The Calculus of Operations and the Rise of Abstract Algebra

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Communicated by C. B. BOYER & C. TRUESDELL

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1. Introduction

During the eighteenth century, mathematics in Great Britain was quiescent. Cambridge University, home of one of the founders of calculus, ISAAC NEWTON, had sunk so low that, as CARL BOYER has stated, "Cambridge University in the earliest years of the nineteenth century was scarcely the place to which one would have looked for new developments in mathematics."[1] One of the probable causes of this condition was the isolation of the English mathematicians from their fellow workers on the Continent. While modern calculus was being created by the BERNOULLIS, EULER, LAGRANGE and LAPLACE, the NEWTONIAN school clung to a clumsy notation and, perhaps even more important, to a reliance on geometric methods out of a misguided belief that these represented the spirit of NEWTON. They remained unaware that the true heirs of NEWTON were those who took the differential notation and methods of LEIBNIZ and applied them to the NEWTONIAN cosmology. The separation was not complete, but unfortunately when the English used Continental writings, they combined the worst of each system. For instance, the text by GUILLAUME DE L'HOSPITAL, *Analyse des infiniments petits* (1696) was translated into English in 1730 by EDMUND STONE and in 1801 a translation by JOHN COLSON of the *Istituzioni analitiche ad uso della gioventù Italiana* (1748) by MARIA AGNESI appeared. In both works the notation of fluxions is grafted onto the infinitesimals of LEIBNIZ. The result is a mixture of English symbols and phraseology with Continental concepts, which forms, as FLORIAN CAJORI has said, "a system, destitute of scientific interest."[2]

The isolation is usually attributed to the bitterness engendered by the NEWTON-LEIBNIZ priority controversy, which left the English with the conviction that

[1] CARL B. BOYER, *A History of Mathematics*. New York, 1968: 620–621.

[2] FLORIAN CAJORI, *A History of the Conceptions of Limits and Fluxions from Newton to Woodhouse*. Chicago, 1919: 254.

to abandon the notation and methods of NEWTON would be an insult to his memory. As DIRK STRUIK has asserted, "Until well into the nineteenth century, the Cambridge and Oxford dons regarded any attempt at improvement in the theory of fluxions as an impious revolt against the sacred memory of NEWTON."[3] STRUIK suggests an additional reason for the insularity of the English. During much of the eighteenth century, England was at war with France, and her victories in war and in trade, as well as the admiration of Continental philosophers for her political system, fostered an intellectual arrogance which made the English blind to the possibility of learning anything from abroad. It should also be noted, in partial defense of the English, that due to the wars, foreign books were difficult to procure and very expensive when available. On the other hand, the isolation was peculiarly English. The Scottish Universities remained in contact with scientific developments on the Continent. And the only British mathematician of note to use analytic methods during the eighteenth century was Sir JAMES IVORY, a Scotsman.

Whatever the reason, isolation is the most conspicuous feature of the English school of mathematics during the eighteenth century. Change occurred at the beginning of the nineteenth century through the efforts of ROBERT WOODHOUSE, GEORGE PEACOCK, CHARLES BABBAGE and JOHN HERSCHEL. These men were all connected with Cambridge University, and aimed their efforts primarily at the reform of the method of teaching mathematics at that school. By 1830 they had succeeded in securing the adoption of the differential notation and analytic methods. As the reformers had hoped, there followed a remarkable renaissance in English mathematics. The 1830's through the 1860's not only saw the emergence of a new concept of algebra in the works of GEORGE PEACOCK, DUNCAN GREGORY, AUGUSTUS DEMORGAN and GEORGE BOOLE, but also the appearance of a brilliant school of mathematical physicists, including GEORGE GREEN, G. G. STOKES, Lord KELVIN and JAMES CLERK MAXWELL.

The interest in this paper is in the first group. For it seems curious that following the introduction of the differential notation the first important English contributions to mathematics were made in algebra rather than in analysis. It is my claim that this was not merely a coincidence, but that the work in algebra was a direct response of the English to a specific aspect of the work of Continental analysts which became accessible to them. This subject came to be called, by the English, the calculus of operations. It was related to the analogy between repeated operations and the law of exponents: that is, the equations

$$x^n \cdot x^m = x^{n+m}, \qquad \frac{d^n}{dx^n}\left(\frac{d^m u}{dx^m}\right) = \frac{d^{n+m} u}{dx^{n+m}}, \qquad f^n\big(f^m(x)\big) = f^{n+m}(x). \tag{1.1}$$

The symbols of operation are then manipulated, often apart from the symbols they operate on, as if they were magnitudes, with multiplication replaced by iteration. For example, based on the similarity between the two expansions

$$\begin{aligned} e^{h\frac{du}{dx}} - 1 &= h\frac{du}{dx} + \frac{1}{1\cdot 2} h^2\left(\frac{du}{dx}\right)^2 + \frac{1}{1\cdot 2\cdot 3} h^3\left(\frac{du}{dx}\right)^3 + \cdots \\ \Delta u = u(x+h) - u(x) &= h\frac{du}{dx} + \frac{1}{1\cdot 2} h^2\frac{d^2u}{dx^2} + \frac{1}{1\cdot 2\cdot 3} h^3\frac{d^3u}{dx^3} + \cdots \end{aligned} \tag{1.2}$$

[3] DIRK STRUIK, *A Concise History of Mathematics*. 3rd. ed., rev. New York, 1967: 168.

it was concluded that

$$\Delta u = \left(e^{h \frac{d}{dx}} - 1\right) u \tag{1.3}$$

and finally that

$$\Delta^n u = \left(e^{h \frac{d}{dx}} - 1\right)^n u, \tag{1.4}$$

the latter expression being expanded by the binomial theorem, with multiplication replaced by iteration.

I might point out that this result can be stated in fluxional notation, and did in fact first appear in English literature in that guise. But in that form it is exceedingly cumbersome, and the calculus of operations leans heavily on notation for its plausibility and usefulness. The formula (4) appeared first in the writings of JOSEPH LAGRANGE, and was discussed at length by SIMON LAPLACE. In fact, the English found not only the tools for the calculus of operations, but many of the basic theorems and even the basis of the theoretical explanation in the writings of such men as LOUIS ARBOGAST, B. BRISSON, J. F. FRANÇAIS, AUGUSTIN CAUCHY, ANTON-MARIO LORGNA and F. J. SERVOIS. The French themselves, however, never really trusted the method, and certainly they did not relate it to a theory of algebra.

The British, on the other hand, not only used the method extensively; they considered the concept of operation to be a unifying theme in mathematics, and one of the utmost interest. The writings of BABBAGE, HERSCHEL, and to a somewhat lesser extent, PEACOCK all testify to their high regard for the importance of the calculus of operations. GREGORY, DEMORGAN, BOOLE and HAMILTON all did research in the subject. Furthermore, especially in the case of GREGORY and BOOLE, the relationship between these efforts and their innovations in algebra and logic is clear. For in their attempts to put the calculus of operations on a firm logical basis the English mathematicians were led to the notion that it was not the nature of the objects under consideration which was most significant, but rather the laws of combination of their symbols. They were then led to study abstract systems which could be interpreted either as magnitudes, or as symbols of operation, or, in the case of BOOLE, of logic. Clearly this represents a step towards an abstract approach to mathematics. This interpretation yields a much more unified picture of the developments in Britain during the first part of the nineteenth century. For we now see the work of the Continental analysts acting not simply as a catalyst through the introduction of the differential notation and analytic methods into England but also as a point of departure for the foundation of an abstract algebra. It also shows that BABBAGE and HERSCHEL were not simply the vehicles for that introduction, but that in their own mathematical research they anticipated much of what their followers were to find important.

2. Origins of the Calculus of Operations

The calculus of operations can be traced to the analogy between the raising of a sum to a power and the differential of a product. In a letter to JOHANN BERNOULLI, dated 1695, LEIBNIZ noted the analogy between "numeros potestatum a binomio, et differtiarum rectanular, ...,"[4] that is, using his notation, between

$$\overline{1|x+y} = 1\,x + 1\,y = 1\,x^1y^0 + 1\,x^0y^1 \quad dxy = 1\,y\,dx + 1\,x\,dy = 1\,d^1xd^0y + 1\,d^0xd^1y;$$

$$\overline{2|x+y} = 1\,xx + 2xy + 1\,yy \qquad d^2xy = 1\,y\,ddx + 2dy\,dx + 1\,xd\,dy$$

where, he noted that the analogy becomes clearer if we realize that xx and $y\,ddx$ can be written as $y^0\,xx$ and $d^0\,y\,ddx$, respectively. LEIBNIZ then went on to state what is now known as LEIBNIZ' Rule: to obtain the expression for $d^n\,xy$, consider the binomial expansion $(x+y)^n$ and change $x^n\,y^m$ into $d^n\,xd^m\,y$. Furthermore, he added, this result holds if n is negative, using $d^{-1} = \int$, and for trinomials. LEIBNIZ published his observation in a memoir entitled "Symbolismus memorablis calculi algebraici et infinitesimalis in comparatione potentiarum et differtiarum, et de lege homogeneorum transcendentali," which appeared in 1710. In this work the analogy is made striking by LEIBNIZ' use of the notation $p^e\,x$ and $p^e(x+y)$ for x^e and $(x+y)^e$.[5] In a letter written to JOHN WALLIS in 1697, LEIBNIZ used as an argument for the advantage of his method over that of Newton the fact that it brought that analogy into prominence.[6] JOHANN BERNOULLI made use of it in order to express an integral as an infinite series.[7] In a letter to LEIBNIZ he noted that if we assume that $\int n\,dz$ should be the mean proportional between $d^0(n\,dz)$ and $d(n\,dz)$, then we have

$$\int n\,dz = \frac{d^0n\,dz}{d^0nd\,dz + dn\,dz} \tag{2.1}$$

and hence, by dividing

$$\begin{aligned}\int n\,dz &= d^0n\,d^0z - dn\,d^{-1}z + d^2n\,d^{-2}z - d^3n\,d^{-3}z \quad \text{etc.}\\ &= nz - dn\int z + d^2n\int^2 z - d^3n\int^3 z \quad \text{etc.}\end{aligned} \tag{2.2}$$

One outgrowth of this formal aspect of LEIBNIZ' work was the combinatorial analysis of C. F. HINDENBURG. This concerns itself with methods of computing the coefficients in various types of expansions. But the type of formalism which led to the calculus of operations, though it too is concerned with that question, is slightly different. And this is found next in a paper, dated 1772, by JOSEPH LAGRANGE, entitled "Sur une nouvelle espèce de calcul relatif à la différentiation et à l'intégration des quantités variables." LAGRANGE referred to LEIBNIZ and BERNOULLI as the sources of his ideas. In this memoir he was concerned with the problem of expressing a finite difference as a series of terms involving differentials. That is, letting u be a function of $x, y, z, t, \ldots$ LAGRANGE studied the expansion of

$$\Delta u = u(x+\xi, y+\psi, z+\zeta, t+\theta, \ldots) - u(x, y, z, t, \ldots) \tag{2.3}$$

in powers of $\xi, \psi, \zeta, \theta, \ldots$ where these are given constants. First he noted that the expansion of $u(x+\xi, y+\psi, z+\zeta, t+\theta, \ldots)$ in powers of $\xi, \psi, \zeta, \theta, \ldots$ is given by

$$\frac{M\,\xi^\mu\,\psi^\nu\,\zeta^\omega\,\theta^\varrho\ldots}{1\cdot2\cdot3\cdot\ldots\cdot(\mu+\nu+\omega+\varrho)\ldots}\cdot\frac{d^{\mu+\nu+\omega+\varrho+\cdots}}{dx^\mu\,dy^\nu\,dz^\omega\,dt^\varrho\ldots} \tag{2.4}$$

[4] GOTTFRIED WILHELM LEIBNIZ, *Mathematische Schriften*. ed. C. I. GERHARDT. 7 vols. 1849–1863. Reprint. Hildesheim, 1962: vol. 3/1, 175.

[5] *Ibid.*, vol. 5, 377–382.

[6] *Ibid.*, vol. 4, 25.

[7] *Ibid.*, vol. 3/1, 199.

where M is the coefficient of the term $x^\mu y^\nu z^\omega t^\varrho \ldots$ in the polynomial

$$(x+y+z+t+\cdots)^{\mu+\nu+\omega+\varrho+\cdots}.$$

Thus, LAGRANGE said, in order to find the desired expansion it suffices to consider the series

$$\frac{(x+y+z+t+\cdots)^1}{1}+\frac{(x+y+z+t+\cdots)^2}{1\cdot 2}+\frac{(x+y+z+t+\cdots)^3}{1\cdot 2\cdot 3}+\cdots \tag{2.5}$$

and after developing it change x into $\frac{\xi}{dx}$, y into $\frac{\psi}{dy}$, z into $\frac{\zeta}{dz}$, t into $\frac{\theta}{dt}$, ... and then multiply each term by $d^\lambda u$ where λ is the sum of the powers. And he continued, this expression can be simplified still further by noting that the series (5) is equal to

$$e^{x+y+z+t+\cdots}-1. \tag{2.6}$$

From this remark, LAGRANGE derived one of the basic theorems in the calculus of operations. He wrote[8]

> De là il est facile de conclure que si l'on considère l'expression
>
> $$e^{\frac{du}{dx}\xi+\frac{du}{dy}\psi+\frac{du}{dz}\zeta+\cdots}-1$$
>
> et qu'après l'avoir développée suivant les puissances de du, on applique les exposans de ces puissances à la caractéristique d pour indiquer des différences du même ordre que les puissances, c'est à dire qu'on change du^λ en $d^\lambda u$, on aura l'accroissement cherché de la fonction u lorsque $x, y, z, \ldots$ y deviennent $x+\xi, y+\psi, z+\zeta \ldots$

Or,

$$\Delta u=e^{\frac{du}{dx}\xi+\frac{du}{dy}\psi+\frac{du}{dz}\zeta+\cdots}-1. \tag{2.7}$$

And by repeating this process

$$\Delta^\lambda u=\left(e^{\frac{du}{dx}\xi+\frac{du}{dy}\psi+\frac{dz}{du}\zeta+\cdots}-1\right)^\lambda, \tag{2.8}$$

where the terms are computed by the convention given above if λ is a positive integer and where

$$d^{-1}=\int,\ d^{-2}=\int^2\ldots\ \Delta^{-1}=\Sigma,\ \Delta^{-2}=\Sigma^2\ldots \tag{2.9}$$

if λ is negative. By studying the expansion of $(e^\omega-1)^\lambda$, where ω is any quantity and using the notion of switching from powers to iterates, LAGRANGE obtained several theorems of the same type. LAGRANGE realized that he had not proved his results. But, he said, the analogy between powers and indices of differentiation had led him to discover many new theorems which would have been "très difficile

[8] JOSEPH LAGRANGE, "Sur une nouvelle espèce de calcul relatif à la différentiation et à l'intégration des quantités variables," 1772, *Oeuvres*: vol. 3, 441.

de parvenir par d'autres voies."[9] And, he added, it was in all cases possible to supply proofs of the theorems, though he gave no details.

The first published proofs of LAGRANGE's theorems were given by PIERRE SIMON LAPLACE in 1776. He considered the case of a single variable and based his proof on the observation that in the development

$$\Delta^\lambda u = \frac{d^\lambda u}{d x^\lambda} h^\lambda + A' \frac{d^{\lambda+1} u}{d x^{\lambda+1}} + A'' \frac{d^{\lambda+2} u}{d x^{\lambda+2}} + \cdots \tag{2.10}$$

the coefficients $A', A'', \ldots$ are independent of the function u and depend only on λ. Thus they can be determined by choosing a particular function u; and letting $u = e^x$, he said, one obtains the result of LAGRANGE.[10] LAPLACE returned to this question in 1780, giving another proof and deriving several corollaries. In this paper which dealt with the theory of generating functions, LAPLACE freely used the idea of switching from powers to indices of differentiation.[11]

The next step towards a calculus of operations is found in the work of ANTON-MARIO LORGNA. In a memoir "Théorie d'une nouvelle espèce de calcul fini et infinitésimal," published in 1787, LORGNA, who was professor of mathematics at Verona and a correspondent of LAGRANGE, attempted to justify and extend theorems based on the analogy between exponentiation and iteration. He asserted that what was involved was the consideration of symbols of operation as if they were algebraic quantities. That is, he wrote,[12]

> La nouvelle espèce de calcul dont il est question dans cette Mémoire, exige que les caractéristiques Δ, d, Σ, $\int$ dont on se sert dans les calculs ordinaires fini & infinitésimal soient considerées sous deux différents aspects, c'est à dire, tantôt commes les signes représentatif destinés à marquer les états variés des grandeurs avec lesquelles ils se trouvent préfigés, tantôt comme des quantités algébriques.

That is, LORGNA asserted, if y is a function and y' is a "successivement variées," that is, a difference or differential, one could treat repeated variations, which he denoted by $y^{\lambda'}$ as if the indices were ordinary integers, and then apply the primes, *i.e.*, in any true statement one could go through and replace $(y^{\lambda'})^n$ by $y^{(\lambda n)'}$, and the result would be a true statement. The central tenet of his calculus was thus expressed as[13]

> if faut faire abstraction, pour le moment de l'opération, de tous signes de variation, & regarder les nombres symboliques comme des nombres absolus.

Although LORGNA stated clearly what was to be a basic idea in the calculus of operations, namely the treatment of symbols of operation as if they were

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[9] *Ibid.*, 442.

[10] PIERRE SIMON LAPLACE, "Mémoire sur l'inclinasion moyenne des orbites des comètes, sur la figure de la terre et sur les fonctions," 1776, *Oeuvres*: vol. 8, 314–321.

[11] PIERRE SIMON LAPLACE, "Mémoire sur l'usage du calcul aux différences partielles dans la théorie des suites," 1780, *Oeuvres*: vol. 9, 313–380.

[12] ANTON-MARIO LORGNA, Théorie d'une nouvelle espèce de calcul fini et infinitésimal," *Mém. de l'Acad Roy. de Turin*, 1786–1787, **8**: 411.

[13] *Ibid.*, 418.

algebraic entities, his own applications of the idea are obscure. And he gave no rules as to when this might be done nor why it was possible. Furthermore, LORGNA'S work does not seem to have become widely known, either on the Continent or in England and is rarely mentioned in later works in the field.

A full scale development of the calculus of operations did not occur until LOUIS FRANÇOIS ARBOGAST introduced the idea of the separation of symbols. ARBOGAST, professor of mathematics at Strasbourg, published a book *Du calcul des derivations* in 1800. In this work ARBOGAST set out to furnish a new type of calculus which would contain as a special case the differential calculus. It is essentially a method for determining the coefficient of x^n in the expansion of $\varphi(a+bx+cx^2+\cdots)$ for any function φ. In this respect it is highly combinatorial and recalls the work of the German school. But the work also contains two major contributions to the calculus of operations. First of all, he emphasized the concept of operation—a concept he felt was fundamental to all of mathematics. His work, ARBOGAST wrote, was based on a general way of considering quantities as being derived from one another. And, he added, anticipating an attitude we will find important to the English algebraists:[14]

> les derivées que je considère sont moins des derivées de quantités que des derivées d'opérations, comme l'Algèbre est moins un calcul de quantités que d'opérations arithmétiques ou géométriques a exécuter sur les quantités.

In addition to these general considerations, ARBOGAST made an important specific advance towards the calculus of operations in the form of the method of the separation of symbols, or, as he termed it, the "méthode de séparation des échelle d'opérations." In this method the symbols of operation, combined with constants, are treated as single entities and as they would be if they were symbols of quantity. The resulting expression is then applied to the original function. As ARBOGAST described it:[15]

> cette méthode consiste à détacher de la fonction des variables, lorsque cela est possible, les signes d'opération qui affectent cette fonction, et à traiter l'expression formée de ces signes mêlés avec des quantités quelconques, expression que j'ai nommée échelle d'opérations à la traiter, dis-je, tout de même que si les signes d'opérations qui y entrent étoient des quantités; puis à multiplier le résultat par la fonction.

As an example of his work, consider ARBOGAST'S treatment of LAGRANGE'S theorem (8) for functions of a single variable. ARBOGAST expressed this theorem in the following form:

$$\Delta^n u = \left\{ e^{\frac{du}{dx}\xi} - 1 \right\}^n \tag{2.11}$$

where it is necessary to expand the left hand side in ascending powers of $\frac{du}{dx}\xi$ and then change $(du)^r$ into $d^r u$. The statement of this theorem and its proof,

[14] LOUIS FRANÇOIS ARBOGAST, *Du Calcul des Dérivations*. Strasbourg. 1800: i.
[15] *Ibid.*, vii–ix.

ARBOGAST continued, can be simplified considerably by avoiding the necessity of having to change from du^r to $d^r u$. This he achieved by writing $\delta u = \frac{du}{dx}$ and "détachant l'échelle de la fonction." Thus he wrote

$$\begin{aligned} (1+\Delta)\, u &= \left(1+\xi\delta+\frac{1}{1\cdot 2}\xi^2\delta^2+\frac{1}{1\cdot 2\cdot 3}\xi^3\delta^3+\cdots\right) xu \\ (1+\Delta)\, u &= e^{\xi\delta}\, xu \\ \Delta\, u &= (e^{\xi\delta}-1)\, xu \\ \Delta^n\, u &= (e^{\xi\delta}-1)^n\, xu. \end{aligned} \tag{2.12}$$

And, he noted "il n'y a rien à changer après le développement."[16]

The essence of his method is contained in the fact that he also expressed the second equation in the form

$$1+\Delta = e^{\xi\delta} \tag{2.13}$$

and then concluded that

$$F(1+\Delta)\, xu = F(e^{\xi\delta})\, xu \tag{2.14}$$

for any function F, where F applies only to the operations.[16] Here the function F was to be expanded in a power series. Clearly, by his consideration of the symbols of operation apart from the subjects on which they operate, manipulating them as if they were algebraic quantities, ARBOGAST was working in the calculus of operations. Furthermore, he applied his theory to the solution of differential equations. In this he had many followers, some building on his work, while others came to the same general method independently.

One of the first who did so, apparently without knowledge of ARBOGAST'S work, was BARNABÀ BRISSON. BRISSON, a graduate of the Ecole Polytechnique, developed a method, published in 1808, of solving linear partial differential equations with constant coefficients which was based on the observation that if A and B are constants, then

$$A\,\frac{d^n\left(B\,\frac{d^m z}{dy^m}\right)}{dx^n} = AB\,\frac{d^{n+m} z}{dx^n\, dy^m}, \tag{2.15}$$

a result which is also obtained if the indices denote powers and the terms are multiplied. This result BRISSON said, generalizes to any expression containing differentials. He applied this observation to equations of the form

$$Az + B\frac{dz}{dx} + C\frac{dz}{dy} + \cdots + G\frac{d^2 z}{dx^2} + H\frac{d^2 z}{dx\,dy} + I\frac{d^2 z}{dy^2} + \cdots = 0, \tag{2.16}$$

which he denoted by $\nabla z = 0$. Writing $\nabla' z = 0$ for the corresponding algebraic equation in which iteration is replaced by multiplication, *i.e.*, $\frac{d^2 z}{dy\,dz}$ is replaced by $\frac{dz}{dy}\cdot\frac{dz}{dx}$, he factored this, and from $\nabla' z = \delta_0'\,\delta_1' \ldots \delta_{n-1}'\, z$ he obtained $\nabla z = \delta_0\,\delta_1 \ldots \delta_{n-1}\, z$, where δ_i is obtained from δ_i' by replacing the powers by

[16] *Ibid.*, 350.

iterations. BRISSON then solved his equation by noting that since the terms $\delta_0, \delta_1, \dots \delta_{n-1}$ can be written in any order $\delta_i z = 0$ implies $\nabla z = 0$. He went on to show that $\delta_i z = 0$ could generally be solved when the δ_i are linear.[17] BRISSON anticipated many later attacks on the same problem, but his work relied on switching back and forth between powers and indices of operation which made it awkward and imprecise. BRISSON carried many of his ideas further, but his later memoirs were not published. However, they were used as a starting point by CAUCHY for his own work in this area and will be considered later.

JACQUES-FRÉDERIC FRANÇAIS spoke admiringly of BRISSON's work, though he criticized it for lack of rigor. In a paper published in the 1812–13 volume of the *Annales de mathématiques* FRANÇAIS included among those who had made use of the analogy between powers and indices of operation LAGRANGE and BRISSON. However, he most admired the work of ARBOGAST, who, he claimed, had first stated the proper way in which the analogy should be used. Thus, FRANÇAIS wrote,[18]

> Arbogast est le premier qui se soit proposé de débarrasser cette methode des inconvéniens qu'entraîne le passage alternatif des indices aux exposans, et des exposans aux indices. L'idée heureuse qu'il a eu de détacher les caractéristiques ou *échelles d'opérations* des fonctions qu'elles affectent, pour les traiter comme des symboles de quantités, remplit parfaitement le but qu'il s'est proposé.

FRANÇAIS used the method to solve linear differential equations with constant coefficients and he also considered finite difference and partial differential equations. His early work on this subject was criticized as being not rigorous, and FRANÇAIS admitted, was rejected by the Institute (as the Academy was known at that time) on that ground. The following example giving the way in which he solved the first degree linear equation shows why. Following ARBOGAST, he wrote $\delta\varphi$ for $\frac{d\varphi}{dx}$ and $E\varphi$ for $\varphi(x+1)$. Then from

$$\begin{aligned} E\,\varphi(x) &= \varphi(x+1) = \varphi x + \delta\,\varphi(x) + \frac{1}{1\cdot 2}\,\delta^2\,\varphi(x) + \cdots \\ &= e^{\delta\,\varphi(x)}, \end{aligned} \tag{2.17}$$

by "détachant les échelles" FRANÇAIS concluded

$$E = e^{\delta}. \tag{2.18}$$

Expressing his equation in the form

$$(\delta - a)\;\varphi(x) = 0 \tag{2.19}$$

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[17] BARNABÀ BRISSON, "Mémoire sur l'intégration des équations différentielles partielles," *J. de l'Ecole Poly.*, 1808, **7**: 191–261.

[18] JACQUES-FRÉDERIC FRANÇAIS, "Mémoire rendant à démontrer la légitimité de la séparation des échelles de différentiation et d'intégration des fonctions qu'elles affectent; avec des applications à l'intégration d'une classe nombreuse d'équations," *Ann. des Math. pures et app.*, 1812–1813, **3**: 244.

and again "détachant les échelles," FRANÇAIS obtained

$$\begin{aligned} \delta - a &= 0 \\ \delta &= a \\ e^{\delta} &= e^{a}. \end{aligned} \tag{2.20}$$

But then by (18) and (20)

$$\begin{aligned} E &= e^{a} \\ E^{k} &= e^{ak} \\ 1 &= e^{ak}\, E^{-k} \end{aligned} \tag{2.21}$$

for any k. Multiplying by $\varphi(x)$, and then letting $x=k$

$$\begin{aligned} \varphi(x) &= e^{ak}\, E^{-k}\, \varphi(x) = e^{ak}\, \varphi(x-k) \\ \varphi(k) &= e^{ak}\, \varphi(0) = C\, e^{ak}. \end{aligned} \tag{2.22}$$

Hence, again letting $x=k$, one can write the general solution

$$\varphi(x) = C\, e^{ax}. \tag{2.23}$$

As FRANÇAIS was aware, this result can be found by other methods. However he felt that his approach was best. He also applied similar methods to equations of higher degree using the same basic idea found in BRISSON and ARBOGAST. Thus, given the equation

$$\delta^{n}\, \varphi(x) + a_1\, \delta^{n-1}\, \varphi(x) + \cdots + a_n\, \varphi(x) = 0 \tag{2.24}$$

he said that if $\alpha_1, \alpha_2, \ldots, \alpha_n$ are the roots of the corresponding algebraic equation, this can be written (separating the symbols)

$$(\delta - \alpha_1)(\delta - \alpha_2) \ldots (\delta - \alpha_n) = 0 \tag{2.25}$$

and hence (25) is satisfied if

$$\delta - \alpha_1 = 0,\ \delta - \alpha_2 = 0, \ldots,\ \delta - a_n = 0. \tag{2.26}$$

But then, by (23)

$$\varphi(x) = C_1\, e^{\alpha_1 x},\ \varphi(x) = C_2\, e^{\alpha_2 x}, \ldots,\ \varphi(x) = C_n\, e^{\alpha_n x} \tag{2.27}$$

each furnish a solution of the original equation (24). And since the equation is linear, their sum is a solution, which if the α_i are distinct, is the complete integral. In the case of multiple roots this is not the case, but FRANÇAIS showed that the method of separation of symbols could still be applied to obtain the complete integral. As we shall see, FRANÇAIS' methods were very similar to those which became popular in England during the late 1830's.

J. B. JOSEPH FOURIER, in his *Théorie de la chaleur*, a work known and admired in Great Britain, used the calculus of operations though in a somewhat different way from the authors considered earlier. That is, he did not use it to solve equations, but rather to express and verify solutions of various partial differential equations found by other means. For example, he wrote the solution of the fundamental equation

$$\frac{dv}{dt} = \frac{d^2 v}{dx^2} \tag{2.28}$$

in the form

$$v = e^{tD^2} \varphi(x), \quad \varphi(x) \text{ arbitrary.} \tag{2.29}$$

The meaning of this expression he explained as follows:[19]

> On développer l'exponentielle, selon des puissances de D, et l'on écrira $\frac{d^i}{dx^i}$ au lieu de D^i, en considerant i comme indice de différentiation. On aura ainsi
>
> $$v = \varphi(x) + t \frac{d^2}{dx^2} \varphi(x) + \frac{t^2}{1 \cdot 2} \frac{d^4}{dx^4} \varphi(x) + \frac{t^3}{1 \cdot 2 \cdot 3} \frac{d^6}{dx^6} \varphi(x) + \text{etc.}$$

And to verify the above solution, he differentiated both sides of (29) with respect to t, which gives

$$\frac{dv}{dt} = D^2 e^{tD^2} \varphi(x) = D^2 v = \frac{d^2 v}{dx^2}. \tag{2.30}$$

FOURIER'S work also contains a notational innovation that is characteristic of the calculus of operations. This was the use of a single symbol, in his case D, for a compound operation. Thus, in considering the equation[20]

$$\frac{d^2 v}{dt^2} = \frac{d^2 v}{dx^2} + \frac{d^2 v}{dy^2} \tag{2.31}$$

FOURIER wrote

$$\frac{d^2 v}{dx^2} + \frac{d^2 v}{dy^2} = Dv, \tag{2.32}$$

and he expressed the equation and the solution symbolically as

$$\begin{aligned} \frac{d^2 v}{dt^2} &= Dv, \\ v &= \cos(t\sqrt{-D})\, \varphi(x, y), \end{aligned} \tag{2.33}$$

where $\cos(t\sqrt{-D})$ is to be expanded in powers of tD, and D^i replaced by

$$\left(\frac{d}{dx} + \frac{d}{dy}\right)^i.$$

Another writer whose work in the calculus of operations was well known to the English mathematicians was AUGUSTIN CAUCHY. In his *Exercices de mathématiques, Seconde année,* published in 1827, CAUCHY included three lengthy articles on the calculus of operations.[21] These contain many general theorems as well as methods of solution of various types of equations. CAUCHY referred to two papers by BRISSON as the main source of his material. These memoirs, dated 1821 and 1823, he said, had unfortunately never been published. However CAUCHY

[19] JEAN-BAPTISTE JOSEPH FOURIER, *Théorie Analytique de la Chaleur*. Paris, 1822: 517.

[20] *Ibid.*, 519–520.

[21] AUGUSTIN CAUCHY, "Sur l'analogie des puissances et des différences; Addition à l'article précédent," and "Sur la transformation des fonctions qui représentent les intégrales générales des équations différentielles linéaires," *Oeuvres*: ser. 2, vol. 7, 198–235, 235–254 and 255–266.

gave a broad outline of their contents and he acknowledged BRISSON'S priority in some specific results. But the articles are characterized by the rigor and elegance one expects to find in a work by CAUCHY and his own contribution is clearly a major one.

CAUCHY began by noting that the analogy between powers and indices of differentiation leads easily to the idea of representing a linear expression involving the function u of the variables $x, y, z, \ldots$ and its successive differentials in the form

$$f(\alpha, \beta, \gamma, \ldots)\, u \tag{2.33}$$

$f(\alpha, \beta, \gamma, \ldots)$ denoting a polynomial of degree m. Now BRISSON, said CAUCHY, had generalized this by allowing m to become infinite, attaching a meaning to the expression (33) for any function f that could be developed in positive integral powers of the variable. Actually, a similar idea is found in ARBOGAST'S work. BRISSON, said CAUCHY, used the expression (33) to formulate the solution of both the homogeneous and non-homogeneous linear partial differential equations with constant coefficients. He also considered equation (33) when the function f could be developed in a series in descending powers of the variable only and applied these results to find the solution of certain partial differential equations in symbolic form. But CAUCHY said, despite the fact that with the extended meaning equation (33) sometimes led to correct results, if the work is to be rigorous, it is necessary to restrict attention to the case in which the function f is either a polynomial or a rational function.[22]

CAUCHY began his own discussion by defining the expressions

$$F(D), \quad F(\Delta), \quad F(D, \Delta) \tag{2.34}$$

where $F(x)$ and $F(\alpha, \beta)$ are polynomials and $D y = \frac{dy}{dx}, \Delta y = y(x + \Delta x) - y(x)$. They denoted, he said, the linear function of $y, Dy, \Delta y$, etc., which arise when the expressions (34) are developed in terms of the form $A\, D^m\, \Delta^n y$. CAUCHY then obtained the following important results in the calculus of operations: for any polynomial F[23]

$$\begin{aligned} F(D)\,[e^{rx} f(x)] &= e^{rx} F(r + D)\, f(x) \\ F(\Delta)\,[e^{rx} f(x)] &= e^{rx} F\left(e^{rh}(1 + \Delta) - 1\right) f(x) \\ F(D, \Delta)\,[e^{rx} f(x)] &= e^{rx} F\left(r + D,\, e^{rh}(1 + \Delta) - 1\right) f(x)\,. \end{aligned} \tag{2.35}$$

CAUCHY used these equations to obtain solutions to non-homogeneous linear equations with constant coefficients. Thus, the first equation in (35) shows that if

$$(D - r)\, y = f(x)\,, \tag{2.36}$$

then

$$y = \int e^{-rx} f(x)\, dx\,. \tag{2.37}$$

This result CAUCHY attributed to BRISSON. He then went on to consider higher degree equations by factoring them. That is, he said that if $r_1, r_2, \ldots r_n$ are the

[22] *Ibid.*, 198–199.
[23] *Ibid.*, 200–201.

roots (real or imaginary) of the equation $a_0 r^n + a_1 r^{n-1} + \cdots + a_{n-1} r + a_n = 0$, then we can write

$$a_0 \frac{d^n y}{dx^n} + a_1 \frac{d^{n-1} y}{dx^{n-1}} + \cdots + a_{n-1} \frac{dy}{dx} + a_n = f(x) \tag{2.38}$$

as

$$(D - r_1)(D - r_2) \ldots (D - r_n)\, y = \frac{f(x)}{a_0}, \tag{2.39}$$

and to integrate, let

$$\begin{aligned} (D - r_1)\, y_{n-1} &= \frac{f(x)}{a_0} \\ (D - r_2)\, y_{n-2} &= y_{n-1} \\ \ldots&\ldots\ldots\ldots \\ (D - r_{n-1})\, y_1 &= y_2 \\ (D - r_n)\, y &= y_1. \end{aligned} \tag{2.40}$$

Then, using (37) repeatedly, we obtain

$$y = \frac{e^{r_n x}}{a_0} \int e^{(r_{n-1} - r_n) x} \left(\int e^{(r_{n-1} - r_{n-2}) x} \left(\ldots \int e^{(r_2 - r_1) x} f(x) \right) \right) dx \ldots dx. \tag{2.41}$$

This method works equally well for distinct or multiple roots. CAUCHY also considered difference equations, using the second equation in (35). Furthermore he obtained analogous formulas in the case of polynomials in several variables and applied them to linear partial differential equations.

CAUCHY'S work represents the highest degree of development of the calculus of operations on the Continent during the first half of the nineteenth century. It contained many results which were to be used by English mathematicians as their starting point. But CAUCHY did not trust the method, and he neither carried it further nor did he give a justification of its basic principles, preferring to gain rigor by careful specifications of the functions and operations which he dealt with.

For the next few decades England was clearly the center for research in the calculus of operations, considered here to be defined as calculation in which symbols of operation are manipulated as if they were algebraic symbols. Except for a short paper by BARNABÀ TORTOLINI published in 1853, in which he used what he called the symbolic form of TAYLOR'S Theorem, *i.e.*, $\Delta = e^{h \frac{dy}{dx}} - 1$, in order to integrate finite difference equations, there seems to have been nothing published on the Continent in this vein until the latter part of the century.[24]

The analogy between powers and indices of differentiation suggests not only applications like those given above but also the possibility of extending the analogy to define symbols of operation with non-integral indices. This topic was discussed by many of the mathematicians considered in this paper and was related by some of them to the calculus of operations.

[24] BARNABÀ TORTOLINI, "Sopra gli integrali a differenze finite espressi per integrali definiti," *Ann. di sci. mat. e fis.*, 1853, 4: 209–231.

The extension to non-integral exponents had occurred to LEIBNIZ, who wrote in 1695 to GUILLAUME l'HOSPITAL, that from the analogy between powers of $(x+y)$ and the differentials of (xy): "on peut exprimer par une serie infinie une grandeur comme $d^{\frac{1}{2}}(xy)$, ..." He added, these considerations seemed to give paradoxical results, but he said, it is worth studying since "il n'y a gueres de paradoxes sans utilité."[25] LEONHARD EULER considered the question in 1731 in a way which anticipated the methods of many of the nineteenth century analysts who worked in this area.[26] The general problem, EULER said, was to determine the ratio of $d^n p$ to dx where n is a fraction and p a function of x. This, he added, is in general very difficult, and can be done only in certain special cases. Thus, for example, since for integers e and n,

$$1\cdot 2\cdot 3\ldots e=\int dx(-lx)^e$$

$$\frac{d^n z}{dz^n}=z^{e-n}\frac{\int dx(-lx)^e}{\int dx(-lx)^{e-n}} \tag{2.42}$$

$$\frac{d^{\frac{1}{2}} z}{dz}=\sqrt{z}\,\frac{\int dx(-lx)^e}{dx\sqrt{-lx}}$$

where lx is the natural logarithm of x.

There are passing references to the possibility of fractional indices of operation in the work of ARBOGAST, LAPLACE and FOURIER. ARBOGAST claimed that n could be fractional, or even irrational, in the expression

$$\Delta^n u=(e^{\xi\delta}-1)^n\, xu \tag{2.43}$$

since the right hand side could always be expanded in integral powers.[27] LAPLACE, in his *Théorie analytique des probabilités*, after deriving the formula

$$\nabla^i y_x=\int T\,dt\cdot t^{-x}\left(a+\frac{b}{t}+\cdots+\frac{q}{t^n}\right)^i \tag{2.44}$$

for integral values of i stated that this could be considered as defining the meaning of ∇^i when i was a fraction.[28] FOURIER's remark was in a similar vein. The theorem

$$f(x)=\frac{1}{2\pi}\int_{-\infty}^{\infty} d\alpha\, f(\alpha)\int_{-\infty}^{\infty} dp\cos(px-p\alpha) \tag{2.45}$$

gives, he said,

$$\frac{d^i f(x)}{dx^i}=\frac{1}{2\pi}\int_{-\infty}^{\infty} d\alpha\, f(\alpha)\int_{-\infty}^{\infty} dp\, p^i\cos\left(px-p\alpha+\frac{i\pi}{2}\right) \tag{2.46}$$

and we can allow i to be any number whatever.[29]

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[25] LEIBNIZ, vol. 2, 301–302.

[26] LEONHARD EULER, "De Progressionibus Transcendentibus seu quarum dari nequent," 1730–1731, *Opera Omnia*: ser. 1, vol. 14, 22–24.

[27] ARBOGAST, 351.

[28] PIERRE SIMON LAPLACE, *Théorie analytique des probabilités*. 3rd. ed. rev. Paris, 1820: 85.

[29] FOURIER, 561–562.

But the first attempt to create a coherent theory of fractional indices did not appear until 1832. It is found in a work of JOSEPH LIOUVILLE, professor at the Collège de France in Paris: *Mémoire sur quelques questions de géométrie et de mécanique, et sur un nouveau genre de calcul pour résoudre ces questions.* He based his definition on the assumption that every function y of x can be written in the form

$$y = \Sigma A_m e^{mx} \tag{2.47}$$

where Σ might also denote $\int$. And he defined

$$\frac{d^\mu y}{dx^\mu} = \Sigma A_m e^{mx} m^\mu, \tag{2.48}$$

where μ was any number.[30] Several points that, as we shall see, were to be crucial in discussions of fractional indices first appeared in LIOUVILLE's work on this topic. For example, he obtained

$$\frac{d^\mu \frac{1}{x^n}}{dx^\mu} = \frac{(-1)^\mu \Gamma(n+\mu)}{\Gamma(n)\, x^{n+\mu}} \tag{2.49}$$

where

$$\Gamma(n) = \int_0^\infty e^{-\theta} \theta^{n-1} d\theta. \tag{2.50}$$

The Γ function, which had been introduced by EULER played an important role in many theories of fractional exponents. In particular the thorny question of its value for negative arguments led to difficulties. Thus, in his first memoir LIOUVILLE had put no restrictions on n or μ, but in a later work he noted that since the Γ function was infinite for negative values, his formula (49) was indeterminate if both n and $n+\mu$ were negative. He got around this by altering the definition of the Γ function for negative arguments.[31] In another memoir, the fact that the results of differentiation with fractional exponents are not unique was discussed by him. LIOUVILLE attempted to get around this by introducing what he called complementary functions which he defined as[32]

> certaines quantités qu'il est souvent nécessaire d'ajouter aux valeurs de différentielles, pour les rendre complètes, et leur donner toute la généralité dont elles sont susceptibles.

He found that for $\frac{d^u y}{dx^u}$ the complementary function was of the form

$$C_0 + C_1 x + \cdots + C_{n-1} x^{n-1}, \tag{2.51}$$

$C_0, C_1, \ldots, C_{n-1}$ constants, where if u is a positive integer, all the C_i are 0, if u is a negative integer, there are $n = -u$ terms, while for other values of u the number of terms is finite but indeterminate.

[30] JOSEPH LIOUVILLE, "Mémoire sur quelques questions de géométrie et de mécanique et sur un nouveau genre de calcul pour résoudre ces questions," *J. de l'Ecole Poly.*, 1832, **13**: 3.

[31] JOSEPH LIOUVILLE, "Mémoire sur le théorème des fonctions complémentaires," *J. für die reine und angew. Math.*, 1834, **11**: 4–8.

[32] JOSEPH LIOUVILLE, "Mémoire sur le calcul des différentielles à indices quelconques," *J. de l'Ecole Poly.*, 1832, **13**: 94.

LIOUVILLE did not use the analogy between repeated operations and the law of exponents as the starting point in his theory, but he did consider it to be important. He noted that his definition "contient la clef véritable de ce qu'on a nommé *l'analogie des puissances et des différences ...*" and he proved that, using his definition

$$\frac{d^{\mu}\left(\frac{d^{\alpha} F(x)}{dx^{\alpha}}\right)}{dx^{\mu}} = \frac{d^{\mu+\alpha} F(x)}{dx^{\mu+\alpha}} \tag{2.52}$$

for any values of μ and α.[33] This remark, as we shall see was taken as a starting point for the study of the theory of fractional exponents in the work of some of the English mathematicians.

LIOUVILLE applied his theory to various problems in analysis both in the papers already cited and in others.[34] Furthermore, it is correct, provided one properly restricts the class of functions which can be "differentiated." Despite this it does not seem to have aroused much interest on the Continent. SIMON SPITZER, in a short paper which appeared in 1859, accepted LIOUVILLE's definition and studied the question of finding the successive derivatives and differences of the function $f(x)$ defined as the x^{th} derivative of $\varphi(r)$ evaluated at λ, $\varphi(r)$ being a known function.[35]

BERNHARD RIEMANN, in a paper dating from his student days, took a totally different approach to the problem: he defined the ν^{th} derivative of $z(x)$ as the coefficient of h^{ν} times a constant in a series expansion of $z(x+h)$, *i.e.*, by the equation

$$z(x+h) = \sum_{\nu=-\infty}^{\nu=+\infty} k_{\nu}\, \delta_x^{\nu}\, z(x)\, h^{\nu}, \tag{2.53}$$

where k_{ν} depends only on ν.[36] If the exponents are positive integers, this gives LAGRANGE's definition of the derivative. However, RIEMANN never published his paper. It appeared only in the posthumous collection of his papers. And, as with other topics in the calculus of operations, the center of research in the theory of fractional indices of differentiation was in England during the middle part of the nineteenth century.

Another subject which was, in its early history, considered to be part of the calculus of operations was the study of functional equations. This was because $f(x)$ was interpreted as the operation f acting on x. D'ALEMBERT, LAGRANGE and EULER had all studied certain specific equations. But the subject seems to have come into prominence at the end of the eighteenth century in connection

[33] *Ibid.*, 114.

[34] JOSEPH LIOUVILLE, "Mémoire sur l'intégration de l'équation

$$(mx^2+nx+p)\frac{d^2y}{dx^2}+(qx+r)\frac{dy}{dx}+sy=0$$

à l'aide des différentielles à indices quelconques," *J. de l'Ecole Poly.*, 1832, **13**: 163–186 and "Mémoire sur une formule d'analyse," *J. für die reine und angew. Math.*, 1834, **12**: 273–287.

[35] SIMON SPITZER, "Note über Differenz- und Differential-quotienten von allgemeiner Ordnungszahl," *Arch. der math. und phys.*, 1859, **33**: 116–118.

[36] BERNHARD RIEMANN, "Versuch einer allgemeinen Auffassung der Integration und Differentiation," *Gesammelte Werke*, Dover ed.: 354.

with the determination of the arbitrary functions which enter into the solution of partial differential equations. Pioneering work was done on this subject by GASPARD MONGE and LAPLACE in the 1770's. Both men reduced their problem to that of solving a finite difference equation.[37]

The subject was put on a new plane by CAUCHY, who published in 1821 an important paper in which he studied in great detail, and quite rigorously, the solution of the important functional equations

$$\begin{aligned} \varphi(x+y) &= \varphi(x)+\varphi(y) \\ \varphi(x+y) &= \varphi(x)\cdot\varphi(y) \\ \varphi(xy) &= \varphi(x)+\varphi(y) \\ \varphi(xy) &= \varphi(x)\cdot\varphi(y) \end{aligned} \tag{2.54}$$

both in the case of real and complex valued functions.[38] It should be noted that LEGENDRE had studied the first equation earlier.[39] Although CAUCHY'S work is very significant, he did not develop general methods, nor did he attempt to relate his work to any general considerations on the nature of algebra. Here again, the interest in this subject is found not on the Continent, but in England. In 1821 JOSEPH GERGONNE published a translation of a work by BABBAGE. GERGONNE, in an introduction to the translation, emphasized the importance of the study of functional equations, and expressed regret that it was "encore peu connu et peu cultivé en France ...".[40]

The fact that the techniques of the calculus of operations led to correct results certainly calls for an explanation, and this was apparent to its early users. Thus, JOHANN BERNOULLI wrote to LEIBNIZ[41]

> Nihil elegantis est, quam consensus quem observasti inter numeros potestatum a binomio et differentiarum rectangulo; haud aliquid arcani subest.

He went on to say that it seemed to be a matter of considering d, d^2, d^3, ... as if they were algebraic quantities, but gave no indication of why this was legitimate. LAGRANGE stated explicitly that he did not understand the underlying principles

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[37] GASPARD MONGE, "Mémoire sur la construction des fonctions arbitraires qui entrent dans les intégrales des équations aux différences partielles," *Mém. de math. et de phys.*, 1776, **7**: 267–300; "Mémoire sur la détermination des fonctions arbitraires qui entrent dans les intégrales des équations aux différences partielles," *Ibid.*, 305–327 and PIERRE SIMON LAPLACE, "Recherches sur l'intégration des équations différentielles au différences finies et sur leur usage dans la théorie des hasards," 1776, *Oeuvres*: vol. 8, 103–108.

[38] AUGUSTIN CAUCHY, *Cours d'Analyse de l'Ecole Royale Polytechnique*, Première Partie, 1821, *Oeuvres*: ser. 2, vol. 3. The real case is considered on 98–112 and the complex case on 220–229.

[39] ADRIEN-MARIE LEGENDRE, *Eléments de Géométrie*. 14th ed. Brussells, 1832: 186–187.

[40] CHARLES BABBAGE, "Des équations fonctionelles," trans. J. Gergonne. *Ann. des Math. pures et app.*, 1821–1822, **12**: 102.

[41] LEIBNIZ, vol. 3/1, 179.

of his results in the calculus of operations, though he was sure that they were correct.[42]

LAPLACE as we have seen claimed that the fact that the coefficients of $q, q', \ldots$ in the expansion

$$\Delta^n u = \alpha^n \frac{d^n n}{dx^n} + q\,\alpha^{n+1} \frac{d^{n+1} u}{dx^{n+1}} + q'\,\alpha^{n+2} \frac{d^{n+2} u}{dx^{n+2}} + \cdots \tag{2.55}$$

where $\Delta u = u(x+\alpha) - u(x)$ are independent of α furnishes the explanation.[43]

Both ARBOGAST and FOURIER seemed to regard the method of using symbols of operation as if they were symbols of quantity as an elegant way of discovering, expressing, or verifying theorems, rather than as a valid method of proof. Thus we find in ARBOGAST[44]

> Cette séparation de l'échelle met plus de facilité dans les calcus et fait en outre arriver facilement à des théorèmes bien plus étendus.

And FOURIER remarked on his own use of the calculus of operations[45]

> Ces notations abrégés et connues dérivent des analogies qui subsistent entre les intégrales et les puissances. Quant à l'usage que nous en faisons ici, il a pour objet d'exprimer les séries, et de verifier sans aucune développement.

CAUCHY did not have a theoretical basis for his own work in the calculus of operations and for this reason was wary of its use. As he wrote of his own work[46]

> Toutefois, ces formules, ainsi déduite d'une équation symbolique, ne pourrait encore être considérées comme rigoureusement établies, la methode qui les aura découvrir n'étant en réalité qu'une méthode d'une induction ...

He continued by observing that it did not specify when the series solutions converged or under what conditions the methods could be applied. CAUCHY considered these questions in a later paper, but his results express explicit conditions on the functions and series involved rather than a general explanation of why one should ever be able to derive valid results from the analogy.[47]

LORGNA and FRANÇAIS did attempt to state general principles which would justify the methods of the calculus of operations. Both had the general idea that the reason one could treat the symbols of operation of calculus like symbols of quantity was because the two sets obey the same laws of combination. Thus LORGNA considered powers y^λ and indices of variation y^{λ_l} where λ_l indicated that a certain operation had been performed λ times on y. One could then, in

[42] LAGRANGE, 441–442.

[43] LAPLACE, "Sur l'inclinasion," 314.

[44] ARBOGAST, 350.

[45] FOURIER, 518.

[46] AUGUSTIN CAUCHY, "Note sur des théorèmes nouveaux et de nouvelles formules qui se deduisent de quelques équations symboliques," 1843, *Oeuvres*: ser. 2, vol. 8, 27.

[47] AUGUSTIN CAUCHY, "Mémoire sur l'emploi des équations symboliques dans le calcul infinitésimal et dans le calcul aux différences finies," 1843, *Oeuvres*: ser. 2, vol. 8, 28–38.

certain circumstances, go from one to the other, treating the $\lambda_/$ as if they were powers because, said LORGNA, the index of variation $\lambda_/$ expresses only[48]

> les nombres de traits destinés à représenter les états consécutivement variés des fonctions ou les ordres de différences & des intégrales, si l'on suppose que a, b soient les diviseurs du nombre absolu λ, on peut mettre a la place du $\lambda_/$ les produits de ses diviseurs en appliquent l'accent à tel d'eux qu'on voudra.

That is, if $\lambda = ab$, $\lambda_/ = ab_/$, $\lambda = ba_/$, where $y^{ab_/}$ signifies the fact that the operation performed to get $y^{b_/}$ is repeated a times. LORGNA felt that this observation provided an adequate proof of LAGRANGE'S result (8). He wrote[49]

> on ne saurait disconvenir, ça me semble, après ce qu'on vient d'exposer, que ce n'est pas simple analogie entre les puissances positives, & les différentiations. C'est une liason intime & nécessaire qu'elles ont ensemble, dont cette analogie n'est qu'une suite tenant à des principes qu'il fallait developper ...

Although LORGNA was correct in so far as he went, restricting the analogy to powers, his principle was not sufficient to explain the method of the separation of symbols in which the symbols of operation are not combined merely by iteration but also by addition and multiplication by constants.

FRANÇAIS attempted to give an *à priori* demonstration of the legitimacy of these procedures. He did this by comparing the series of equations[50]

$$F(x, y) = 0 \tag{2.56}$$

$$\begin{gathered} aF(x, y) + bF(x, y) + cF(x, y) + \cdots = 0 \\ f_1(a, b, c, \ldots)\, F(x, y) + f_2(a, b, c, \ldots)\, F(x, y) + \cdots = 0 \end{gathered} \tag{2.57}$$

$$\begin{gathered} (a + b + c + \cdots)\, F(x, y) = 0 \\ \big(f_1(a, b, c, \ldots) + f_2(a, b, c, \ldots) + \cdots\big)\, F(x, y) = 0 \end{gathered} \tag{2.58}$$

with the series

$$\begin{array}{ll} \partial F(x, y) = 0 & \Delta F(x, y) = 0 \\ \partial^2 F(x, y) = 0 & \Delta^2 F(x, y) = 0 \\ \cdots\cdots\cdots & \cdots\cdots\cdots \\ \partial^n F(x, y) = 0 & \Delta^n F(x, y) = 0 \end{array} \tag{2.59}$$

$$\begin{gathered} \partial^n F(x, y) + a\,\partial^{n-1} F(x, y) + b\,\partial^{n-2} F(x, y) + \cdots + k\, F(x, y) = 0 \\ \partial^n F(x, y) + a\Delta\; \partial^{n-1} F(x, y) + b\Delta^2 \partial^{n-2} F(x, y) + \cdots + k\Delta^n F(x, y) = 0. \end{gathered} \tag{2.60}$$

Now, FRANÇAIS said, equations (57) and (58) say nothing more nor less than (56) (clearly this is so only if they hold identically); and the same is true of (59) and (60). But this means, he continued[51]

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[48] LORGNA, 413.
[49] *Ibid.*, 430.
[50] FRANÇAIS, 245.
[51] *Ibid.*, 246.

les *échelles* ou signes de différentes espèces de différentiation se comportent donc de la même manière a l'égard de l'équation proposée qu'elles effectent, que les constantes des équations [58]. *On peut donc considérer ces constantes comme des échelles; et réciproquement on peut traiter des échelles comme des quantités constantes ...*

and hence, equations (60) can be written as

$$\begin{gathered}(\partial^n + a\,\partial^{n-1} + b\,\partial^{n-2} + \cdots + k)\,F(x,y) = 0\\ (\partial^n + a\Delta\,\partial^{n-1} + b\Delta\,\partial^{n-2} + \cdots + k\Delta^n)\,F(x,y) = 0.\end{gathered} \tag{2.61}$$

FRANÇAIS was groping towards the idea that it was because the symbols of operation and of quantity obey the same laws of combination that they can be treated in a similar manner. However, he did not state this clearly, nor did he isolate the laws. This step was taken by FRANÇOIS-JOSEPH SERVOIS in two papers published in the *Annales de mathématiques*. SERVOIS' work was directed primarily towards the establishment of a firm theoretical basis for the calculus. The work was written in part as a polemic against some recently published works by the Pole HOËNE WRONSKI; his *Réfutation de la théorie des fonctions analytiques de Lagrange* (Paris, 1812) and the earlier *Introduction à la philosophie des mathématiques* (Paris, 1811). SERVOIS accepted WRONSKI's criticism of LAGRANGE's theory, but he did not accept WRONSKI's sweeping philosophical claims for his own solution to the problem.[52] (WRONSKI claimed that all of modern mathematics was based on one supreme law, which was not mathematically derived but given by transcendental philosophy.) SERVOIS, on the other hand, felt that no single theory could claim necessarily to represent the true foundations of that science, but that each method had its own advantages. And two of the advantages of his own approach, he claimed, were that it served "lier solidement le calcul différentiel avec l'analyse algébrique ordinaire" and that it furnished an adequate explanation of ARBOGAST's method of separation of symbols.[53] SERVOIS' theory was based on the consideration of functions which were, in terms introduced by him, "distributif" and "commutatif entre elles."[54] That is, functions which satisfied respectively

$$\varphi(x+y+\cdots) = \varphi(x) + \varphi(y) + \cdots \tag{2.62}$$

and

$$f\,F(x) = F\,f(x). \tag{2.63}$$

He proved that if f and F are distributive, so is Ff; that if $F(z) = f(z) + f'(z) + \cdots$, where $f, f', \ldots$ are distributive and pairwise commutative, then F and F^n are distributive for any integer n. Furthermore, he declared, F^n can be found by applying ordinary algebraic laws to $(f(z) + f'(z) + \cdots)^n$. SERVOIS applied this to the differential calculus by noting first that if $\Delta\,\varphi(x) = \varphi(x+\Delta x) - \varphi(x)$, then Δ

[52] FRANÇOIS-JOSEPH SERVOIS, "Réflexions sur les divers systèmes d'exposition des principes du calcul différentiel, et en particulier, sur la doctrine des infiniment petits," *Ann. des Math. pures et app.*, 1814–1815, **5**: 141–170.

[53] *Ibid.*, 141 and 152.

[54] FRANÇOIS-JOSEPH SERVOIS, "Essai sur un nouveau mode d'exposition des principes du calcul différentiel," *Ann. des Math. pures et app.*, 1814–1815, **5**: 98.

is distributive and commutes with constant factors. He then defined the differential dz by

$$dz = \Delta z - \tfrac{1}{2}\Delta^2 z + \tfrac{1}{3}\Delta^3 z - \cdots. \tag{2.64}$$

By his general theory, it then followed that d is also distributive and commutes with constants. And it was here that he related his work to ARBOGAST'S method of the separation of symbols. These two properties, he said show clearly why one can treat symbols like Δ and d as if they were symbols of quantity. He wrote[55]

> Chemin faisant, d'autres rapports entre la différentielle, la différence, l'état varié et les nombres, se sont manifestes; il a fallu en rechercher la cause, et tout est expliqué fort heureusement, quand après dépouillé, par une sévère abstraction, ces fonctions de leurs qualités spécifiques, on a eu simplement a considérer les deux propriétés qu'elles possedent en commun, d'être *distributives et commutatives entre elles.*

The relationship between these notions and the calculus of operations was then made explicit by SERVOIS who noted that the analogy between the expansions involving exponentiation and indices of differentiation is easily explained by his theory. He quoted an evaluation of his work by LEGENDRE and SYLVESTRE-FRANÇOIS LACROIX to that effect. They had written,[56]

> En montrant que s'est à leur nature *distributives et commutatives entre elles* et avec les facteurs constants, que les états variés, les différences et les différentielles doivent leurs propriétés et les analogies de leur développements avec ceux des puissances (l'auteur) en donne la *véritable origine* et éloigne cette idée de *séparation des échelles* qu'Arbogast avait imaginée d'après Lorgna pour expliquer le même circomstances, et qui a paru un peu hasardée.

But neither SERVOIS, nor any of his Continental contemporaries, seemed to be interested in developing the calculus of operations or its implications for the logical foundations of mathematics further. Thus, in 1819 LACROIX summarized the history of the method which he described as one in which "les caractéristiques d'opérations paraissent se comporter comme des symboles de quantités," and he concluded that although it had been considered by several mathematicians, it had not been widely adopted.[57] This, he added, may have resulted from the fact that when it was first introduced the logical foundation was wanting. But, he noted, SERVOIS had now supplied the deficiency. However, the method still did not become popular on the Continent. As we shall see, it was the English who developed this work in the calculus of operations both in extending the scope of its applications and in relating it to a theory of abstract algebra.

3. Introduction of Continental Ideas Into England

The acceptance of the methods and notation of the Continental analysts by the English mathematical world was surprisingly quick, considering the length

[55] SERVOIS, "Réflexions," 142.

[56] *Ibid.*, 152.

[57] SYLVESTRE FRANÇOIS LACROIX, *Traité du Calcul Différentiel et du Calcul Intégral.* 2nd. ed., rev. 3 vols. Paris, 1810–1819: vol. 3, 726.

of time that the isolation had been maintained. The first polemical work directed towards this aim, ROBERT WOODHOUSE'S *Principles of Analytical Calculation,* appeared in 1803, and by 1830 the fluxionary notation and the emphasis on geometric arguments in the calculus had virtually disappeared. WOODHOUSE is generally given credit for being the first to attempt the reform, while its achievement is attributed to CHARLES BABBAGE, JOHN HERSCHEL and GEORGE PEACOCK. But the work of these men in the cause of reform is usually treated as a catalyst which had no causal relation to the subsequent development of mathematics in Great Britain. As I shall show, this is not the case: in their "crusading" writings these men emphasized certain critical ideas they found in the French works connected with the calculus of operations. And these ideas were to be important in the thinking of those later mathematicians who were to be concerned with the problem of the nature of mathematics.

ROBERT WOODHOUSE spent his entire working life at Cambridge University as student, fellow and finally professor. His first book on the subject of the reform of mathematics in England was the *Analytical Calculation* mentioned above. In it he addressed himself primarily to the question of the foundations of the calculus. He reviewed the theories known to him, including those of NEWTON, LEIBNIZ, D'ALEMBERT, LANDEN, LAGRANGE and ARBOGAST. It was the latter two authors whose views came closest to satisfying him, because they linked the calculus to algebra. He wrote "the differential calculus ... is to be considered as a branch of common Algebra, or rather as a part of the common symbolical language in which quantity is treated of."[58] None of the theories which he reviewed were found totally satisfying by WOODHOUSE. In particular, he criticized LAGRANGE for assuming that any function can be expanded in a power series. His own treatment is one which, while not directly tied to the calculus of operations or abstract algebra is not wholly unconnected with them either. WOODHOUSE took what can be described as a strictly formal view of series. That is, he claimed that one could give an extended meaning to the symbol of equality, so that in the theory of series it does not denote numerical equality but rather the result of an operation; thus in analysis the convergence or divergence of a series is irrelevant. He justified this by his overall view on the nature of mathematics which he described as follows:[59]

> The axioms or self-evident principles, such as, when equal quantities are added to, or subtracted from equal quantities, the sums, or remainders are equal, etc. being true in all sciences form in part the base of Analytical Calculation; from these the process of deduction begins, and is expressed by arbitrary characters and their combinations, the meaning of which is to be fixed by definition and convention.

And he continued, the meaning of the symbol "=" should not be taken to mean numerical equality when used between a function and its series expansion. Rather, he said, in such a situation = is used[60]

[58] ROBERT WOODHOUSE, *The Principles of Analytical Calculation.* Cambridge 1803: 212.

[59] *Ibid.*, 1.

[60] *Ibid.*, 14–15.

> to denote the expansion, or the result of any operation, whether it be of multiplication, of division, of involution, or of evolution: and assuming this signification of the sign $=$, when an arithmetical equality results between the function and its expansion, such an equality results not necessarily but contingently; ... in the process of analytical deduction, however, it is the law of the expansion, the connexion of the coefficients of its terms, which it is useful to consider and the convergency or divergency of the series is then a useless consideration.

The following example illustrates WOODHOUSE's point of view. If, he wrote, $\frac{1}{1+x}$ is the symbol of the series which results from dividing 1 by $1+x$, then

$$\frac{1}{1+x} = 1 + x + x^2 + x^3 + \cdots. \tag{3.1}$$

If $\frac{1}{x+1}$ is the symbol of the series which results from dividing 1 by $x+1$, then

$$\frac{1}{x+1} = \frac{1}{x} - \frac{1}{x^2} + \frac{1}{x^3} - \cdots. \tag{3.2}$$

But then, WOODHOUSE concluded, "with reference to their expansions it cannot be affirmed that $\frac{1}{1+x} = \frac{1}{x+1}$."[61] Many earlier mathematicians had gotten into trouble by assuming that since $\frac{1}{1+x} = \frac{1}{x+1}$, series (1) was equal to series (2). This is avoided by WOODHOUSE. He also showed great insight in his realization that equality in mathematics need not denote numerical equality.

Returning to the question of the introduction of the notation in use on the Continent into England, WOODHOUSE's approach was extremely critical. He discussed at length the importance of a good notation. But, WOODHOUSE realized, the choice is in a sense aesthetic. After comparing various expressions written in the fluxional and differential notation, he admitted that if the advantage of the latter does not strike the eye of the reader, he can offer no verbal arguments for it. The notation he finally accepted as best was that introduced by ARBOGAST, because of its usefulness in expressing the TAYLOR series expansion of a function. Thus he compared

$$\begin{aligned} (x+i)^m &= x^m + \frac{\dot{\overline{x^m}}}{\dot{x}}\, i + \frac{\ddot{\overline{x^m}}}{1\cdot 2\,\dot{x}^2}\, i^2 + \cdots \\ (x+i)^m &= x^m + \frac{d\,(x^m)}{1\cdot 2\,dx}\, i + \frac{d^2\,(x^m)}{1\cdot 2\,dx^2}\, i^2 + \cdots \\ (x+i)^m &= x^m + D\,x^m \cdot i + \frac{D^2\,x^m}{1\cdot 2}\, i^2 + \cdots . \end{aligned} \tag{3.3}$$

The first, he said, is very awkward; the second is better but still requires the differential coefficient to be expressed as a fraction. The third avoids this, and can be improved still further by another innovation due to ARBOGAST, namely writing $\not{D}^2\, V, \not{D}^3\, V, \ldots$ for $\frac{D^2 V}{1\cdot 2}, \frac{D^3 V}{1\cdot 2\cdot 3} \ldots$[62] In the text he used this notation

[61] *Ibid.*, 57.

[62] *Ibid.*, xxvii–xxix.

while he is establishing the theoretical foundation, but in all his calculations he used the differential notation.

The book does not seem to have made much impression on the Cambridge scene. In an article on the introduction of the differential notation into Great Britain, J. M. DUBBEY denies that it played any role at all.[63] However, it seems that, contrary to DUBBEY'S statement, CHARLES BABBAGE, who was to play a key role, actually learned the differential notation from it. Not only does BABBAGE describe the work as that "from which I learned the notation of Leibniz," but the other books which he lists as those he knew when he entered Cambridge bears this out. The only foreign works he lists are AGNESI'S *Analytical Institutions*, which, as noted earlier, appeared in England in fluxional garb, and LAGRANGE'S *Calcul des fonctions*.[64]

The *Analytical Calculation* was not WOODHOUSE'S only attempt to institute changes in the English approach to mathematics. In a paper of 1802 "On the independence of the analytical and geometrical methods of investigation and on the advantages to be derived from their separation," he attacked the reliance on geometric methods which probably was, even more than a poor notation, the cause of the lack of progress in English mathematics. WOODHOUSE argued that the introduction of geometric methods into analytical investigations was always unnecessary, owing to the nature of algebra which as a universal language "... must be sufficient to express all the conditions belonging to any subject of investigation."[65] And he used his theory of series to solve analytically problems which had been generally assumed to require a geometric approach.

But WOODHOUSE'S most influential work was his textbook *Plane and Spherical Trigonometry*, first published in 1809. It is much less polemical in tone than his earlier work, and he uses the differential notation throughout, without comment, simply restating each problem in the fluxionary notation in a footnote. He defined the trigonometric functions by their series expansions and derived from these a large number of formulas. These he applied to problems of physical astronomy which would have been extremely difficult to solve geometrically. That this was part of a conscious campaign seem clear from the following quote from that work:[66]

> The student, perhaps, may now be inclined to believe that the formulae demonstrated in the preceding pages, are not entirely without their use, nor invented and shewn as mere specimens of analytical dexterity.

According to GEORGE PEACOCK, a student of WOODHOUSE who was himself to play a major role in the revitalization of mathematics at Cambridge and in

[63] J. M. DUBBEY, "The introduction of the differential notation to Great Britain," *Ann. of Sci.*, 1963, **19**: 39–40.

[64] *Charles Babbage and his Calculating Machines. Selected Writings by Charles Babbage and Others.* ed. PHILIP MORISON & EMILY MORISON. New York, 1961: 22.

[65] ROBERT WOODHOUSE, "On the independence of analytical and geometrical investigation and on the advantages to be derived from their separation," *Phil. Trans.*, 1802, **92**: 86–87.

[66] ROBERT WOODHOUSE, *A Treatise on Plane and Spherical Trigonometry.* 3rd. ed. rev. Cambridge, 1819: 116.

the development of a new concept of algebra, this book aroused a great deal of opposition from the Cambridge dons. It was criticized, he said, because it tended[67]

> to produce a dangerous innovation in the course of academical studies, and to subvert the prevalent taste for the geometrical form of conducting investigations and of exhibiting results which had been adopted by Newton in the greatest of his works, and which it became us, therefore, from a regard to the national honour and our own, to retain unaltered.

However, PEACOCK continued, the opposition, violent as it was, did not persist, and the book came to be universally adopted. In fact, he characterized this work as that which "more than any other work contributed to revolutionize the mathematical studies of this country."[68]

Most writers on English mathematics do not rate WOODHOUSE'S contribution to the cause of reform so highly, and it is probably true that without the efforts of BABBAGE, HERSCHEL and PEACOCK he would not have succeeded. But it should also be noted that these men were influenced by WOODHOUSE. He was their teacher, and PEACOCK'S views on algebra were closely related to those of WOODHOUSE.

Another name always cited in accounts of the reform is that of the Analytical Society. A full account of its founding can be found in CHARLES BABBAGE'S autobiography.[69] Composed of several young men who were dissatisfied with the quality of mathematical instruction at Cambridge, the group met regularly to discuss the work of Continental analysts. The most active members of the organization were GEORGE PEACOCK, JOHN HERSCHEL and CHARLES BABBAGE. The lives of these men were very different. Each of them achieved renown, but in very disparate fields: PEACOCK as Dean of Ely and biographer of THOMAS YOUNG, HERSCHEL as an astronomer, and BABBAGE for his computing machines. All three shared a lifetime devotion to the cause of improving the quality of science in England.

Their earliest contribution was their attack, while they were still undergraduates at Cambridge, on the provincialism of that school. They wished to replace the fluxional notation and geometric methods in vogue there by those used in Continental writings, thus making these works accessible to their fellow students. Towards this end they published several works dating from 1813 to 1820. These included a translation of LACROIX'S elementary treatise on differential and integral calculus, prepared by all three; *A Collection of Examples of the Differential and Integral Calculus* by PEACOCK and *A Collection of Examples of the Applications of the Calculus of Finite Differences* by HERSCHEL. The latter work contained an appendix on the solution of functional equations by BABBAGE. Perhaps even more important, PEACOCK served for several years as examiner for the Tripos examination, and was a lecturer at Trinity College from 1815 to 1823 and a tutor from 1823 to 1839. In all of these positions he used his influence both on

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[67] GEORGE PEACOCK, "Report on the recent progress and present state of certain branches of analysis," *Brit. Ass. Rep.*, 1833: 296.

[68] *Ibid.*, 295.

[69] BABBAGE, 23–24.

students and on his colleagues to further the cause of reform. By 1820 the fluxionary notation had disappeared from the Tripos examination. Works by other authors, also using the differential notation began to appear and by 1830 the analytical methods and differential notation had replaced their geometrical and fluxional counterparts not only at Cambridge but throughout England.

The contributions of the three men were not limited to educational reform alone, a fact generally acknowledged in the case of PEACOCK but not for the other two. As we will see, all three emphasized the relationship between calculus and algebra, and they felt that the French work in the calculus of operations particularly well illustrated the superiority of the Continental approach.

We can see this clearly in their treatment of the LACROIX translation. This work, entitled *An Elementary Treatise on the Differential and Integral Calculus* was published in 1816. The section on the differential calculus was translated by BABBAGE, the integral calculus by PEACOCK and HERSCHEL with the addition of a series of notes by PEACOCK and an original treatise on finite difference equations by HERSCHEL. Although the work is essentially a condensation of LACROIX'S classic three volume treatise on the calculus, it differed from that book in that LACROIX had used LAGRANGE'S theory of functions as the foundation in the original work while in the shortened version he used the theory of limits. This change did not meet the approval of the translators, who wrote about the work:[70]

> It may be considered as an abridgement of his great work on the *Differential and Integral Calculus*, although in the demonstration of the First Principles, he has substituted the method of limits of D'Alembert in the place of the more correct and natural method of Lagrange which was adopted in the former.

As PEACOCK explained it, the theory of limits was not acceptable because it leads to "a tendency to separate the principles and departments of the Differential Calculus from those of Common Algebra ... ."[71] A similar note is found in the discussion of the method of fluxions, which is criticized by PEACOCK for introducing extraneous ideas (geometrical and mechanical) into the study of purely algebraic problems. A further reason given for preferring the differential to the fluxional notation is directly related to the calculus of operations. The differential notation, PEACOCK wrote, is to be preferred because it is "equally convenient for representing both operation and quantity."[72]

Throughout the text, whenever there is an argument based on the theory of limits, PEACOCK supplies a note deriving that result from LAGRANGE'S theory. Though the text was probably more influential in bringing the new notation and methods into wider use in England, it was not the first publication of the reformers. In 1813 there appeared the first (and only) volume of the *Memoirs of the Analytical Society*. It was published anonymously, but according to BABBAGE the preface

[70] SYLVESTRE FRANÇOIS LACROIX, *An Elementary Treatise on the Differential and Integral Calculus*. trans. CHARLES BABBAGE, JOHN HERSCHEL and GEORGE PEACOCK. Cambridge, 1816: iii.

[71] *Ibid.*, 612.

[72] *Ibid.*, 620.

is by himself and HERSCHEL; the first paper is also his, and the remaining two are by HERSCHEL.[73] The authors comment on their anonymity illustrates the zeal which they brought to the task of mathematical reform and is a fitting introduction to their work. They commented[74]

> But some account will naturally be expected of the source itself, from which this work emantes. Of this however, very little need be said, but, that it consists of a few individuals, perhaps too sanguine in their hopes of promoting their favorite science, and of adding at least some trifling aid to that spirit of enquiry, which seems lately to have been awakened in the minds of our country-men, and which will no longer suffer them to receive discoveries in science at second hand or to be thrown behind in that career, whose first impulse they so eminently partook.

In the preface BABBAGE and HERSCHEL emphasized: (1) the relationship between algebra and calculus, exemplified by their preference for the approach of LAGRANGE and ARBOGAST to the foundations of the differential calculus; (2) the impotance of the functional notation and its relationship to a functional calculus; (3) the analogy between repeated operations in the calculus and exponentiation; and (4) the method of the separation of symbols. They praised the analytic method because of the simplicity of its language and conciseness of its notation. Despite these advantages, they went on, symbolic reasoning was not immediately successful, primarily because at the start its resources were poor and it was not well used. But they continued[75]

> to employ as many symbols of operation and as few of quantity as possible, is a precept which is now found invariably to ensure elegance and brevity.

The example they chose was the use of an abstract symbol to denote a function. Thus, they wrote, a good notation can serve to advance science and that[76]

> no single instance of the improvement or extension of notation, better illustrates this opinion, than the happy idea of defining the result of every operation that can be performed on quantity, by the general term of function, and expressing this generalization by a single letter.

The result of this, they went on, was a calculus more general than any known, which they called the calculus of functions. This dealt in fact with the solution of functional equations, and BABBAGE wrote extensively on the subject.

The range of references quoted shows that the authors were very well acquainted with foreign literature. In discussing the origins of the calculus they stated that it was discovered by FERMAT, made analytical by NEWTON and enriched with a powerful and comprehensive notation by LEIBNIZ.[77] And, they went on, the proper foundation of the theory is found in TAYLOR's theorem, as first

[73] BABBAGE, 373.
[74] [CHARLES BABBAGE & JOHN HERSCHEL], "Preface," *Mem. Anal. Soc.*, 1813, **1**: xxi.
[75] *Ibid.*, i.
[76] *Ibid.*, xvi.
[77] *Ibid.*, iv.

recognized by LAGRANGE and ARBOGAST, who "invented it anew, and established it as the true basis of the differential calculus."[78] This also formed the basis of the theory of finite differences, they added, as is obvious if one makes use of the analogy between repeated operations and exponentiation. In this regard, they mention with great admiration the theorems of LAGRANGE and single out for special praise the work of ARBOGAST, who, they declare,[79]

> by a peculiarly elegant mode of separating the symbols of operation from those of quantity, and operating *upon them* as upon analytical symbols; ... derives not only these, but many other much more general theorems with unparalleled conciseness.

The influence of these ideas on the *Memoirs* is apparent. BABBAGE'S paper "On continued products" concerns the solution of functional equations. In it he reduces the solution of

$$\psi x \cdot \varphi x = \chi x \tag{3.4}$$

to a problem depending on successive orders of a single function f. He uses the notation f^n for the n-fold iteration of f and of course notes the analogy between this operation and exponentiation.[80]

HERSCHEL, in his paper "On trigonometrical series" also considered this question, and letting $n = -m$, he used the equation

$$f^n f^m(x) = f^{n+m}(x) \tag{3.5}$$

to define $f^{-m}(x)$. He went on to investigate $f^z(x)$, when z was not an integer. He concluded that this could be defined in terms of z and x, provided one could find a formula for $f^z(x)$, when z was integral. Thus, for example, if

$$f(x) = \frac{a x}{b + c x}, \qquad f^z(x) = \frac{a^z}{b^z + c x \dfrac{a^z - b^z}{a - b}} \tag{3.6}$$

makes sense even when z is not integral. And he said, if z is fractional or imaginary "the only meaning we can assign to $f^z(x)$ is, that *it is* that function of z and x which is here connected to it by the sign of equality."[81] HERSCHEL'S main mathematical interest was in the solution of finite difference equations. This gave rise to the investigation of fractional indices of differentiation, for, as he pointed out, in solving equations of mixed differences formulas occur in which the index of differentiation is variable. His technique here was similar to that used above for functions. Thus he concluded that the problem was solvable for a given function f provided that we can find a function φ such that

$$D^n f(x) = \varphi(n, x) \tag{3.7}$$

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[78] *Ibid.*, iv–v.

[79] *Ibid.*, xi.

[80] [CHARLES BABBAGE], "On continued products," *Mem. Anal. Soc.*, 1813, **1**: 1–31.

[81] [JOHN HERSCHEL], "On trigonometric series; particularly those whose terms are multiplied by tangents, co-tangents, secants, etc. of quantities in arithmetic progression; together with some singular transformations," *Mem. Anal. Soc.*, 1813, **1**: 48.

for positive integers n. We can then look on this equation as the definition of D^n for other values of n.

In the last paper in the volume HERSCHEL considered BABBAGE's favorite topic—functional equations. Like MONGE and LAPLACE, he solved various equations by reducing them to finite difference equations.[82]

BABBAGE's later work in pure mathematics was concentrated on the solution of functional equations. In one of his earliest works on that subject "An Essay towards the calculus of functions, 1815" he gave a definition of function which bears out the contention that this subject belongs to a history of the calculus of operations. He wrote[83]

> the term function has long been introduced into analysis with great advantage, for the purpose of designating the result of every operation that can be performed on quantity.

In a later paper, "Observations on the analogy which subsists between the calculus of functions and other branches of analysis," BABBAGE referred not only to the obvious one between powers and repeated operations but also to those concerned with the solutions of specific functional equations. Thus, he compared the relations which hold among the n^{th} roots of unity to those among the solutions of the equation $\psi^n x = x$. And, he noted, many of the methods for solving functional equations which he gives were derived from the corresponding integral problems by analogy.

To BABBAGE, the importance of the concept of function was not only that it supplied a unifying theme in mathematics but also that its suggestive notation helped lead to future investigations. As an example, he cited the ramifications of equation (5). Like HERSCHEL, he used this equation as a starting point for a discussion of the meaning of f^α where α was not a positive integer. His solution, too, was similar to that of HERSCHEL, in that he used an established equation to define the further meaning of the symbol. Thus, the index n was now to indicate "such a modification of the function to which it is attached that that equation shall be verified."[84] Thus, $f^m x = f^0 f^m x$ gives $f^0 y = y$; and letting $m = -1$, $n = 1$, $f f^{-1} x = x$, or f^{-1} is the function of x such that if you perform f on it the result is x. The fact that such a function is not necessarily unique was noted by BABBAGE, but did not appear to him to present a problem. He merely mentioned the fact, and added that among all such functions there was only one which also satisfied $f^{-1} f x = x$.

BABBAGE's general philosophy of mathematics is found in one of his last papers on pure mathematics, "On the influence of signs in mathematical reasoning." His approach was that of a formalist and is similar to that of WOODHOUSE and ARBOGAST. He also anticipated later views when he wrote[85]

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[82] [JOHN HERSCHEL], "On equations of differences and their applications to the determination of functions from given conditions," *Mem. Anal. Soc.*, 1813, **1**: 65–114.

[83] CHARLES BABBAGE, "An essay towards the calculus of functions," *Phil. Trans.*, 1815, **105**: 389.

[84] CHARLES BABBAGE, "Observations on the notation employed in the calculus of functions," *Trans. Camb. Phil. Soc.*, 1819–1821, **1**: 64–65.

[85] CHARLES BABBAGE, "On the influence of signs in mathematical reasoning," *Trans. Camb. Phil. Soc.*, 1822–1826, **2**: 326–327.

The nature of the quantities with which the mathematical sciences are conversant, is undoubtedly one of the first causes for the certainty of its conclusions; in Geometry it has been well remarked that its foundations rest on definitions, and if this do not altogether hold in algebraical enquiries, at least the meaning of the symbols employed must be regulated by definition; ...

In the calculus of functions, as with so much that BABBAGE was to do in his life, after a very promising beginning he dropped the subject, leaving, in the end, only an interesting fragment. This does not diminish his real contributions. He attacked and solved some important equations and introduced the method of determining the general solution from a particular one in certain cases. Furthermore he was one of the prime movers of the mathematical reform, and we find in his arguments for the importance of a good notation and the analytic method; in his emphasis on the relationship between common algebra and analysis; and in his attempts to develop a calculus of functions many of the ideas that were to influence the subsequent development of British mathematics.

The same is true of his fellow worker JOHN HERSCHEL. HERSCHEL, well known for his work in astronomy and chemistry was also a first rate mathematician. His primary field of interest was, as has already been noted, the solution of finite difference equations. He was the first Englishman to write on the application of ARBOGAST'S method of separation of symbols to the solution of these and other equations. In his paper "Consideration of various points of analysis, 1814" he explained the basis of the method and its importance:[86]

In the following pages I have uniformly made use of the functional or characteristic notation, together with the method of separating (where it could conveniently be done) the symbols of operation from those of quantity. This method, I have, perhaps, extended and carried somewhat farther than has hitherto been customary; but, I trust, without losing sight of its grand and ultimate object, the union of extreme generality with conciseness of expression.

HERSCHEL made several remarks which indicate one of the ways in which these developments were to broaden the concept of algebra—by giving rise to an algebra in which the objects of calculation were symbols of operation rather than of quantity. He noted that if any number of functions of x be combined algebraically, the resulting function was to be denoted by the same combination of their characteristics; that is $\varphi \cdot \psi(x)$ was defined to be $\varphi(x) \cdot \psi(x)$. He used the symbol D, due, as he noted, to ARBOGAST, for the sign of derivation. And, HERSCHEL pointed out, it denoted an operation performed not on quantity, but on the functional characteristic which follows it. He wrote[87]

It is properly speaking the sign of an operation performed, not on quantity, but on the characteristic which it immediately precedes; by which the operation denoted by that characteristic is altered.

[86] JOHN HERSCHEL, "Considerations of various points of analysis," *Phil. Trans.*, 1814, **114**: 441.
[87] *Ibid.*, 443.

The idea that what is involved in the calculus of operations was that the symbols of operation behave like those of quantity also appears in this paper. HERSCHEL wrote "every functional characteristic is affected by all the characters preceding it, in the same manner as if it were a symbol of quantity."[88]

In the body of the paper, HERSCHEL applied the method to the study of generating functions, a theory due to LAPLACE, solving in certain cases both the problem of finding the general term in an expansion and the converse problem of summing a series. He also considered the solution of the linear differential equation which he denoted

$$0 = u + {}^1A\, Du + {}^2A\, D^2u + \cdots + {}^nA\, D^n u. \tag{3.8}$$

And he solved it in much the same way as ARBOGAST, CAUCHY and FRANÇAIS. That is, he divided by ${}^nA$ and factored the corresponding algebraic equation and reduced (8) to the form $0 = (D - p)(D - q) \ldots ; u$, which, he said, could easily be solved.[89]

HERSCHEL also considered LAGRANGE'S theorem, which he wrote as

$$\Delta^n u = \left\{ e^{\frac{d}{dx}} - 1 \right\}^n u,$$

and which he described as[90]

> The beautiful theorem of Lagrange which affords us an opportunity to develop the principles of a method of notation which seems to unite in the most perfect manner the properties of conciseness, simplicity and elegance, and appears peculiarly well adapted to open new and enlarged views of the extent and meaning of analytical operations.

What HERSCHEL is referring to is essentially the method of the separation of symbols. Thus he proved LAGRANGE'S theorem as follows: writing TAYLOR'S theorem as

$$\Delta = \frac{d}{dx} + \frac{1}{1 \cdot 2} \frac{d^2}{dx^2} + \cdots \tag{3.9}$$

he noted that[91]

> It is of no consequence to our present purpose in what light we regard the symbol Δ whether as a quantity, or merely as an intrument by means of which, ..., we are enabled to produce the numerical coefficients of the series affected with their proper powers of the same.

Here we see an approach similar to that of WOODHOUSE. HERSCHEL then expanded $\left\{ e^{\frac{d}{dx}} - 1 \right\}$ in powers of $\frac{d}{dx}$ and comparing coefficients, concluded that the series of operations so denoted is equivalent to Δ. Thus, HERSCHEL asserted, the operation Δ repeated n times is clearly equivalent to $\left\{ e^{\frac{d}{dx}} - 1 \right\}^n$ which is the theorem to be proved. However, he went on to say[92]

[88] *Ibid.*
[89] *Ibid.*, 467–468.
[90] LACROIX, *Elementary Treatise*, 478.
[91] *Ibid.*, 479.
[92] *Ibid.*, 487.

The discovery of this theorem and its consequences have formed in some respects an epoch in mathematical literature, and as it seems in general to be regarded as involving a certain degree of obscurity, we shall proceed to a more particular demonstration of it.

The proof he gives is due to Dr. JOHN BRINKLEY, at that time Andrews Professor of Astronomy at Trinity College, Dublin. BRINKLEY'S work on this subject appeared in the 1807 volume of the *Philosophical Transactions*. It is of interest because it shows that while it is not impossible to treat theorems of this sort in the fluxional notation, it is certainly a disadvantage in terms of clarity and conciseness. BRINKLEY'S statement of his result illustrates this: $\Delta^n u$ and $S^n u$ denoting the first terms of the n^{th} order differences and the series of which the first term of the n^{th} order is u respectively[93]

1. $\Delta^n u = (e^{\dot{u}/\dot{x}} - 1)^n$ and 2. $S^n u = (e^{\dot{u}/\dot{x}} - 1)^{-n}$ provided that in the expansion of $(e^{\dot{u}/\dot{x}} - 1)^n$ $\frac{\overset{2}{\dot{u}}}{\dot{x}^2}$, $\frac{\overset{3}{\dot{u}}}{\dot{x}^3}$, etc. be substituted for $\left(\frac{\dot{u}}{\dot{x}}\right)^2$, $\left(\frac{\dot{u}}{\dot{x}}\right)^3$ etc. and provided that in the expansion of $(e^{\dot{u}/\dot{x}} - 1)^{-n}$ fl.^{n} $u\dot{x}^n$, fl.$^{n-1}$ $u\dot{x}^{n-1}$ be substituted for $\left(\frac{\dot{u}}{\dot{x}}\right)^{-n}$, $\left(\frac{\dot{u}}{\dot{x}}\right)^{-n+1}$ etc. and $\frac{\overset{2}{\dot{u}}}{\dot{x}^2}$, $\frac{\overset{3}{\dot{u}}}{\dot{x}^3}$ etc. be substituted for $\left(\frac{\dot{u}}{\dot{x}}\right)^2$, $\left(\frac{\dot{u}}{\dot{x}}\right)^3$ etc.

The problem then is to determine the coefficients in the series expansion on the right hand side of the equation. It is interesting to note that BRINKLEY'S argument, which is a combinatorial one, is related, as he points out, to that of ARBOGAST.

HERSCHEL and BRINKLEY both discussed generalizations of LAGRANGE'S theorem, which they attribute to ARBOGAST. In a later paper on this topic HERSCHEL expressed the basic result as:[94] for any algebraic function f

$$f(1+\Delta)\, u_x = f(e^{\Delta x, D})\, u_x. \tag{3.10}$$

He used this result to find various series expansions, many of which had also been obtained by BRINKLEY. The formulas given by the two men, although they give the same numerical answers are quite different in form. HERSCHEL'S deep interest in this subject is shown by the fact he returned to it in 1860, long after he had obtained renown in other fields. In this work HERSCHEL makes use of the calculus of operations in order to show that his own result can be derived from that of BRINKLEY.[95]

HERSCHEL did not often express general views on the nature of mathematics. One of his few pronouncements is found in a work which is of some interest in its own right. This was the *Mathematical Essays* by WILLIAM SPENCE, which was published posthumously, and edited by HERSCHEL. SPENCE, a Scotsman, was a self-taught mathematician. His work was primarily devoted to problems

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[93] JOHN BRINKLEY, "An investigation of the general term of an important series in the method of finite differences," *Phil. Trans.*, 1807, **97**: 114–115.

[94] JOHN HERSCHEL, "On the development of exponential functions; together with several new theorems relating to finite differences," *Phil. Trans.*, 1816, **116**: 26.

[95] JOHN HERSCHEL, "On the formulae investigated by Dr. Brinkley for the general term in the development of Lagrange's expression for the summation of series and for successive integration," *Phil. Trans.*, 1860, **150**: 319–321.

of the series expansion of functions, both direct and inverse. It was thus in the combinatorial school and, as HERSCHEL pointed out, his approach was very similar to that of ARBOGAST. The final essay in the volume, "Outline of a theory of algebraical equations," was left unfinished at the time of SPENCE's death, and HERSCHEL supplied the final pages. He concluded on a note which emphasized one of the central points of view of the revivers of mathematics in England. The object of SPENCE's paper, he noted, is to[96]

> offer a satisfactory link of connexion between the ordinary algebra and the profounder theorems of the differential calculus—subjects which are too commonly, at least in this country, regarded as essentially disjoined and dependent on different principles.

Thus, HERSCHEL's mathematical work, which was concentrated on the solution of equations of finite differences and their applications to functional equations, freely used the method of the separation of symbols of operation from those of quantity, emphasized the relationship between the calculus and ordinary algebra and anticipated many of the themes which were to be current in English mathematics throughout the first half of the century.

The other member of the Analytical Society who published noteworthy mathematical works is GEORGE PEACOCK. As we have seen, PEACOCK was, in his roles as examiner, lecturer and tutor, in many ways the man most responsible for the acceptance of Continental methods at Cambridge. He was also the only one among this group who formulated explicitly a theory of algebra, and probably for this reason he is—wrongly, in my view—the only one whose writings are generally considered in the history of algebra. His important works are his *Treatise on Algebra*, first printed in 1830 and issued in revised form in 1842, and his "Report on the Recent Progress and Present State of Certain Branches of Analysis," delivered at the 1833 meeting of the British Association for the Advancement of Science. The latter work, which amply exhibits the erudition of the reformers, is an invaluable source in the history of mathematics. Because of their nature and their date these works will be considered in later sections.

The four men who labored to reform the study of mathematics at Cambridge achieved not only that aim, but also contributed, though each in a different way, to the shaping of a new view of algebra which was to be formulated in England following their work. WOODHOUSE and PEACOCK were specifically interested in such problems. But while BABBAGE and HERSCHEL did not suggest any formal philosophy of algebra, they did, in their work on the calculus of functions and in their use of the method of the separation of symbols, introduce concepts which were to be influential in the extension of the concept of algebra.

4. Calculus of Operations in Great Britain

During the 1830's there occurred, as the reformers had hoped, a marked increase in mathematical research. Much of this was concerned with the application of the method of the separation of symbols, or, as it came to be termed, the calculus

[96] WILLIAM SPENCE, *Mathematical Essays*. ed. JOHN HERSCHEL. London, 1820: 295.

of operations, to various problems in analysis. As E. T. BELL has pointed out the years 1835–1860 saw much exploitation of the method in England. BELL was hardly admiring of the use made by the British mathematicians of the calculus of operations, describing the period as a "somewhat shady episode of symbolic methods," which, he added "despite their utility ... were scarcely reputable mathematics, because no explicit formulation of the conditions under which they give correct results accompanied their use."[97] Although this charge is essentially true, it was not universally so, and, as I shall show, the main significance of the episode is not to be found in the methods themselves, but rather in the attempts to extend them and to put them on a firm logical basis. This served to focus attention on the laws of the combination of symbols without regard to specific operations, fostering an abstract view and clearly influencing many of the men who were to give, in the 1840's the beginnings of an abstract definition of algebra.

The earliest work, following HERSCHEL, in which the symbolic method is used, appears to be a paper by S. S. GREATHEED: "A new method of solving equations of partial differentials," published in the 1837 *Philosophical Magazine*. GREATHEED was a Cambridge man and in fact came from Trinity College, the college of the reformers. In this memoir he began by noting[98]

> Separation of the symbols of operation from quantity has, so far as I know, been hitherto applied only to the calculus of finite differences, and to the differential calculus where both are involved. It appears to me that if any much greated eminence than that to which analysis has already been brought, remains to be attained by it, that process is the most obvious and likely path.

From this quote, it seems likely that GREATHEED's knowledge of the method derived from HERSCHEL's work. GREATHEED based his work on the assumption that symbols of operation could be treated as if they were symbols of quantity in certain situations. He also used a theorem in the calculus of operations which we have encountered in several forms before. GREATHEED referred to it as the "symbolic form of Taylor's Theorem" and he wrote it as

$$f(x+h) = e^{h\frac{d}{dx}} f(x). \tag{4.1}$$

He used this to help solve the partial differential equation

$$a\frac{dz}{dx} + b\frac{dz}{dy} = c, \tag{4.2}$$

where a, b and c are contents, in the following manner. If, he noted, z were a function of x alone and n a constant, the ordinary differential equation

$$a\frac{dz}{dx} + nbz = c \tag{4.3}$$

[97] ERIC TEMPLE BELL, *The Development of Mathematics*. 2nd ed. New York, 1945: 413.

[98] S. S. GREATHEED, "A new method of solving equations of partial differentials," *Phil. Mag.*, 1837, **11**: 239–247.

has the solution

$$z = \frac{c}{nb} + e^{-\frac{nbx}{a}} C', \tag{4.4}$$

C' an arbitrary constant. But then, if we replace n by $\frac{d}{dy}$, the result will be a solution of the given partial equation (2). Making the substitution, he obtained

$$z = \frac{c}{b}\left(\frac{d}{dy}\right)^{-1} + e^{-bx\frac{d}{d,ay}} f(ay), \tag{4.5}$$

an arbitrary function f replacing the constant C'. But then, he continued, since $\left(\frac{d}{dy}\right)^{-1}$ represents integration with respect to y, applying (1) to the last term, GREATHEED got as the solution to his equation

$$z = \frac{cy}{b} + f(ay - bx). \tag{4.6}$$

GREATHEED'S approach is interesting because of the free use which he makes of treating the symbol of operation as if it were a symbol of quantity. But his only explanation of the method was a pragmatic one, that is, that it yielded a correct solution and it is clear why more rigorous generations of mathematicians shuddered.

The next mathematician whose work we shall consider did attempt to justify his methods. This was DUNCAN GREGORY. GREGORY, a Scotsman, descended from a family of scientists which included the mathematicians JAMES and DAVID GREGORY. He attended the University of Edinburgh before entering Trinity College, Cambridge, in 1833, where he remained until shortly before his untimely death at the age of 31.

Most of GREGORY'S papers appeared in the *Cambridge Mathematical Journal.* This periodical was founded by GREGORY and ROBERT ELLIS, also from Trinity College, to provide a place for the publication of short research papers in mathematics, and hence encourage young researchers. AUGUSTUS DEMORGAN, whose work we shall consider later, described the *Journal* and GREGORY'S contributions to it in a letter to JOHN HERSCHEL written in 1845:[99]

> You should not forget the Cambridge "Mathematical Journal." It is done by the younger men. Four octavo vols. are published. It is full of very original communications. It is, as is natural in the doings of young mathematicians, very full of symbols. The late D. F. Gregory, ...—gave his extensions of the Calculus of Operations, what used to be the separation of the symbols of operation and (quantity) in it ... .

GREGORY'S first paper on the separation of symbols treated the linear differential equation with constant coefficients, in a manner very similar to that of

[99] SOPHIA DEMORGAN, *Memoir of Augustus DeMorgan.* London, 1882: 150–151.

his predecessors.[100] Thus he factored

$$\frac{d^n y}{dx^n}+A\frac{d^{n-1}y}{dx^{n-1}}+\cdots+R\frac{dy}{dx}+S=X \tag{4.7}$$

into the form

$$\left(\frac{d}{dx}-a_1\right)\left(\frac{d}{dx}-a_2\right)\cdots\left(\frac{d}{dx}-a_n\right)y=X. \tag{4.8}$$

In order to solve the equation, GREGORY used the theorem

$$\left(\frac{d}{dx}\pm a\right)^n X=e^{\mp ax}\left(\frac{d}{dx}\right)^n e^{\pm ax}X, \tag{4.9}$$

which he proved by expanding the left hand side by the binomial theorem, applying the usual convention to $\frac{d}{dx}$. A solution of the equation was found by multiplying both sides of the equation by $\left(\frac{d}{dx}-a_1\right)^{-1}$. The theorem (9), with $n=-1$ and $\left(\frac{d}{dx}\right)^{-1}$ treated as $\int x$, gives

$$\left(\frac{d}{dx}-a_2\right)\left(\frac{d}{dx}-a_3\right)\cdots\left(\frac{d}{dy}-a_n\right)y=\left(\frac{d}{dx}-a_1\right)^{-1}X=e^{a_1x}\int e^{-a_1x}X\,dx. \tag{4.10}$$

The general solution is obtained by repeating this for $i=2, 3, \ldots, n$. The result,

$$y=\frac{e^{a_1x}\int e^{-a_1x}X\,dx}{(a_1-a_2)\ldots(a_1-a_n)}+\frac{e^{a_2x}\int e^{-a_2x}X\,dx}{(a_2-a_1)\ldots(a_2-a_n)}+\cdots+\frac{e^{a_nx}\int e^{-a_nx}X\,dx}{(a_n-a_1)\ldots(a_n-a_{n-1})}, \tag{4.11}$$

is elegant, though, as GREGORY did not point out, fails if the roots are not all distinct.

In another paper in the same volume of the *Cambridge Mathematical Journal*, GREGORY used exactly the same technique for finite difference equations.[101] GREGORY'S methods are very similar to those we found in the work of CAUCHY, differing primarily in the free use that GREGORY makes of the idea of treating the symbol of operation as if it were a symbol of quantity, for example in his proof of (9). In fact, GREGORY was familiar with CAUCHY'S work in this subject. In the paper on differential equations, he credits BRISSON'S work in 1821 as being the first in which the method of the separation of symbols was applied to the solution of differential equations, and he cites CAUCHY'S *Exercises* as his source of information in regard to BRISSON'S work.[102] (This is in fact not true, as we have seen in Section 2.) In a later paper he acknowledged the work of HERSCHEL, which however, he said, he had read only after his own work had been done.[103]

GREGORY also applied the separation of symbols to partial differential equations. He described his method as follows:[104]

[100] DUNCAN GREGORY, "On the solution of linear differential equations with constant coefficients," 1839, *Mathematical Writings*: 18–21.

[101] DUNCAN GREGORY, "On the solutions of linear equations of finite and mixed differences," 1839, *Math. Writings*: 47–52.

[102] GREGORY, "Differential equations," 15.

[103] DUNCAN GREGORY, "Demonstrations of theorems in the differential calculus and calculus of finite differences," 1839, *Math. Writings*: 122.

[104] DUNCAN GREGORY, "On the solution of partial differential equations," 1839, *Math. Writings*: 63.

Linear partial differential equations between any number of variables with constant coefficients, are to be treated exactly like ordinary differential equations with regard to one of the variables, the symbols of operation of the others being treated as constants.

Like GREATHEED he considered the equation (2), but his treatment differed somewhat since he used his own method for the ordinary equation. GREGORY mentioned GREATHEED'S work, but gave as the source of his own ideas, the Frenchman FOURIER. He wrote that FOURIER had been the first to show that the solutions of partial differential equations could be expressed by the separation of symbols. But, he added, FOURIER had not used it as a method of obtaining solutions:[105]

His idea was apparently that the expressions he obtained as solutions might be conveniently expressed by separating the symbols of operations, and not that the symbolical expressions are the proper solutions of the equations, and the series merely the expansion of them.

Other French writers, he went on, do not use the symbolical solutions at all, citing especially POISSON, who, in dealing with FOURIER'S work, omitted them altogether. The reason for the neglect of the method, GREGORY felt, was that its basic principles were not understood. Thus, he noted, the separation of symbols was used originally only to express known theorems, like that of LAGRANGE. But such results were treated as analogies and "few seemed willing to trust themselves to a method, the principles of which did not appear to be very sound."[106] But this lack had been remedied, he added, citing the work of SERVOIS. He wrote[107]

Servois was, I believe, the only mathematician who attempted to explain its principles, ... and it was in pursuing this investigation that he was led to separate functions into distributive and commutative, which he perceived to be the properties which were the foundation of the method of the separation of the symbols, as it is called. This view which, so far as it goes, coincides with that which it is the object of this paper to develop, at once fixes the principles of the method on a firm and clear basis. For, as these operations are all subject to common laws of combination, whatever is proved true by means only of these laws is necessarily true of all operations.

GREGORY used this observation to apply the method of separation of symbols to the solution of simultaneous ordinary differential equations. Thus, he began his study of that subject by observing that[108]

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[105] *Ibid.*, 62.

[106] DUNCAN GREGORY, "On the real nature of symbolical algebra," 1840, *Math. Writings*: 7.

[107] *Ibid.*

[108] DUNCAN GREGORY, "On the integration of simultaneous differential equations," 1839, *Math. Writings*: 95.

> Since we have shewn that the symbols of differentiation are subject to the same laws of combination as those of numbers, they may be always treated in the same manner if the coefficients be all constant, which is the only case we shall consider. We have therefore only to separate the symbol of differentiation from its subject, and then proceed to eliminate one of the variables between the given equations, exactly as if the symbol of differentiation were an ordinary coefficient.

That is, the system of equations

$$\left(\frac{d}{dt}\right) x + a y = 0$$
$$\left(\frac{d}{dt}\right) y + b x = 0 \tag{4.12}$$

can be solved by "multiplying" the first equation by $\frac{d}{dt}$ and subtracting a times the second equation from the result. This gives

$$\left(\frac{d^2}{dt^2} - a b\right) x = 0 \tag{4.13}$$

which is integrable, and y may then be found from the first equation.

Not only did GREGORY defend the use of the symbols of differentiation and differencing as if they were symbols of quantity on the grounds that they obey the same laws of combination, he argued that in fact there was no valid distinction between the two types of symbols. "We have spoken," he wrote," as if there were a distinction between what are usually called symbols of operation, and those which are called symbols of quantity. But we might with perfect propriety call these last also symbols of operation."[109] That is, he noted, we can consider x the operation (x) performed on unity, x^n the same operation performed n times in succession on unity; $n x$, n of the operations (x) on unity taken simultaneously, and $a x$ the operation a applied not to 1 but to x. The symbols $a, b, \ldots,$ then satisfy, GREGORY claimed, the laws of combination[110]

$$a^m \cdot a^n x = a^{m+n} x$$
$$a[b(x)] = b[a(x)] \tag{4.14}$$
$$a(x) + a(y) = a(x+y),$$

and hence

> If f, f_1, etc. being any other general symbols of operation (f, f_1 being of the same kind) subject to the same laws of combination, so that
>
> $$f^m f^n(x) = f^{n+m}(x) \tag{1}$$
>
> $$f[f_1(x)] = f_1[f(x)] \tag{2}$$
>
> $$f(x+y) = f(x) + f(y) \tag{3}$$

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[109] GREGORY, "Differential equations," 24.
[110] *Ibid.*, 25.

then whatever we may have proved true of a, b, etc. depending on these laws must necessarily be equally true of f, f_1, etc.

GREGORY referred to these laws as the index, commutative and distributive laws respectively. If (2) is interpreted with f_1 as multiplication by a constant, these are obeyed by $\frac{d}{dx}$ and Δ. This observation was used by GREGORY in his paper "Demonstrations of theorems in the differential calculus and calculus of finite differences" written "to bring together the more important of the theorems in the Differential Calculus and in the Calculus of Finite Differences, which depending on one common principle, can be proved by the method of the separation of symbols."[111] The principle referred to is the binomial theorem, for GREGORY claimed, this theorem was shown by EULER to follow, even when the exponent is fractional or negative, from the laws listed above. That is, he said[112]

> it will be found, on examining Euler's demonstration, that it includes not only these cases, but also those in which a, b, and n are operations subject to certain laws; for it may be seen, that in the proof no other properties are presumed than that a, b, and n are commutative and distributive functions and that a^n and b^n are subject to the laws of index functions.

He went on to add, since the operations of the differential calculus and those of finite differences satisfy these laws, the binomial theorem may be assumed true of them. Among the results he derived from this remark is LEIBNIZ' rule for the repeated derivative of a product. GREGORY wrote

$$\frac{d}{dx} uv = \left(\frac{d^1}{dx} + \frac{d}{dx}\right) uv \tag{4.15}$$

where $\frac{d^1}{dx}$ acts on v alone and $\frac{d}{dx}$ on u alone, then $\frac{d^1}{dx}$ and $\frac{d}{dx}$ are commutative and the binomial theorem applies, and hence

$$\begin{aligned}\frac{d^n}{dx^n} uv &= \left(\frac{d^1}{dx} + \frac{d}{dx}\right)^n uv \\ &= u\frac{dv}{dx} + n\frac{du}{dx}\frac{d^{n-1}v}{dx^{n-1}} + \frac{n(n-1)}{1\cdot 2}\frac{d^2u}{dx^2}\frac{d^{n-2}v}{dx^{n-2}} + \cdots.\end{aligned} \tag{4.16}$$

This GREGORY felt was now proved for all rational values of n.[113] However, if n is negative or fractional, the right hand side is an infinite series, and to modern minds this raises the question of convergence, which GREGORY completely ignored. In 1848 GREGORY's procedure was criticised on this ground by JOHN YOUNG, an Irish mathematician. YOUNG pointed out that GREGORY's result was not correct for all values of n, in particular, that if n was negative it was necessary to add a series of correction factors. Young praised the calculus of operations,

[111] GREGORY, "Demonstrations of theorems," 108.
[112] *Ibid.*, 108–109.
[113] *Ibid.*, 110–111.

calling it "a method which has deservedly received much attention of late." But he went on[114]

> the employment of this refined principle of investigation requires, however, more than ordinary caution and circumspection: among other things it must be observed, that the theorems to which it leads cannot generally be true when they assume the form of series, whose character is such that, when the symbols of operation are replaced by those of quantity, divergency takes place.

GREGORY'S use of the calculus of operations was defended by CHARLES GRAVES, also an Irishman and a close friend of WILLIAM ROWAN HAMILTON. Although GRAVES agreed with YOUNG that the form of the iterated integral given by GREGORY was incomplete, he argued that the fault was not inherent in the calculus of operations, "which, if applied to this problem with proper caution, will furnish the correct result in a direct and elegant manner."[115] The error, he claimed, was in the use of an incomplete form of the binomial theorem; the remainder term should have been retained. This result, he said, made[116]

> manifest the danger, noticed by Professor Young, of substituting symbols of operation for those of quantity in divergent series, they indicate that, whenever we know how to express in a finite form the value of the remainder after any given number of terms of an infinite series, there is a safe way of effecting such a substitution.

Returning now to GREGORY, he also attempted to popularize the calculus of operations in his textbook, *Examples of the Processes of the Differential and Integral Calculus*, which first appeared in 1841, and then in a slightly revised version in 1846. In the preface he noted that he had used the method of the "Separation of the Symbols of Operation" extensively, since it not only shortens and simplifies problems, but also because it "offers to the student one of the most instructive examples of Analytical Generalization." Furthermore, he added, any idea that it is not rigorous "is formed on a limited view of the nature of Analysis ... ."[117] He used the technique both in developing a theory of fractional indices of differentiation and also in the chapter on "General theorems in the differential calculus." GREGORY gave as his major sources both SERVOIS and ROBERT MURPHY, whose work will be discussed below.

Despite GREGORY'S lapses in rigor, he did obtain many interesting results, and his efforts led to the acceptance of symbolic methods as legitimate by many English mathematicians. Even more important, as we shall see, his theoretical explanations were to shape his views on the nature of mathematics.

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114 JOHN YOUNG, "On the extension of the theorem of Leibniz to integration," *Phil. Mag.*, 1848, **33**: 337.

115 CHARLES GRAVES, "On the calculus of operations," *Phil. Mag.*, 1849, **34**: 60.

116 *Ibid.*, 62.

117 DUNCAN GREGORY, *Examples of the Processes of the Differential and Integral Calculus*. 2nd ed. rev. ed. W. WALTON. Cambridge, 1846: iv.

GREGORY had restricted himself to the study of equations with constant coefficients, as in fact, he was forced to by his theory. This is because he assumed that all the operations he was dealing with commuted with each other, and while

$$b\frac{dy}{dx}=\frac{d(by)}{dx} \tag{4.15}$$

if b is constant, this is not so if the multiplier is variable. Hence the next step required was a study of noncommutative operations. Such a work was published in 1837 by ROBERT MURPHY. MURPHY, an Irishman, had a short, tempestuous career. From a lower class background he was sent to Cambridge in 1825 on a scholarship which was raised by subscription. His academic career was successful—B. A. as Third Wrangler in 1829—and then a fellow. Unfortunately he was not careful about money, and he was forced to leave in 1832 with his fellowship in sequestration for his creditors. After some years in Ireland he went to London. In 1838 he became examiner in Mathematics and Natural Philosophy at the University of London, where he remained until his death in 1843 at the age of 37.

Although he was not prolific, his work shows extreme originality. The memoir which is relevant here, "First memoir on the theory of analytical operations," clearly illustrates this. The subject matter of the treatise is the theory of linear operations, which MURPHY defines as follows: "Linear operations in analysis are those of which the action on any subject is made up by the several actions on those parts connected by the sign + or −, of which the subjects is composed."[118] He was careful to define what it meant to call two operations equal. Writing the "subject", as he called it, to the left of the operation, MURPHY noted that if we define the operations ψ and Δ by $[x]\,\psi=x+h$, $[u]\,\Delta=u(x+h)-u(x)$, then

$$\begin{aligned}[u]\,(\psi-1)&=[u]\,\Delta\\ [u]\,(\Delta+1)&=[u]\,\psi\end{aligned} \tag{4.16}$$

for any function u; and, he added[119]

> When general relations such as these, between different symbols exist independently of the particular value of the subject, we may abstract the consideration of the latter, and the sign = between symbols of operation being understood to indicate that they are universally equivalent, the symbols ... would have the following relations ...

$$\psi-1=\Delta \quad \text{and} \quad \Delta+1=\psi. \tag{4.17}$$

MURPHY did not use the terminology of SERVOIS, but rather referred to two operations θ and θ' which satisfied $\theta\theta'=\theta'\theta$ as "relatively free" while those for which $\theta\theta'\neq\theta'\theta$ were said to be "relatively fixed."[120]

[118] ROBERT MURPHY, "First memoir on the theory of analytical operations," *Phil. Trans.*, 1837, **127**: 181. That is, θ is linear if $(x\pm y)\,\theta=x\,\theta\pm y\,\theta$. Modern analysts include the condition $(a x)\,\theta=a(x\,\theta)$ for all scalars a. The two are equivalent if θ is continuous. MURPHY did assume the second condition was satisfied as well.

[119] *Ibid.*, 180.

[120] *Ibid.*, 182.

MURPHY proved that the sum and composite (he called it the compound) of two linear operations is linear. In regard to the binomial theorem he believed, like GREGORY, that it applied to any two operations which were relatively free, even when the exponent is negative or non-integral. In general, in applications he ignored problems of convergence and had no hesitation about considering infinite series of arbitrary operations. But his treatment of inverses was quite sophisticated and avoided the pitfalls into which many of his contemporaries were to fall due to the general indeterminateness of the inverse. He defined the inverse θ^{-1} of the operation θ by the equation

$$[y]\,\theta^{-1} = u \quad \text{when} \quad [u]\,\theta = y, \tag{4.18}$$

and he proved not only that the inverse of a linear operation was itself linear but also that to invert a composite operation one must invert not only the nature, but also the order of the constituents, *i.e.* that $(\theta\psi)^{-1} = \psi^{-1}\,\theta^{-1}$.

MURPHY'S discussion of the inverse centered around what he called the appendage of a linear operation. He pointed out that if P is a subject such that $[P]\,\theta = 0$, where θ is a linear operation, then if $[x]\,\theta = y$, we have also $[x+P]\,\theta = y$ so that $[y]\,\theta^{-1} = x + P$. He went on to say[121]

> The appendage, therefore, in a linear operation is the result of its action on zero; P will express a *form*, but its magnitude must be susceptible of an infinity of values, that is, it contains arbitrary constants which enter as multipliers, for if a be such a constant, we have in general $[X]\,a\theta = [X]\,\theta a$ and supposing $X = 0$, we have $[0\,a]\,\theta = [0]\,\theta a$; therefore whatever value is given to $[0]\,\theta$, a more comprehensive value is attained by its arbitrary multiplication by a.

A good way to illustrate MURPHY'S theory is to look at an example. He considered the operation ψ_x (defined above) and said that if it acts on a function of two variables, say x and y, the appendage is $f(y)$, an arbitrary function of y; but when it acts on x alone, there is no appendage at all, since in that case $[u]\,\psi_x = 0$ implies $u(x+h) = 0$ for all values of x and hence that $u = 0$.

MURPHY anticipated some other problems of interest in later developments of transformation theory as well. He studied the form $(\theta - \theta^1)^{-1}$ when θ and θ^1 do not commute, and also the equation $\theta\iota = \iota\eta$ given ι and either θ or η, that is the determination of $\theta = \iota\eta\iota^{-1}$ or $\eta = \iota^{-1}\theta\iota$. He studied the operation e^θ defined by

$$e^\theta = 1 + \theta + \frac{\theta^2}{1\cdot 2} + \frac{\theta^3}{1\cdot 2\cdot 3} + \cdots \tag{4.19}$$

where θ is a linear operation, and showed that if θ and θ' are commutative then

$$e^\theta \cdot e^{\theta'} = e^{\theta+\theta'}. \tag{4.20}$$

The most striking aspect of MURPHY'S work is the extreme degree of generality and abstraction. Although in regard to applications to the calculus, the rigor is marred by the lack of attention to problems of convergence, the paper is a valuable

[121] *Ibid.*, 188–189.

contribution to the algebra of linear operations. In fact, in an account of the abstract theory of linear operations, S. PINCHERLE, who himself did research in that subject, discussed many of MURPHY's results and praised the paper highly.[122]

MURPHY's work influenced GEORGE BOOLE who used his approach in order to extend GREGORY's studies of linear equations to the case of variable coefficients. BOOLE is of course best known for his work in logic. And we will see, this work was related to his mathematical studies. One of BOOLE's earliest works, inspired by GREGORY's paper on linear differential equations, was a simplification of the latter's method for the solution of equations with constant coefficients. BOOLE began by assuming that $a_1, \ldots, a_n$ are distinct roots of

$$m^n + A_1 m^{n-1} + A_2 m^{n-2} + \cdots + A_n = 0. \tag{4.21}$$

But rather than factoring the corresponding differential equation into linear factors, as GREGORY and others had done, BOOLE used the partial fraction expansion; in other words, he wrote

$$\begin{aligned} u &= \left(\frac{d^n}{dy^n} + A_1 \frac{d^{n-1}}{dy^{n-1}} + \cdots + A_n\right)^{-1} X \\ &= \left\{N_1\left(\frac{d}{dx} - a_1\right)^{-1} + N_2\left(\frac{d}{dx} - a_2\right)^{-1} + \cdots + N_n\left(\frac{d}{dx} - a_n\right)^{-1}\right\} X, \end{aligned} \tag{4.22}$$

where $N_1, N_2, \ldots, N_n$ are the constants which occur in the partial fraction expansion of

$$\frac{1}{(m-a_1)(m-a_2)\ldots(m-a_n)}. \tag{4.23}$$

In justifying this method BOOLE wrote[123]

> the method of the resolution of this into a sum of partial fractions is independent of any properties of the variable, except the three which have been shown by Mr. Gregory (Vol. I, p. 31) to be common to the symbol $\frac{d}{dx}$, and to the algebraical symbols generally supposed to represent numbers.

But BOOLE's most important work in the calculus of operations was his paper "On a general method of analysis" published in 1844. This work, which won the Mathematical Medal of the Royal Society, was very significant, both in influencing later workers in the calculus of operations and in shaping BOOLE's views on the nature of mathematics. BOOLE opened his paper by noting that "much attention has of late been paid to a method in analysis known as the calculus of operations, or as the method of the separation of symbols." He went on to say that GREGORY "had both clearly stated the principles on which the method is founded, and shown its utility."[124] The other sources cited by BOOLE are MURPHY, SERVOIS and DEMORGAN (his work will be discussed below). The object of BOOLE's work was to apply symbolical methods to the solution of linear

[122] S. PINCHERLE, "Equations et opérations fonctionelles," *Encyclopédie des sciences mathématiques pures et appliquées*. ed. J. MOLK, Tome 2, vol. 5, 5 and 8–9.

[123] GEORGE BOOLE, "On the integration of linear differential equations with constant coefficients," *Camb. Math. J.*, 1841, **2**: 115.

[124] GEORGE BOOLE, "On a general method of analysis," *Phil. Trans.*, 1844, **134**: 225.

equations with variable coefficients. In order to do this he had to remove GREGORY'S assumption that all the operations that can be considered must commute. In his own words[125]

> The object of this paper is to develop a method in analysis, which while it operates with symbols apart from their subjects, is nevertheless free from restrictions.

The first section of the paper gives the requisite "symbolical" background. Assuming that the function $f(x)$ can be developed in a power series in x, BOOLE finds a series expansion for $f(\pi+\varrho)$ in ascending powers of ϱ, under the assumption that π and ϱ are distributive operations which combine according to the law

$$\varrho f(\pi) = \lambda f(\pi)\,\varrho \tag{4.24}$$

where λ acts on π so that $\lambda f(\pi) = f(\varphi\pi)$. The result is

$$\begin{aligned} f(\pi+\varrho) &= \Sigma f_m(\pi)\,\varrho^m u, \\ f_m(\pi) &= \frac{(\lambda-1) f_{m-1}(\pi)}{(\lambda^m-1)\,\pi}, \qquad f_0(\pi) = f(\pi). \end{aligned} \tag{4.25}$$

He also showed that

$$\begin{aligned} f(\pi)\,\varrho^m\,u &= \varrho^m f(\pi+m)\,u \\ f(\pi)\,\varrho^m &= \varrho^m f(m) \\ \pi(\pi-1)\ldots(\pi-n+1)\,u &= x(x+r)\ldots\big(x+(n-1)\,r\big)\left(\frac{\Delta}{\Delta x}\right)^n u. \end{aligned} \tag{4.26}$$

In the second equation, the subject u is equal to 1, and in the third $r=\Delta x$. Then if one lets $r=0$ (tend to zero to be more exact), $\pi = x\frac{d}{dx}$, $\varrho = x$ or $x=e^\theta=\varrho$, $\pi = \frac{d}{d\theta} = D$, these equations become

$$\begin{aligned} f(D)\,e^{m\theta}\,u &= e^{m\theta} f(D+m)\,u && \text{(a)} \\ f(D)\,e^{m\theta} &= e^{m\theta} f(m) && \text{(b)} \\ D(D-1)\ldots(D-n+1)\,u &= x^n\left(\frac{d}{dx}\right)^n u. && \text{(c)} \end{aligned} \tag{4.27}$$

These last three equations, BOOLE noted, were already known. But, he said, for the sake of the "maintainence of an unbroken analogy, it has been thought better to deduce them from the properties of the more general system in π and ϱ."[126] Other applications were derived by letting

$$\pi = \frac{x\,e^{r\frac{d}{dx}} - x}{r}, \qquad \varrho = x\,e^{r\frac{d}{dx}}. \tag{4.28}$$

The equations (27) were those which he used for his most general result. In particular, from (27c) he derived his basic theorem[127]

125 *Ibid.*, 226.
126 *Ibid.*, 232.
127 *Ibid.*, 282.

Every linear differential equation which can, with or without expansion of its coefficients, be placed in the form

$$(a+bx+cx^2+\cdots)\frac{d^n u}{dx^n}+(a'+b'x+c'x^2+\cdots)\frac{d^{n-1}u}{dx^{n-1}}+\cdots=X$$

may be reduced to the *symbolical* form

$$f_0(D)\,u+f_1(D)\,e^{\theta}\,u+f_2(D)\,e^{2\theta}\,u+\cdots=U \qquad \text{(VIII)}$$

wherein $f_0, f_1, f_2, \ldots$ are functional symbols, and U is a function of e^{θ}.

BOOLE then gave a general method for solving equations of the form (VIII) in which he made use of equation (27b).

By assigning different values to π and ϱ, BOOLE was able to solve other types of equations as well. In particular, letting

$$\pi=x-xe^{-\frac{d}{dx}}, \qquad \varrho=xe^{-\frac{d}{dx}}$$

or $\qquad\qquad\qquad\qquad\qquad\qquad$ (4.29)

$$\pi=x-n\,\varphi(x)\,e^{-\frac{d}{dx}}, \qquad \varrho=\varphi(x)\,e^{-\frac{d}{dx}},$$

BOOLE studied finite difference equations. Clearly BOOLE was in this work studying an abstract system, in that his symbols were considered only from the point of view of their laws of combination in deriving the basic equations. BOOLE ended the paper by saying that such considerations were very important and had a wide range of applicability. He wrote[127]

> Fearful of extending this paper beyond its due limits I have abstained from introducing any researches not essential to the development of that general method of analysis which it was proposed to exhibit. It may however be remarked that the principles on which the method is founded have a much wider range. They may be applied to the solution of functional equations, to the theory of expansions, and to a certain extent, to the integration of nonlinear differential equations. The position which I am most anxious to establish is, that any great advances in the higher analysis must be sought for by an increased attention to the laws of the combination of symbols. The value of this principle can scarcely be overrated.

BOOLE's own later writings include some further work along these lines. He considered again the problem of the expansion of $f(\pi+\varrho)$ in powers of ϱ, changing his assumption on the laws of combination of the distributive operations π and ϱ.[128] He also applied his method to several differential equations of physics.[129]

BOOLE wrote two texts which were very widely used and in fact are still in print, one on differential and the other on finite difference equations. In both

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[128] GEORGE BOOLE, "On the theory of developments. Part I.," *Camb. Math. J.*, 1845, **4**: 214–223.

[129] GEORGE BOOLE, "On the equation of Laplace's function," *Camb. and Dub. Math. J.*, 1846, **1**: 10–22; "On the attraction of a solid of revolution on an external point," *Camb. and Dub. Math. J.*, 1847, **2**: 1–7 and "On the differential equations of dynamics," *Phil. Trans.*, 1863, **153**: 485–501.

works he devoted considerable space to what he called symbolical methods.[130] What he meant by this term, BOOLE explained in a paper "On a certain symbolical equation" which appeared in 1847. By definition, he wrote, the terms symbolical equation and symbolical solution mean that the results are such that "Their validity does not depend on the significance of the symbols which they involve, but only on the truth of the laws of their combination."[131] As we shall see, BOOLE's work in the calculus of operations was intimately connected with his views on logic and the nature of mathematics. It also influenced a number of (mainly obscure) British mathematicians to work along the lines he set out, though not all with the same care as BOOLE, particularly with respect to arbitrary constants and functions which might have to be introduced. During the 1840's and 1850's the pages of the *Cambridge and Dublin Mathematical Journal* (this superseded the *Cambridge Mathematical Journal* in 1846), the *Philosophical Magazine* and the *Philosophical Transactions* were filled with attempts to apply and extend BOOLE's results.

Some of the problems encountered are illustrated in the work of the Reverend BRICE BRONWIN, one of the earliest and most prolific adherents to the new method. In his first paper on this subject, "On the integration and transformation of certain differential equations" BRONWIN used the notation $D=\frac{d}{dx}$ and the equations[132]

$$\begin{aligned} &x\,D^n y = D^n\,xy - n\,D^{n-1}y\\ &x^2\,D^n y = D^n\,x^2 y - 2n\,D^{n-1}xy + n(n-1)\,D^{n-2}y \qquad (4.30)\\ &x^3\,D^n y = D^n\,x^3 y - 3n\,D^{n-1}x^2 y + 3n(n-1)\,D^{n-2}xy - n(n-1)\,D^{n-3}y\\ &\vdots \end{aligned}$$

which he claimed were true for both positive and negative values of n in order to solve the equation

$$x\left(\frac{d^2y}{dx^2}+K^2y\right)+2p\frac{dy}{dx}=0, \quad p \text{ a positive integer.} \qquad (4.31)$$

Letting $y=(D^2+K^2)^p\,u$, he reduced the equation to the form

$$\frac{d^2u}{dx^2}+K^2\,u=0 \qquad (4.32)$$

and obtained as his solution

$$y=(D^2+K^2)^{p-1}0. \qquad (4.33)$$

This result was criticised by BOOLE, who noted first that BRONWIN's result was a special case of the theory in his 1847 paper on symbolical equations. BRONWIN's transformations, he noted, depended on the properties of two compound factors

[130] GEORGE BOOLE, *A Treatise on Differential Equations*. 5th ed. Reprint from 4th ed. 1877. With supplementary volume. ed. I. TODHUNTER. New York, 1959: 381–460 and 675–699; *The Calculus of Finite Differences*. 4th ed. Reprint from 3rd ed. 1880. ed. J. MOULTON. New York, n.d.: 236–263.

[131] GEORGE BOOLE, "On a certain symbolical equation," *Camb. and Dub. Math. J.*, 1847, **2**: 7.

[132] BRICE BRONWIN, "On the integration and transformation of certain differential equations," *Phil. Mag.*, 1846, **29**: 494–500.

π_m and ϱ which combine according to the law (1) $\pi_m \varrho = \varrho \pi_{m-1}$, where the equation to be solved was $\pi_m u = 0$. Based on his earlier work, BOOLE added, the general solution was seen to be

$$u = \varrho^m \pi_0^{-1} \varrho^{-m} 0, \tag{4.34}$$

which is true "whatever may be the interpretation of the symbols π_m and ϱ provided that they satisfy the combination law (1)."[133] This applied to BRONWIN's problem (letting $p = m$ and $y = u$) where

$$\pi_m = x(D^2 + K^2) + 2m\,D, \qquad \varrho = D^2 + K^2.$$

Hence the solution to the equation, using (33), is

$$u = (D^2 + K^2)^m \{x(D^2 + K^2)\}^{-1} (D^2 + K^2)^{-m} 0, \tag{4.35}$$

and, says BOOLE, when $m > 0$ it is necessary to retain exactly two of the constants which arise from $(D^2 + K^2)^{-m}$; BRONWIN's solution, he stated, lacks both the x^{-1} and these constants.

In a later paper, BRONWIN corrected has method to take care of BOOLE's criticism.[134] He became more sophisticated in the use of BOOLE's methods and published several papers on applications to ordinary and partial differtial equations and finite difference equations.[135] BRONWIN clearly acknowledged his debt to BOOLE. In an 1848 paper "On some theorems of use in the integration of linear differential equations," he wrote[136]

> within the last few years or since we have become acquainted with the theorems
>
> $$f(D+a)\,y = e^{-ax} f(D)\, e^{ax}\, y$$
>
> $$f(D+\varphi)\,y = e^{-\varphi} f(D)\, e^{\varphi}\, y$$
>
> (φ a function of x) the latter theorem given by Mr. Boole in the *Mathematical Journal*, the method of integrating Linear Differential Equations has undergone a great change.

Another mathematician influenced by BOOLE was CHARLES HARGREAVE. HARGREAVE was a lawyer by profession, but he published several interesting papers in mathematics. His first publication in the calculus of operations was a criticism of BRONWIN's work. He made the same observation that BOOLE had,

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133 GEORGE BOOLE, "On the Rev. B. Bronwin's method for differential equations," *Phil. Mag.*, 1847, **30**: 7.

134 BRICE BRONWIN, "On a new method of integrating linear differential equations," *The Mathematician*, 1847. Reprint, 1856: 204–208.

135 BRICE BRONWIN, "On the integration of certain differential equations," *Camb. and Dub. Math. J.*, 1846, **1**: 154–160; "On the integration of certain equations in finite differences," *Ibid.*, 1847, **2**: 42–47; "On some theorems of use in the integration of linear differential equations," *Ibid.*, 1848, **3**: 35–43; "On the solution of linear differential equations," *Phil. Trans.*, 1851, **141**: 461–482 and "On the integration of linear differential equations," *Phil. Mag.*, 1851, **2**: 477–483; 1852, **3**: 187–192.

136 BRONWIN, "On some theorems," 35.

and he obtained the same solution, though his method was different.[137] HARGREAVE'S most important work is found in the paper "On the solution of linear differential equations", which won the Royal Society medal in mathematics in 1848. The first section of the paper is headed "General theorems in the calculus of operations."[138] Letting D represent differentiation with respect to the independent variable, HARGREAVE'S basic theorems are

$$\begin{aligned}\varphi(D)\{\psi x \cdot u\} &= \psi(x)\,\varphi(D)\,u + \psi'(x)\,\varphi'(D)\,u \\ &\quad + \frac{1}{2}\psi''(x)\,\varphi''(D) + \frac{1}{2\cdot 3}\psi'''(x)\,\varphi'''(D) + \cdots \\ \varphi(x)\,\psi(D)\,u &= \psi(D)\{\varphi(x)\cdot u\} - \psi'(D)\{\varphi'(x)\cdot u\} \\ &\quad + \frac{1}{2}\psi''(D)\{\varphi''(x)\cdot u\} - \frac{1}{2\cdot 3}\psi'''(D)\{\varphi'''(x)\cdot u\} + \cdots\end{aligned} \tag{4.36}$$

where φ and ψ are functions which can be developed in ascending or descending integral powers of D. Clearly, if this is not so, the formulas do not make sense, or, in HARGREAVE'S term, are not interpretable. HARGREAVE did not worry about convergence, and probably led by the analogy with the role of imaginary numbers in trigonometry as it was then understood, he, like BOOLE, assumed that in the course of calculation it was only necessary that the end result be interpretable. He wrote "we shall therefore not hesitate to pronounce any interpretable result derived from the free use of these theorems true, although the intermediate steps of the process are not capable of rational interpretation."[139]

HARGREAVE used these theorems to solve linear differential equations, both in the above paper and in one published in 1850. Many of the examples were taken from BOOLE'S 1844 paper, though, as HARGREAVE noted, the methods were different.[140]

The form of the equations given above, particularly the use of $\varphi'(D)$, $\varphi''(D)$, etc., suggested to HARGREAVE some general results in the calculus of operations. He denoted by ∇ the operation of deriving $\varphi'(D)$ from $\varphi(D)$. The symbol ∇ thus had the same relation to D that D had to x, and, he wrote,[141]

> if we apply the established theorems of the Calculus of Operations to the new symbol, we shall acquire enlarged and more convenient forms for the expression of complex operations; ...

HARGREAVE noted that ∇ obeyed the index laws, using the terminology of GREGORY, and that it was distributive and commutative with respect to x or

[137] CHARLES HARGREAVE, "Observations on the Rev. B. Bronwin's paper on the integration and transformation of certain differential equations," *Phil. Mag.*, 1847, **30**: 8–10.

[138] CHARLES HARGREAVE, "On the solution of linear differential equations," *Phil. Trans.*, 1848, **139**: 31–54.

[139] *Ibid.*, 31.

[140] CHARLES HARGREAVE, "General methods in analysis for the resolution of linear equations in finite differences and linear differential equations," *Phil. Trans.*, 1850, **141**: 261–286.

[141] CHARLES HARGREAVE, "Applications of the calculus of operations to algebraical expansions and theorems," *Phil. Mag.*, 1853, **6**: 352.

any function of x, but that it possessed neither of these properties with respect to D.

HARGREAVE and BRONWIN were most concerned with that aspect of BOOLE'S work which dealt with operations of the calculus. However the general study of the laws of combination of symbols and their consequences contained in BOOLE'S work was not overlooked by his contemporaries. An interesting paper on this topic was written by WILLIAM DONKIN, Savilian professor of astronomy at Oxford in 1850. In this work DONKIN studied symbols of operation ϱ and ω which combined according to the following laws:[142]

$$\begin{aligned} \omega\varrho - \varrho\omega &= \varrho_1 \\ \omega\varrho_1 - \varrho_1\omega &= \varrho_2 \\ &\vdots \\ \omega\varrho_n - \varrho_n\omega &= \varrho_{n+1}. \end{aligned} \tag{4.37}$$

Assuming that $f(x)$ is a function which can be developed in integral powers of x, he derived series expansions for

$$f(\omega)\,\varrho, \quad \varrho f(\omega), \quad f\left(\omega + \frac{1}{\varrho}\,\varrho_1\right), \quad f\left(\omega + \varrho_1 \frac{1}{\varrho}\right)$$

in terms of ϱ_i. For example,

$$f(\omega)\,\varrho = \varrho f(\omega) + \frac{\varrho_1}{1} f'(\omega) + \frac{\varrho_2}{1\cdot 2} f''(\omega) + \cdots. \tag{4.38}$$

He applied his results to the differential calculus, letting $\omega = D$, $\varrho = x$, and to finite differences with $\omega = e^D$, $\varrho = \sqrt{x}$.

Another follower of BOOLE who was interested in the study of non-commutative symbols of operation was W. H. L. RUSSELL. His first work in the calculus of operations, which appeared in the 1854 volume of the *Cambridge and Dublin Mathematical Journal* was concerned only with applications.[143] But his major effort in that field, a series of three papers which appeared in the *Philosophical Transactions* in 1862–1863, was very much symbolically oriented. RUSSELL'S account of the genesis of the method is interesting in that he gives major credit to LAPLACE. It is a good description of the growth of the method and BOOLE'S key role. RUSSELL wrote[144]

> The calculus of generating functions, discovered by Laplace, was, as is well known highly instrumental in calling the attention of mathematicians to the analogy which exists between differentials and powers. This analogy was perceived at length to involve an essential identity, and several analysts devoted themselves to the improvement of the new methods of calculation which were thus called into existence. For a long time the modes of combination assumed to exist between different classes of symbols were those of ordinary

[142] WILLIAM DONKIN, "On certain theorems in the calculus of operations," *Camb. and Dub. Math. J.*, 1850, **5**: 10.

[143] W. H. L. RUSSELL, "On the integration of linear differential equations," *Camb. and Dub. Math. J.*, 1854, **9**: 104–112.

[144] W. H. L. RUSSELL, "On the calculus of symbols, with applications to the theory of differential equations," *Phil. Trans.*, 1861, **151**: 69.

algebra; and this sufficed for investigations respecting functions of differential coefficients and constants ... The laws of combination of ordinary algebraical symbols may be divided into the commutative and distributive laws; and the number of symbols in the higher branches of mathematics which are commutative with respect to one another is very small. It became then necessary to invent an algebra of non-commutative symbols. This important step was effected by Professor Boole ....

RUSSELL's own studies began with a consideration of "functional" symbols π and ϱ which combine according to

$$\varrho^n f(\pi)\, u = f(\pi - n)\, \varrho^n u. \tag{4.39}$$

If P, Q and R are functions of π and ϱ such that $PQ = R$, RUSSELL called P an external and Q an internal factor of R. He then studied the question of when a given function was an internal or external factor of some expression. For example, he asked when an expression of the form $\varrho\,\psi_1(\pi) + \psi_0(\pi)$ is an internal or external factor of

$$\varrho^n\, \varphi_n(\pi) + \varrho^{n-1} \varphi_{n-1}(\pi) + \cdots + \varrho\, \varphi_1(\pi) + \varphi_0(\pi). \tag{4.40}$$

RUSSELL applied his results to the integration of linear differential equations, letting $\varrho = x$ and $\pi = x \frac{d}{dx}$. Thus, the equation

$$x^2(x+1)^3 \frac{d^3 u}{dx^3} + 3\,x(x+1)^3 \frac{d^2 u}{dx^2} + (x^3 + 4x^2 + 3x) \frac{du}{dx} - (2x - 3)\, u = X_\chi \tag{4.41}$$

takes the "symbolical form"

$$\varrho^3 \pi^3 u + \varrho^2 (3\pi^3 + \pi - 1)\, u + 3\varrho(\pi^3 + 1)\, u + \pi(\pi^2 - 1)\, u = X_\chi. \tag{4.42}$$

RUSSELL's theory then enabled him to factor (42), which gave him

$$\big(\varrho(\pi - 2) + \pi\big)\big(\varrho(\pi - 1) + (\pi + 1)\big)(\varrho\pi + \pi - 1)\, u = X_\chi;$$

or

$$\left\{(x^2 + x)\frac{d}{dx} + 2x\right\}\left\{(x^2 + x)\frac{d}{dx} - (x-1)\right\}\left\{(x^2 + x)\frac{d}{dx} - 1\right\} u = X_\chi. \tag{4.43}$$

And the latter equation he was able to integrate.

In the second paper, RUSSELL used the same laws for his symbols π and ϱ and studied questions like how to extract the square root of certain symbolical expressions and ways to find the highest common internal factor of two such expressions. The latter gave him a method for determining when two linear differential equations had a common solution.[145] The last paper of the series contained the consideration of symbols of operation which combined according to a slightly different law. Again it was concerned mainly with problems of "factoring" symbolical equations.[146]

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[145] W. H. L. RUSSELL, "On the calculus of symbols, II," *Phil. Trans.*, 1862, **152**: 253–272.

[146] W. H. L. RUSSELL, "On the calculus of symbols, III," *Phil. Trans.*, 1863, **153**: 517–523.

RUSSELL'S work was extended by WILLIAM SPOTTISWOODE. He studied the question of when $\psi_1(\varrho)\,\pi + \psi_0(\varrho)$ is an internal or external factor of

$$\varphi_n(\varrho)\,\pi^n + \varphi_{n-1}(\varrho)\,\pi^{n-1} + \cdots + \varphi_0(\varrho) \tag{4.44}$$

and also how to determine the quotient. He also used more complex divisors, and like his predecessors applied his work to the solution of differential equations.[147] Although all these men applied their work to the calculus, we clearly can consider parts of it to be, as RUSSELL himself called it, the study of a "non-commutative algebra."

As we have seen, GREATHEED and GREGORY had applied the method of the separation of symbols to partial differential equations. But, although BOOLE had stated his basic theorems for functions of several variables, he himself applied them only to the ordinary case. The first systematic extension of BOOLE'S method to this subject was supplied by ROBERT CARMICHAEL, who was a fellow at Trinity College, Dublin. He began his studies with a paper which appeared in 1851. He introduced what he called the index symbol ∇, which he defined as follows:[148]

$$\nabla = x_1 \frac{d}{d x_1} + x_2 \frac{d}{d x_2} + \cdots + x_n \frac{d}{d x_n}. \tag{4.45}$$

He pointed out that if $u_m = F(x_1, x_2, \ldots, x_n)$ is a homogeneous function of degree m, EULER'S theorem can be stated as

$$\nabla u_m = m u_m. \tag{4.46}$$

But then, applying the calculus of operations, $\nabla^\varrho u_m = m^\varrho u_m$ and hence $f(\nabla)\, u_m = f(m)\, u_m$. (Assuming tacitly that f can be developed in a power series.) CARMICHAEL'S work was based on the mistaken belief that $\frac{d}{d x_1}, \frac{d}{d x_2}, \ldots, \frac{d}{d x_n}$ were always pairwise commutative. He justified this principle by noting that it must be true since, for example, $x_2, x_3, \ldots$ are constant relative to x_1. CARMICHAEL utilized the extension of BOOLE'S theorems in the calculus of operation to the index symbol ∇. Thus, for example, BOOLE'S result

$$D(D-1)\ldots\big(D-(n-1)\big) = x^n \left(\frac{d}{dx}\right)^n \tag{4.47}$$

became

$$\nabla(\nabla - 1)\ldots(\nabla - n + 1) = \nabla_n = \nabla^n. \tag{4.48}$$

He applied his theorems to the solution of both ordinary and partial differential equations.

CARMICHAEL also extended some results in the calculus of operations due to his countryman CHARLES GRAVES, who, as we have seen, was a staunch defender of the calculus of operations. GRAVES had studied the meaning of expressions

[147] WILLIAM SPOTTISWOODE, "On the calculus of symbols," *Phil. Trans.*, 1862, **152**: 99–120.

[148] ROBERT CARMICHAEL, "On the index symbol of homogeneous functions," *Camb. and Dub. Math. J.*, 1851, **6**: 277–284; 1853, **8**: 81–91.

such as a^{xD_x} and e^{ψ} where ψ is a distributive function satisfying $\psi uv = u\psi v + v\psi u$.[149] CARMICHAEL generalized this by establishing interpretations for expressions of the form

$$e^{\varphi(x)\frac{d}{dx}+\psi(y)\frac{d}{dy}+\cdots} f(x, y, \ldots). \tag{4.49}$$

He applied his results primarily to finite difference equations in several variables.[150] In 1853 SPOTTISWOODE published an interesting generalization of the index symbol of CARMICHAEL.[151] However, in general, the application of the calculus of operations to partial differential equations was not particularly significant with respect to the development of general theories.

As we have seen, the calculus of operations had its roots in the symbolical expression of certain series expansions, for example the statement of TAYLOR'S theorem as $e^{h\frac{d}{dx}} f(x) = f(x+h)$ and its generalization by HERSCHEL $f(1+\Delta)\, u_x = f(e^{\Delta xD})\, u_x$. This last result was extended by WILLIAM ROWAN HAMILTON in 1837.[152] But HAMILTON'S most important work in the calculus of operations was his application of that subject to the evaluation of certain definite integrals. This subject had acquired considerable importance with the work of FOURIER, CAUCHY and FRESNEL, and in fact, from general developments in physics. As ROBERT ELLIS, biographer of GREGORY and cofounder with him of the *Cambridge Mathematical Journal* wrote in 1845:[153]

> Since the beginning of the century the general aspect of mathematics has greatly changed. ... the new requirements of natural philosophy have greatly influenced the progress of pure analysis. In the mathematical theories of heat, light, elasticity and magnetism an idea is prominent, which comparatively occurs but seldom in purely dynamical enquiries. This is the idea of discontinuity ... the power, therefore, of symbolizing discontinuity, ..., is essential to the progress of the more recent applications of mathematics to natural philosophy, and it is well known that this power is intimately connected with the theory of definite integrals.

In 1848 BRONWIN had done some work on the use of the calculus of operations for the evaluation of definite integrals, but his work was not rigorous.[154] HAMIL-

[149] CHARLES GRAVES, "On a generalization of the symbolic statement of Taylor's theorem," *Proc. Roy. Ir. Acad.*, 1850–1853, **5**: 285–287.

[150] ROBERT CARMICHAEL, "Theorems in the calculus of operations," *Camb. and Dub. Math. J.*, 1853, **8**: 165–171.

[151] WILLIAM SPOTTISWOODE, "On certain theorems in the calculus of operations," *Camb. and Dub. Math. J.*, 1853, **8**: 25–33.

[152] WILLIAM ROWAN HAMILTON, "On differences and differentials of zero," *Trans. Roy. Ir. Acad.*, 1837, **17**: 235–236.

[153] ROBERT ELLIS, "Biographical memoir of Duncan Farquharson Gregory," *Mathematical Writings of Duncan Farquharson Gregory*. ed. W. WALTON. Cambridge, 1865: xx–xxi.

[154] BRICE BRONWIN, "Application of certain symbolical representations of functions to integration," *Camb. and Dub. Math. J.*, 1848, **3**: 243–251.

TON's work along these lines, published in the *Philosophical Magazine* in 1857 was much more careful.[155] He used the operations

$$I_t = \int_0^t dt$$

$$J_t = \int_t^\infty dt \qquad (4.50)$$

$$K = I_t + J_t = \int_0^\infty dt$$

to study the series

$$F_{n,r}(t) = I_t^n (1 + x I_t^2)^{-r-\frac{1}{2}} 1 \qquad \text{(a)}$$

or (4.51)

$$F_{n,r}(t) = I_t^n (1 + x I_t^2)^{-r} f(t) \qquad \text{(b)}$$

where

$$f(t) = F_{0,0}(t) = (1 + x I_t^2)^{-\frac{1}{2}} 1 = \frac{2}{\pi} \int_0^{\frac{\pi}{2}} d\omega \cos(2t\omega). \qquad (4.52)$$

The integral (52), as HAMILTON pointed out, appears in the theory of heat and had been studied by FOURIER and POISSON. For $n > 0$ he defined the function $f_n(t)$ by the relationship

$$f_n(t) = \left(I_t^n - (-J_t^n)\right) f(t) = I_t^n f(t) - D_t^{-n} f(t). \qquad (4.53)$$

That $-J^n f(t) = D^{-n} f(t)$, he is careful to point out, is true since $f(t)$ vanishes at infinity. The bulk of the paper is concerned with the problem of finding approximations for $f_n(t)$. Although the paper ends with a "to be continued", HAMILTON did not publish anything further on the subject. However, he did not drop it either, and his further work is found in letters, dated December 1857, addressed to the English mathematician AUGUSTUS DEMORGAN, with whom he carried on a voluminous correspondance.

In these letters, HAMILTON obtained a number of theorems of which the following is representative:[156]

Theorem A: If we have two developments, ascending and descending

$$\psi x = a_0 - a_1 x + a_2 x^2 - \text{etc.} \qquad (1)$$

$$\psi x = b_1 x^{-1} - b_2 x^{-2} + b_3 x^{-3} - \text{etc.} \qquad (2)$$

for any function ψx, algebraical or transcendental; and if the following third series

$$f t = a_0 - a_1 t + \frac{a_2 t^2}{1 \cdot 2} - \frac{a_3 t^3}{1 \cdot 2 \cdot 3} + \text{etc.} \qquad (3)$$

[155] WILLIAM ROWAN HAMILTON, "On the calculation of the numerical values of a certain class of multiple and definite integrals," *Phil. Mag.*, 1857, **14**: 375–382.

[156] HAMILTON to DEMORGAN, December 4, 1857. Sir WILLIAM ROWAN HAMILTON Papers, Trinity College, Dublin.

can be summed so as to give a function of t which is real, finite and continuous, with its diff[1] coeff[s] of all orders from $t=0$ to $t=\infty$, and which vanishes, with all those coeff[s], at this last limit; then

$$b_n = \int_0^\infty dt \left(\int_t^\infty dt \right)^{n-1} ft. \qquad (A)=(4)$$

In the course of discussing his theorems, HAMILTON used the notation of his paper, modified somewhat by dropping the subscript, for example writing $I = \int_0^t dt$. In this notation, the conclusion of the theorem became

$$b_n = K\, J^{n-1}\, ft. \qquad (4.54)$$

HAMILTON did a number of numerical verifications of this result and also sketched a proof, but he did not claim to complete rigor.[157] In the course of his discussion, he made considerable use of the calculus of operations, for example writing (1) and (2) in the form $\psi x = K(J+x)^{-1} ft$ and $\psi x = K(x+J)^{-1} ft$. While not complete, HAMILTON'S work is interesting in itself and also because it shows that HAMILTON was aware of the work of the English school, one of his approaches being to use the study of laws of combination of noncommutative symbols which could then be interpreted as his symbols I, J and K.

In the course of their correspondance, HAMILTON and DEMORGAN also mentioned the functional calculus. In September 1849 HAMILTON wrote that PEACOCK had remarked that there was an analogy between the calculus of functions and quaternions, and DEMORGAN replied: "What has not analogy with the functional calculus?"[158]

Actually, the calculus of functions, considered in BABBAGE'S sense, that is—seeking the solution to functional equations—did not, as BABBAGE had hoped it might, become a major area of research during the period we are studying, although some work was done. Thus in 1817 WILLIAM HORNER extended BABBAGE'S solution of $\psi^n x = x$ by showing that the general solution could be obtained from a particular solution in a way similar to that which BABBAGE had used in other cases.[159] And in 1840, WILLIAM WALLACE, who had been GREGORY'S teacher at the University of Edinburgh, studied the equation

$$f(x_0)\, f(x_1) = c\,[f(x_0+x_1) - f(x_0-x_1)], \qquad (4.55)$$

where x_0 and x_1 are any two fixed values of x and c is a constant.[160]

Both GREGORY and ELLIS wrote on functional equations. GREGORY generalized his and BOOLE'S methods for dealing with linear differential equations with

[157] HAMILTON to DEMORGAN, December 23, 1857. Sir WILLIAM ROWAN HAMILTON Papers, Trinity College, Dublin.

[158] ROBERT PERCEVAL Graves, *Life of Sir Willam Rowan Hamilton*. 3 vols. Dublin, 1882–1889: vol. 3, 277.

[159] WILLIAM G. HORNER, "Solution of the equation $\psi^n x = x$," *Ann. of Phil.*, 1817, **10**: 341–346.

[160] WILLIAM WALLACE, "Solution of a functional equation, with its application to the parallelogram of forces and to curves of equilabration," *Trans. R. S. of Edin.*, 1840, **14**: 625–676.

constant coefficients to functional equations of the form

$$\varphi(\omega^n x) + a_1 \varphi(\omega^{n-1} x) + a_2 \varphi(\omega^{n-2} x) + \cdots + a_n \varphi(x) = X, \qquad (4.56)$$

where ω is a known function φ is to be determined. He did this by introducing the operation π defined by $\pi \varphi(x) = \varphi(\omega x)$. He then showed that π satisfied the index law, the distributive law and commuted with constants. And, he wrote, since "these are the laws which are used in applying the method of the separation of symbols to the solution of linear differential equations; ... the same method may be applied to our functional equation."[161] ELLIS' work, published in the same volume of the *Mathematical Journal* was concerned with correcting and generalizing some results due to BABBAGE in solving functional equations involving derivatives.[162]

The most extensive work on the calculus of functions which appeared during the period we are considering was an article by AUGUSTUS DEMORGAN which was written for the *Encyclopedia Metropolitana* and published in 1843. In his paper, DEMORGAN included historical background and a critical evaluation of the problems and methods involved in the theory, as well as solutions of specific equations. DEMORGAN felt that the subject was important more for its methodology than for any specific results. Thus, he wrote that the advantage to the student of a study of the functional calculus is that it will lead him to "a knowledge of forms and familiarity with the results of general operations as will render his grasp of ordinary mathematical language more intellectual and less mechanical."[163] DEMORGAN cited as those who had studied the language of mathematics for its own sake BABBAGE, HERSCHEL and PEACOCK among English writers; CARNOT, CAUCHY and ARBOGAST among the French, and he added, while the subject was not studied much by the Germans such a list would probably include FUSS, ABEL and JACOBI.[164]

DEMORGAN defined the calculus of functions as follows:[165]

> Given a function, and a *general* form of another, required, (if possible) a *specific* form, such that it and the given function shall be reducible to identity by the same operations.

In this form, he noted, the origins of the calculus of functions was to be found in the works of EULER, LAMBERT, D'ALEMBERT, LAGRANGE, MONGE and LAPLACE. However, he added, it was the work of BABBAGE which gave the subject substance.[166] DEMORGAN discussed many examples taken from the work of BABBAGE and HERSCHEL. He was very much concerned with the difficulties which arise from the lack of uniqueness in the solutions of many types of equations. As we

[161] DUNCAN GREGORY, "On the solutions of certain functional equations," 1843, *Math. Writings*: 250.

[162] ROBERT ELLIS, "On the solution of functional differential equations," *Camb. Math. J.*, 1843, **3**: 131–138.

[163] AUGUSTUS DEMORGAN, "Calculus of functions," *Encyclopedia Metropolitana*. London, 1843: vol. 2, 305.

[164] *Ibid.*

[165] *Ibid.*, 306.

[166] *Ibid.*, 366.

shall see in the following section, DEMORGAN related many of these problems to general questions concerning the nature of mathematical laws.

In general, in considering the work of this period, one must agree with ELLIS, who remarked that "it cannot be denied, that hitherto the Calculus of Functions has not led to many results of much interest." But also, I think, one must agree with his conclusion that this does not detract from its importance. As he wrote, "Its value arises directly from the wide views it gives of the science of the combination of symbols."[167]

Another subject which, as we have seen, was considered as part of the calculus of operations is that of assigning a meaning to $\frac{d^\alpha}{dx^\alpha}$ when α is not integral. The Continental work, as well as that of HERSCHEL and BABBAGE has already been discussed. In England, following them, we find that PEACOCK devoted a fairly lengthy section of his *British Association Report* to the question, as he considered the theory to be an application of his principle of the permanence of equivalent forms. This principle, which is central to PEACOCK's theory of algebra states that[168]

> *Whatever algebraical forms are equivalent, when the symbols are general in form but specific in value will be equivalent likewise when the symbols are general in value as well as in form.*

This leads to an approach similar to that of BABBAGE and HERSCHEL, in that a formula proved for integral values is used to define the meaning for non-integral values. Thus, PEACOCK proceeds as follows. He noted that among those formulas which limit one or more of their variables in form is $1 \cdot 2 \cdot 3 \cdot \ldots \cdot r$, in which r must be an integer. Thus in writing

$$\frac{d^r x^n}{dx^r} = n(n-1)(n-2)\ldots(n-r+1)\,x^{n-r} \tag{4.57}$$

n is not limited, but r appears to be. However, says PEACOCK, since we can find a form of the coefficient in which r is not restricted, we can define $\frac{d^r x^n}{dx^r}$ where r is not integral. The form he gives is

$$\frac{d^r x^n}{dx^r} = \frac{\Gamma(1+n)}{\Gamma(1+n-r)}\,x^{n-r}. \tag{4.58}$$

PEACOCK was familiar with LIOUVILLE's work on this subject, but their theories differed in several significant points, in particular with regard to the problems involved when the Γ function becomes infinite in both numerator and denominator, and the nature of the complementary function.

During the 1840's there was, in Great Britain, a flurry of interest in fractional exponents of differentiation, clearly related to the general interest in the calculus of operations. In the first volume of the *Cambridge Mathematical Journal* GREATHEED criticized PEACOCK's approach and emphasized the fact that it was not

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[167] ELLIS, "Functional differential equations," 138.
[168] PEACOCK, "Report," 198.

the form of the formulas which is basic, but rather the rules governing their laws of combination.[169] That is, he took as the starting point for his theory the equations

$$\begin{aligned} \frac{d^\alpha(u+v)}{dx^\alpha} &= \frac{d^\alpha u}{dx^\alpha} + \frac{d^\alpha v}{dx^\alpha} \qquad &\text{(A)}\\ \frac{d^\alpha}{dx^\alpha}\,\frac{d^\beta}{dx^\beta}\,u &= \frac{d^{\alpha+\beta}u}{dx^{\alpha+\beta}} \qquad &\text{(B)}\\ \frac{d^\alpha}{dx^\alpha}\,\frac{d^\beta}{dy^\beta}\,u &= \frac{d^\beta}{dy^\beta}\,\frac{d^\alpha}{dx^\alpha}\,u. \qquad &\text{(C)} \end{aligned} \tag{4.59}$$

From (A) he is able to prove that

$$\frac{d^\alpha}{dx^\alpha}\,cu = c\,\frac{d^\alpha u}{dx^\alpha} \qquad \text{(D)}$$

for c rational, and he extends it to all real values "by assumption." Using this, he is able to prove LIOUVILLE's basic formula

$$\frac{d^\alpha e^{nx}}{dx^\alpha} = n^\alpha e^{nx} \tag{4.60}$$

and from this, obtain his general result

$$\frac{d^\mu}{dx^\mu}\,x^n = \frac{P(n)}{P(n-\mu)}\,x^{n-\mu}, \tag{4.61}$$

where $P(n)$, a generalization of the gamma function, is defined by the two properties $P(n) = n\,P(n-1)$ and $P(n)$ finite, except where n is a negative integer. His method, as he acknowledged, is derived from that of LIOUVILLE.

A similar approach was taken in 1840 by PHILIP KELLAND, professor of mathematics at the University of Edinburgh. In a short historical introduction to his work, KELLAND cites LAPLACE and FOURIER as the followers of LEIBNIZ and EULER in this subject. FOURIER's work, he noted, "showed how general differential coefficients might be deduced by means of definite integrals."[170] Like GREATHEED, he preferred LIOUVILLE's approach to that of PEACOCK. But his method was somewhat different. KELLAND assumed the formula (D), and then proved (A) and (B). The problem, as he sees it, is, then, to extend the Γ function. Like GREATHEED he does this by introducing a new function $\lceil P$, which is to agree with $\Gamma(P)$ whenever both are defined, and which is to satisfy $\lceil 1+P = P\lceil P$. His fundamental formula is then

$$\frac{d^\mu}{dx^\mu}\,\frac{1}{x^n} = \frac{1}{\lceil n}\,(-1)^\mu\,\frac{\lceil n+\mu}{x^{n+\mu}}. \tag{4.62}$$

KELLAND claimed that his work proved that PEACOCK's principle of the permanence of equivalent forms to be false, because the form of the differential coefficient was not unique. He argued that

$$\frac{d^\mu x^n}{dx^\mu} = A\,x^{n-\mu} \tag{4.63}$$

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[169] S. S. GREATHEED, "On general differentiation, numbers 1 and 2," *Camb. Math. J.*, 1839, **1**, 2nd ed. rev. 1846: 12–22, 120–128.

[170] PHILLIP KELLAND, "On general differentiation, I," *Trans. R. S. Edin.*, 1840, **14**: 567.

will always give

$$A = \frac{f(n+1)}{f(n-\mu+1)}, \tag{4.64}$$

but that the form of $f(n+1)$ is not necessarily $\lfloor n+1$—that, for example, it might also be $(-1)^n (\sin n\pi) \lfloor n+1$—since the only requirement is that $n\,f(n) = f(n+1)$. This reasoning is correct, if one assumes the laws (B) and (D). Hence if PEACOCK's principle is interpreted to mean that not only the form of the coefficient but also these laws must remain valid in the extended system, then KELLAND has shown it to be false.

Another example of this same general method is found in the work of the Reverend WILLIAM CENTER.[171] Like GREATHEED and KELLAND, he preferred the approach of LIOUVILLE to that of PEACOCK, and also felt that the difficulty lay in the proper generalization of the Γ function to be employed.

The relationship between this topic and the calculus of operations is clear in the work of these men. They all stated at some point that since in their system the symbols combine in accordance with the same laws as if the indices were integral, they could apply the methods of the calculus of operations to the solution of differential equations involving fractional indices and presented many examples.[172] The subject is quite complicated due to the lack of uniqueness of solutions, and the procedures employed were far from rigorous. However, their labors serve to illustrate the extent to which the British tried to push the calculus of operations and some of the difficulties encountered.

The popularity of the calculus of operations was enhanced by its appearance in elementary textbooks. I have already mentioned those of GREGORY and BOOLE. DEMORGAN, in his text, *On the Differential and Integral Calculus* which appeared in 1842, devoted some space to the calculus of operations. He discussed GREGORY's work on the foundation of the method of the separation of symbols, symbolic theorems like those of LAGRANGE and HERSCHEL, and the use of symbolic methods for the transformation of divergent series.[173] He also considered the question of fractional indices of differentiation, but here he concluded that the subject is "unsettled."[174]

DEMORGAN also taught the calculus of operations to his classes at the University College, London where he was professor of mathematics from 1828 to 1831 and then again from 1836 to 1866, both times resigning on a question of principle. DEMORGAN's syllabi are known inasmuch as he was in the habit of

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171 WILLIAM CENTER, "On the value of $(d/dx)^\theta x^0$, when θ is a positive proper fraction," and "On differentiation with fractional indices, and on general differentiation," *Camb. and Dub. Math. J.*, 1848, **3**: 274–285; **5**: 206–217.

172 GREATHEED, "General differentiation," 120–128; Kelland, "On general differentiation, III," *Trans. R. S. Edin.*, 1840, **14**: 250–284 and Center, "On differentiation," 275–276.

173 AUGUSTUS DEMORGAN, *The Differential and Integral Calculus*. London, 1842: 163–167, 308–310, 561.

174 *Ibid.*, 598.

preparing notebooks containing examples and illustrations of various topics for the use of his students. These books are deposited in the University of London library. There are two books, dated 1853 and 1854, labelled "Higher Seniors" which give a straightforward account of the theory, similar to the material found in the article on the calculus of functions.[175]

The first text devoted exclusively to the calculus of operations was written by ROBERT CARMICHAEL and published in 1855. In his introduction, CARMICHAEL wrote,[176]

> The Calculus of Operations, in the greatest extension of the phrase, may be regarded as that science which treats of the combination of symbols of operation conformably to certain laws, and of the relations by which these symbols are connected with the subject on which they operate.

The work consists mainly of a summary and some extensions of the memoirs discussed above. But there is one work which CARMICHAEL mentions as his major source, which we have not encountered before. This is *An Elementary Treatise on the Calculus of Variations*, published in Dublin in 1850 by the Reverend JOHN HEWITT JELLETT, who, like CARMICHAEL, was a graduate of Trinity College, Dublin. JELLETT treated that subject in the context of the calculus of operations as follows. A variable quantity, he wrote, is a function of others when its value depends on the value of the others. "The nature of the relation subsisting between the first, or dependent variable, and the others, or independent variables, is termed the *form* of the function."[177] And, he went on to say, the value of the dependent variable depends on two things: the values of the independent variable and the form of the function. The calculus of variations is primarily concerned with changes in the value which arise from a change in form. In particular, if the form of one function depends on the form of some other function (for example, the differential quotient), we say that the second function is derived from the first, and"... to investigate the change in a derived function, in consequence of a change in the form of its primitive is the object of the Calculus of Variations."[178] It is in this study of change of forms, considered as the result of an operation on the function, that the subject derives its relationship with the calculus of operations.

In summary: the calculus of operations, which had been imported from France and extended by BABBAGE and HERSCHEL, was a focal point for mathematical research in Great Britain between 1835 and 1865. In particular, as DEMORGAN pointed out, the *Cambridge Mathematical Journal* and its successor the *Cambridge and Dublin Mathematical Journal* were full of "symbolical reasonings." These included not only the papers discussed above, but also additional papers by

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[175] DEMORGAN Papers, Box XII. University of London, London.

[176] ROBERT CARMICHAEL, *A Treatise on the Calculus of Operations*. London, 1855: 1.

[177] JOHN HEWITT JELLETT, *An Elementary Treatise on the Calculus of Variations*. Dublin, 1850: 1.

[178] *Ibid.*, 1–2.

BOOLE, C. GRAVES and others.[179] Furthermore, interest in the subject did not end in the 1860's. There were additional works in Great Britain and increasingly by Continental mathematicians through the end of the century and into the twentieth century. The earlier work was made more rigorous and as it was better understood it led eventually to the modern theory of linear operations. A bibliography of this work, though unfortunately the citations are often not exact, can be found in articles by S. PINCHERLE. The role of integral equations in leading to the general theory of function spaces has been treated by MICHAEL BERNKOPF.[180]

To return to the summary of the early work in Great Britain, GREGORY'S explanation of the validity of the separation of symbols was accepted as correct by his contemporaries. For example, CARMICHAEL wrote; "Much of the importance now attributed to the Calculus of Operations is due to the vindication and illustration of its claims by that mathematician."[181] But GREGORY'S view of the subject was limited, inasmuch as he assumed that all the laws of algebra must be satisfied —in particular, that his operations commuted with constants—which limited him to equations with constant coefficients. Non-commutative operations were first studied by MURPHY, whose 1837 paper deserves an important place in the history of the algebra of linear transformations. The study of such operations was continued by BOOLE, who applied his results to the integration of linear differential equations with variable coefficients. This work was extended and applied by many other workers during the middle part of the century. As pointed out at the beginning of this section, the early work in the calculus of operations has been severely criticized for its lack of rigor. It is true that the unrestricted use of the method did lead at times to incorrect results, primarily because of the indeterminacy of the inverse operations and lack of attention to problems of convergence when the solutions occurred, as they often did, in the form of infinite series. However, the importance of the method should not be judged solely by the actual results obtained through its use. As we shall see, it was involvement with this subject which was one of the major influences on GREGORY, BOOLE, and to a somewhat lesser extent, PEACOCK and DEMORGAN, in their formulation of a new concept of algebra during the same period.

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[179] GEORGE BOOLE, "On the transformation of definite integrals," *Camb. Math. J.*, 1843, **3**: 216–224; "On the inverse calculus of definite integrals," *Ibid.*, 1845, **4**: 82–87; BRICE BRONWIN, "On certain symbolical representations of functions," *Camb. and Dub. Math. J.*, 1847, **2**: 134–140; CHARLES GRAVES, "On the solution of linear differential equations, and other equations of the same kind by the separation of symbols," *Proc. Roy. Ir. Acad.*, 1845–1847, **3**: 536; "On the method of solving a large class of linear differential equations by the application of theorems in the calculus of operations," *Ibid.*, 1854–1857, **6**: 34–37; ARTHUR CURTIS, "On the integration of linear and partial differential equations," *Camb. and Dub. Math. J.*, 1854, **9**: 272–290 and FRANCIS NEWMAN, "On Γa, especially when a is negative," *Ibid.*, 1848, **3**: 57–61 are some representative titles.

[180] PINCHERLE, "Equations fonctionelles"; "Pour la bibliographie de la théorie des opérations distributives," *Bibl. Math.*, 1899, **13**: 13–18; MICHAEL BERNKOPF, "The development of function spaces with particular reference to their origins in integral equation theory," *Arch. for Hist. of Ex. Sci.*, 1966, **3**: 1–96.

[181] CARMICHAEL, *Treatise*, ix.

5. The Idea of Abstract Algebra in Great Britain

The rise of an abstract view of algebra was not the work of a single man, or even of a single school. Among the major influences were the work of ABEL and GALOIS in the theory of equations, leading to the development of group theory, GAUSS' work in the theory of numbers, the creation of linear algebra, symbolic logic and the formalization of elementary algebra. However, as BOURBAKI has pointed out, it was the last three items which were most important in moving towards a specifically abstract point of view—and these were carried out in Great Britain. Thus, BOURBAKI, in discussing the work of GALOIS and GAUSS, says[182]

> leurs travaux n'eurent pas d'action immediate sur l'évolution de l'Algèbre abstraite. C'est dans une troisième direction que se font les progrès les plus nets vers l'abstraction: à la suite de réflexions sur la nature des imaginaires (dont la représentation géométrique avait suscité, au début du XIX^e siècle, d'assez nombreux travaux), les algèbristes de l'école anglaise dégagent les premiers, de 1830 à 1850 la notion abstraite de loi de composition, et élargissent immediatement le champ de l'Algèbre en appliquant cette notion à une foule d'êtres mathématiques nouveaux: algèbre de la logique avec Boole, ... vecteurs, quaternions et systèmes hypercomplexes généraux avec Hamilton ... matrices et lois non associatives avec Cayley ....

However it was not their work on imaginary numbers but rather the developments in the calculus of operations which directed the attention of the English towards the abstract study of laws of combination. The desire to explain in a satisfactory was the principles of the calculus of operations, and to extend its applicability, was important to GREGORY, BOOLE and DEMORGAN, and influenced, though to a lesser extent, HAMILTON as well. The significance of this fact is great, since by linking the introduction of Continental notation and methods in the calculus with the ensuing work in algebra, it yields a much more unified picture of mathematics in Great Britain during the first part of the nineteenth century than is usually presented.

The progress of PEACOCK, GREGORY, BOOLE and DEMORGAN towards an abstract view of algebra has been studied by DANIEL CLOCK in his dissertation, *A New British Concept of Algebra: 1825–1850.* CLOCK addressed himself to the question[183]

> Exactly when and to whom should we attribute the concept that algebra is an abstract deductive calculus that can have more than one model?

Although he does not answer that question categorically, he does give a full account of the progress made by the men whose work he discusses towards that goal. His principal conclusions are[184]

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[182] NICOLAS BOURBAKI, *Elements d'Histoire des Mathématiques*. Paris, 1960: 74.

[183] DANIEL ARWIN CLOCK, "A new British concept of algebra: 1825–1850," Ph. D. dissertation, University of Wisconsin, 1964: 2. The development of the English school of algebra is also treated in LUBOS NOVY, "L'Ecole algébrique anglaise," *Rev. Synth.*, 1968, **89**: 211–222.

[184] *Ibid.*, 2–3.

a) Peacock was a teacher who accidently or otherwise used the concept in a relatively unsophisticated way.
b) Gregory refined the concept but did not live to make a major statement.
c) DeMorgan expanded the concept and did make a fairly large work based on it.
d) Boole constructed a full scale algebra essentially different from "ordinary" algebra, an early example of a non-quantitative calculus.

In light of CLOCK's work, I will simply review briefly the views of algebra of these four men. PEACOCK defined algebra as the[185]

> science of symbols and their combinations constructed upon its own rules, which may be applied to arithmetic and to all other sciences by interpretation ....

However the generality of this statement is misleading, because he insisted that the laws of arithmetic must always be valid. In his words[186]

> Though the science of arithmetic, or of arithmetical algebra, does not furnish an adequate foundation for the science of symbolical algebra, it necessarily *suggests* its principles or rather its laws of combination.

The problem is, of course, that he writes *necessarily*. Thus, for example, he would deny the name of algebra to a non-commutative system. In his textbook he was quite explicit on this point, writing[187]

> I believe that no views of the nature of Symbolical Algebra can be correct or philosophical which makes the selection of its rules of combination arbitrary and independent of arithmetic.

GREGORY's views of algebra were more refined. Although his definition of algebra sounds much like PEACOCK's, there is a difference. And it is one which may explain the advance towards abstraction. GREGORY wrote that symbolical algebra is[188]

> the science which treats of the combination of operations defined not by their nature, that is by what they are or what they do, but by the laws of combination to which they are subject.

Notice that whereas PEACOCK had referred to the combination of symbols, GREGORY talks of operations. As we have seen, he studied laws of combination abstractly laid down—particularly the distributive, commutative and index laws—and then considered various interpretations of them.

The contrast between the work of PEACOCK and GREGORY is well illustrated by their treatment of the symbols + and −. PEACOCK considered them to be

[185] PEACOCK, "Report," 194–195.
[186] *Ibid.*, 195.
[187] GEORGE PEACOCK, *A Treatise of Algebra.* 2nd ed. rev. 2 vols. 1842–1845. Reprint. New York, 1942: vol. 2, 453.
[188] GREGORY, "Symbolical algebra," 2.

"signs of affectation," that is signs which do not affect magnitude, but only direction.[189] GREGORY discussed these symbols in a paper "On a difficulty in the theory of algebra." The difficulty referred to arose from the fact that GREGORY felt that any distinction between the symbols "a" and "$+$" in kind is only valid in arithmetical algebra, in which the meaning of the symbol $(a+b)$ is fixed, and no attention is paid to the laws of the combination of symbols. "But," he added, "in Symbolical Algebra, where any symbol represents an operation," a and $+$ have no general meaning.[190] In fact, according to GREGORY, the symbols $+$ and $-$ do not represent addition and subtraction in symbolical algebra. This is because[191]

> a symbol is defined *algebraically* when its laws of combination are given; and ... a symbol represents a given operation when the laws of combination of the latter are the same as those of the former.

Now, he went on, $+$ and $-$ combine according to

$$+(-a) = -a \quad \text{and} \quad -(+a) = -a. \tag{5.1}$$

But addition and subtraction are inverse operations, and any two inverse operations, say f and φ combine according to

$$f(\varphi(a)) = a; \qquad \varphi(f(a)) = a. \tag{5.2}$$

Hence, he concludes, $+$ and $-$, not obeying the same laws of combination as addition and subtraction, cannot represent these operations. We are deceived, he added, because we write the sum and difference as $a+x$ or $a-x$, whereas ordinarily the result of applying f to a is denoted $f(a)$. He then proceeded to study the operation of addition, using $A_x(a)$ to represent the addition of x to a, that is $x+a$. He gave the following three laws of combination for his symbols:[192]

$$\begin{aligned} A_x A_y(a) &= A_y A_x(a) \\ A_x(a) &= A_a(x) \\ A_x A_y(a) &= A_{A_y(x)}(a). \end{aligned} \tag{5.3}$$

The first law expresses, as GREGORY said, the fact that A_x and A_y are commutative operations. (It is the second that we would say expresses the fact that addition is commutative.) The third law, says GREGORY, states that if we first add y to a, and then x to the sum, the result is the same as if we first add y to x and then add the result to a, *i.e.* that $x+(y+a) = (y+x)+a$. GREGORY was close to the associative law, which was to be isolated by HAMILTON and had been missed completely by PEACOCK. As CLOCK has pointed out, this was probably because the latter did not conceive of addition as a binary operation.[193]

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[189] PEACOCK, "Report," 225 and *Treatise*, vol. 2, 120–121.

[190] DUNCAN GREGORY, "On a difficulty in the theory of algebra," 1843, *Math. Writings*: 241.

[191] *Ibid.*, 236.

[192] *Ibid.*, 239.

[193] CLOCK, 19.

GREGORY was clearly on the right track, but unfortunately he did not live to carry out his ideas further. DEMORGAN, on the other hand, left a large and coherent work on the subject, including a series of four papers "On the Foundations of Algebra" which appeared in the *Cambridge Philosophical Society Transactions*. His general goal in this work—at least in the first three papers—was a clear theory of the complex numbers. In them he gave a list of eight axioms which contained, at least implicitly, most of the axioms which characterize a field. The degree of abstraction found in this work seems to be between that of PEACOCK and GREGORY. Thus, in the first paper, which was read in 1839, DEMORGAN distinguished between technical and logical algebra:[194]

> Technical algebra is the art of using symbols under regulations which, when this part of the subject is considered independently of the other, are prescribed as the definition of the symbols. Logical algebra is the science which investigates the method of giving meaning to the primary symbols, and of interpreting all subsequent symbolic results.

DEMORGAN discriminated between defining, explaining and interpreting symbols. A symbol, he said, is "*defined* when such rules are laid down for its use as will enable us to accept or reject any transformation of it, or by means of it."[195] The distinction between the explanation and interpretation of a symbol is essentially the point at which it occurs. Thus[196]

> a simple symbol is *explained*, when such a meaning is given to it as will enable us to accept or reject the application of its definition, as a consequence of that meaning; and a compound symbol is interpreted when, having occurred as a result of explained elements, used under prescribed definitions, a necessary meaning can be given to it ....

DEMORGAN cites PEACOCK as the first to have made the distinction between two kinds of algebra, but, he says, he prefers the term technical to symbolical, "because the latter does not distinguish the use of symbols from the explanation of symbols."[197] In the first paper he studied the "possible explanation of one given technical algebra"; but he indicated that there might be others, for he writes of "the symbolical algebra which we have, and ... any other which we might have."[198] But he was not consistent in this, and in his book *Trigonometry and Double Algebra*, which appeared in 1849, DEMORGAN wrote[199]

> Any system of symbols which obey these rules and no others, except they be formed by combinations of these rules—and which uses the preceding symbols and no others— except they be new symbols invented in abbreviation of combinations of these symbols—is *symbolical algebra*.

[194] AUGUSTUS DEMORGAN, "On the foundations of algebra, I," *Trans. Camb. Phil. Soc.*, 1837–1842, 7: 173.
[195] *Ibid.*, 174.
[196] *Ibid.*
[197] *Ibid.*, 176.
[198] *Ibid.*
[199] AUGUSTUS DEMORGAN, *Trigonometry and Double Algebra*. London, 1849: 103–104.

The rules referred to first appeared in his second paper on the foundations of algebra and are essentially the axioms which characterize the field of complex numbers.[200] It was the system of complex numbers which he called logical algebra. And in his treatment of it we can see the influence of the operational point of view. DEMORGAN wrote[201]

> The first step to logical algebra is the separation of the rules of the ordinary science from its principles, or rather of its laws of operations from the explanation operated upon or with.

Although DEMORGAN included the study of the symbols previous to explanation as necessary, he felt that this was not by any means what we should call mathematics. He wrote that[202]

> ... the art of operation, previously to the explanation of its symbols, is precisely what Dugald Stewart imagined every mathematical science to be, namely a pure consequence of definitions, which upon other definitions might have been another thing. This opinion was not, and perhaps is not, without its followers.

(DUGALD STEWART was a member of the Scottish "common sense" school of philosophy.) DEMORGAN went on to reject this view and said that the study of rules without meaning is not a science and certainly bears no resemblance to the work of the mathematician. A person "who makes the transformations of algebra by the defined laws of operation only," he added, "is comparable to one who puts a dissected map together by the backs of the pieces alone; whereas the person who looks at the front, and uses his knowledge of geography to help more resembles the investigator and mathematician."[203] (DEMORGAN's views are not without their supporters today.)

Although DEMORGAN did not base his idea of algebra on the concept of arithmetic as a suggesting science in the sense of PEACOCK, his views were somewhat limited in a similar fashion. In essence, in the main body of his work, he was looking for a single universal system, in which all operations led to interpretable results and in which the laws were the axioms characterizing the complex numbers. DEMORGAN felt that he had achieved this, when, in his third paper in the series, he gave a definition for A^B, where A and B represent both magnitude and direction, *i.e.* are complex.[204] DEMORGAN's aims remained the same, even in the last paper in the series, which was a work inspired by the publication of HAMILTON's first paper on quaternions in 1844. In this memoir DEMORGAN studied what he called systems of algebra of the n^{th} character. Such a system was defined to be

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[200] AUGUSTUS DEMORGAN, "On the foundations of algebra, II," *Trans. Camb. Phil. Soc.*, 1837–1842, **7**: 287–300.

[201] *Ibid.*, 287.

[202] *Ibid.*, 288–289.

[203] *Ibid.*

[204] AUGUSTUS DEMORGAN, "On the foundations of algebra, III," *Trans. Camb. Phil. Soc.*, 1849, **8**: 139–142.

one in which there were n symbols $\xi_1, \xi_2, \xi_3, \ldots$ and where

$$a_1 \xi_1 + a_2 \xi_2 + \cdots = b_1 \xi_1 + b_2 \xi_2 + \cdots \tag{5.4}$$

implies that $a_1 = b_1$, $a_2 = b_2$, *etc.* "This condition, however," he wrote, "is connected with the interpretation: a perfect symbolical system might very well exist without it."[205] DEMORGAN proceeded to study various ways in which one can define the products $\xi_i \xi_j$, which he then extended to general products by assuming the distributive law. But he tried to do this so that $AB = BA$ and $A(BC) = (AB)C$. Any definition that failed to do this, he labelled "imperfect", because, he said, his object was "to detect systems in which the symbolic forms of common algebra are true ... ."[206] In effect, DEMORGAN was working in n dimensional vector spaces over the reals and trying to define a product which would make them fields. This is, of course, not possible unless $n = 1$ or 2.

DEMORGAN'S views on the nature of algebra were also influenced by the fact that he was a logician. In a paper entitled "On infinity; and on the signs of equality," published in 1871, he made some interesting, if somewhat obscure, comments on the difference between algebra and logic. Algebra, he wrote, was not yet a formal science in the sense that logic was, because in a formal algebra every form and every transformation had to be universal. But the forms of algebra admit exceptions; for example $ab = b$ implies $a = 1$, but $2x = x$ does not give $2 = 1$. (Here a and b are symbols in arithmetical algebra, *i.e.* they represent natural numbers, and hence the cancellation law for multiplication is not restricted.) PEACOCK'S principle of the permanence of equivalent forms came closest to the assertion that algebra is a formal science, wrote DEMORGAN, "but, it lacked the distinction between form and matter."[207]

The last member of the English school we are considering here is BOOLE. He did not write explicitly on the formalization of algebra but expressed his views on the nature of algebra and mathematics in his logical works. In the introduction to his first logical work, *The Mathematical Analysis of Logic*, 1847, BOOLE wrote[208]

> We might justly assign it as the definitive character of a true Calculus, that it is a method resting upon the employment of Symbols, whose laws of combination are known and general, and whose results admit of a consistent interpretation.

In *The Laws of Thought*, 1854, he stated that "it is not of the essence of mathematics to be conversant with the ideas of number and quantity."[209] His strictures on the nature of symbolic reasoning show that he, like PEACOCK, believed that some suggesting science was necessary, though he went beyond PEACOCK by

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[205] AUGUSTUS DEMORGAN, "On the foundations of algebra, IV," *Trans. Camb. Phil. Soc.*, **8**: 241.

[206] *Ibid.*, 254.

[207] AUGUSTUS DEMORGAN, "On infinity and on the sign of equality", *Trans. Camb. Phil. Soc.*, 1871, **11**: 181.

[208] GEORGE BOOLE, *The Mathematical Analysis of Logic*. 1847. Reprint. Oxford, 1951: 4.

[209] GEORGE BOOLE, *An Investigation of the Laws of Thought*. 1854. Reprint. New York, n.d.: 12.

asserting that this science need not be arithmetic. BOOLE described the construction of a symbolic science as follows: One must start with a fixed interpretation of the symbols in order to establish the laws of combination; in the formal process of reasoning, no attention should be paid to the interpretability of the intermediate steps; only the final result must be interpretable.[210] In logical works, the so-called suggesting science is, BOOLE claimed, the laws of thought. CLOCK is very critical of BOOLE for insisting on a suggesting science and calls his work a step backward from that of GREGORY and DEMORGAN in the process of abstraction.[211] But this is not, I think, quite fair, as it fails to take into account BOOLE's work on the combination of symbols, done in connection with differential equations. As we have seen, GREGORY restricted himself to equations with constant coefficients, because his justification of the method of the separation of symbols rested on the assumption that his symbols obeyed the ordinary laws of algebra. BOOLE, on the other hand, freed himself from this restriction and studied non-commutative operations. And, of course, he also gave his famous algebraic interpretation of logic.

BOOLE stated his basic position in the *Laws of Thought* as follows:[212]

> *All the operations of Language, as an instrument of reasoning may be conducted by a system of signs composed of the following elements, viz.:*
>
> 1st. *Literal symbols, as x, y, etc., representing things as subjects of our conceptions.*
>
> 2nd. *Signs of operation, as +, −, ×, standing for those operations of the mind by which the conceptions of things are combined or resolved so as to form new conceptions involving the same elements.*
>
> 3rd. *The sign of identity, =. And these symbols of Logic are in their use subject to definite laws agreeing with and partly differing from the laws of the corresponding symbols in the science of algebra.*

The laws which BOOLE isolated for his symbols are

$$\begin{aligned} xy &= yx \\ x^2 &= x \\ z(x+y) &= zx + zy. \end{aligned} \tag{5.5}$$

Thus, he said, since we have the special law $x^2 = x$, it is not worthwhile to trace the analogy between ordinary algebra and the system of logic. There are only two symbols of number subject to that law, namely 0 and 1. BOOLE concluded[213]

> Let us conceive, then, of an Algebra in which the symbols x, y, z, etc. admit indifferently of the values 0 and 1, and of these values alone. The laws, the axoims, and the process, of such an Algebra will be identical in their whole extent with the laws, the axioms, and the processes of an Algebra of Logic. Difference of interpretation will alone divide them.

[210] *Ibid.*, 66–70.
[211] CLOCK, 157.
[212] BOOLE, *Laws of Thought*, 27.
[213] *Ibid.*, 37–38.

Looking at the work of the English school of algebra during the first half of the nineteenth century, CLOCK's summary seems fair:[214]

> the principle aspects of a concept of an abstract algebra were rather clearly enunciated during that period. The actual construction of a genuinely abstract algebra remained yet to be accomplished.

CLOCK did not discuss the work of HAMILTON, which played an important role in the rise of abstract algebra. HAMILTON's concept of algebra differed considerably from that of his English contemporaries, though, as we shall see, he did come to feel a certain degree of sympathy with their views. His most important contributions to algebra are his work in the formalization of elementary algebra, his theory of complex numbers and his discovery of quaternions. The fact that the quaternions did not turn out to be the important tool in geometry and physics which HAMILTON and some of his followers claimed they were led to a decline in his reputation during the first part of the twentieth century. But in recent years there have been important reassessments of HAMILTON's contributions to algebra.

MICHAEL CROWE, in his *History of Vector Analysis*, has argued convincingly that modern vector analysis developed historically as a modification of HAMILTON's quaternion system.[215] The significance of HAMILTON's work in the formalization of elementary algebra was pointed out by C. C. MACDUFFEE who wrote that[216]

> Hamilton's careful, detailed and logical criticisms of the foundations of algebra have been important steps in the development of modern abstract algebra.

And JEROME MANHEIM, in his *Genesis of Point Set Topology*, credited HAMILTON with the first modern scientific treatment of the irrationals.[217]

I suspect that the reason for the neglect of HAMILTON's work on the foundations of algebra is that it is contained in the "Essay on algebra as the science of pure time," whose metaphysical sounding title must have discouraged many from reading it. Its philosophical content has been succintly summarized by G. WINDRED as being based on three fundamental concepts: 1. the notion that time is connected with existing algebra; 2. the notion, or intuition, that time may be developed into an independent pure science; and 3. that the science so developed is identical with algebra, insofar as algebra is a science.[218]

HAMILTON took as his starting point the assumption that algebra, like geometry, should be "deduced by valid reasonings from its own intuitive principles."[219]

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214 CLOCK, 166.

215 MICHAEL J. CROWE, *A History of Vector Analysis*. Notre Dame, Indiana, 1967.

216 C. C. MACDUFFEE, "Algebra's debt to Hamilton," *Colletion of Papers in Memory of Sir William Rowan Hamilton*, 25–35. New York, 1945: 25.

217 JEROME MANHEIM, *Genesis of Point Set Topology*. Oxford, 1964: 78.

218 G. WINDRED, "The history of mathematical time," *Isis*, 1933, **19**: 150.

219 WILLIAM ROWAN HAMILTON, "Theory of conjugate functions or algebraic couples; with a preliminary and elementary essay on algebra as the science of pure time," 1837, *Math. Papers*: vol. 3, 5.

The principle which he chose as a basis is that of pure time, or "the closely connected (and in some sort coincident) notion of *Continuous Progression*."[220] HAMILTON cited some interesting authorities as lending credence to his idea: NEWTON, "whose revolutionary work in the higher parts of both pure and applied Algebra was founded mainly on the notion of *fluxion* which involves the notion of *time*;" J. NAPIER, who in his discovery of logarithms used the idea of "Continuous Progression, in describing which, he speaks expressly of *Fluxions, Velocities* and *Times*", and, surprisingly, LAGRANGE. Now LAGRANGE, HAMILTON admitted, had tried to banish the idea of time and to reduce the theory of fluxions "to a system of operations upon symbols, analogous to the earliest symbolic operations of algebra." But, in regarding algebra as the "*Science of Functions*" HAMILTON felt that LAGRANGE had unwittingly let the idea of time back in—because the clearest idea of a function is that of a "*Law connecting Change with Change*. But where *Change* and *Progression* are, there is TIME."[221] Starting with his intuitive base, HAMILTON took a constructive point of view. From the notion of instants of time, he defined the integers and the rationals. He stated clearly the laws of number, including the commutative, associative and the distributive, the existence of an identity for addition and multiplication and the existence of inverses, and at each step proved these for the class of numbers under consideration. It should be noted that the term associative law seems to have been coined by HAMILTON, who first used it in a paper of 1844 on quaternions.[222]

Based as it was on the ordinal motion of number as primary, HAMILTON's work led him to an appreciation of the importance of the idea of order, a concept that DEMORGAN had missed completely, since he concentrated on the complex numbers. From his ordering of the rationals and the assumption that time is continuous, HAMILTON derived his theory of irrationals. Thus, assuming the identity between instants of time and the set of real numbers, he showed that not all numbers are fractional and that there are fractional numbers which are not the squares of other such numbers. HAMILTON then proved that, under his assumptions, every positive ratio does have a square root, which can be approximated as closely as desired by rationals, or as he called them, fractional numbers. This he achieved by a series of lemmas:[223]

I. If x is positive, then as x increases continuously, x^2 increases continuously.

II. Between any two unequal ratios, there is another ratio.

As a corollary to II, he gets: Given the two series $a', b', c', \ldots$ and $a'', b'', c'', \ldots$. If the smallest ratio in the second series is larger than the largest ratio in the first series, then there is a ratio a such that

$$a > a', \quad a > b', \quad a > c', \ldots$$
$$a < a'', \quad a < b'', \quad a < c'', \ldots.$$

[220] *Ibid.*

[221] *Ibid.*, 5–6.

[222] WILLIAM ROWAN HAMILTON, "On a new species of imaginary quantities connected with the theory of quaternions," 1844, *Math. Papers*: vol. 3, 114.

[223] HAMILTON, "Pure time," 56–58.

III. If $\underline{b} > 0$, there exists a unique positive ratio a which satisfies

$$\underline{a} > \frac{n'}{m'} \quad \underline{a} \leqq \frac{n''}{m''}$$

where m', n', n'', m'' are any positive whole numbers which satisfy

$$\frac{n' n'}{m' m'} < \underline{b} \quad \frac{n'' n''}{m'' m''} > \underline{b}.$$

IV. Between any two unequal positive ratios there is the square of a fractional number.

Using these lemmas, HAMILTON was able to prove his result in the following theorem:

> The square $\underline{a}\underline{a}$ of a determined positive ratio $\underline{a}$, of which the existence was shown in the IIIrd Lemma, is equal to the proposed positive ratio $\underline{b}$ in the same Lemma; that is
>
> $$\text{if} \quad \underline{a} > \frac{n'}{m'} \quad \text{whenever} \quad \frac{n' n'}{m' m'} < \underline{b}$$
>
> $$\text{and } \underline{a} < \frac{n''}{m''} \quad \text{whenever} \quad \frac{n'' n''}{m'' m''} > \underline{b}$$
>
> $$\text{then } \underline{a}\underline{a} = \underline{b},\ \underline{a} = \sqrt{\underline{b}},$$
>
> m', n', m'', n'', being any positive whole numbers which satisfy the conditions here mentioned, and $\underline{b}$ being any determined positive ratio.

HAMILTON'S theory clearly has its roots in EUDOXUS' definition of proportion, a definition he knew and admired. It also bears a striking resemblance to the work of RICHARD DEDEKIND. The relationship between HAMILTON'S theory and that of DEDEKIND was discussed briefly by H. E. HAWKES, in 1901. HAWKES pointed out that although there is a similarity, HAMILTON'S work was incomplete in several respects when compared with DEDEKIND'S. He also stated that it was doubtful that DEDEKIND was influenced by HAMILTON, since the former based his theory on the continuity of space rather than of time.[224] Despite its flaws, HAMILTON'S work remains an impressive attack on a very difficult problem, and one which has been unfairly neglected.

HAMILTON extended his number system to include complex numbers, by defining them as ordered pairs of reals. He laid down the rules for the addition and multiplication of two pairs and again proved all the field axioms were satisfied. The definitions, he noted, were made so as to preserve as great an analogy with the theory of singles, *i.e.*, reals, as possible. In this regard, the degree of abstraction he was willing to accept is delineated in the following quote:[225]

> In general the definitions of mathematical science are not altogether arbitrary, but a certain discretion is allowed in the selection of them, although once selected, they must then be consistently reasoned from.

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[224] EDWIN HERBERT HAWKES, "Note on Hamilton's determination of irrational numbers," *Bull. Amer. Math. Soc.*, 1901, **7**: 306–317.

[225] HAMILTON, "Pure time," 81.

But, later on, he seemed to draw back a bit. At the end of the section on algebraic couples (complex numbers), we find him defending his theory of pure time. Thus, he wrote[226]

> Were those definitions altogether arbitrary, they would not contradict each other, nor the earlier principles of Algebra, and it would be possible to draw legitimate conclusions, by rigorous mathematical reasoning, from premises arbitrarily assumed: but the persons who have read with attention the forgoing remarks of this theory, and have compared them with the Preliminary Essay, will see that these definitions are really not *arbitrarily chosen*, and that though others might have been assumed, no others would be equally proper.

HAMILTON's theory was not well received; even his friends felt that he was mistaken in bringing in the notion of time, and that he should have referred only to the idea of continuous progression. In answer, he wrote to DEMORGAN in 1841 that[227]

> One thing I am, and was prepared to admit, nay if it had seemed needful, to contend for, that Algebra does not require for its foundation as a Science, any knowledge or conception of the actual succession of events or of the relation of cause and effect; continuous progression appeared, and still appears to me sufficient: but this I thought and think, is the essential element in the conception of what I call *pure time*.

HAMILTON then quoted a passage from KANT to the effect that "we can think to ourselves no line, without *drawing* it in thought ...." And, he went on,[228]

> I cannot say whether the passage was in my recollection when I was drawing up my Paper on Algebra, but I remember that a similar train of thought prevented me from yielding to the suggestion of some friends, who were of the opinion that without much impairing the statement of my own views I should be likely to escape much opposition if I contented myself with speaking of continuous succession or progression, without introducing the jealousy-exciting name of Time.

DEMORGAN referred to the theory in a friendly spirit in the "Calculus of Functions," where he wrote[229]

> A distinguished analyst calls Algebra the science of *pure time* ... the notion of *time*, *succession*, and number are so clearly related that we have no doubt very admissible conventions would make this true.

On the other hand, it was severely criticized by ARTHUR CAYLEY, first Sadlerian Professor of mathematics at Cambridge and an important figure in the history of algebra. CAYLEY wrote in 1864[230]

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[226] *Ibid.*

[227] R. GRAVES, vol. 3, 426.

[228] *Ibid.*, 426–427.

[229] A. DEMORGAN, "Calculus of functions," 347.

[230] ARTHUR CAYLEY, "On the notion and boundaries of algebra," 1864, *Math. Papers*: vol. 4, 292.

I do not admit the assertion, that the idea of number is derived from that of time, it appears to me that it is derived from that of succession in time or space indifferently.

In 1883 he went even further, denying that continuous progression of any kind was fundamental.[231] The question of whether the cardinal or ordinal concept of number is primary is one which is still argued by philosophers of mathematics.

Although HAMILTON's work was important in the development of a formal view of mathematics, he himself, as we have seen, did not subscribe to it. However, he was in close contact with members of the English school, especially DEMORGAN. And, although he never totally abandoned his KANTIAN position, he did become more receptive to the views of PEACOCK, GREGORY and DEMORGAN. Thus, in 1846, he wrote that although he had not given up his ideas, he had come to believe that[232]

there is a sort of symbolical science, or *science of language* which well deserves to be studied, abstraction being made for a while of *meaning*, or of interpretation; and *forms of expression* being treated as themselves the subject-matter to be studied: in short I feel an increased sympathy with, and fancy that I better understand that *Philological School* ....

Among the authors included by HAMILTON in that school were PEACOCK, GREGORY and MARTIN OHM. OHM was the brother of the physicist GEORG OHM. From 1811 to 1821 he worked, according to his own testimony, on an attempt to unify all of mathematics into one system. BOURBAKI credits his work as being the first to attempt to base all of analysis on arithmetic.[233] An exposition of the general principles on which his work rests was translated into English in 1843 under the title *The Spirit of Mathematical Analysis and its Relation to a Logical System.* OHM began with seven basic operations: $+$, $-$, $\times$, $\div$, powers, roots and logarithms. He then defined mathematical analysis as "the doctrine of these (seven) (mental) acts to one another, to which we are led by the consideration of (whole, indenominate) number."[234] OHM's theory is rather involved, but it clearly was a forerunner of later attempts to base all of mathematics on the integers. The work impressed HAMILTON, and by his admission, influenced his thinking.

In the preface to the *Lectures on Quaternions*, which appeared in 1853, HAMILTON again linked OHM and PEACOCK. He wrote that his[235]

own old views respecting Algebra, perhaps modified in some respects by subsequent thought and reading are not fundamentally and irreconcilable

[231] ARTHUR CAYLEY, "Presidential address," 1883, *Math. Papers*: vol. 11, 429–459.

[232] R. GRAVES, vol. 2, 521–522.

[233] NICOLAS BOURBAKI, *Eléments de Mathématiques* XXII. Livre I. *Théorie des Ensembles. Chapitre 4. Structures*. Actualités Scientifiques et Industrielles. 1258. Paris, n.d.: 94.

[234] MARTIN OHM, *The Spirit of Mathematical Analysis and its relation to a Logical System*. trans. A. J. ELLIS, London, 1843: 12.

[235] WILLIAM ROWAN HAMILTON, *Lectures on Quaternions*. Dublin, 1853: (14). In this work the page numbers of the preface were enclosed in parentheses.

> opposed to the teaching of writers whom I so much respect as Drs. Ohm and Peacock.

HAMILTON described what he believed to be the method of these authors as follows:[236]

> I by no means dispute the possibility of constructing a consistent and useful system of algebraical calculations, by starting with the notion of *integer number*; unfolding that notion into its necessary consequences; expressing those consequences with the help of *symbols*, which are already general in their *form*, although supposed at first to be limited in their signification, or *value*; and then, by *definition*, for the sake of *symbolic generality*, *removing* the *restrictions* ... .

This, HAMILTON felt, described the approach of both PEACOCK and OHM, though, he wrote, the former concentrates on the "*permanence* of equivalent forms", while the latter emphasizes "the *relations* between the fundamental *operations*."[237] The problem with this approach, says HAMILTON, is that it takes the cardinal concept of number as the starting point, whereas he feels that it is the ordinal concept which is more basic. Thus, he said, "I cannot fancy myself as *counting* any set of things, without first *ordering* them, and *treating* them as successive ... ."[238] It was possible, he added, to use the theory of pure time as the "suggestive science" in PEACOCK's sense, and this had the advantage of delaying the introduction of uninterpreted symbols. But finally, he admitted that whatever the basis of algebra[239]

> the actual *calculations* suggested by this, or by any other view, must be performed according to some fixed laws of *combination of symbols*, such as Professor DeMorgan has sought to reduce, for ordinary algebra, to the smallest possible compass, ...

Beginning in 1846 HAMILTON published a series of papers entitled "On Symbolical Geometry." Here again he cited PEACOCK and OHM as the authors who had led him to a deeper appreciation of the new school of algebra. He began as follows:[240]

> The present paper is an attempt towards constructing a symbolical geometry, analogous in several important respects to what is known as symbolical algebra, but not identical therewith; since it starts from other suggestions and employs in many cases other rules of combination of symbols.

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236 *Ibid.*, (15).

237 *Ibid.*

238 *Ibid.*

239 *Ibid.*, (16).

240 WILLIAM ROWAN HAMILTON, "On symbolical geometry," *Camb. and Dub. Math. J.*, 1846, **1**: 45.

His object in writing the paper was two-fold, he added; first to illustrate the quaternions in another light, and secondly, what would probably interest his readers more, to[241]

> furnish some new materials towards judging of the general applicability and usefulness of some of the principles of symbolical language which have been put forward in modern times.

As early as 1835, in criticizing the symbolic school of algebra, HAMILTON had noted that "Geometry itself might be presented in a merely logical or symbolical form, ..." though, he added, he did not think it should be.[242]

At the end of his paper on algebraic couples, HAMILTON had spoken of his hopes of extending the theory to triplets, and in fact to arbitrary sets of moments and steps of time.[243] As his notebooks testify, he spent much time on the attempt to define multiplication for such objects. Like DEMORGAN, he wanted, at first, a system which would form an associative, commutative, division algebra over the reals. But, led in part by geometric considerations, he took, in 1843, the step of sacrificing the commutative law of multiplication for four-tuples. And hence, creating one of the first non-commutative algebraic systems, which he called the system of quaternions. Thus, in assessing HAMILTON'S contributions to the growth of an abstract view of algebra, we find that it was much more technical than philosophical. He gave a constructive view of the irrationals and the complex numbers and isolated the axioms which characterize a field. Furthermore, his quaternions furnished an example of a non-commutative system. But his own views on the nature of mathematics looked back towards KANT rather than forward to a more abstract point of view.

In most histories of mathematics, the rise of abstract algebra is linked with the increased necessity to deal with complex analysis and hence to the need for the justification of so-called imaginary quantities. The geometric interpretation of the complex numbers is a fascinating example of simultaneous discovery, followed by a time lag before the theory was generally accepted. It has been studied in detail by several scholars.[244] The part played by ideas on imaginary numbers on the growth of algebra has also been elucidated.[245] The British mathematicians whose work we are considering here were familiar with, and discussed

[241] *Ibid.*

[242] R. GRAVES, vol. 2, 143.

[243] HAMILTON, "Pure time," 96.

[244] WOOSTER WOODRUFF BEMAN, "A chapter in the history of mathematics," *Proc. Amer. Ass. for the Adv. of Sc.*, 1897, **46**: 33–50; PHILLIP S. JONES, "Complex numbers: an example of recurring themes in the development of mathematics," *Math. Teacher*, 1954, **47**: 106–144; 257–263 and G. WINDRED, "The history of the theory of imaginary and complex quantities," *Math. Gaz.*, 1929, **14**: 533–541 are among the most complete.

[245] ERNEST NAGEL, "Impossible numbers: a chapter in the history of logic," *Studies in the History of Ideas* III. ed. Dept. of Phil. Columbia Univ. New York, 1935: 429–475; FEDERIGO ENRIQUES, *The Historic Development of Logic.* trans. J. ROSENTHAL, New York, 1929 and CROWE.

at length, the works of JOHN WARREN, Abbé BUÉE, ROBERT ARGAND, CHARLES MOUREY, AUGUSTIN CAUCHY and KARL GAUSS, as well as the early work of JOHN WALLIS on this subject. But despite this, the role of the geometric interpretation of the complex numbers on their thinking was not really strong, since it was not regarded as a sufficient foundation for their use. For example, ROBERT WOODHOUSE would not accept any geometric interpretation as a sound basis for the use of imaginaries, since one of his aims was to free analysis from any dependence on geometry. WOODHOUSE wrote a paper, published in the 1801 volume of the *Philosophical Transactions* giving his view of the proper foundation for the use of imaginaries. It was written as a direct reply to a paper by JOHN PLAYFAIR. PLAYFAIR had suggested that the reason that computations involving imaginary quantities lead to correct results was to be found in the analogy between circular and hyperbolic arcs, and that because of this, such quantities could not be used to prove results, but only to suggest them; any such true result could and should be proved independently.[246] This attitude WOODHOUSE rejected completely; in his view operations on imaginary quantities led always to true conclusions, and hence there had to be a logical explanation of this fact. As he said[247]

> It would indeed be a singular paradox or a rare felicity if truth not always attained by meditation should unexpectedly result from unideal operations conducted without principle, purpose, or regularity.

WOODHOUSE's own view was strictly formal, and was related to his theory of series. Thus, he said, we can attach a meaning to the symbols $\times$ and $+$ and prove that $(a+b)\times(c+d) = ac+ad+bc+bd$, if a, b, c, d are real. But we cannot prove that $(a+b\sqrt{-1})(c+d\sqrt{-1})$ and $ac-bd+(ab+bc)\sqrt{-1}$ are equivalent. What we must do is to assume that this is so; that is to extend the rule demonstrated for the real quantites to ones involving $\sqrt{-1}$. Thus the fact that $x\sqrt{-1}-x\sqrt{-1}$ is 0, or that $\frac{x\sqrt{-1}}{x}$ is $\sqrt{-1}$ is due to the assumption that "x is to combine with $\sqrt{-1}$ as with real quantities."[248] WOODHOUSE's views here clearly presage PEACOCK's principle of the permanence of equivalent forms.

Furthermore an expression like $e^{x\sqrt{-1}}$ was to be defined by

$$e^{x\sqrt{-1}} = 1 + x\sqrt{-1} - \frac{x^2}{1\cdot 2} - \frac{x^3}{1\cdot 2\cdot 3}\sqrt{-1} + \cdots \tag{5.6}$$

where the right hand side was to be computed according to that rule.

The geometric interpretation of imaginaries did play an important part in leading HAMILTON to his concept of quaternions. In the first published account of quaternions HAMILTON asserted that it was the attempt to generalize JOHN

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[246] JOHN PLAYFAIR, "On the arithmetic of impossible quantities," *Phil. Trans.*, 1788, **68**: 318–343.

[247] ROBERT WOODHOUSE, "On the necessary truth of certain conclusions obtained by means of imaginary quantities," *Phil. Trans.*, 1801, **91**: 91.

[248] *Ibid.*, 100.

WARREN'S definition of the proportion between four lines in a plane to lines in space which had led him to the quaternions.[249] JOHN WARREN'S *Treatise on the Geometrical Representation of the Square Root of Negative Quantities*, published at Cambridge in 1828 was the first nineteenth century work in English to expound the geometric interpretation. (Abbé BUÉE'S work, though published in the *Philosophical Transactions* previous to that date, was in French.) WARREN had defined quantity to mean a line drawn from the origin, and hence completely determined by its length and direction. His definition of proportion was that the ratio of two lines was the same as that of two others if their lengths had the same numerical ratio and if they formed equal angles.[250]

HAMILTON described his attempt to generalize this idea to lines in space as follows: Letting $i=\sqrt{-1}$ and j another $\sqrt{-1}$, perpendicular to the plane of 1 and i, $x+iy+jz$ denotes the line from (0, 0, 0) to (x, y, z). If WARREN'S theory extends to space, $(x+iy+jz)^2$ must represent the third proportional between (1, 0, 0) and (x, y, z) as indicators of lines. Thus its length should be the third proportional between 1 and $\sqrt{x^2+y^2+z^2}$. Assuming the ordinary rules of arithmetic, HAMILTON found that the square was given by

$$x^2-y^2-z^2+2ixy+2jxz+2ijyz. \tag{5.7}$$

But, he added, the ij term must vanish if the length is to have its proper value. This term, he went on, would be omitted either by letting $ij=0$, or $ij=-ji$. Since on the first assumption the length of a product is not the product of the lengths, he preferred the second, and letting $ij=k$, he found $k^2=-1$.[251] This account of HAMILTON'S has convinced most historians, who emphasize the role of the complex numbers over that of his algebraic theory in shaping HAMILTON'S thought.[252] But HAMILTON kept returning to the idea of "pure time." Thus in the first extended publication on quaternions he presented them as quadruples, and described them as[253]

> at least in their first aspect and conception, a continuation of those speculations concerning Algebraic Couples, and respecting Algebra itself, regarded as the science of Pure Time.

And in 1852 he described the role played by the two strands of thought:[254]

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[249] WILLIAM ROWAN HAMILTON, "On quaternions, or a new system of imaginaries in algebra (a letter to J. GRAVES, dated 17 October 1843)", 1844, *Math. Papers*: vol. 3, 106.

[250] JOHN WARREN, *Treatise on the Geometrical Representation of the Square Roots of Negative Quanitites*. Cambridge, 1828: 6–7.

[251] WILLIAM ROWAN HAMILTON, "Quaternions," Manuscript dated 1843, *Math. Papers*: vol. 3, 103.

[252] E. T. WHITTAKER, "The sequence of ideas in the discovery of quaternions," *Proc. Roy. Ir. Acad.*, 1944–1945, **50**: 93–98.

[253] WILLIAM ROWAN HAMILTON, "Researches respecting quaternions, First Series," 1848, *Math. Papers*: vol. 3, 159.

[254] R. GRAVES, vol. 3, 307.

$\sqrt{-1}$ had haunted me long, and I did know the outlines of double algebra, not only before I thought of the quaternions, but also *(I think)* before I had formed, with the help of Kant, any very definite views about pure time. ... (and) when those views ... were formed, I was naturally led to see that *any* number of independent progressions might be imagined as easily as *two* and thus formed early the notion of triplets and sets, of moments, steps and numbers.

Thus we had the complex numbers derived as pairs of reals, without any reference to geometry, and this idea extended to quaternions as quadruples. And HAMILTON himself modified the early statement on the role of geometric thinking in the discovery of quaternions. He later wrote that while in the first published work he had emphasized geometric considerations[255]

yet I have memoranda which show that I had been recently reading my own paper on algebra, and seeking to illustrate its first principles to my boys, especially as related to equal and to successive *steps in time*. So that, although, at the last moment, it was *geometry* that *moulded* and *fixed* my conception of the *ijk*, I had been *prepared* for accepting it, by recent as well as by other speculations, of a more *abstract* sort.

That the geometric approach to the complex numbers was not sufficient was stated explicitly by PEACOCK. The ability to give a geometric interpretation of $\sqrt{-1}$, and to imaginaries in general, he wrote[256]

does not in any respect affect the general theory of their introduction or of their relation to other signs: for in the first place, it is not an essential or necessary property of such signs: and in the second place, it in no respect affects the form or equivalence of symbolical results, though it does affect both the extent and mode of their application.

DEMORGAN also rejected the idea that algebra could or should be based on geometric ideas. In a *Penny Cyclopedia* article, "Negative and impossible quantities," he stated that the theory of double algebra (as he called the complex numbers), was founded not on geometry, but rather on symbolical algebra, that is, drawn from arithmetical suggestion and then cut loose. The resulting system, complete in its own right, could then be applied to geometry.[257] And this is the program he carried out in his text *Trigonometry and Double Algebra*. That is, the rules of combination are considered as the basis of the subject and the geometry is introduced as an interpretation.[258]

The geometric interpretation of the complex numbers has sometimes been thought to have led directly to the notion that the subject matter of mathematics

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[255] *Ibid.*, 307–308.

[256] PEACOCK, "Report," 131.

[257] AUGUSTUS DEMORGAN, "Negative and impossible quantities," *Penny Cyclopedia*, London, 1840: vol. 16, 136.

[258] AUGUSTUS DEMORGAN, *Trigonometry and Double Algebra*. London, 1849.

is more than simple quantity.[259] And it is true that in the work of many of the men who introduced the idea, the objects of study appear as magnitude affected by direction. But still the notion of magnitude seemed essential. The next advance towards abstraction was to drop any use of magnitude and study objects defined by their laws of combination. And this seems to have occurred first in the calculus of operations. As we have seen, all of the men who were active in producing a new concept of algebra had some degree of connection with the calculus of operations. This was no coincidence. Particularly in the case of GREGORY, DEMORGAN and BOOLE the calculus of operations played a strong role in forming their way of thinking about the nature of mathematics. Thus, GREGORY opened his paper on the nature of symbolical algebra by stating that[260]

> The following attempt to investigate the real nature of Symbolical Algebra ... took its rise from certain general considerations, to which I was led in following out the principle of the separation of symbols of operation from those of quantity.

In fact, GREGORY preferred to consider the symbols of ordinary algebra as symbols of operation. In his paper, "On a difficulty in the theory of algebra," which has been mentioned before GREGORY actually identified the two subjects. He argued that the symbols $+$, $-$, $\times$, $\div$ were invented for the purpose of indicating operations on numbers; but that it was found that the symbol $\times$ might be omitted, the operation being indicated by the juxtaposition of symbols, $a x$ being written for $a \times x$. He continued[261]

> From this the transition was easy to the conception of a as the symbol of operation; a change of great importance, as leading to the view that Symbolical Algebra is a Calculus of Operations.

In the same paper, he says[262]

> Symbolical Algebra must be considered as a science of operations represented symbolically: ... it will, I am convinced, be found that there is no other way of explaining the difficulties of Algebra in a uniform and consistent manner.

And DEMORGAN seems to have shared this view. In introducing double algebra, the wrote,[263]

> It thus appears that what we here denominate *addition* is truly not addition of *magnitude* to produce *magnitude*, but junction of *effects* to produce joint *effects*.

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[259] NAGEL, "Impossible numbers."
[260] GREGORY, "Symbolical algebra," 1.
[261] GREGORY, "Difficulty in the theory of algebra," 242.
[262] *Ibid.*
[263] A. DEMORGAN, *Trigonometry*, 118.

Furthermore, in his first paper on the foundations of algebra, he noted that a symbol may be used to denote magnitude, or the operation by which magnitude is obtained; and, he added, "modern algebraists usually dwell on the second notion, that of operation."[264] In the introduction to the paper, DEMORGAN explicitly pointed both to the imaginary numbers and the calculus of operations as the resons for an inquiry into the foundations of algebra, writing[265]

> The extent to which explanation of the meaning of the symbolical results of Algebra has been carried within the last half century; and the complete interpretation of all which formerly appeared incongruous; the separation, as it was called, of the symbols of operation and quantity, which amounts to the use of an algebra in which the symbols represent something more than simple magnitude; will for some time to come suggest inquiry into the *logic* of this many-handeled instrument of reasoning, which seems able of presenting under fixed laws of operation, all the results which arise from very distinct primary conceptions as to the things operated on.

The idea that ordinary algebra is itself a calculus of operations was expanded upon by DEMORGAN in his article on the calculus of functions. There, he wrote that "strictly speaking, we have in the letters of Algebra our first *arbitrary symbols of operation.*" The science of "abstract number", he added, is best defined as "the method of reasoning upon the operations which may be performed on 1."[266] DEMORGAN then used this to justify the method of the separation of symbols in the following manner. The step from $u_x + \Delta u_x$ to $(1+\Delta) u_x$, considering $+\Delta u_x$ as the operation which converts u_x to u_{x+1} is the same as that from $a + ba$ to $(1+b) a$, when a is considered as an operation on 1 and $(1+b)\, a$ the same operation on a as $1+b$ on 1. But then, he added, the expression

$$(1+\Delta)^n u_x = u_x + n \Delta u_x + \frac{n(n+1)}{2} \Delta^2 u_x + \cdots \tag{5.8}$$

which treats Δ as a quantity on one side of the equation, and as a symbol of operation on the other, is not different from the way in which "the enlarged view of algebra" treats the expression

$$(1+a)^n b = b + nab + \frac{n(n+1)}{2} a^2 b + \cdots . \tag{5.9}$$

And, DEMORGAN concluded[267]

> The preceding view may perhaps remove some of the feeling of unmixed astonishment with which the student always regards (or ought to regard) the now common method of the *separation of the symbols of operation from those of quantity* in the higher mathematics.

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[264] A. DEMORGAN, "Foundations, I," 175.
[265] *Ibid.*, 173.
[266] A. DEMORGAN, "Calculus of functions," 306.
[267] *Ibid.*, 311.

How hard it was to break away from the rules of arithmetic is shown by DEMORGAN's further considerations. He stated that the notation $(1+\Delta)u_x$ does not require any extension of the meaning of the symbol $+$, as was required in writing $a+b\sqrt{-1}$, but that the general use of

$$(\varphi+\psi)\,x=\varphi x+\psi x \tag{5.10}$$

is not really acceptable, if the separation of symbols is to be applied. For then, he claimed, it must be defined so that

$$(\varphi+\psi)^2\,x=\varphi^2\,x+2\,\varphi\psi x+\psi^2 x \tag{5.11}$$

which seems to require that $\varphi\psi x=\psi\varphi x$. DEMORGAN felt that without this condition being satisfied, or a better definition of $+$, separation of symbols could not validly be applied here. In other words, he seems to limit his discussion to systems in which the laws of arithmetic and their consequences are maintained. But in a footnote he apparently recognized that this is not a necessary condition. These considerations show, he wrote, "how apt we are to take the convention derived from our own first view of numbers as the limit of our method of considering symbols."[268]

That the new views of algebra were related to the calculus of operations is also shown by the fact that although PEACOCK did not specifically write on the calculus of operations, DEMORGAN credits him with a large role in establishing it. In his article "Operations" in the *Penny Cyclopedia* DEMORGAN outlined the history of the method of the separation of symbols. He observed that in the early works by LAGRANGE, ARBOGAST, BRINKLEY—and even in the translation of LACROIX by BABBAGE, PEACOCK and HERSCHEL—it was regarded only as giving a strong presumption of truth, not as a method of proof. The first general account of why it should or should not give true results in certain cases, DEMORGAN continued, is found in the work of SERVOIS. But[269]

> The last step was virtually made by Dr. Peacock, in his *Algebra* (1830); for though this work does not mention the subject, yet it is the first which lays down the principles on which the theory of separation is neither a resemblance of algebra, nor a calculus of functions founded on algebra, but an algebra, or if the reader pleases, algebra itself; so that its conclusions rest upon the same foundations as those of ordinary algebra.

DEMORGAN's views of the relationships between the various questions being considered here—the calculus of operations, the geometric representation of the complex numbers, and the development of abstract algebra—were stated clearly in 1871. He wrote[270]

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[268] *Ibid.*, 312–313.

[269] AUGUSTUS DEMORGAN, "Operations," *Penny Cyclopedia*, London, 1840: vol. 16, 443.

[270] A. DEMORGAN, "On infinity," 169.

> Those who have seized the spirit of the relation between the different forms of algebra, the ascent from arithmetic to single and thence to double algebra, and to such triple algebra as has been given, the divergence to the calculus of operations, algebra divested of some of its laws which has been made an extension of the calculus of operations, and the method of quaternions, seek for illustration of difficulties by allowing the formal science to remain untouched, and looking for other matter of meaning to the symbols under which all the relations of form shall be preserved.

The reference to the extension of the calculus of operations is probably to the work of MURPHY and BOOLE on non-commutative operations.

The role played in BOOLE's thinking about logic by the calculus of operations was pointed out by ROBERT HARLEY, a friend and biographer of his. After discussing the paper of 1844, "On a general method of analysis," HARLEY says,[271]

> In the course of these speculations, and others of a like nature which grew out of them, Mr. Boole was led to consider the possibility of constructing a calculus of deductive reasoning. The severe discipline of his efforts to extend the powers of analysis had given him not only a complete mastery over its mechanical processes, but also, what was of far greater advantage a profound insight into its logical principles. In tracing out these principles he discovered that they admitted of an application to other objects of thought than number and quantity.

In a biographical sketch of her husband, MARY BOOLE asserted that the idea of symbolizing the relations of logic had occurred to him when he was a boy of seventeen.[272] BOOLE himself said that the impetus to publish his first logical work came from the controversy between DEMORGAN and the Scots philosopher and logician Sir WILLIAM HAMILTON on the quantification of the predicate.[273] However, this does not invalidate the point, suggested by HARLEY, and others, including Mrs. SOPHIE BRYANT and P. E. B. JOURDAIN, that BOOLE'S work in the calculus of operations was of great importance in shaping his theory of logic.[274]

Thus, BOOLE opened *The Mathematical Analysis of Logic* with an appeal to algebra. He wrote[275]

> Those who are acquainted with the present state of the theory of Symbolical Algebra, are aware that the validity of the processes of analysis does not

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[271] ROBERT HARLEY, "George Boole, F. R. S.," 1866. *Studies in Logic and Probability by George Boole.* LaSalle, Illinois, 1952: 444.

[272] MARY BOOLE, "Home-side of a scientific mind," *University Mag.*, 1878, **1**: 326.

[273] G. BOOLE, *Mathematical Analysis*, Preface.

[274] SOPHIE BRYANT, "The relation of mathematics to general formal logic," *Proc. of the Airst. Soc.*, 1902, **2**: 105–134 and PHILIP E. B. JOURDAIN, "The development of theories of mathematical logic and the principles of mathematics," *Quart. J. of Math.*, 1910, **41**: 324–352.

[275] G. BOOLE, *Mathematical Analysis*, 3.

> depend upon the interpretation of the symbols which are employed, but solely upon the laws of their combination.

The relationship between BOOLE's work in logic and the calculus of operations is apparent in the comments which he made in criticizing a paper by BRONWIN. The heightened degree of controversy in mathematics of late, BOOLE wrote, was due to "the unmeasured capabilities of modern analysis for the expression of general theorems." The primary reason for dispute was to be found, he added, "in the almost entire absence of any direct study of the laws of correct reasoning in connexion with the practical discipline of modern science."[276]

The close connection between BOOLE's theory of logic and his work in analysis is also shown in the chapters on symbolic methods in his textbook on differential equations. In the preface to that work he pointed out that the true value of symbolic methods depends only in part on their simplicity and power. Their true importance, he went on, lies in their connection with the general relationship between language and thought. Thus, he wrote, in regard to such methods,[277]

> in order to form a just estimate, we must consider them in another aspect, viz. as in some sort the visible manifestation of truths relating to the intimate and vital connexion of language with thought.

Throughout the discussion of the method of the separation of symbols, the connection with the logical theory is apparent. For example, we find[278]

> In thus expressing an operation by a symbol, in studying the laws of that symbol, and in founding processes and methods upon those laws, we introduce no strange or novel principles of Language; for it is the very office of Language to express by symbols the procedure of Thought.

BOOLE went on to consider the meaning of such expressions as $f\left(\frac{d}{dx}\right)$, and he concluded that "as a general principle, ... the mere processes of symbolical reasoning are independent of the conditions of their interpretation." This, he added, is not a mathematical principle, but rather "it claims a place among the *general* relations of Thought and Language."[279]

Further evidence that the idea of operation was basic in the work of the British mathematicians is found in the fact that in BOOLE's first logical work, *The Mathematical Analysis of Logic*, the basic symbols are symbols of operation. Thus he used the symbol 1 to represent the universe, symbols X, Y, Z to represent generic names; and a further class of symbols $\underline{x}$, $\underline{y}$, $\underline{z}$ conceived of as follows:[280]

> The symbol $\underline{x}$ operating on any subject comprehending individuals or classes, shall be supposed to select from that subject all the X's it contains.

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[276] G. BOOLE, "On a paper by the Rev. Brice Bronwin," 418.
[277] G. BOOLE, *Differential Equations*, vii–viii.
[278] *Ibid.*, 381.
[279] *Ibid.*, 399.
[280] G. BOOLE, *Mathematical Analysis*, 15.

It is these symbols of operation, which he called elective symbols whose laws of combination are then studied. And BOOLE noted that they satisfy the commutative and distributive laws, properties shared with symbols of quantity[281]

> in virtue of which, all the processes of common algebra are applicable to the present system. The one and sufficient axiom involved is that equivalent operations performed on equivalent subjects produce equivalent results.

In regard to the solution of equations involving elective symbols, BOOLE remarked that[282]

> from the very nature of elective symbols they are necessarily linear, and ... their solutions have a very close analogy with those of linear differential equations.

The similarity of approach between this and GREGORY'S work in the calculus of operations is striking.

The idea of operations as the fundamental concept in algebra, and a connection with the separation of symbols is also found in the work of HAMILTON. If we look at the theory developed in the 1835 essay on "pure time," we see that numbers and number couples are defined as operations on time steps and pairs of steps respectively. And in his discussion of that essay, in the preface to the *Lectures on Quaternions*, HAMILTON pointed out that $\sqrt{-1}$, in the theory of couples "is an operator on a couple of time steps."[283] After deriving the law for the multiplication of number couples in general, considering them as operations which act on step couples, HAMILTON noted in a footnote that[284]

> the principles of such derivation were only hinted at in the Essay of 1835... but it was perhaps sufficiently obvious that they depended on the 'separation of symbols' or on the abstraction of a common operand.

In his attempt to get the quaternions accepted, HAMILTON presented many different ways of considering them. But they all had in common the fact that the quaternions were essentially operations. Thus, in the *Lectures* he defined a quaternion as "a geometrical quotient" whose fundamental property is that "by *operating*, as a *multiplier* (or at least in a way *analogous* to multiplication), on the *divisor-line a*, it *produces* (or generates) the *dividend-line b*."[285] In the series of researches of 1848, HAMILTON, going back to the concepts of the 1835 essay, defined the quaternions as operations on quadruples of time steps, whose laws of combination were then derived by the method of the separation of symbols. Thus, for example, letting $q = (a, b, c, d)$, and defining the quaternion i by $i q = (-b, a, -d, c)$, HAMILTON noted that $i \cdot i q = (-a, -b, -c, -d) = -q$, so that

[281] *Ibid.*, 18.
[282] *Ibid.*, 70.
[283] HAMILTON, *Lectures*, (12).
[284] *Ibid.*
[285] *Ibid.*, (60)–(61).

"detaching the symbols of operation from those of the common operand" we have $i^2 = -1$.[286]

In a letter to DEMORGAN, written in 1854, on what he called "the Calculus of Quaternions," HAMILTON again emphasized quaternions as operations. He wrote[287]

> we may sum up all the essential properties of the three peculiar symbols i, j, k and consequently very concisely state the whole symbolical foundation of the Calculus of Quaternions, by saying that i, j, k are *distributive and associative symbols* of operations, *unconnected by any linear relation* but satisfying this *fundamental formula*: $i^2 = j^2 = k^2 = ijk = -1$.

One other important situation in which HAMILTON used the idea of operation and the separation of symbols was in his study of the operations S, V, K, T. These were defined as follows: if $Q = \mathrm{w} + i\mathrm{x} = j\mathrm{y} + k\mathrm{z}$, then $SQ = \mathrm{w}$, the scalar part of Q; $VQ = i\mathrm{x} + j\mathrm{y} + k\mathrm{z}$, the vector part of Q; $KQ = \mathrm{w} - i\mathrm{x} - j\mathrm{y} - k\mathrm{z}$ is the conjugate of Q and $TQ = \mathrm{x}^2 + \mathrm{y}^2 + \mathrm{z}^2$ its tensor modulus. These are significant in that by using S and V to separate the scalar and vector parts and studying them separately, HAMILTON and his followers, as CROWE has pointed out, were actually doing what is now known as vector analysis.[288]

With respect to HAMILTON's relationship to the symbolic school, it is interesting to note that after proving $T \cdot TQ = TQ$, using a geometric argument, HAMILTON added[289]

> even though it is possible thus to employ geometrical considerations to *suggest* and even to *demonstrate* the validity of many general transformations, yet it is always desirable to know how to obtain the same *symbolic results*, from the *laws of combination of the symbols*: nor ought the calculus of quaternions be regarded as complete, till all such *equivalences of form* can be deduced from such symbolic laws, by the fewest and simplest principles.

Important as the focus on symbols of operation and their laws of combination was on the formation of the abstract view of algebra, it clearly was not the only relevant factor. One that should be mentioned, and which deserves further study, is the influence of new ideas in geometry. ERNEST NAGEL has pointed out that it was projective geometry, rather than non-EUCLIDEAN geometry, which first made the mathematical community aware of the need to enlarge geometry from its EUCLIDEAN confines. He also pointed out the similarity between PEACOCK's principle of the permanence of equivalent forms and J. V. PONCELET's principle

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[286] HAMILTON, "Quaternions, First series," 168.

[287] HAMILTON to DEMORGAN, May 6, 1854. Sir WILLIAM ROWAN HAMILTON Papers, Trinity College, Dublin.

[288] CROWE, 32.

[289] WILLIAM ROWAN HAMILTON, "On quaternions; or on a new system of imaginaries in algebra," 1844–1850, *Math. Papers*: vol. 3, 241.

of continuity in geometry.[290] Also, as CARL BOYER noted, the full title of the second volume of PEACOCK's text on algebra, *On Symbolical Algebra, and its Applications to the Geometry of Position*, suggests that he was influenced by the work of LAZARE CARNOT.[291] Certainly the work of the French geometers, MONGE, PONCELET and MICHEL CHASLES, was well known and much admired in Great Britain during the period under discussion.

There is also the question of the influence of work in the theory of equations. While PEACOCK devoted a long section in his "Report on Analysis" to the subject, and HAMILTON did some research in it, this work does not seem to have contributed directly to their speculations on the "metaphysics" of algebra.

The last point that should be mentioned here is the role of the development of group theory. HAMILTON himself, in his late work on the icosian calculus, derived several group-theoretic results, but he regarded this work as a generalization of the quaternions, and it followed his major pronouncements on the nature of mathematics.[292] Of course, the major British contributor to group theory was ARTHUR CAYLEY. CAYLEY however tended to shy away from comments on the general nature of mathematics. But a short paper "On the Notion and Boundaries of Algebra," published in 1864, seems to show that he shared the views of the symbolical school. It also reflects the fact that early group theory was primarily concerned with groups of transformations, that is, of operations. CAYLEY wrote that "Algebra is an Art and a Science; qua Art it defines and prescribes operations which are either tactical or else logistical ..."; tactical operations refer to permutations or "the arrangement in any manner of a set of things;" logistical operations are those which are arithmetical. And he added, "qua Science Algebra affirms *a priori*, or predicts the result of any such tactical or logistical ... operation."[293] CAYLEY's work, and its relationship to that of his contemporaries, certainly deserves further study and I think would show that it too is closely related to the ideas discussed in this paper.

6. Conclusions

In any attempt to delineate the origins of a complex idea, such as an abstract view of algebra, one must be wary of making too strong a case for any one cause. For, as DEMORGAN pointed out in a letter to HAMILTON, a slight shift in emphasis may, for example, lead to the crediting of one or the other man for a certain discovery. DEMORGAN wrote[294]

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[290] ERNEST NAGEL, "The formation of modern conceptions of formal logic in the development of geometry," *Osiris*, 1939, **7**: 142–224.

[291] BOYER, 622.

[292] WILLIAM ROWAN HAMILTON, "Memorandum respecting a new system of roots of unity," 1856, *Math. Papers*: vol. 3, 610 and "Account of the icosian calculus," 1858, *Math. Papers*: vol. 3, 609.

[293] CAYLEY, "Notion of algebra," 293.

[294] R. GRAVES, vol. 3, 467.

The essentials of a subject are *subjective* things so that different people are really and truly the inventors to different people.

I have, in this paper, followed other scholars in giving credit for innovation and discovery: to WOODHOUSE, HERSCHEL, BABBAGE and PEACOCK for their roles in the introduction of Continental views to England and the subsequent reform, and to PEACOCK, GREGORY, DEMORGAN, BOOLE and HAMILTON for the work in algebra which followed. However, by concentrating on one key idea, namely the calculus of operations, I have presented a much more coherent picture of these developments than that usually put forth.

During the eighteenth century, England remained in intellectual isolation from the Continent. The work of the great Continental analysts—the BERNOULLIS, EULER, LAGRANGE and LAPLACE—was not assimilated. This had several causes. One external factor, undoubtedly, was the fact that England and France were at war much of the time. Also, there may have been a feeling of arrogance among English intellectuals raised by the admiration of Continental philosophers for the English political system and her achievements in industry and commerce. Furthermore, the NEWTON-LEIBNIZ priority battle left those in academic circles with the belief that it was a dishonor to NEWTON to abandon his notation or methods.

The rebirth of an active school of mathematical research at the beginning of the nineteenth century is always tied to the acquisition by the English of Continental, particularly French ideas. Among these was the calculus of operations, a subject discussed in the works of LAGRANGE, LAPLACE, ARBOGAST, CAUCHY, FRANÇAIS, SERVOIS and others. Those who effected the introduction of the French ideas knew and admired the French work in the calculus of operations, and HERSCHEL, BABBAGE and PEACOCK embodied this in their own research. HERSCHEL used ARBOGAST'S method of the separation of symbols for the solution of finite difference and differential equations, and generalized LAGRANGE'S work on the expression of finite differences by symbolic means; BABBAGE, in his work on functional equations, considered functional symbols to be symbols of operation; PEACOCK studied the meaning of $\frac{d^\alpha}{dx^\alpha}$, where α is not integral; all these topics belong to some degree to what came to be called the calculus of operations.

As the reformers had hoped, the introduction of Continental ideas was followed by a revitalization in science, particularly mathematics. Several new journals appeared, devoted wholly or in part to mathematics, and scientific societies sprung up on every side. The intellectual benefits include the work in abstract algebra, the invariant theory of CAYLEY and SYLVESTER, and the mathematical physics of GREEN, STOKES, KELVIN and MAXWELL. The follow up of the introduction of a new method in analysis by the formulation of an abstract concept of algebra was not fortuitious. For, as I have shown, they are connected by the calculus of operations.

GREGORY, BOOLE and DEMORGAN, who along with PEACOCK are generally considered the promulgators of the new algebra, all worked in the calculus of operations. GREGORY was an ardent supporter of the method of the separation of symbols, which he applied to differential, finite difference and functional equations. BOOLE extended this work to linear equations with variable coefficients, thereby of necessity considering non-commutative operations. DEMORGAN worked on functional equations. Both BOOLE and DEMORGAN devoted much space to the subject in their popular elementary textbooks. But even more important, we find that their views on the nature of mathematics are explicitly related to this work in the calculus of operations. GREGORY stated specifically that he was led to his theory of algebra by his desire to justify the use of the separation of symbols. DEMORGAN and BOOLE both included this as a development which called for a broader notion of mathematics, since one had an algebra in which the objects were not symbols of magnitude. Furthermore, and this could not just be coincidence, the idea of operations pervades their work. BOOLE considered his logical symbols to be symbols of operation. DEMORGAN and GREGORY suggested that the ordinary symbols of algebra might best be treated as if they too were symbols of operation. HAMILTON in several of his different treatments of the quaternions defined them as symbols of operation. Even in his work on algebra as the science of pure time, the idea of operation is central and the method of the separation of symbols a much used tool. Thus, we see that by concentrating on the calculus of operations, one can trace a direct development from the writings of various French authors through the work of the members of Analytical Society to the formulation of a new concept of algebra in England.

It is interesting to note also that the other contemporary developments in mathematics which influenced the English writers also come to them from the French. Projective geometry during the first part of the nineteenth century was very much a French science. Certainly knowledge of that subject by the British mathematicians seems to have come primarily from that source. BUÉE and ARGAND were among the earliest to give geometric interpretations of the complex numbers, and the work of the former was published in England. CAUCHY gave one of the first non-geometric justifications of the complex numbers. Nor was there a total lack of interest in the foundations of mathematics in France. The early work of CARNOT and LAGRANGE on the basis of the differential calculus was carried forward by ARBOGAST and SERVOIS; while CAUCHY made rigorous the idea of limit. But it was in England that the idea of abstract algebra was first formulated.

It is interesting to speculate on the reason for the fact that these ideas had to be transported to England before they were combined into a general concept of algebra as a formal system. I think one may say that it seems to bear out PIERRE DUHEM'S theory of the national character of the two countries. DUHEM distinguished the "esprit de finesse" or weak but ample mind of the English from the "esprit géométrique" or narrow but rigorous mind of the French.[295] And it is true that we find that the French never really trusted the calculus of operations, because it seemed to have no basis other than analogy and they could not give general limits as to the extent of its applicability. In their more

[295] PIERRE DUHEM, *The Aim and Structure of Physical Theory*, trans. P. WEINER. New York, 1962: 60–69.

pragmatic fashion, the British accepted the method happily, since it worked in so many cases. And in extending it to non-commutative operations and trying to justify its use they developed a broad theory which in effect led to a formal view of the nature of algebra.

This work is a revised version of a dissertation submitted in partial fulfillment of the requirements for the Ph. D. degree at The Johns Hopkins University in 1969. I would like to thank Professor CARL BOYER of Brooklyn College of the City University of New York and Professor HARRY WOOLF of Johns Hopkins for their assistance.

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(Received August 13, 1971)