$ ,  $a \cdot a \cdot a = a^3$ ; what is  $a^{-1}$ ?]: required the extension of the definition which will enable it to explain its own results.” It was important to keep the interpretative process a generalizing one, to “let the meaning of the intelligible results be such as will make the formulae of the [un]intelligible ones true of them.”<sup>33</sup> When successfully completed, the interpretative step firmly established the new results.

A critical aspect of this process lay in its historicity, the relationship of the new, wider interpretation to the old one. In the book *Trigonometry and Double Algebra*, where he synthesized many of the results of his four papers “On the Foundations of Algebra, I–IV,” De Morgan discusses this point explicitly. The problem he poses for himself is this: “Taking the rules of symbolic algebra [the field axioms he had laid out earlier], we . . . ask for an assignment of meaning to  $(-1)^{1/2}$  which would make all those rules true of it.” He goes on to note:

There may be many significant algebras in which this is done. But the demand made by common consent is, that our completely significant algebra shall be an *extension* of the defective system with which we commence: meaning, that so far as that system goes, significantly, it shall be a part of the new system. It would not help us, with reference to the mathematics now established, if fifty completely significant systems were produced, unless in one or more of them the same story were told as in the old algebra, so far as this last tells any story at all. We must have, if possible (and I am to show that it is possible), *all that we do understand still understood in the same sense*, with such enlargement of meaning as will give significance to symbols which we do not now understand [emphasis added].<sup>34</sup>

The process of interpretation De Morgan has here sketched for mathematics is strikingly like the one Whewell had propounded for the process of scientific discovery in general. The advance of science, Whewell claimed, resulted from the correction and enlargement of previous ideas. It was a progressive, generalizing process; advances did not negate previous results: “The great changes which . . . take place in the history of science, the revolutions of the intellectual world, have, as a usual and leading character, this, that they are steps of *generalization*;—transitions from particular truths to others of a wider extent, in which the former are included.” Continuity was a critical aspect of this generalizing process: “The

<sup>32</sup> *Ibid.*, pp. 131, 133.

<sup>33</sup> *Penny Cyclopaedia*, s.v. “Interpretation,” pp. 509–510. De Morgan has “intelligible.”

<sup>34</sup> Augustus De Morgan, *Trigonometry and Double Algebra* (London: Taylor, Walton & Maberly, 1849), p. 109.

earlier truths are not expelled but absorbed, not contradicted but extended; and the history of each science, which may . . . appear like a succession of revolutions, is, in reality, a series of developments."<sup>35</sup> Its outcome was scientific progress.

De Morgan's faith in the possibility of his program to find interpretation for algebraic forms was based on an essential faith in mathematical progress. He recognized that there was no internal, necessary reason why one would be able to find interpretations for meaningless algebraic forms. In the passage quoted earlier he qualifies his call for interpretation with the phrase "if possible," and his demonstration of its possibility rested simply on his having *done it*. He was convinced, however, that by constructively riding historical processes of discovery, mathematicians could be assured that, in time, interpretations would be found that would rigorously ground their results. In the meantime, as he said in "Negative and Impossible Quantities," "we may demand the use of those errors as demonstrated means of finding truth."<sup>36</sup>

Suspending judgment and relying on empty, symbolically generated forms in algebraic work was a powerful, but temporary, mode of procedure. De Morgan recommended relying on it in the teaching of algebra, where "the beginner [unfamiliar with the underlying ideas] is obliged to content himself with a less rigorous species of proof, though equally conclusive, as far as moral certainty is concerned."<sup>37</sup> The same powerful moral certainty was also available to the mathematical thinker who wrote the *Viga Ganita* and to the young De Morgan, whose faith in the validity of negative numbers had been so fully rewarded. Similarly, De Morgan wrote in 1840, early workers in the calculus of operations were able to proceed using their uninterpretable symbolic methods with a confidence "amounting to moral certainty, which indeed they were justified in doing."<sup>38</sup> In all of these cases the historical record showed that apparently blind symbolic manipulations would eventually find meaning in new, more general interpretations.

#### HISTORY, ALGEBRA, AND THE FOUNDATIONS OF CALCULUS

De Morgan's historical orientation, which was intimately connected with his conceptual view of mathematics, played an important role in his work throughout his life. It is particularly evident in his work in the calculus. Like most of his compatriots, De Morgan did not accept the formal view of the calculus that was developed by Augustin Cauchy and his followers in the 1820s and 1830s. At first, like the members of the Analytical Society, De Morgan had viewed the calculus as an essentially algebraic study whose foundations rested on the symbology of the Taylor series. Even after he abandoned this Lagrangian view in the mid 1830s and grounded the calculus in limits, however, De Morgan rejected Cauchy's attempt to construct a formal definitional structure adequate to support all of the calculus. De Morgan's resistance to Cauchy's approach is essentially entwined

<sup>35</sup> Whewell, *History of the Inductive Sciences* (cit. n. 22), Vol. I, pp. 9, 8.

<sup>36</sup> *Penny Cyclopaedia*, s.v. "Negative and Impossible Quantities," p. 131 (full passage quoted above callout for note 32).

<sup>37</sup> Augustus De Morgan, *On the Study and Difficulties of Mathematics* (London: Library of Useful Knowledge, 1836), p. 62.

<sup>38</sup> *Penny Cyclopaedia*, s.v. "Operation," p. 443.

with the conceptual and organic view of mathematics he felt had been revealed through its history. The practical mathematical implications of De Morgan's historical viewpoint are clearly manifested in his views of the calculus.

De Morgan's *Differential and Integral Calculus* was the first full-length British work to break from the Lagrangian interpretation of the calculus. In his preface to this work De Morgan self-consciously denounced that approach: "The method of Lagrange, founded on a very defective demonstration of the possibility of expanding  $\phi(x + h)$  in whole powers of  $h$ , had taken deep root in elementary works; it was the sacrifice of the clear and indubitable principle of limits to a phantom, the idea that algebra without limits was purer than one in which that notion was introduced."<sup>39</sup>

De Morgan's focus on the idea of limit as the foundation of the calculus appears to bring his approach to the foundations of calculus in line with the French analytic school of mathematicians led by Cauchy. Cauchy's approach to the calculus, powerfully presented in his *Cours d'analyse de l'Ecole Royale Polytechnique* in 1821, was also grounded on the limit. However, the importance of the Frenchman's work did not consist of this focus per se. There was a long eighteenth-century tradition of basing the calculus on this idea. Cauchy's originality lay, rather, in his attempt to define the term *limit* in such a way that the definition itself, as opposed to the concept it described, served as the foundation of the calculus. He tried to clarify the calculus by replacing the vagaries of conceptual meaning with unambiguous mathematical definitions. His work marked the beginning of a concerted effort to create a definitional structure that would fix the meaning of terms like *limit*, *continuity*, *convergence*, and *divergence* so unambiguously that the whole of legitimate calculus could be erected upon them. The delta-epsilon proof structure for testing limiting processes, which developed from the program Cauchy suggested in the *Cours*, epitomizes the precision he was trying to introduce in order to ground the calculus rigorously.<sup>40</sup>

In *Differential and Integral Calculus* De Morgan also based his calculus on the limit, but he did not adopt Cauchy's philosophical stance. Rather than beginning with a neat definition of the limit that would allow its subsequent use in logical proofs, De Morgan launched into a twenty-nine-page "Introductory Chapter" discussing the *idea* of the limit, attempting to show how the concept arose from considerations of number and magnitude. Before plunging into this discussion he explicitly laid out his reasons for believing that this elaboration was necessary:

Remembering the acknowledged difficulty of the subject, he [the student of the differential calculus] must be prepared to stop his course until he can form exact notions, acquire precise ideas. . . . To do this sufficiently . . . formal definitions would be useless; for he cannot be supposed to have one single notion in that precise form which would make it worth while to attach it to a word. One reason of the great difficulty which is found in treatises on this subject has always appeared to us to be the tacit assumption that nothing is necessary previously to actually embodying the terms and rules of the science, as if mere statements of definitions could give instantaneous power of using terms rightly.<sup>41</sup>

<sup>39</sup> Augustus De Morgan, *The Differential and Integral Calculus* (London: Baldwin & Cradock, 1842), p. iv. This work was published under the auspices of the Society for the Diffusion of Useful Knowledge and first appeared as a series of cheap pamphlets between 1836 and 1842.

<sup>40</sup> For a fuller discussion of Cauchy's work see Judith V. Grabiner, *The Origins of Cauchy's Rigorous Calculus* (Cambridge, Mass.: MIT Press, 1981).

<sup>41</sup> De Morgan, *Differential and Integral Calculus* (cit. n. 39), p. 6.

De Morgan's focus on conceptual clarity rather than definitional structure as the essence of mathematical understanding continued beyond this opening chapter. Nowhere in the pages that follow did he develop a precise definitional structure with which to construct the calculus. The following is a typical definition:

DEFINITION:—The function is said to have the value  $A$  when  $x$  has the value  $a$ , either when the common arithmetical sense of these phrases applies, or when by making  $x$  sufficiently near to  $a$  we can make the function as near as we please to  $A$ . In the first case  $A$  is simply called . . . an ordinary value of the function; in the second case  $A$  is called a *singular value*.<sup>42</sup>

Nowhere did he make the idea of “as near as we please to  $A$ ” more precise.

The conceptual way De Morgan reasoned throughout his text is illustrated by his discussion of the postulate that immediately follows this definition:

*Postulate 1.*—If  $\phi a$  be an ordinary value of  $\phi x$ , then  $h$  can always be taken so small that no singular value shall lie between  $\phi a$  and  $\phi(a + h)$  . . .

The truth of this statement, De Morgan asserted, is a “matter of observation. We always find singular values separated by an infinite number of ordinary values.”<sup>43</sup> Here, as throughout the book, De Morgan relied on conceptual understanding and observation rather than on definitionally constructed proofs to establish the truth of his postulate.

De Morgan's reliance on a conceptual understanding of the *subject matter* of calculus as the basis of its truth completely distinguished his approach from Cauchy's. The Englishman's calculus entailed the scientific description of a real, historically generated concept rather than the prescription of rules for generating internally consistent formal statements. For De Morgan the exactness of mathematical definitions, or their rigor, lay in the exact correlation of concept with definition. This contrasted starkly with Cauchy's approach, wherein rigor depended on the exactness with which the definitional structure could be used in proofs.

De Morgan's conceptual interpretation of the foundations of the calculus was intimately entwined with his sense of history. In part it derived from some specifics of the interpretation of scientific history he shared with Whewell. In his *History of the Inductive Sciences* Whewell had recognized the existence of two different kinds of historical epochs, each supporting a different form of scientific work. On one hand there were progressive “inductive epochs” when processes of conceptual generalization were being pursued. On the other hand there were “stationary periods” where “. . . the process which we have spoken of as essential to the formation of real science [induction in Whewell's conceptual interpretation] was interrupted; and in such cases, men employed themselves in reasoning from principles, and they arranged, and classified, and analyzed their ideas, so as to make their reasonings satisfy the requisitions of our rational faculties.”<sup>44</sup> Whewell's characterization of stationary periods is drawn from his view of the

<sup>42</sup> *Ibid.*, p. 44.

<sup>43</sup> *Ibid.*, pp. 44–45.

<sup>44</sup> Whewell, *History of the Inductive Sciences* (cit. n. 23), Vol. I, p. 12.

medieval period. However, the parallel between the “reasoning from principles” of Whewell’s stationary periods and Cauchy’s program of analytical mathematics is clear. For those, like De Morgan, who agreed with Whewell’s historical analysis and who aspired to keep the science of mathematics progressing, Cauchy’s formulation of foundations had little appeal.

The difference between De Morgan’s view of the calculus and that of Cauchy’s school was manifested on a different level as well. The implications of adopting one or the other of their interpretations of the basis of the calculus extended beyond the organization of past knowledge into decisions about how work should proceed in the future. Here, too, De Morgan had serious reservations about the work of Cauchy and his school. De Morgan feared that in their attempts to rationalize the calculus the French risked deflecting mathematicians from the strategies that had been successful in the past. Algebraic development had been the key to this success, and De Morgan was highly suspicious of developments that seemed to point mathematics in another direction.

The fundamental importance of De Morgan’s historically justified belief in algebra can be seen in a paper on divergent series which he directed against a pillar of the French analytic school, Siméon-Denis Poisson. In 1844 De Morgan challenged Poisson’s attempts to distinguish between legitimate forms of divergent series and those which were essentially ambiguous and untrustworthy. Poisson had made a distinction between “infinitely divergent series” like  $1 + 2 + 4 + 8 \dots$  and “finitely divergent series” like  $1 - 1 + 1 - 1 \dots$ . The first he rejected as meaningless; the second he accepted. For De Morgan this classification represented a dangerous break from history. In the eighteenth century, he noted, infinitely divergent series had been used, whereas finitely divergent ones were ignored or rejected.

Technically, the crux of De Morgan’s argument with Poisson lay in the treatment of definite integrals. Since the 1820s Cauchy had argued that these integrals were central. His precise definition of such integrals as sums, rather than as often elusive antiderivatives, was essential to his attempts to reformulate the foundations of the calculus.<sup>45</sup> This redefinition of definite integrals as sums was pivotal to Cauchy’s mathematical program but problematic for the use of divergent series. Defining the integrals in this way one might obtain, for example:

$$\int_0^{\infty} 2^x dx = \int_0^1 2^x dx + \int_1^2 2^x dx + \int_2^3 2^x dx + \dots$$

In this equation the left side,  $2^x dx$ , is infinite, whereas the sum on the right side is  $(\log 2)^{-1}(1 + 2 + 4 + \dots)$ . Therefore the series  $(1 + 2 + 4 + 8 + \dots)$  seems to be infinite. Hence, Poisson classified it as “infinitely divergent.”

This conclusion was problematic, however, because algebraic derivations suggested a different sum for the same series. Letting  $y = (1 + 2 + 4 + \dots)$ , simple algebraic transformation turns the series into the equation  $1 + 2y = y$ , which, in turn, leads to the conclusion  $y = -1$ . The same result can also be generated by using Peacock’s principle of equivalent forms. It was generally recognized that the series  $1 + x + x^2 + \dots$  was equal to  $1/(1 - x)$  for  $|x| < 1$ . Lifting

<sup>45</sup> For a treatment of Cauchy’s view of the definite integral see Judith V. Grabiner, “The Origins of Cauchy’s Theory of the Definite Integral,” in *The Origins of Cauchy’s Rigorous Calculus* (cit. n. 40), pp. 140–163.

this restriction on the value of  $x$  and allowing it to equal 2 also led to the conclusion that

$$1 + 2 + 4 + 8 \dots = -1.$$

Negative one was the value De Morgan defended as the sum of this series. His commitment to this solution was strong enough that he rejected Poisson's reliance on the definite integral defined as a sum. Throughout "On Divergent Series" De Morgan staunchly defended the vague eighteenth-century definition of the integral as an antiderivative, completely unaffected by the kind of foundational power Cauchy and his followers had been able to generate from their more precise definition. "I am prepared to contend that, when integration is not employed, there has not been produced one single instance in which divergency, properly treated, has led to error," he wrote.<sup>46</sup>

In his defense of an open-ended as opposed to unambiguously defined integral, the Englishman again demonstrated his commitment to a conceptual approach to mathematics wherein the fit of definition to concept rather than clear and tight internal structure was the key to mathematical exactness and rigor. In addition, though, he affirmed the importance of the creative patterns he saw revealed in mathematical history. The conceptual problems associated with divergent series were huge. That  $1 + 2 + 4 \dots$  should be equal to  $-1$  is certainly not conceptually transparent. Nevertheless, according to De Morgan, the legitimacy conferred on such series by their algebraic genesis far outweighed the problems they presented. He found Poisson's self-conscious break with this algebraic tradition to be unconscionable. He included the following quotation, liberally italicized, in his divergent series paper as an example of the Frenchman's reasoning:

On enseigne dans les élémens, qu'une série divergente ne peut servir à calculer la valeur approchée de la fonction dont elle résulte par le développement: mais *quelquefois* on a paru croire qu'une telle série peut être employée dans les calculs analytiques à la place de la fonction; et *quoique cette erreur soit loin d'être générale parmi les géomètres*, il n'est cependant pas inutile de la signaler, car les résultats auxquels on parvient par l'intermédiaire des séries divergentes, sont *toujours incertains et le plus souvent inexacts*.

Because of the unreliability of divergent series, Poisson concluded that they should be rejected from analysis. This rejection of mathematical forms that had been unexpectedly encountered in the course of algebraic manipulation shocked De Morgan. "I hardly know which of the passages in my Italics ought to excite more surprise," he wrote. "Divergent series, at the time Poisson wrote, had been nearly universally adopted for more than a century, and *it was only here and there* that a difficulty occurred in using them" (emphasis added).<sup>47</sup>

Rather than regarding such difficulties as marks of foundational flaws, De Morgan saw them as essential parts of progressive historical process. "We must admit" he wrote, "that many series are such as we cannot at present safely use, except as means of discovery, the results of which are to be subsequently verified." "But," he continued, "to say that what we cannot use no others ever can, to refuse that faith in the future prospects of algebra which has already realised

<sup>46</sup> Augustus De Morgan, "On Divergent Series and Various Points of Analysis Connected with Them," *Trans. Cambridge Phil. Soc.*, 1844, 8:182–203, on p. 184.

<sup>47</sup> *Ibid.*, pp. 183–184 (emphasis added by De Morgan), 184 (emphasis added by me).

so brilliant a harvest, . . . seems to me a departure from all rules of prudence. The motto which I should adopt against a course which seems to me calculated to stop the progress of discovery would be contained in a word and a symbol—remember  $\sqrt{-1}$ .<sup>48</sup>

The same argument is elaborated in the “Series” article for the *Penny Cyclopaedia*, where De Morgan again defended the use of algebraically generated divergent series. Here he explicitly developed the parallels between the history of divergent series and that of negative and imaginary numbers:

The divergent series, that is, the equality between it and a finite expression, is perfectly incomprehensible in an arithmetical point of view; so was the impossible quantity. The use of divergent series has been admitted, by one on one explanation, and by another on another, almost ever since the commencement of modern algebra; and so it was with the impossible quantity. It became notorious that such use generally led to true results, with now and then an apparent exception, which most frequently ceased to be such on further consideration; this is well known to have happened with impossible quantities. In both cases these apparent exceptions led some to deny the validity of the method which gave rise to them, while all were obliged to place them both among those parts of mathematics (once more extensive than now) in which the power of producing results had outrun that of interpreting them.

In the case of impossible numbers, De Morgan continued, a complete explanation became available upon the realization that the definitions on which the symbolic system had been erected had been too narrow. “Why,” he lamented, “should the divergent series, of all the results of algebra which demand interpretation, be the only one to be thrown away without further inquiry, when in every other case patience and research have brought light out of darkness?”<sup>49</sup>

In this argument De Morgan was countering a prescriptive, analytical view of mathematical foundations with a historical, conceptual one. The issue, for him, had little to do with the strength or weakness of the specific definitional structure that Cauchy, Poisson, and their followers were trying to develop. Rather than responding on the Frenchman’s timeless, logical terms, De Morgan approached their work as a self-consciously historical figure who was willing to accept present ambiguities in the hope of future rewards. He felt that divergent series had been generated in the course of legitimate explorations of algebra. This historical fact forced the conscientious mathematician to try to understand them.

De Morgan did not simply preach patience in the face of problems of interpretation that were generated from legitimate mathematical development. On the contrary, he actively searched for clear, conceptual interpretations that would resolve what he saw as the unsolved problems of mathematics. This orientation explains some of his later work that otherwise seems virtually incomprehensible. In 1864, for example, De Morgan explored the meaning of the equals sign in a paper entitled “On Infinity; and on the Sign of Equality.”<sup>50</sup> De Morgan’s discussion in this paper derived from the specific equation  $2 \times 0 = 0$ . This equation can be generalized into the symbolic form  $2x = x$ . Following the basic rules of algebraic combination this general equation can be manipulated to give  $2x/x =$

<sup>48</sup> *Ibid.*, pp. 183.

<sup>49</sup> *Penny Cyclopaedia*, s.v. “Series,” p. 264.

<sup>50</sup> Augustus De Morgan, “On Infinity; and on the Sign of Equality,” *Trans. Cambridge Phil. Soc.*, 1871, 11:145–189.

$x/x$  or  $2 = 1$ , a conclusion that is clearly impossible with the usual interpretation of the meaning of the equals sign. De Morgan devoted his energies in the 1864 paper to developing a new interpretation for the equals sign that would both incorporate its former meaning and make this equation intelligible.

At first glance De Morgan's problem seems merely absurd; any high school mathematics student knows to beware of generalizing results reached through the special powers of zero. But this restriction is analogous to the one that the young De Morgan had learned was inappropriate in his considerations of equations like  $(a^2 - b^2) = (a + b)(a - b)$ . The lesson De Morgan had drawn from his earlier experience emphasized the power of the results that could be obtained if such symbolic forms were accepted without restrictive prejudice. Difficult though understanding them might be, they should be embraced as hints about rich lodes of knowledge, pushed up by the historical process.

De Morgan's paper on equality and others like it are striking expressions of his lifelong determination to resist attempts to tamper with historical processes by those who would rationally control and define mathematical development. In 1865, he wrote to J. S. Mill: "With regard to the acceptance of . . . [Peacock's] system, the time is not yet come. The algebraists almost all make algebra obey their preconceived notions. . . . So long as an algebraist has preconceptions which his science must obey, so long is he incapable of true generalization." For De Morgan, restrictive axiomatic systems were the embodiment of preconceptions that turned mathematical attention away from the central problems, like generalizing the equals sign. Such true generalization was what Peacock's principle of equivalent forms had promised, it was the direction wherein mathematical progress lay, and it remained De Morgan's goal throughout his life. In the same letter to Mill he noted: "It is my taste, if you please, that I *will have* a formal algebra, in which every form, every law of transformation is *universal*."<sup>51</sup> By this he meant that every symbolic form thrown up by the manipulation of mathematical signs could be given a meaning that would render the whole structure comprehensible. Although he recognized that the possibility of realizing such an interpretation was not logically necessary, his belief in the power of historical forces lent his efforts a "moral certainty."

#### CONCLUSION

De Morgan's letter to Mill hints that by the end of his life the mathematician felt quite alone in his pursuit of the mathematical ideal of total algebraic interpretation he had gleaned from his understanding of its history. As in his speech of the same year to the London Mathematical Society, De Morgan seems to have felt beleaguered in his defense of a historically legitimated, conceptual view of algebra.

This tone of somewhat frustrated isolation can be attributed partly to the increasing cantankerousness of De Morgan's old age, which led him to cut almost all of his ties—including those to University College—in the final years of his life. With respect to his interests in algebra, though, De Morgan's sense of alienation also had an element of truth. By the 1860s his mathematical colleagues were of a

<sup>51</sup> Quoted in Sophia De Morgan, *Memoir of Augustus De Morgan* (London: Longmans, Green, 1882), pp. 328, 329.

younger generation that, for the most part, had been educated after the excitement over symbolical algebraic development in which he had been so involved. As more and more people became accustomed to trusting algebra's powers in a variety of fields, theoretical interest in its foundations waned. The centrality of interpretation to the truth of an algebraic form continued to be asserted in the educational context. In practice, however, to use De Morgan's terminology, algebra became more of an art than a science.<sup>52</sup> The power of its symbology began to be used more as a road to knowledge about other things, like physics, than as more or less disjointed clues to the reality of a single, still elusive, underlying concept. The very variety of its successful applications undermined the assurance that a single one could tie them all together.

However, although by the end of his life De Morgan's work in algebra was idiosyncratic, the historical spirit behind it was not. The progressive orientation De Morgan had introduced into his historical view of mathematics continued to manifest itself strongly in British work, although in geometry more than in algebra. Projective geometry, in particular, fit beautifully into the picture of a progressive mathematical field that De Morgan had painted. In this new approach to geometry, which became a central feature of the British mathematical scene after the 1860s, a new generation of British mathematicians found the same combination of historical continuity and promised progress that had earlier captivated De Morgan in algebra. Like De Morgan's algebra, projective geometry held out tantalizing conceptual challenges in the form of its uninterpreted imaginary points and points at infinity. For many its success was grounded on a "principle of continuity" that played a role analogous to the one Peacock's "principle of equivalent forms" had played in algebra. The historical development of projective ideas was routinely cited as a legitimating factor for poorly defined but effective geometrical practices. The optimism of De Morgan's historically grounded "moral certainty" lived on in the work of his countrymen, who continued to rejoice in their conceptual freedoms, undaunted by the logical strictures that concerned their counterparts on the Continent.<sup>53</sup>

In important ways, then, De Morgan's immediate legacy to his countrymen was not merely the legacy of strict logic and axiomatics with which current mathematical historians tend to associate his name. Equally important was a historical legacy that allowed a younger generation to agree with him that such things were often only marginally relevant to mathematical progress.

<sup>52</sup> For a further consideration of the terms *art* and *science* as De Morgan used them see Richards, "Art and Science of British Algebra" (cit. n. 3).

<sup>53</sup> For a fuller discussion of this approach to projective geometry see Joan L. Richards, "Projective Geometry and Mathematical Progress in Mid-Victorian Britain," *Studies in the History and Philosophy of Science*, 1986, 17:297-325.