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The Making of Peacock's Treatise on Algebra: A Case of Creative Indecision

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0. Introduction

When the small group of Cambridge undergraduates led by Charles Babbage, John Herschel and George Peacock[1] founded the Analytical Society in 1811, they envisaged more than a forum for enthusiasts of continental analysis to compare notes. The founding members of the Society aspired to more than *self*-improvement. Their declared mission was to bring the mathematics taught and practiced at Cambridge back in line with state-of-the-art continental developments. To this end they declared their immediate and most urgent target to be the calculus.[2] "Discovered by Fermat, concinnated and rendered analytical by Newton, and enriched by Leibniz with a powerful and comprehensive notation," wrote Herschel and Babbage in their joint preface to the *Memoirs of the Analytical Society* 1813 (the only volume of memoirs the Society would ever publish), "as if the soil of this country [was] unfavourable to its cultivation, it soon drooped and almost faded into neglect; and we now have to re-import the exotic, with nearly a century of foreign improvement, and to render it once more indigenous among us".[3] The young rebels knew fully well that in order to regain its past glory, Cambridge would, in important respects, have to turn its back on its past achievements. They also realized that in order to succeed in "re-importing the exotic" into the University it would not be enough for learned mathematical research papers to be published out of Cambridge. The University's curriculum, and most of all its examination system would have to be

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[1] According to Babbage (1817) the Analytical Society was founded by seven students (Babbage, Peacock and Michael Slegg of Trinity, and Richard Gwatkin, Herschel, John Whittaker, and Henry Wilkinson of St. John's) and one recent graduate (Edward Bromhead, at the time a scholar of Caius). For further details on the founders and their backgrounds see Enros (1979, 103–22).

[2] On the Analytical Society see Enros (1979), (1981) and (1983), Becher (1980a) and (1980b), Schweber (1981, 54–67), Fisch (1991a, §2.221–3) and (1994b). Some disagreement exists regarding the date of the Society's formation. Compare Schweber (1981, 56f.) and Fisch (1991a, 175 n.15) to Hyman (1982, 23–4) and especially Enros (1979, 103ff.).

[3] Herschel and Babbage (1813, iv).

thoroughly revised. But for that to happen suitable mathematics textbooks had first to be made available.[4]

According to Babbage, it was he who had originally proposed the formation of such a Society, and indeed to have done so with the translation into English of a particular textbook in mind. The textbook selected by Babbage was the celebrated French textbook writer, Silvestre Lacroix's 1802 *Traité elémentaire de calcul différential et du calcul intégral*, translated jointly by Babbage, Herschel and Peacock in 1816 as *An Elementary Treatise of the Differential and Integral Calculus*. There exists, however, a curious discrepancy between Babbage's latter-day recollection of his plan and the manner in which

[4] Work published on the Analytical Society since Philip Enros's pathbreaking dissertation of 1979, especially by Sam Schweber (1981) and the present author (1991a) and (1994b), has tended to contest two of Enros's main conclusions. The two are related, and concern the aims and the lifespan of the Society. According to Enros (1979, 157) the Analytical Society disbanded in 1813 with the graduation of most of its members. It had existed, on his reckoning, for a period of just over two years. (He mistakenly follows Babbage in claiming that the Society was founded in May 1812 – see previous note.) Enros also insists that although dissatisfaction with the mathematics taught at the university "had been a factor" in the formation of the Society, it "was not founded to change or reform the mathematical studies at Cambridge ... but rather to further analysis" (1979, 121, 165). He portrays the Society primarily as "a mathematical society" that functioned to "aid in the mathematical research of some of its members" (165). He is forced, therefore, to view Herschel, Babbage and Peacock's campaign to change things at the University as a later and quite distinct episode, for which the Society "was the precursor". In one obvious sense Enros is right: prior to obtaining College positions, members of the Society could have very little direct influence on the curriculum (a task to which Peacock and later Whewell set themselves in earnest after their Trinity Fellowships had been secured). The only way for a Society of undergraduates to do so indirectly, from the outside, was by making available to the University a suitable English language textbook. Enros ignores the fact that even Babbage, who, unlike his two colleagues, never had much to do with students or teaching, claimed repeatedly that the need for an English language edition of Lacroix had motivated the formation of the Society from the start. He himself translated the first part, with Herschel and Peacock finishing the job and providing the Notes. The three Society co-founders had also resolved early on to collaborate in the production of a multivolume compendium of problems, which although was only published in 1820, was publicly promised in the advertisement to their edition of Lacroix in 1816. Had the Society indeed disbanded in 1813, Enros could still have argued that all of this belonged to a later phase, but apparently it did not. The most decisive piece of evidence against Enros's account are the minutes of the last known meeting of the Society, preserved at the British Museum as Add 37182 f64 (cited at length by Schweber (1981, 64)) – a meeting reported to have been held at the house of Edwin Ffrench Bromhead in Thurlby near Lincoln on December 20, 1817! Why is all of this important? By ignoring the Society's protest and rebellious self-image, playing down its plans for reform (even though they were only realized later), and portraying it as geared primarily toward research, Peacock, who did not engage in original research during the early years, is virtually written out of Enros's account, and is relegated, by implication, to the role of an awestruck groupie (p. 120). Content with an all too general definition of 'analytics' as implying "an algebraic or formal, operational approach to the subject matter" (5), and focusing exclusively on the original mathematical work of Herschel and Babbage, Enros misses the foundational and tactical disagreements between Babbage and Peacock evidenced in the latter's correspondence and Notes.

it had in fact been realized forty-eight years earlier. "At this period", wrote Babbage in an oft quoted passage of his 1864 autobiography,

> Cambridge was agitated by a fierce controversy. Societies had been formed for printing and circulating the Bible. One party proposed to circulate it with notes, in order to make it intelligible; whilst the other scornfully rejected all explanations of the word of God as profane attempts to mend that which was perfect.
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> The walls of the town were placarded with broadsides, and posters were sent from house to house. One of the latter form of advertisement was lying upon my table (...) Taking up the paper and looking through it, I thought it, from its exaggerated tone, a good subject for parody.
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> I then drew up the sketch of a society to be instituted for translating the small work of Lacroix on the Differential and Integral <Calculus>. It proposed that we should have periodical meetings for the propagation of D's; and consigned to perdition all who supported the heresy of Dots. It maintained that the work of Lacroix was perfect that any comment was unnecessary.[5]

Lacroix's 1802 *Traité elémentaire* does indeed provide a lucid introduction to the calculus clearly casting it in Leibniz's 'd' notation. In this respect it was unarguably perfect. The English edition was not ready for publication until 1816. The cause of the delay was Peacock who, quite contrary to Babbage's witty parody, had insisted that the English-language Lacroix be circulated among the English readership annotated by over a hundred additional pages of copious Notes. Peacock apparently found the famous French textbook less perfect than Babbage's clever quip suggests that he, Babbage, had believed at the time. Peacock's Notes left Lacroix's "d" notation intact. Whatever it was, the point of contention between the two men was not about notation as such.[6] Whatever Peacock found wanting in the Frenchman's presentation of the calculus, and he evidently found much, seems not to have initially impressed Babbage – if, of course, the latter's autobiography can be trusted.

Personal, motivated memoirs written late in life and long after the event are bound to be distorted by age, hindsight, and a latter-day desire to settle scores. Babbage's memoir certainly manages to get some of the dates wrong.[7] Still, even that long after the event, knowing fully well what the Society's rendition of Lacroix ended up looking like, it is hard to imagine how Babbage might have unwittingly confused the parties to the Bible Society dispute to the extent of imagining that he had parodied the wrong poster. It makes better sense, I think, to assume that, despite the time difference, the discrepancy between Babbage's recollection of his initial motives and plan for the Society, and the textbook eventually produced on Peacock's insistence, indicates real disagreement. But disagreement between whom, when and about what? As far as I can tell Babbage's recollected satire represents a position he had held uninterruptedly for the best of fifty

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[5] Babbage (1864, 28–9).

[6] At this juncture I am referring exclusively to the twelve Notes penned by Peacock. The position adopted in Herschel's share of the Notes, as we shall see shortly, was different, and closer to that of Babbage. Needless to say, neither did Herschel's Notes in any way qualify Lacroix's original notation.

[7] Compare Babbage (1864, 26–7) and Hyman (1982, 23–4) to Schweber (1981, 56–8) and Fisch (1991a, 25–6).

years. He certainly spoke up strongly for it during the late 1810s, as we shall see – in which case, the fact that the Society's Lacroix failed to live up to this ideal would have to owe to Peacock initially having a rather different view of matters.

Peacock looms large in the growing literature on the history of nineteenth century British mathematics, and justly so. He is best known for his *Treatise on Algebra* of 1830 – a work regarded as a minor milestone in being the first major work published in England to endorse a significantly formalistic approach to algebra. It was certainly a bold and extremely well-informed work. Still, in retrospect, one would be pressed to point to a lasting mathematical or metamathematical achievement contained within it. Even as a precursor of abstract algebra the position detailed by the *Treatise* was swiftly eclipsed by the keener writings of D.F. Gregory, Augustus DeMorgan and George Boole. And yet, historically speaking, it was, in at least two major respects, the most important mathematical publication of the decade: first, for the way it meticulously, if unwittingly concretizes and preserves for latter-day perusal *the* central metamathematical dilemma of its day; and second, for the profound impact it had on others. The historical and historiographical significance of Peacock's algebra pertains first to it being a response to problems, and second to the ways it was conceived as problematic by others and was responded to. In both these respects the *Treatise* affords historians a major resource. I have elsewhere, in preliminary fashion, attempted to deal with its importance in the latter respect,[8] arguing that Peacock's *Treatise* of 1830 along with his British Association "Report on the Recent Progress and Present State of Certain Branches of Analysis" of 1834[9] were jointly responsible, almost single-handedly, for the foundational debate on the nature of mathematics in general, and of algebra in particular, that raged throughout the decade. By the problems it was thought to pose, rather than by the example it was thought to set, it provoked major works by William Whewell, DeMorgan, William Rowan Hamilton, and the younger D.F. Gregory, whose impact on the next, and better known generation of Cambridge trained mathematicians and physicists would be decisive. There was nothing unwitting about this response. All four writers acknowledged the importance of Peacock's algebra to their own thinking, expressed great respect for its achievement, and engaged with it energetically. The present paper concentrates for the most part on the importance of the *Treatise* in the former respect; on reading it as an anxious and problematic response to what Peacock perceived as *the* metamathematical challenge of the day.

Much of what has been written on Peacock in recent years has tended to describe the innovative and controversial claims set forth in the *Treatise* as a direct and natural continuation of his equally innovative and controversial involvement in the Analytical Society almost two decades earlier. The *Treatise,* according to this view, did little more than to detail and systematically expose a view of algebra to which its author had been committed all along.[10] The Society had endorsed Lagrange's formalistic approach

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[8] Fisch (1994b).

[9] The *Treatise* was essentially a Cambridge textbook. The BAAS "Report", Peacock (1834), on the other hand, targeted a wider and in a sense a more 'professional' readership from among the 'gentlemen of science'.

[10] See, for instance, Novy (1973) especially pp. 189f.

to the calculus, and Peacock, in his later work, simply extended it to algebra. I shall try to show that this portrayal of Peacock is largely mistaken in both its claims; that his endorsement of Lagrange was originally undertaken with a very different view of algebra in mind, a view from which the *Treatise* represents a significant, if interestingly incomplete retreat.[11] Peacock, I believe, set out to do one thing and ended up fifteen years or so later, rather unwillingly, doing another. It is to this preliminary, and largely unstudied phase in Peacock's development, and to the special role I now realize Babbage had played in it, that I devote the present study.

1. Babbage's "Philosophy of Analysis"

I have remarked in the past that Peacock's intellectual biography is destined to remain something of a mystery.[12] He appears to have left no drafts, notebooks or journal – nothing that might help shed light on his deliberations during the crucial period between the publication of his "Notes" to the Society's 1816 edition of Lacroix and that of the *Treatise* of 1830.[13] Apart from the works he published, all we have to go on, at this stage are a small number of relevant letters. One such letter, that has come to my attention since making that remark in 1994, is, I now realize, of major significance. It was written by Peacock to Babbage on May 7, 1822 to inform the latter that the essays he had sent Peacock had been read and would be returned by coach the following day. Peacock admits to have been "completely baffled" by the latter part of the first essay, and the greater part of the second, but goes on to say of the entire batch that as

[11] One reason for this is a tendency on behalf of many historians of mathematics to concentrate on determining the extent to which the *Treatise* of 1830 was as an early precursor of later developments. Peacock's algebra is deemed, quite whiggishly, a rather interesting development in the history of mathematics in being a forerunner of the kind of formalistic approach to algebra that would come of age in the 1840's and '50's. Nagel (1935, 448–55), Bell (1945, 180–1), Clock (1964, 4–34), Novy (1973, 189–94), Koppelman (1971/2, 215ff.) are all cases in point. These assessments are generally true, but are of little historical significance for those working on Peacock. Reading a work retrospectively, as anticipating or failing to anticipate subsequent developments, can contribute nothing to an understanding of why the antecedent work was written, or, subsequently, to explaining its immediate impact. Questions such as these – rendered all the more pressing by the *prima facie* novelty of the work under consideration – require a forward, rather than backward gazing perspective on the field. To make historical sense of the *Treatise* is first to ask *why* Peacock wrote it, and to then go on to explain its reception and impact. Interesting as it may be, determining by hindsight the extent to which it had, or had not foreshadowed later developments is quite irrelevant to answering these questions.

[12] Fisch (1994b, 262).

[13] I can detect no substantial differences between the views of algebra conveyed by the *Treatise* and the BAAS Report of four years later. The second edition of the *Treatise* published in two volumes in 1842 and 1845 does register important changes as we shall see in conclusion to the present study. See also Fisch (1994b, 274 ft. 96).

> I have before expressed my opinion concerning them; they must form when completed a work of very great interest, abounding as they do with so much of original research & with illustrations of the most interesting kind.[14]

Peacock's letter registers puzzlement, disagreement, acknowledgment of Babbage's originality, and a sense of having been through it all with him before. John Dubbey has identified the essays to which Peacock's letter refers as the collection of papers written by Babbage around 1821 promisingly entitled: "Essays on the Philosophy of Analysis". He doubts, though, that the version of them perused by Peacock coincides with the one now preserved at the British Museum.[15] Dubbey bases this assessment on the fact that Peacock's remarks about the length and difficulty of the first two papers "are not borne out by an examination" of the two first essays listed in Babbage's table of contents – "On notation" and "Of the Influence of General Signs in Analytical Reasoning". These, however, are *not* the first two essays in the manuscript preserved at the British Museum. The former seems at the time not to have got beyond a couple of pages, while the latter, had been submitted by Babbage to the Cambridge Philosophical Society and read there six months earlier on December 1821. This is an essay with which Peacock would have been well acquainted long before the correspondence of 1822, and it is doubtful that Babbage would have included it with the new batch of writings he had sent for his old friend's opinion. But if we assume, contrary to Dubbey, that the batch of essays comprising the British Museum "Philosophy of Analysis" manuscript are the ones to which Peacock's letter refers (and there is no evidence whatsoever to the contrary), and take Peacock's remark to refer not to the order of titles on Babbage's roughly sketched table of contents, but to the first two essays of the batch itself, a very different picture emerges from the one Dubbey tells.[16]

Dubbey, however, is absolutely right in drawing attention to the historical significance of Babbage's brief, yet, as we shall see, fascinating early foray into the foundations of analysis, and to its special significance to Peacock's development. So that although no new archival material of Peacock's has come to light, these papers of Babbage, now

[14] Peacock to Babbage, May 7, 1822, British Museum Additional Manuscripts 37182, No. 323, cited Dubbey (1978, 95).

[15] British Museum, Additional Manuscripts 37202, referred thereon as "Philosophy of Analysis".

[16] Folios 5 and 6 of "Philosophy of Analysis" appears to be the draft of a cover letter to the Cambridge Philosophical Society accompanying the paper he delivered in December 1821, announcing and outlining the entire project. In which case, it is doubtful that it would have been included in the package to Peacock. Dubbey, mistakenly in my opinion, describes it as a draft of the introduction to the projected book. The opening paragraph begins as follows: "In presenting to the Cambridge Philosophical Society the first of a series of *essays on the Philosophy of Analysis* some account of the object and nature of the discussions they will contain as well as a sketch of the path and purpose [they] pursue may render their intimate connections more evident...". It is quite clear that "Philosophy of Analysis" represents work in progress of which only the first had been completed by the time Peacock had read it. I doubt that Babbage, who quickly became engrossed in the schemes for his difference engines, ever worked much further on any of the papers. So that in all likelihood the material Peacock had read and had written back about was very close, if not identical, to the version of "Philosophy of Analysis" in hand.

fully deciphered, present an excellent opportunity for me to attempt once more, and in greater detail, to speculate on the erotetic process responsible for Peacock's *Treatise*.[17]

For those familiar with the relatively little written on the mathematical writings of Babbage and Peacock, the suggestion that the two men might have disagreed from the start would seem rather strange. Prior to my suggestion published in 1994 (but nowhere further developed) that perhaps the young rebels failed fully to agree on the nature of the algebraical foundation of the calculus, all historical accounts of the Analytical Society and its aftermath have assumed that Peacock, Babbage and Herschel – to whom the Society owes everything that was published in association with its name – had perhaps pursued slightly different topics in their research,[18] but, with regard to fundamentals, had been of one mind and had worked in full agreement. In his biographical sketch of Herschel, Schweber suggests that perhaps "all was not sweet reasonableness within the rank of the [Analytical] Society and that some animosity existed between the Johnian members and the others". Schweber cites a letter written by William Maule to Babbage in which he makes mention of "the great difference of opinion which distract[s] the internal constitution of the society".[19] Schweber did not pursue the point any further, but gives the distinct impression that, in his opinion, the tension and "great difference of opinions" had to do more with the running of the society than with matters of substance.

Indeed so close are the views of Peacock and Babbage on the nature of mathematics alleged to have been, that Dubbey registers surprise at the fact that the two men could have remained friends after Peacock published his *Treatise on Algebra*, which, claims Dubbey, all but plagiarizes the "Philosophy of Analysis" Babbage had shown him eight or nine years earlier.[20] There are, of course, obvious similarities between Peacock's mature position, and the position outlined in Babbage's "Philosophy of Analysis". A closer look at the two men's early and subsequent mathematical writings indicates, however, that their initial points of departure where significantly different. Writing in 1822, Peacock gives the impression of being baffled, but not at all surprised by Babbage's position of which he reports to have before expressed his opinion. Assuming that their differences dated back at least to the late 1810s, this might very well account for the discrepancy between Babbage's memoir and Peacock's Lacroix.

[17] I was unaware of Babbage's unpublished "Philosophy of Analysis" at the time, and had ascribed Peacock's change of heart to other factors Fisch (1994b, 262–3). In this respect, that early study is now superseded. For a preliminary attempt to articulate the philosophical grounding and historiographical implications of the erotetic method see Fisch (1994a) and more recently Fisch (1997, 1–38). I hope, in the near future, to devote a separate book to the further elaboration of the theory of rationality and progress sketched in these works.

[18] Peacock produced no original work during the early years. Babbage concentrated for the most on developing the calculus of functions, and Herschel on the calculus of finite differences. Detailed assessments and comparisons of their early papers can be found in Schweber (1981), Enros (1979, Ch. V), and Koppelman (1971/2).

[19] Schweber (1981, 59–60) citing W.H. Maule to Babbage, January 16, 1812, British Museum Add MSS 37182, f3.

[20] Dubbey (1978, 93–107) – a view wholly endorsed by Hyman (1978) though mollified in Hyman (1982, 36).

In 1835, reacting directly to Peacock's *Treatise*,[21] William Rowan Hamilton formulated his oft quoted tripartite taxonomy of approaches to algebra. Despite a certain lack of finesse, it provides an authentic, if tendentious mapping of the field.[22] According to Hamilton, the three "schools" distinguish themselves by the type of problems they address. They represent three algebraic projects, fully capable, in Hamilton's view, of peaceful coexistence. In this, we shall see, he was not only biased but wrong. The type of approach advocated by Babbage firmly and knowingly contradicted the kind of approach Peacock was reluctant to abandon.

"The Study of Algebra", wrote Hamilton in his essay of 1837 on Algebra as the Science of Pure Time, "may be pursued in three very different schools, the Practical, the Philological or the Theoretical, according as Algebra itself is accounted an Instrument, or a Language, or a Contemplation". The "practical", or applied algebraist regards algebra primarily as an art, a technique, as a system of rules prized for its practical usefulness. The "philological algebraist", or formalist, regards it as a language valued for the conciseness of its notation and its logical coherence. While the "theoretical algebraist", regards it as a science, as a system of connected propositions, grounded on a clear "contemplation" of ideas and valued primarily for their truth.

What would appear to be a foundational disagreement concerning the very nature of algebra turns out to be a mere difference of emphasis. In truth, Hamilton implies, algebra is all of these things together: an instrument, a language, and a science. It so happens that different people are attracted to different kinds of problems.

> The Practical Algebraist complains of imperfection when he finds his Instrument limited in power; when a rule, which he could happily apply to many cases, can be hardly or not at all applied by him to some new case ... The Philological Algebraist complains of imperfection, when his Language presents him with an Anomaly; when he finds an Exception disturb the simplicity of his Notation, or the symmetrical structure of his Syntax ... The Theoretical Algebraist complains of imperfection, when the clearness of his Contemplation is obscured; ... when he ... cannot look beyond the signs to the things signified.[23]

Ideally one would like to see one's chosen area of mathematics perfected in all three respects, as, he argues, is largely the case in geometry. The problem with algebra is that despite having made considerable practical and philological progress, its theoretical basis has been neglected. In fact algebra is now a theoretician's nightmare plagued by "confusions of thought and errors of reasoning". His delightful, almost Berkeleyian prose is worth quoting at length.

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[21] Hamilton to Peacock, 23 July 1835, Trinity College Library, Add.ms.b.49[44].

[22] For criticism of Hamilton's taxonomy see Fisch (1994b, 258–9). Despite their flaws, Ivor Grattan-Guinness makes good use of Hamilton's distinctions in a recent paper on the nature of Euclid's *Elements*. He sets them up very differently from the way I believe Hamilton intended them to be presented (by viewing the various styles of algebra in relation to the nature of its application to non algebraic domains, rather than, as Hamilton phrased them, in relation to what are to count as algebraic problems), but usefully alludes to them anyway. See Grattan-Guinness (1996, 359).

[23] Hamilton (1837, 293).

> . . . it requires no peculiar scepticism to doubt, or even to disbelieve, the doctrine of Negatives and Imaginaries, when set forth (as it has commonly been) with principles like these: that a greater magnitude may be subtracted from a less, and that the remainder is less than nothing; that . . . [two] numbers denoting magnitudes each less than nothing, may be multiplied the one by the other, and that the product will be a positive number, or a number denoting a magnitude greater than nothing; and that although the square of a number, or the product obtained by multiplying that number by itself, is therefore always positive, whether the number be positive or negative, yet that numbers, called imaginary, can be found or conceived or determined, and operated on by all the rules of positive and negative numbers, as if they were subject to those rules, although they have negative squares, and must therefore be supposed to be themselves neither positive nor negative, nor yet null numbers, so that the magnitudes which they are supposed to denote can neither be greater than nothing, nor less than nothing, nor even equal to nothing.

"It must be hard to found a SCIENCE on such grounds as these",[24] he concludes.

Hamilton had every right to complain about the state of "theoretical" algebra. But he was quite wrong to imply that the problem was merely a matter of algebraists getting their priorities right. The project of theoretical, or scientific algebra was not merely suffering from lack of attention. The very idea of treating algebraical activity as contemplative, and algebraical formulae as (therefore) admitting of truth values was under direct attack.[25] Paradoxically, Peacock, who Hamilton took to be the representative philologist, was, at the time, more of an ally than Hamilton could have appreciated. But I am getting ahead of myself.

In the terms of Hamilton's taxonomy, Babbage's "Philosophy of Analysis" represents the work of a fully-fledged, unabashed, self-conscious philologist. If Hamilton and DeMorgan expressed deep discomfort during the early 1830s at the idea of algebra being, as the latter would put it in his review of Peacock's *Treatise*, "something like symbols bewitched, and running about the world in search of a meaning",[26] Babbage's "Philosophy of Analysis" of almost fifteen years earlier, argued forcefully that failure to wholly "dissever" algebra from its applications, and render it an independent, self-sufficient "language of signs" (or, as he put it elsewhere, in shameless defiance of Hamilton's later taxonomy: a "science of analysis") is the greatest obstruction to its development. "The cause that has mainly contributed to fetter the language of signs", he observed, "may be found in this circumstance; that being in itself a method of reasoning of extreme generality it was discovered through the mediation of one of its particular applications: that of number".[27] Unapplied, of itself, algebra constituted a language and

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[24] Hamilton (1837, 294).

[25] Ten years or so later, his work on quaternions and symbolic geometry proceeding nicely despite wide dissatisfaction with his idea of algebra as the science of pure time, Hamilton seems to have softened to the point of defying his original terminology in the course of acknowledging the existence not merely of people who are drawn to algebra's philological difficulties, but to there being a sort of "*symbolical science*, or *science of language* which well deserves to be studied . . . [in which] *forms of expression* [are] 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." [Graves (1975, II 521–2)]. See also Hamilton (1853, 14–16) and Koppelman (1971/2, 226ff.).

[26] DeMorgan (1835, 311).

[27] "Philosophy of Analysis", f. 42.

only a language. In Babbage's view, philologists and theoreticians were not disputing the relative urgency, but the legitimacy of the problems they worked on; they were disputing the very nature of algebra. From this perspective theoretical concerns regarding analysis were not unimportant, they were misplaced. On this point, so fundamental to Babbage's position, the difference between his and Peacock's view was glaring. Where Babbage saw a misdirected, restricting fetter, Peacock, we shall see, saw an indispensable lifeline.

For those acquainted with Babbage's earlier work, as Peacock's brief letter indicates, the "Philosophy of Analysis" hardly represented a new development. Many of the key elements of the radically formalistic position presented in this work can be found in earlier writings and letters – most importantly, an unpublished work dating from 1817 entitled: "The History of the Origin and Progress of the Calculus of Functions During the Years 1809, 1810 . . . 1817".[28] The only major development his work does exhibit during this period is a move from the calculus of functions he was working on to an ambitious attempt to ground all of mathematical analysis on the principle of identity.[29] In terms of Hamilton's taxonomy, Babbage was determined to banish all theoretical considerations from analysis to its applications, thereby rendering analysis a matter of pure philology. Mathematical analysis, considered of itself, was not at all a science governed by a truth relation, but a purely syntactical system governed by a grammar. "The object which I propose to attempt", he wrote in one of the draft essays he had sent to Peacock, "is to separate entirely Analysis as the language of signs from all its various applications".[30] Babbage envisaged a comprehensive "science of analysis" presupposing nothing more than a series of well-defined operations subject to a formal principle of identity whenever applied. These early unpublished works of Babbage clearly present a view of algebra which is widely thought to have only seriously begun its career twenty-five or so years later in the work of D.F Gregory, the later DeMorgan and Boole.[31] At most Babbage is credited for the role he played in the introduction of continental mathematics into England, and for the innovative way in which he had approached specific problems. The following remark by Elaine Koppelman is typical:

> 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

[28] Museum of the History of Science, Oxford University, Buxton ms. 13.

[29] Babbage to Bromhead, Aug. 6, 1815, private collection of Sir Edward Ffrench Bromhead's correspondence in the possession of Sir Benjamin Denis Gonville Bromhead, Thurlby Hall, Thurlby, Linconshire, cited Enros (1979, 179).

[30] An essay entitled: "<Of> General Notions Respecting Analysis", "Philosophy of Analysis", ff. 41–55; quotation at f. 44.

[31] As we shall see Babbage was not merely proposing *a* calculus of operations, but was redefining algebra itself as a wholly abstract, non-quantitative calculus. Working almost exclusively from published material, standard histories of the development of abstract algebra in England, such as Clock (1964), Koppelman (1971/2) and Novy (1973), have completely missed this early contribution, which in view of Peacock's acquaintance with it, is of crucial importance. Though perhaps not fully appreciative of the metamathematical import of Babbage's proposals, Enros (1979), by contrast, takes full count of its existence. Unfortunately, for reasons explained above (ft. 4), Enros chose to leave Peacock out of the story, and as a result, despite his superior appreciation of Babbage's unpublished work, he misses its single real impact.

> separation of symbols, introduce concepts which were to be influential in the extension of the concept of algebra.[32]

Babbage never published such a philosophy, but he certainly suggested one. He worked hard at the "Philosophy of Analysis" for several years,[33] eventually loosing interest and abandoning the project by about 1822. By then he had shown it to Peacock who, I believe, in direct response wrote the *Treatise*.

"In divesting analysis of all relation to number", wrote Babbage in 1817,

> ...I should consider every equation as an abridged representation of the operations[,] which if actually performed at length[,] would cause all the terms mutually to destroy each other.[34]

The point was elaborated in the "Philosophy of Analysis". Viewed thus, analysis

> ultimately resolves itself into propositions which are purely identical[,] or at least that the signification of every equation amounts to nothing more than that when all the operations which are indicated on each side are actually executed[,] every letter which occurs on the one side will be found occurring under precisely similar circumstances on the other[,] and in case any letters stand as representatives for others[,] these latter must be substituted for them before the identity becomes apparent.[35]

In Babbage's opinion, algebraic equations do not stand for 'truths of contemplation'. They do not *reduce* (though, of course, may be applied) to statements found true of some realm of contemplated objects or concepts. They represent formal identities regulated by operations performed upon featureless objects. A well-formed analytical proposition is an equation in which the two sides can be reduced to identical 'letters' by a series of operations that Babbage apparently believed exclusively constitute the 'language of signs'. It is quite clear that Babbage's redefinition of analysis was undertaken in conscious opposition to what Hamilton would later describe as "theoretical" algebra and William Whewell as "pure" (as opposed to "inductive") science.[36] But it also differed, equally consciously, from other trends current among the young 'analyticals' at the time. Babbage's "Philosophy of Analysis" is not concerned in developing a calculus of operations as such, nor does it seem in any way concerned with the separation of symbols (those denoting quantity from those signifying operations) – the two transitional

32 Koppelman (1971/2, 187), but compare Enros (1979, 179ff.) (See previous fn.).

33 The general plan to found "the whole system on the principle of identity" was apparently formulated and discussed with friends in some detail by as early as 1816 (Buxton ms. 13, p. 202). Enros (1979, 202 ft. 2) draws attention in this regard to a misfiled letter from Babbage to Brewster of June 20, 1821 (Babbage ms., British Library) which I have so far been unable to consult.

34 Buxton ms. 13, p. 217.

35 "Philosophy of Analysis", f. 44.

36 Not unlike Hamilton, Whewell conceived of a "pure" science as a well-formed, axiomaticized, 'Euclidean', deductively connected scheme of propositions derivable from a set of true axioms. The notion of axiomatic truth is central to his system. Axioms of a "pure" science (or the pure component of an "inductive" science), are intuitively recognized as stating true features of a "fundamental idea". They do not reduce to arbitrary definitions, but to basic truths concerning basic ideas. On Whewell's theory of the truth of "pure" science see Fisch (1991a, 154–9). For his unsuccessful attempt to include higher algebra within this scheme see Fisch (1994b, 266–76).

stages credited by historians of mathematics to Babbage's generation prior to the birth of a fully-fledged abstract algebra in the hands of DeMorgan and Boole. Koppelman is right in noting that, like those of Herschel and to a lesser extent Peacock, "The Writings of Babbage (...) testify to [his] high regard for the importance of the calculus of operations".[37] But his "Philosophy of Analysis" was geared to more than to further exploiting the formal analogies or disanalogies between, say, repeated operations and the law of exponents. It insisted on the study of pure analysis in its purity, devoid of meaning, prior to application, prior to interpretation; to conceive of it as what we would today term a purely syntactical system. It was not an attempt to establish *a* calculus of operations, but an attempt to distill and articulate the formal, operational system that, in his view, grounds and informs all forms of mathematical analysis. More than twenty years later, in an oft quoted passage from a series of papers devoted to the foundations of algebra, DeMorgan, who is widely considered a major contributor to the development of abstract algebra, deemed such a view overly formalistic and deprived of much of what mathematics is really about. A person

> who makes the transformations of algebra by the defined laws of operation only 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.[38]

For Babbage the point of foundational studies in mathematics was not merely to emulate working mathematicians, but rather to deconstruct, and break down the mathematical thought process to its logically ordered elements. "It is my intention in the following essays", he wrote in the cover letter to the Cambridge Philosophical Society,

> to attempt an examination of some of those modes by which mathematical discoveries have been made[,] to point out some of those evanescent links which but rarely appear in the writings of the discoverer but which passing perhaps imperceptibly in his mind, have acted as his unerring although his unknown guides.

The "Philosophy of Analysis" is not a study of the nature of mathematical entities. Contrary to Hamilton's concerns during the mid-1830's, Babbage's "Philosophy of Analysis" shows no direct interest in ontology. It aspires to analyze, disclose and render explicit the thought processes by which mathematical discovery is achieved.

> All that I have proposed is by an attentive examination of the writing of those who have contributed most to the advancement of mathematical science and by continued attention to the operations of my own mind[,] to state in words some of those principles which appear to me to exercise a very material influence in directing the intellect in its transition from the known to the unknown.

To this end Babbage sought to differentiate sharply between the way mathematical reasoning operates in general and how it operates with respect to specific realms of objects. One of the most interesting aspects of Babbage's conclusions in this respect is his observation that the relationship between mathematical analysis generally construed and its various applications, is by no means necessarily a matter of simple interpretation.

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37 Koppelman (1971/2, 157).
38 DeMorgan (1842, 288–289).

By ridding the symbols (or, as Babbage has it, the 'letters') of pure analysis of all inherent reference, the domain of analysis, no longer constrained by the particularities of subject matter, is naturally expanded. But, in an important sense, he points out, the process of abstraction also acts to "contract the boundaries of the science".[39] In generalizing up from mathematical application to pure mathematical analysis not all is preserved; in doing so the system is significantly reified. First, Babbage notes, some symbols must be restricted at least some of the time. There is the obvious need to limit some indexicals to whole positive numbers. Thus, for example:[40]

$$n = \frac{1-a^n}{1-a} + \frac{(1-a^n)(1-a^{n-1})}{1-a^2} + \frac{(1-a^n)(1-a^{n-1})(1-a^{n-2})}{1-a^3} + \cdots$$

> is only true in the case of n being a whole number and it is necessary to point out the distinction between the letter n and the letter a: the latter is perfectly unrestricted (...) all that is asserted of it is merely that when certain operations are executed, a will be destroyed by a and each of its powers will be destroyed by an equal one. With respect to n the case is different, it is essentially a whole number. The formula was probably discovered by the trial of a few of the simpler cases[,] and the character n is merely used to signify that any number found in the natural series would give a true result. In all cases *where the meaning of a letter is confined solely to signify repetition it is to be considered as a mere number* and the origin of any formula in which such [letters] occur will easily suggest means of distinguishing them[.]

Similarly:

$$\frac{2\sin 2\theta}{(2\sin 2^{n+1}\theta)^{2^{1/n}}} = (\tan 2\theta)^{1/2} \cdot (\tan 2^2\theta)^{1/4} \cdots (\tan 2^n\theta)^{2^{1/n}}$$

> which is only true when n is a whole positive number the identity can only be made apparent after a considerable number of steps and n must always be regarded as a mere number.[41]

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39 "Philosophy of Analysis", f. 44.

40 "Philosophy of Analysis", f. 46–8 (my italics).

41 This is not to say that one should resist generalizing indexicals in all cases. Babbage and Herschel were among the first in England to take the analogy between

$$x^n \cdot x^m = x^{n+m}, \qquad \frac{d^n}{dx^n}\left(\frac{d^m u}{dx^m}\right) = \frac{d^{n+m}u}{dx^{n+m}}, \qquad \text{and} \qquad f^n\left(f^m(x)\right) = f^{n+m}(x)$$

seriously enough to consider cases of the latter in which n and m are fractional or imaginary. Herschel in his contribution to the 1813 *Memoirs of the Analytical Society* allowed for $f^z(x)$ in which z is not an integer, but only went as far as to define $f^{-m}(x)$. Babbage, in a paper read to the Cambridge Philosophical Society on May 1, 1820 – Babbage (1822) – proceeded to obtain $f^0 z = z$ from $f^n x = f^0 f^n x$ and from that to interpreting $f^{-1}x$. But these are special cases which require special examination (like x^m itself). And in the "Philosophy of Analysis", f. 53 he urges explicitly that the definitions of the differential calculus be such as to "include fractional indices of differentiation as well as those which are whole numbers". Not in every case can the symbol denoting the number of repeated operations be thus generalized. (Cyclical functions are a good example.) Such considerations generally belong to specific applications. Babbage's point at this juncture is that, although pure analysis is not about number, one must allow in advance for indexicals that enter definitions of operations to be limited to the integers.

Babbage's next step, however, is the radical one, and as we shall see further down, squarely contradicts the foundational principle on which Peacock's algebra would be grounded. "This view of the subject" (namely, to insist on the principle of strict identity)

> leads us *to exclude entirely from pure analysis all series consisting of an infinite number of terms; such expressions properly belonging to its application to number*: this does not however exclude series containing an indefinite number and in which the function containing the remainder after n terms is accounted for; thus
>
> $$\frac{1}{1-x} = 1 + x + x^2 + x^3 + \cdots$$
>
> is not legitimately an analytical expression in the sense we have assigned to the term because the expression cannot be rendered identical; but –
>
> $$\frac{1-x^{n+1}}{1-x} = 1 + x + x^2 + x^3 + \cdots + x^n$$
>
> which is a series consisting of an indefinite number n of terms may be regarded as within the province of pure signs; its identity is easily established and is perfectly independent of the nature of x: when such an expression occurs in any enquiry if a numerical result is the final object (as it most frequently is) we must make use of those rules which apply to the arithmetical interpretation of algebraic signs; the actual value of x must then be taken into consideration if n is supposed infinite and the formula will *only be numerically accurate or rather will only afford an approximation when x is some number less than unity*.

"From these principles", he adds,

> a satisfactory answer may be given to an objection of Berkely's [sic!] relative to the shifting of the hypothesis[42]: as an example of it we will take the following
>
> (1) $$\frac{1}{1+x} = 1 - x + x^2 - x^3 + \cdots$$
>
> if $x = 1$ this becomes
>
> (2) $$\frac{1}{2} = 1 - 1 + 1 - 1 + 1 - \cdots$$

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[42] Babbage is referring to Berkeley's criticism, in *The Analyst*, §12–16, of Newton's proof that the first derivative of x^n is nx^{n-1}. Berkeley accuses Newton of first assuming that x is increased by a finite increment 0, building on this assumption in order to apply the binomial to $(x + 0)^n$ and to then divide by 0, only to then replace it by the contradictory assumption that, in fact, no increment in x occurred and that 0 is really nothing. But in that case, asserts Berkeley, one may not retain the former consequences of the rejected assumption, and is required to begin anew. Berkeley formulates the principle that Babbage, I believe, refers to as "the shifting of the hypothesis" thus: "If, with a view to demonstrate any proposition, a certain point is supposed, by virtue of which certain other points are attained; and such supposed point be itself afterwards destroyed or rejected by a contrary supposition; in that case, all the other points attained thereby, and consequent thereupon, must also be destroyed and rejected, so as from thenceforward to be no more supposed or applied in the demonstration".

which cannot possibly be numerically true; for no combination of whole numbers by mere addition and subtraction can equal a fraction[.] (1) is therefore false when $x = 1$. Multiply it by *dx* and integrate then it becomes

$$\log(1+x) = \frac{x}{1} - \frac{x^2}{2} + \frac{x^3}{3} - \cdots \tag{3}$$

an expression which we conclude to be true when $x = 1$[,] and which from other processes we know to be accurate. Now the conclusion that (3) is true in a case in which (1)[,] from which it is derived[,] is false Berkely [sic!] has justly observed is quite illogical.

His objection is completely removed by keeping the analytical process distinct from its application to arithmetic thus setting out with the expression

$$\frac{1-(-x)^{n+1}}{1+x} = 1 - x + x^2 - \cdots x^n \tag{4}$$

which is easily reduced to identity multiplying by *dx* and integrating

$$\log(1+x) + (-1)^n \int \frac{x^{n+1}dx}{1+x} = \frac{x}{1} - \frac{x^2}{2} + \frac{x^3}{3} - \cdots \frac{x^n}{n} \tag{5}$$

or

$$\log(1+x) + \frac{(-1)^n x^{n+1}}{(n+2)(1+x)} + \frac{(-1)^n}{n+2} \int \frac{x^{n+1}dx}{(1+x)^2} = \frac{x}{1} - \frac{x^2}{2} + \frac{x^3}{3} - \cdots \frac{x^n}{n} \tag{6}$$

Every stage in this proof is reducible to identity and is quite independent of the magnitude of n if it is required to apply it to numbers in order to find the logarithm of 2 it is to be observed that of the three terms on the left side of the equation (6) the two latter are divided by $n+2$ on which account they become less and less as n is a larger number and when n is considered as infinite may be neglected and consequently we have as *an arithmetical form*

$$\log 2 = \frac{1}{1} - \frac{1}{2} + \frac{1}{3} - \cdots$$

In this way of stating the process there is no shifting of the hypothesis[,] and we also see the reason why in the former process that circumstance did not introduce any fallacy[,] for the part of the analytical formula which was omitted in the first stage[,] although at that step of finite magnitude[,] became by means of the integration infinitely small for the real value of x.[43]

Berkeley's "objection" was, of course, originally voiced in his famous (infamous?) critique of the calculus of 1734. His attack, especially on limit-based procedures, was devastating. Increments are real quantities unless they actually vanish. Before they vanish they are quantities, that cannot be ignored in mathematics, however small. If and when they are made to vanish, they are exactly zero, and can no longer be divided by, etc. Berkeley saw only two alternatives. To lower standards, and view the calculus as operating with notions of rigor, truth and exactitude less stringent than other branches of mathematics, or to explain the obvious success of the calculus by a systematic "compensation of errors" within an otherwise rigorous mathematical enterprise. Berkeley opted for the latter. Lagrange's power-series approach, so favored by Babbage and his fellow

[43] "Philosophy of Analysis", f. 48–50.

analyticals, proposed a third way in attempting to avoid the use of limits entirely. But by choosing to meet Berkeley's "objection" by ridding pure analysis not of merely of inconsistencies, but of the very notion of infinity, Babbage, it would seem, had thrown the baby out with the bath water. The Taylor series expansion of functions generates a converging, yet infinite number of terms. Lagrange's version of the calculus should, therefore, also be banished from pure analysis, not for shifting hypotheses in mid-proof, but for manipulating equations that failed to reduce to strict identities![44] Needless to say, Babbage could not settle for any other known version of the calculus. Any appeal to "infinitesimals or limiting ratios or exhaustions", he claimed,[45] necessarily introduced analysis into "the obscure regions of infinity" to a far greater extent than Lagrange's methods. But there is no way Babbage could have maintained, as his text incredibly implies, that Lagrangian calculus avoids the problem entirely. The way things stood, it was impossible for him to include the calculus in his scheme for pure analysis. Or so it would seem.

I believe Babbage was well aware of all this. The problem was simply too obvious for him to have missed. And in any event, he solved it – quite dramatically in fact, though without in any way indicating that he might have found Lagrange's approach more than mildly deficient. He did so by founding the calculus anew, in a way radically different from any other, including Lagrange's. Irascible, pigheaded, direct and rudely outspoken as he could be, Babbage, it seems, was incapable of openly criticizing Lagrange. (This makes him the only one of his generation to attempt to outdo Lagrange at his own game.)[46] He registers certain doubts indirectly, and in the most inoffensive of terms, not

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[44] There were other problems with Lagrange's approach, of course. The very identification of the derivability of a function with its susceptibility to a Taylor expansion was being seriously questioned at the time (with Cauchy furnishing the classical counter-example of $f(x) = \exp(-x^{-2})$, a function derivable yet non-expandable at $x = 0$). More disturbing from Babbage's perspective perhaps was the fact that, regardless of the calculus, working with power series expansions required limit-based reasoning in order to decide convergence. But these were problems peculiar to Lagrange's approach itself and were not especially aggravated by Babbage's new theory of analysis.

[45] "Philosophy of Analysis", f. 51.

[46] The diehard Langrangians were Peacock and especially Herschel, who retained their early allegiances to Lagrange's version of the calculus well after its demise across the English Channel and the publication of Cauchy's algebraical epsilon-delta, limit-based definitions. Peacock explicitly deemed Cauchy's procedures an attempt "to conciliate the direct consideration of infinitesimals with the purely algebraical views of the principles of this calculus, which Lagrange first securely established . . . altogether inconsistent with the spirit and principles of symbolical algebra" (Peacock (1834, 247–8).) Herschel's allegiance to Lagrange and apparent disregard of Cauchy's revision of the calculus is less explicit if equally evident in his long entry "Mathematics" in *The Edinburgh Encyclopedia*, in which Lagrange is hailed "[a] more consummate analyst than Clairaut, – a more profound reasoner than D'Alembert, – a more comprehensive genius than Euler" (Herschel (1832, 458)), and his revision of the calculus, as having accomplished "the greatest revolution which the nature of a science like mathematics could admit in the principles of its most extensive branch" (op. cit., 451). Cauchy is not even mentioned. On the other hand, those who, like Whewell and DeMorgan, rejected Lagrange's version of the calculus during the 1830's did so for reasons precisely opposite to those of Babbage. For them the differential calculus was not an abstract system *applied* to limits, but a systematic articulation *of* the very notion of limit.

before paying eulogistic homage to the patron saint of British analysis, whose "masterly view" of the differential calculus "while it rested the calculus on a foundation not to be contested[,] has at the same time furnished an example which will have no inconsiderable weight on guiding other reforms".[47] Of the great merits of Lagrange's "attempt to place [the calculus] on a better foundation" Babbage promises to speak in another part the essay. When he finally gets round to doing so, twelve tightly-packed pages later, after explaining in great detail how, like in all branches of analysis, the true principles on which the calculus rests have been "obscured or infringed by a connection with any of its applications whose evidence is of a lower [or] even of a different nature", and urging that the remedy be found in "entirely dissevering it from infinitesimals", Babbage announces that he has a suggestion of his own, that

> was unknown to me when I began this essay[,] and arose entirely from pursuing the reasoning which I have explained when examining the meaning of exponents both of quantities and of functions as indicated by the two equations
>
> $$x^{a+b} = x^a \cdot x^b \quad \text{and} \quad f^{n+m}(x) = f^n f^m(x).$$

And it is at this point that Babbage introduces a mildly critical comment regarding Lagrange.

> An objection to the view of Lagrange had often been proposed namely that it required that the possibility of converting all expressions into series should be granted a postulate referring to a fact which although undoubtedly true could hardly be established without considerable discussion. Besides this the very method of converting functions into series may be regarded as one of the first applications of the differential calculus[,] and although it can be accomplished without passing through that step to which reference has been frequently made[,] yet it still leaves the system open to the objection of establishing the principles by means of one of the applications.[48]

Babbage's rhetoric is amazing. On the one hand he makes out that in his own opinion both objections are unfounded, and hence Lagrange's move is impeccable, on the other hand, he cites them as reason for his own rethinking the entire enterprise.

> These reasons induced me to look amongst the properties of differentials for some one from which the others might be deduced and which should be free from the objections that have been stated. The simplest ought of course to be selected if any such could be found suitable to the purpose[.][49]

His suggestion is simple and without precedent. It is to define "the prefixing to a function the letter *d*" as an *operation* such that:

(a) $$d(xy) = xdy + ydx$$

and

$$d(x + y) = dx + dy.$$

[47] "Philosophy of Analysis", f. 41.
[48] "Philosophy of Analysis", f. 53.
[49] *Loc. cit.*, f. 54.

No limits, no infinitesimals, no exhaustions, no infinite series. Babbage's dx is none of these. Nor is it a "derived function" as in Lagrange's *Théories des fonctions analytiques*. It is simply an operation from which he proceeds to derive the "usual expressions of the differentials of functions" as follows:

$$\text{"If } y = x \qquad d(xy) = d\left(x^2\right) = xdx + xdx = 2xdx,$$

$$\text{if } y = x^2 \qquad d(xy) = d\left(x^3\right)\left[= d\left(x^2 x\right)\right] = 2x^2dx + x^2dx = 3x^2dx,$$

and generally when n is a whole number

$$d\left(x^n\right) = nx^{n-1}dx.$$

Next let it be required to find the differential of x^{-n}.

Let $z = x^{-n}$ then

$$zx^n = 1$$

and

$$d\left(zx^n\right)[= d(1)] = x^n dz + nz^{n-1}dx = 0$$

$$dz = -\frac{nx^{n-1}zdx}{x^n} = \frac{nx^{n-1}x^{-n}dx}{x^n} = -nx^{-n-1}dx.$$

Let $z = x^a$ where a is any number[,] then it may be put into the form $z = x^{m/n}$ where m and n are whole numbers [sic!].[50]

Hence $z^n = x^m$ and by (a)

$$nz^{n-1}dz = mx^{m-1}dx$$

$$dz = \frac{m}{n} \cdot \frac{x^{m-1}dx}{z^{n-1}} = \frac{m}{n} \cdot \frac{x^{m-1}dx}{x^{\frac{m}{n}(n-1)}},$$

or

$$dz = \frac{m}{n}x^{\frac{m}{n}-1}dx.\text{"}$$

In general, Babbage concludes, all three cases are contained in the expression:

$$d\left(x^a\right) = ax^{a-1}dx.$$

He goes on to note that the differential of x/y is easily deduced from the consideration of its equivalent: xy^{-1}, and at this point the essay is discontinued.

Such was Babbage's construal of pure analysis: a system consisting of well-formed operations, defined in relation to "letters" that denote mathematical objects generalized in advance to the level of absolute, featureless abstractness. Well-formed propositions within the system are represented by equations constituting statements of strict identity. Pure analysis à la Babbage constitutes an autonomous realm of mathematical activity

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[50] Babbage's assertion that any number may be expressed in this way would imply that he believed all numbers to be rational. I am convinced he didn't, and that the sloppy formulation is due to a mere oversight.

knowingly 'dissevered', as he put it, from all manner of application. It is, in a sense, a metamathematical system, logically prior to, and hence wholly independent of actual and potential subject-matter. So pure was Babbage's pure analysis, that in one important respect it went a significant step further than what would later be dubbed abstract algebra. Babbage refused to allow into the realm of pure analysis anything, as well-defined and abstract as it might be, that failed fully and rigorously to comply with the principle of identity. Even though he wrote the "Philosophy of Analysis" several years before Cauchy published his famous epsilon-delta formulations (and failed to react to them after they were published), one may safely assume that he would not have tolerated the inclusion of inequalities parading as identities, even if they could be shown to be appropriately contained within rigorous Cauchian limits. But if the mathematics of convergent and divergent series is deemed "altogether foreign" to pure analysis, and "assigned" exclusively to "the nature of the subject on which that species of reasoning is employed", it can no longer be strictly considered an *application* of analysis in the strict sense of the term. Babbage's pure analysis is not abstract algebra, but *rarefied* algebra. "Ordinary" algebra cannot be considered a model of the higher system that Babbage envisaged, exactly because parts of it are obtained independently. The application of pure analysis to specific realms of mathematical objects entails more than a narrowing of the scope of its expressions. It also involves concessions to rigor. In this too, as we shall see, Babbage's construal of pure analysis differed significantly from the system Peacock would eventually propose.

3. Peacock's problematic response

There is another, more obvious difference between the metamathematical attitudes of Peacock's *Treatise* and Babbage's "Philosophy of Analysis". Babbage's essays were not written with a view to making sense of algebra as he found it. They do not represent an interpretive or explanatory undertaking, at least not primarily so. They are an attempt, or rather an outline of an attempt, to explore the foundations, reformulate the objectives, and lay down the foundational principles of a new, and austere conception of mathematical analysis. It was the undertaking of a working mathematician; not a *study* of mathematics, but a *work* of mathematics. In this respect Peacock's algebraic project was very different. Published twice – once as a textbook, and once as an Annual BAAS Report – the *Treatise* was essentially a comment *on* algebra, a critique performed with a view to articulating what algebra was, and analyzing what it could therefore achieve. In order to understand it, Peacock's work aspired to dismantle (to deconstruct) an existing area of mathematics. Babbage's essays, by contrast, endeavored to construct one *de novo*. The innovations contained in Peacock's work were metamathematical. His was a new exposition of what algebra *is* and how it should be accounted for. The novelty of Babbage's essays lay in them being a first, incomplete attempt at stating what algebra *should be*. Both works convey and apply a philosophy of algebra. Babbage's does so with a view to putting right something allegedly wrong *in* mathematics, Peacock's does so with a view to putting right something allegedly wrong in our *understanding of* mathematics. I stress this point because knowing that Peacock had been privy to Babbage's essays, his decision to adopt an interpretive rather than a reformative approach, suggests, as did his letter to Babbage of a few years earlier, a knowing disagreement with his friend's line of thinking.

Enros is probably right to note that during the Society's very early days Peacock's knowledge of mathematics was no match to that of Herschel and Babbage.[51] But I doubt he lagged behind for long. His Notes to Lacroix, and certainly the *Treatise* and the BAAS Report are all extraordinarily well informed. Peacock's preference for metamathematics (like Whewell's for metascience for that matter) cannot easily be attributed to a lack of talent, or capacity for first-order research. In many respects Babbage was the most eccentric and unruly member of the group. But with regard to mathematics proper, Peacock was as zealous and as dedicated a supporter of "the true faith".[52] In fact, in being the first of the young rebels to obtain an official position at Cambridge, Peacock not only spearheaded the reform process there, but also drew most of the fire.[53] Had Peacock agreed with Babbage's reformative program, I doubt he would have refrained from following suit.

All this strongly suggests that the *Treatise* can no longer be read as a hesitant step in the direction of a formalist approach to algebra that its author was incapable of fully seeing through. Peacock cannot be said to have been timidly groping towards a position he lacked the ability to formulate for the simple reason that, as it now turns out, that very position was staring him in the face. To put it differently, in view of Babbage's "Philosophy of Analysis" and Peacock's evident knowledge of its content, the *Treatise* can no longer be set against a supposedly linear development in mathematics that allegedly culminated twenty years or so later. What would appear, from judging the

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[51] Enros (1979, 120) citing Herschel (1859, 536). Even Enros, whose work, for reasons noted above, largely ignores Peacock, reserves this remark to the level of Peacock's mathematical knowledge at the time the Society was founded. Two years later, in 1813, Peacock graduated second wrangler and second Smith's Prizeman (with Herschel ranking first in both), and was awarded a Trinity Fellowship the following year.

[52] "The true faith" was a catchword employed by the young dissenters to denote Lagrange's approach to the calculus. See for example: Babbage to Herschel, June 13, 1813, RS HS 2.15; Babbage to Herschel, Dec. 8, 1815, RS HS 2.50; Herschel to Babbage, July 14, 1816, RS HS 2.64; Babbage to Herschel, July 20, 1816, RS HS 2.65; Peacock to Herschel, Dec. 3, 1816, RS HS 13.247, and Whewell to Herschel, March 6, 1817, cited in Todhunter (1876, ii, 16).

[53] The University granted the young examiners of each year extensive freedom in framing the questions on the tripos. This afforded Peacock, who was appointed moderator in 1817 and 1819, the opportunity to introduce the differential notation in his share of the questions. He encountered considerable opposition. Writing to Herschel in December 1816, Peacock was confidently optimistic: "We are proposing college reforms, which will introduce the true faith in Trinity; at least if our wise seniority will adopt them. . . . Trust me, the goldenage of the University is approaching". (Peacock to Herschel, Dec. 3, 1816, RS HS 13.247.). His optimism proved premature, however. "White and Fallows [proved] entirely of the old school", he wrote a few weeks later, "& the influence of their examination was so great as completely to overpower my examination. The introduction of d's into the papers excited much remorse . . . & I believe that I may consider myself as owing entirely to the success of the Johnians in the examination for my escape from some public proceedings against me" (Peacock to Herschel, undated, postmark: 1817, RS HS 13.248). Peacock's initiative was publicly criticized by D.M. Peacock (1819). But the opposition from without eventually died down. For further details see J.M. Dubbey (1978, Ch. 3).

published record alone, as an interesting move away from one conception of algebra *en route* to another, is better described as an uneasy and unstable compromise between two existing and incompatible extremities.

To appreciate it as such, we must briefly return to Peacock's point of departure, to his first published work, namely, his, Herschel's, and Babbage's edition of Lacroix. Peacock's twelve Notes (Lacroix (1816, 581–681)) were self-consciously penned with the single aim of 'translating' Lacroix's text from the language of limits to that of Lagrangian power-series. As the Advertisement to the book has it:

> The first twelve of the Notes were written by Mr. Peacock, and were principally designed to enable the Student to make use of the principle of Lagrange, adopting those statements and examples of our author, which do not involve the theory of limits.

"The others", it adds, "were written by Mr. Herschel".[54] Peacock's Notes are long and tedious but could not be otherwise if the job was to be done properly. If the coefficients of the Taylor series are to define derivatives rather than the other way round, then they need to be obtained independently of the calculus – a procedure long and tedious. It was an impressive undertaking which must have cost its author considerable time and effort. It was also retrospectively deemed superfluous by Babbage's autobiography, as we have seen. We now understand why. Close to the time Peacock was composing his Notes, Babbage was becoming more and more critical of Lagrange. It was not, as we have seen, merely a matter of the Taylor series originating as an application of the calculus. According to Babbage's emerging view of things, Lagrange's calculus belonged among the lower-level, 'arithmetical' applications of pure analysis by virtue of its manipulation of infinite series. Although Lagrange's move was obviously an improvement on rival versions of the calculus, for Babbage, rendering a limit-based calculus Lagrangian, was no longer equivalent to rendering it analytical. This is certainly true of Babbage's later position, which, in all probability had not changed by the time he wrote his autobiography. On such a showing, Peacock's Notes did little more than to translate one lower-level application of pure analysis into another. Peacock believed his Notes enabled Cambridge men to study and appreciate the calculus for what it really was. For Babbage, the aim of the textbook seems to have been to teach the calculus by means of an application, and, to that end, its application to limits was as good as any.

Around the time Peacock was penning his Notes, Herschel too seems to have been less insistent regarding Lagrange's approach. In a section of his Appendix to Lacroix, entitled "On the Connection between the Differential Calculus and that of Differences" he introduced the former liberally as "considered in the light in which Leibniz presented it, or as depending on the theory of limits" without a word of reservation.[55] I am not quite sure what to make of Herschel's attitude at this point in time. He certainly seems to have changed his tune by the time he wrote Herschel (1832).[56] Peacock, however,

[54] Lacroix (1816, iv).

[55] Lacroix (1816, 465–579), at pp. 539–40.

[56] Herschel wrote a considerable amount of mathematics during the mid-1810s (for a detailed survey see Enros (1979, Ch. V)), but refrained from making general statements about the nature of analysis. Still, a paper he read at the Royal Society on December 14, 1815 – Herschel (1816) – suggests that he had adopted Babbage's view to disallow expressions involving an infinite number

was quite clear as to his own position. He stated his preference for Lagrange's version of the calculus in what Hamilton would surely have recognized as strong theoretical terms. He criticizes each of the three "methods" of the calculus separately. Of D'Alembert's "method of limits" he says:

> Our notion, indeed, of a ratio, whose terms are evanescent, is necessarily obscure, however rigorously its existence and magnitude may be demonstrated; and its introduction into all our reasonings in the establishment of this Calculus, is calculated to throw a mystery over all its operations. . . .[57]

The "method of Infinitesimals", he continues, is even worse, being

> liable to the same objections as the system of D'Alembert, and to many others from which that system is free; for its first principles hardly admit of demonstrations, and by considering the successive differentials as infinitesimals of successive orders, we are unable to form any notion whatever of their connection with each other, and with the function from which they are derived.[58]

With the method of fluxions Peacock had the least patience, deeming it "unquestionably inferior to all other methods".

> The consideration of Motion, which is essential to the method of fluxions, is foreign to the spirit of pure Analysis; and the analogy by which the name and properties of a fluxion are transferred to a modification of the difference of a function, is strained and unnatural. The different orders of fluxions also are involved in considerable obscurity, and we are utterly unable to comprehend the connection which they respectively bear to their primitive function.[59]

The problem for Peacock was not that of confusing the true foundations of the calculus with one of its applications. Nor did it stem from what Hamilton would call philological concerns.[60] For Peacock in 1816, the problem for which Lagrange's version of the calculus was presented as a solution was one of foundational clarity. To ground the calculus on limits, infinitesimals, or fluxions, he insisted, was to render it conceptually obscure. He saw the great merit of Lagrange's alternative in the way it allowed the calculus to be "established upon principles which are entirely independent of infinitesimals or limits",[61] let alone fluxions. Unlike Babbage, Peacock does not seem to have been overly motivated by the formalistic aspects of Lagrange's approach, or much interested

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of terms in pure analysis. This may well explain his apparent lack of enthusiasm for Lagrange in his contribution to Lacroix. At present I have no reasonable explanation for his lavish praise for Lagrange's reform of the calculus in later years. On this latter phase in his career see above fn. 46.

[57] Lacroix (1816, 612, Note B). Compare Herschel and Babbage's joint preface to the *Memoirs of the Analytical Society*, which focuses exclusively on the notational, rather than the conceptual merits of the Lagrangian calculus, namely, the "accurate simplicity of its language", "the symmetry" and "the conciseness of its notation" Herschel and Babbage (1813, i–ii).

[58] *Loc. cit.*, p. 614.

[59] *Ibid.*, p. 618.

[60] Peacock's criticism of the "utter insufficiency" of the fluxional notation (pp. 618–20) is decisive, but (a) is limited to fluxions, and (b) in no way implies that fluxions could not be better represented.

[61] *Loc. cit.*, p. 596.

in formalism as such. His Notes sought less to formalize the calculus more efficiently, than to better clarify its foundation with a view to enabling its students to form a clear notion of derived functions and of "the connection which they respectively bear to their primitive function."

However, Peacock was not content to stop at that. After purging the calculus of the "aukwardness" (sic) of Newton's cumbersome notation[62] and establishing it anew upon the allegedly solid, algebraic basis furnished by Lagrange's definitions, he envisaged a second stage to the reform process in which, after completing their edition of Lacroix, they would try their hand at a similar textbook on algebra. While still proofreading the Notes, he tried to interest Herschel in the new project. "An Algebra is much wanted & so is a treatise on the application of Algebra to Geometry: the second is almost partial to a system of radical reform", he wrote in November 1816.[63] Such an Algebra, he wrote three weeks later: "must supersede Waring[64] & must include *a theory of numbers* & indeterminate equations: it ought to enter much more into detail than the cur[rent] English books on *the first principles*: something in short like Lacroix's, but containing more matter . . . ".[65] Thanks to Lagrange, Peacock was able to present the calculus to the English reader in all its richness and as a branch of algebra. It was now time, he felt, to get down to basics and to deal with algebra directly. Herschel did not respond. Babbage, as far as I can tell, was not approached by Peacock, but, as we have seen, seems to have had similar plans – though not for a textbook, and certainly not for the type of algebraic foundation Peacock had envisaged.

In one important respect the two friends' initial programs did coincide. They both sought to distinguish sharply between the calculus and its applications. Neither of them denied the great value of designating limits and curvatures by means of derivatives, but both strongly objected to defining derivatives by means of such *applicanda*. Of itself, unapplied, the calculus, they agreed, pertained to a more basic level of mathematical 'existence'. Peacock located it as part of algebra, Babbage, as part of what he termed pure analysis. With the completion of Lacroix, the two men applied themselves to the task of explicating that more fundamental level. And it is here that one can discern profound disagreement, at the heart of which lay the fact that what Peacock took algebra to be in 1816, was considered a lower-level application of Babbage's emerging notion of pure analysis. Babbage's "Philosophy of Analysis" left no room at all for the type of "theory of numbers" Peacock took to be an essential component of algebra. This alone would account for the discrepancy between Babbage's recollected plan for Lacroix and the way it was executed by Peacock.

For the Peacock of 1816, Lagrange's power-series rendition of the calculus provided the crucial link in the grand scheme of algebraic reduction. That is why translating Lacroix's limit-based text into the language of Taylor-series coefficients was so important. For the Peacock of 1816 the original Lacroix did not speak for itself, and could not be allowed to go out into the English-speaking world unannotated. The Babbage

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62 *Ibid*., p. 619.
63 Peacock to Herschel, November 14, 1816, RS HS 13.246.
64 Referring presumably to Waring (1770).
65 Peacock to Herschel, December 12, 1816, RS HS 13.247.

of the "Philosophy of Analysis", (who, I believe, as far as pure mathematics was concerned, was essentially the same as that of Babbage (1864)), I submit, could hardly have cared less! Having banished Lagrange's calculus from pure analysis, and relegating it along with limits to the level of application (his lip service to Lagrange notwithstanding), it no longer really mattered which application one chose in order to introduce the calculus to beginners. Even if he had thought differently in 1812, it is easy to see how a more mature Babbage might have re-described his initial choice of university textbook. The fact that Peacock evidently took a very different view of Lacroix at the time is also understandable. For him Lagrange had done for the calculus what Descartes had done for geometry: he had rendered it a branch of algebra. The next step, in the process of "reintroducing the exotic" into England was only natural: to publish a work on algebra similar in standard to (the appropriately annotated) calculus of Lacroix: a project Peacock, contrary to Babbage, naively believed could be built upon a theory of number.

Such an evaluation of Peacock's position around 1816 not only makes sense of the little he has left us, but also fares well with what we now know, by virtue of Joan Richards' recent work, of the wider educational, and theological setting of early nineteenth-century British mathematics. Over and above their different notational conventions, the works of French and English mathematicians, she has shown,[66] employed at the time fundamentally different modes of mathematical argumentation, geared to fundamentally different conceptions of mathematical truth. While the former generally sought to ground mathematical verity upon public and formal standards of mathematical rigor, the type of mathematics understood, taught and practiced in England pertained traditionally to a far more introspective, personalist, participatory notion of mathematical certitude. Where the French texts strove to establish mathematical certainty by applying rigorous proof procedures to formal definitions, English adaptations of the very same material sought to ground mathematical truth, much as Hamilton would urge, on a meticulous clarification of concepts. Furthermore, where the French aligned their mathematics with the public, interpersonal, demonstrative methods of the natural sciences, the British, she argues convincingly, appear to have aligned theirs with the more contemplative modes of theological reflection. Richards attributes the peculiar style of British mathematical thinking to a broadly shared, and equally peculiar evangelical or Pauline notion of faith, as she terms it, originally introduced, intimately coupled to mathematics, in the writings of John Locke. In contrast to his better known sensationalism with regard to the natural sciences, Locke portrayed knowledge of God and mathematical knowledge as grounded upon what Whewell would later call "progressive intuition" – personal and private processes of growing acquaintance.[67] Richards' work is crucially important for understanding the cultural and intellectual background against which the reform process developed in England. I doubt, though, that the young founders of the Society's initial, almost boyish fascination with continental analysis, especially with the mathematical

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[66] See especially Richards (1991) and (1992).

[67] For Locke's comparison of mathematical and religious knowledge see the chapter entitled: "Of our Knowledge of the Existence of a God" in Locke (1959, II), esp. p. 306.

works of Lagrange, owed much to any of this.[68] But there is little doubt that the ways in which, in subsequent years, they and the young generation of English mathematicians they inspired modified and integrated the 'new math' into their own pedagogical, religious, and research agendas were deeply effected by such prior commitments. Richards' most recent work demonstrates that the commitment in England to this type of "evangelical", Lockian approach to mathematics ran deepest at Cambridge, where it played a far more fundamental pedagogical role than it did, for instance, at Oxford.[69] It is significant, therefore, and understandable in the light of Richards's analysis, that Herschel and Babbage, the two most ardent early spokesmen for the introduction into England of an approach to mathematics so squarely opposed to traditional English ways of thinking, would encounter serious difficulties adapting to Cambridge, and end up operating from outside the University for the most of their lives. It also explains why Peacock, who, from the start, did take up, and take seriously,[70] a Cambridge position, showed clear signs of aspiring to re-locate Lagrange's very 'French' version of the calculus within such a very 'English', or 'Pauline' theory-of-number orientated algebraical setting.

Equally understandable were Peacock's apparent concerns about the possibility of formulating such an algebra. The discrepancy between the scope of what was being done in, and with algebra at the time, and the level, especially in England, of works attempting to explain the nature and foundations of the subject was enormous. If one was concerned, as Peacock seems to have been in 1816, with what Hamilton was to term the 'theoretical' aspects of algebra, Hamilton's complaints against the sorry state of such studies were as true then as when they were issued in the mid-1830s. At the time, the only conceivable theoretical option was to ground algebraic truth upon some form of generalization of the notion of number. So far, however, all such attempts had failed dismally. In later years, Peacock would cite the algebraic works of Francis Maseres and William Frend as notorious examples is this respect. Both writers had raised the problem of treating negatives

[68] Which is not to say that other cultural factors were not involved. For an interesting attempt to view Herschel and especially Babbage's early approach to algebraic analysis as inspired by their fascination with contemporary industrial culture see Schaffer (1994), Ashworth (1994) and especially Ashworth (1996).

[69] I take this opportunity to thank Joan Richards for sharing with me her work in progress on the differences between the two universities. During the period in question Oxford refused to enroll dissenters. Cambridge, on the other hand, accepted them but would not grant them degrees. (Or as Richards likes to put it: at Oxford they weren't allowed in, while at Cambridge they weren't allowed out.) As a result, what the Oxford curriculum could take the principles of the Faith for granted. Cambridge, by contrast, could not. They had to be taught and defended. This, she suggests, explains why logic was stressed at Oxford and mathematics at Cambridge.

[70] Both Herschel and Babbage sought and acquired Cambridge positions in the course of their careers. Herschel graduated Senior wrangler and first Smith's Prizeman in January 1813, left Cambridge, became Fellow of St. John's in March, spent the fall at Cambridge, but left in January 1814, initially to study law. He came back briefly to tutor the following year but apparently hated it. Desperate for a position after his marriage in July 1814, Babbage applied unsuccessfully for professorships both at Cambridge and Edinburgh. He finally obtained the prestigious Lucasian Professorship at Cambridge (1828–39), but seems never to have lectured or to have taken much interest at all in the University's mathematics curriculum.

and imaginaries as species of quantity, and both had proposed, incredibly, to resolve it by denying them a place in algebra altogether. In this way algebra was construed as a universal or generalized arithmetic.[71] In the *Treatise* and in the BAAS Report Peacock was forced to agree with Maseres and Frend that "arithmetical algebra" is "the only form of [algebra] which was capable of strict demonstration, and which alone, therefore, was entitled to be considered as a science of strict and logical reasoning". The arguments of the two British algebraists, he concludes, "were unanswerable ... and the system of algebra which was formed [devoid of negatives and imaginaries]... was perfectly logical and complete... ".[72] By this I take him to mean that only thus algebra could be considered a science (in Hamilton's sense of the term) susceptible to a truth relation (or, as he put in the *Treatise*, as a system founded upon axioms that are considered true in addition to definitions that are considered well-formed).[73] But he could not agree with Maseres and Frend's outrageous conclusion to dispense, therefore, with the "great multitude of algebraic results and propositions, of unquestionable value and of unquestionable consistency with each other, which were irreconcilable with such a system, or, at all events, not deducible from it ... and which made it necessary to consider negative and even impossible quantities as having real existence in algebra, however vain might be the attempt to interpret their meaning".[74]

By 1830 Peacock had obviously despaired of establishing a science of number rich enough to accommodate the entire system. In 1816, with the experience of one complicated foundations project under his belt, Peacock knew enough algebra, and enough about algebra to appreciate both the urgency and the complexity of the new textbook he envisaged. However, everything he wrote during those early years, strongly indicates that at that point in time his orientation, contrary to that of Babbage, was squarely 'theoretical'. He would still have found the conclusions of Maseres and Frend outrageous, but I doubt that he had already despaired of seeing through a more sophisticated version of the type of truth-governed algebra they had taken for granted.

I have come across nothing that bears directly on Peacock's deliberations between 1816 and 1830. What we do have is a good idea of his options around 1820 or so, and what he ended up publishing from 1830 onwards. The options he faced in 1820, we now realize, posed a cruel dilemma: Maseres and Frends' "generalized arithmetic" approach on the one hand *versus* Babbage's pure analysis on the other. The former proposed sacrificing whole realms of algebraic activity for the sake of preserving for mathematics a solid, meaningfully 'Pauline', truth governed foundational system. The latter insisted

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71 Maseres (1758) and (1800), and Frend (1796). For a discussion of their proposals, the manner in which they wrought havoc in the theory of algebraic equations, and Peacock's later reaction to them, see Pycior (1981, 27–31).

72 Peacock (1834, 190–1). On Herschel's very different treatment of the problem of negatives and imaginaries see Herschel (1833, 439 & 446) and Fisch (1994b, fn. 61).

73 See Peacock (1830, 110–13) where he concludes that, as opposed to geometry, in a "science of general symbols, which by themselves express no relations or magnitudes ... there are, properly speaking, no axioms, since the propositions, *immediately* deducible from the definitions and assumptions, must be considered rather as the necessary and immediate consequences of defined operations, than the necessary and self-evident results of reasoning". (p. 112–13).

74 *Ibid.*

on draining analysis of all meaning and content for the sake of grounding mathematics anew by means of a system pertaining exclusively to pure form and process. It is not hard to imagine Peacock's difficulties with the former. As we have seen, he made them quite clear in the criticism of Maseres and Frend he volunteered in the BAAS report of 1834. The problems he might have had with Babbage's radical formalism are a different matter. He never addressed such an approach publicly. By the time Peacock's mature position was taking form during the late 1820's Babbage – whose work on the "Philosophy of Analysis" and related aspects of theoretical mathematics had long been discontinued in favor of what would become a lifelong obsession with calculating engines – was no longer a player. In his published work thereafter, Peacock appears to have decided against openly confronting a system of thought its author had decided not to publish and of which his readers could not be aware. Lacking Peacock's own testimony we can only speculate. As indicated, I find compelling Joan Richard's' analysis of the extent to which Cambridge was committed to opposing the type of approach to mathematics advocated by Babbage, and suspect that Peacock's initial reluctance to endorse it owed much to the institution within, and out of which he had chosen to operate.

In this respect, Whewell, Peacock's friend and comrade-in-arms at Trinity, is a case in point. Three years younger than Peacock, Whewell came up to Cambridge early enough to be dazzled by the Society's campaign, but too late to play an active role in it. By the time he graduated (second wrangler and second Smith's Prizeman, 1816) and acquired his Trinity Fellowship (1817) he had become, on his own admission, a dutiful disciple of the "true faith", wholly dedicated to the "laudable object" of "reforming the mathematics of the university".[75] Whewell, however, was quick to retreat from his early endorsement of Lagrange. By the mid-1820's, for a variety of pedagogical, didactic and theological reasons I have dealt with elsewhere,[76] Whewell had come to regard the calculus, in what Hamilton termed 'theoretical', and Joan Richards has dubbed 'Pauline' terms, namely, as an elaborate and well-structured articulation of what he would later term the Fundamental Idea of Limit. Generalizing this conception of the calculus, Whewell eventually came to view all well-formed mathematical systems as rigorous Euclidean representations of the intuited and contemplated features of other Fundamental Ideas: that of space for geometry, that of number for arithmetical algebra, etc. Like Hamilton[77], Whewell considered the axioms of such systems of ideas along with all they logically entail, as propositions that are true *of* the Fundamental Ideas they articulate. For Whewell, as for Hamilton, mathematical truth had come to mean more than coherence and logical validity. In their view, the axioms of a "pure science" aptly describe true features of the science's Fundamental Idea.[78]

Among Peacock's close associates, Whewell was undoubtedly the most articulate and outspoken opponent to the type of approach to mathematics Peacock had encountered in Babbage's "Philosophy of Analysis". The two Trinity dons were close friends. They

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[75] See, for instance, Whewell to Herschel, Nov. 1, 1818, cited in Todhunter (1876, ii, 30).
[76] Fisch (1991a, 27–57), (1991b) and (1994b).
[77] On the affinity between the views of the two men see Hankins (1980, 174–80), and Fisch (1991a, 63–7, 93–8).
[78] See for example Whewell (1834) and (1840, i, 54).

saw each other and conferred on a daily basis. During much of the 1820's they were both centrally engaged in major pedagogical mathematical projects (Whewell was writing a series of mechanics textbooks, Peacock, his algebra) in an institution in which pure and 'mixed' mathematics provided the main focus, testing ground and criteria of success for a gentlemanly liberal education. One may assume, therefore, that had the two young dons harbored substantial metamathematical differences, it would have shown. There is nothing to indicate that that was the case. On the contrary, as I have shown in some detail elsewhere, a book-length work on the philosophy of mathematics, written, and eventually abandoned by Whewell during the early 1830's, clearly suggests that he had believed at the time that his own emerging, extremely 'Pauline' system of thought was nicely served by the view of algebra proposed in Peacock's *Treatise*.[79] This too, I believe, goes to indicate that, despite Peacock's early involvement with the Analytical Society, and early collaboration with Herschel and Babbage, his initial views on algebra steered closer to those later voiced by Hamilton and Whewell, and toed the 'official' Cambridge line to a far greater extent than is usually assumed.

There are good reasons, then, to assume that Peacock embarked upon the *Treatise* sufficiently biased in favor of a 'theoretical' approach to algebra to rule out, in advance, any possibility of him following Babbage's thoroughly formalist example. At the same time, he was obviously sufficiently skeptical of the 'official' Cambridge, 'Pauline' alternative, to realize that much of the university's rhetoric was mere lip service to a program that with regard to algebra as he knew it, was quite incapable of being seen through. This is the context in which, I believe, the *Treatise* should be read by those interested in understanding why it was written, and what it sought to accomplish.

Of course it is not the only context in which historians should be interested. The *Treatise* was an extraordinarily influential work. But to understand its impact on others requires reading it in the light of *their* objectives, commitments and deliberations, as well as their personal and professional circumstances. Indeed the variety of different reader responses to the *Treatise* is quite extraordinary. Whewell and Hamilton, whose positions on matters metamathematical and metascientific were near identical, still read the *Treatise* very differently! Whewell, as noted, wrote a book-length study of his own under the impression that Peacock's algebra lent itself naturally to a 'theoretical' interpretation, while Hamilton considered it the very paradigm of algebraic 'philology'.[80] The aim

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[79] I am referring to Whewell (1833a). For a detailed evaluation of that work in relation to Peacock's *Treatise*, see Fisch (1994b).

[80] Upon learning, in July 1835, that Peacock would be unable to attend the August meeting of the British Association in Dublin, Hamilton wrote to him the following:

> Besides the pleasure which I expected from even merely seeing you again, I wished to talk to you about Algebra. I have taken some pains to understand your views, & almost persuaded myself that I do. They interest me much, & I hope to study them more; but you have perhaps perceived enough of the turn of my mind in metaphysics to expect that I should rather admire them than adopt them. So far, indeed, have I gone in the opposite direction, that wishing lately to publish some sketch of my own views on the subject, I found myself forced back on an old thought of mine, & have nearly printed an essay on Algebra as the *Science of Pure Time*. The introductory remarks, however, {without expressly mentioning you *), } recognize the existence (I might say the necessity) of a

of the present study, however, is to account for the making of the book rather than its reception; to explain what prompted Peacock to compose it, rather than to analyze the ways it was read thereafter.[81]

In view of Peacock's predicament, the solution he proposed would seem both elegant and simple. If one is incapable of presenting algebra, in all its complexity, as a fully-fledged science, and if one is unwilling to follow Babbage in viewing it as a thoroughly symbolical system, why not enjoy the best of both worlds by splitting it down the middle? Peacock's *Treatise* proposed just that: to view algebra as comprising two antithetical components: one, a truth-governed science of number extending as far as it can, the other, a purely formal, symbolical system, unconstrained and autonomous, and sufficiently general to accommodate the full range of algebraic expression. Peacock dubbed them 'arithmetical algebra' and 'symbolical algebra' respectively. The former was set up similarly to the generalized arithmetic advocated by Maseres and Frend; the latter, at first blush, much the same as Babbage's pure analysis.

The problem, of course, was to determine the precise relationship between the two. Babbage wouldn't deny there being a science of number or quantity, any more than he would deny the existence of geometry or mechanics. What he objected to was granting them a foundational role for pure mathematics. To borrow the terminology of the day, Babbage treated arithmetical algebra and geometry as areas of 'mixed' mathematics, as applications of pure analysis, on a par with mechanics and optics. Peacock, as we have seen (despite his occasional rhetoric to the contrary), could not agree to viewing the theory of number, arithmetical algebra, merely as a side benefit, a lower-level application of its symbolical counterpart. To some extent, in some way, symbolical algebra had to be shown to meaningfully depend upon, pertain to, if not to actually derive from, a theory of number. And yet this had to be done without allowing symbolical algebra to lose any of its unbounded generality.

Still, despite the difficulties, the very recognition of an inherent two-sidedness *within* algebra was, in itself, a significant concession to Babbage on Peacock's behalf. The opening sentences of his preface to the *Treatise* clearly convey his sense of mission, urgency and, most of all, innovation.

> The work which I have now the honour of presenting to the public was written with view of conferring upon Algebra the character of a demonstrative science, by making its first principles co-extensive with the conclusions which were founded upon them: and it was in consequence of the very particular examination of those principles to which I was led in the course of this enquiry, that I felt myself compelled to depart so very widely from the form under which they have been commonly exhibited. The object which I proposed

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Philological School of Algebra, in which school you perhaps, have been the most bold and consistent teacher." (Hamilton to Peacock, July 23, 1835, Trinity College Library, Add.ms.b.49[44])

*) Inserted as an afterthought.

[81] Thus, even Peacock's own reassessment in subsequent years of the first, 1830 edition of the *Treatise* (which resulted in the significantly modified 2nd edition of 1842/5), though dealt with briefly below, properly belongs in a separate study.

> to effect is undoubtedly one of great importance, and of no small difficulty (...) and I am very sensible of the great responsibility which I incur by an attempt of this nature (...).[82]

In theory, Peacock had two options. He could divorce his two algebras completely and thus avoid having to relate them altogether. On such a view, arithmetical algebra would remain an autonomous, contemplative science of number, grounded exclusively on its own principles, no longer considered an application or model, of a system more fundamental. And symbolical algebra, truly unconstrained, could be developed, Babbage style, wholly "dissevered" from all lower-level, subject-orientated systems.[83] The second option open to Peacock was to retain a sufficiently watered-down, bottom-up dependency of symbolical on arithmetical algebra that would somehow allow to meaningfully ground the former in the latter, yet without imposing upon its generality.

Not surprisingly, Peacock opted for the latter. [84] He did so by assigning to arithmetical algebra the role of "science of suggestion" for symbolical algebra, treating arithmetical algebra

> as the science, whose operations and the general consequences of them should serve as the guides to the assumptions which become the foundation of symbolical Algebra:

[82] Peacock (1830, v).

[83] This is how Whewell, in 1833, in an incredible combination of haste and wishful thinking, initially interpreted the *Treatise*. In order to assimilate Peacock's approach to his own system of thought, Whewell was required to assign a 'Fundamental idea' to each of the two algebras. In a deep sense, a Whewellian mathematical system, is a wholly self-contained autonomous structure that is generated by, and acquires its meaning from the Fundamental Idea it articulates. In order that two systems be logically dependent, their respective Fundamental Ideas have to be nested. By assigning different, non-inclusive Fundamental Ideas to the two algebras, Whewell, in effect, dissevered them from one another. Arithmetical algebra was easy, in it being set up from the start as the theory, or science of number. (Better acquainted with, and more inclined toward a Kantian mindset than Peacock, Whewell, like Hamilton, preferred the ordinal connotations of "pure time" to the nominal connotations of number in this respect. But that is beside the point.) As the Fundamental Idea for symbolical algebra – that purely syntactical system that wasn't supposed to be *about* anything at all! – Whewell chose that of the "absolute generality of representation", Whewell (1833a, 83–4). As I have shown in some detail elsewhere (Fisch (1994b, 272ff.)), it was a lame attempt to achieve the impossible. There was simply no way in which the principles, definitions and equations of symbolical algebra could be described, in Whewellian fashion, as *true of* the fundamental idea of a general system. Whewell, I have shown, eventually gave up the project altogether.

[84] The idea of a thoroughly abstract algebra developed for reasons other than instrumental would take time to take root. Babbage, who in this too was way ahead of his time, justified the pursuit of pure analysis as a way of gaining insight into the workings of the creative mind, an approach proposed for the first time publicly by George Boole over a quarter of a century later. In the early 1840's DeMorgan still recoiled from the idea of an autonomous "technical algebra" (as he termed "the art of using symbols under regulations which (...) are prescribed as the definition of the symbols" DeMorgan (1837, 174)), which, as noted above, he felt should not be considered a science, and bore no resemblance to what mathematicians actually do. He "who makes the transformations of algebra by the defined laws of operation only, 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". De Morgan (1842, 288f.).

> thus granting or assuming in the first instance the universality of the values and of the representations of the symbols (...) assumptions to which there is nothing corresponding in arithmetic, ...[85]

At the outset Peacock makes it all sound innocent enough. He repeatedly stresses the fact that the two algebras

> under no view of their relation to each other, can be considered as one science, whatever be the nature of their connexion with each other; (...) there is nothing in the nature of the symbols of [symbolical] algebra which can essentially confine or limit their signification or value ...[86]

Symbolical algebra, Peacock promises time and again, is to stand on its own two feet. Although its principles may be suggested by arithmetic, once formulated, they are to establish "the science of algebra" as "a science of symbols and their combinations, constructed upon its own rules, which may be applied to arithmetic and to all other sciences by interpretation".[87] In this crucial passage, Peacock goes to great lengths to play down the significance of the role he assigns to arithmetic. The principles of symbolical algebra may be suggested by those of arithmetical algebra, but on no count can they be considered generalizations of them.

> [I]t is an abuse to the term *generalization* to apply it to designate the process of mind by which we pass from the meaning of *a-b* when *a* is greater than *b*, to its meaning when *a* is less than *b*, or from that of the product *ab*, when *a* and *b* are abstract numbers, to its meaning when *a* and *b* are concrete numbers of the same or of a different kind;...[88]

The fact that, despite their initial suggestive role, the principles of arithmetic are ultimately interpretations of those of algebra, imposes a firm logical "order of succession" between the two realms of algebraic activity. Logically speaking, symbolical algebra needs to be established prior to interpretation, and hence prior to arithmetic. "Interpretation", as he puts it, "will *follow*, and not *precede*, the operations of [symbolical] algebra.".[89] But Peacock recoils immediately. Apparently uneasy with the idea of relegating the role of number for algebra to that of a mere inspiration, the very next paragraph takes away much of what the previous one had just given. If one is aware of his dilemma, his indecision is manifest almost from page to page.

It is not enough that the principles of symbolical algebra be merely inspired or suggested by those of arithmetic. In the very next paragraph Peacock goes on to demand that they be so constructed as to ensure *in advance* their capacity to yield their

[85] Peacock (1830, xi).

[86] Peacock (1833, 194). There are no substantial differences between the theories of algebra presented in the *Treatise* and the BAAS Report. The latter, however, not being a textbook, dwells at greater length and in greater detail on the metamathematics at stake, and is, therefore, a better suited source for the concerns of the present essay. Unless indicated otherwise, all italics are Peacock's.

[87] *Loc. cit. pp. 194–5.*

[88] *Loc. cit*. p. 194. Peacock further emphasizes the point in a footnote concluding, no less, that: "The process, therefore, by which we pass from one science to the other is not an ascent from particulars to generals, which is properly called *generalization*, but one which is essentially arbitrary...."

[89] Peacock (1833, 195).

arithmetic counterparts by interpretation. The principles and rules of combination of symbolical algebra, he insists, are to be preserved "in whatever manner they may have been suggested". Arithmetical algebra is required, therefore, not only to suggest symbolical algebra, but to be included in it as a special case. Notice also how the move from mere suggestion to predetermined inclusion is subtly clinched by changing the idiom to that of necessity.

> Though the science 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; for in as much as symbolical algebra, though arbitrary in the authority of its principles, is not arbitrary in their application, being required to include arithmetical algebra as well as other sciences, it is evident that their rules must be identical with each other, as far as those sciences proceed together in common.[90]

The principles of symbolical algebra are expected not merely to be motivated by those of arithmetical algebra, but are required *a priori* strictly to embody them.[91] This is Peacock's first, and explicit, step towards denying symbolical algebra the autonomy and self-sufficiency he had so ceremoniously promised at the outset. Still, visibly dithering, he continues to insist that

> The rules of symbolical combination which are thus assumed have been *suggested only* by the corresponding rules in arithmetical algebra. They cannot be said to be *founded* upon them, for they are not *deducible* from them; ...[92]

It is true that in Peacock's system the principles of symbolical algebra are not *deducible* from those of arithmetical algebra, but, counter to his confident assertions to the contrary, they are none the less wholly *determined* by them. Peacock's argument to the contrary, cited a moment ago, rests on the claim, dubious in itself, that the leap from contemplating the subtraction of a lesser from a larger quantity to the subtraction of any quantity from another is so great as to do violence to the term generalization. I think most people would disagree, I certainly do, but that is beside the point. The problem with this line of argument, so central to Peacock's case, is not that it is invalid, but that it is quite irrelevant to the problem at hand. The move, at the level of principles and rules of operation, from arithmetical to symbolical designation does *not*, at any point, require the mind to entertain the idea of subtracting larger from lesser quantities. The very idea of a symbolical algebra, founded as a "science of symbols" independently of arithmetical considerations, is not to expand and generalize the notions of number or quantity, but to get away from them altogether! In symbolical algebra (as opposed to generalized arithmetic), prior to its application, the *a* and the *b* in *a-b* are utterly featureless, and, in, and of themselves, designate nothing in particular. They don't stand for quantities lesser

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[90] *Ibid*.

[91] It goes without saying that suggestion need not entail inclusion. $a^p \cdot a^q = a^{p+q}$, for example, certainly suggests

$$\frac{d^n}{dx^n}\left(\frac{d^m u}{dx^m}\right) = \frac{d^{n+m}u}{dx^{n+m}},$$

though neither formula includes the other.

[92] *Loc. cit*. pp. 197–8.

or greater, because they don't stand for quantities at all. They stand for nothing; they are mere symbols. When their possible interpretation is considered, one may, of course, consider various contexts and potential *designata*. But the move "up", by suggestion, from the subtraction of numbers and quantities to the symbolical formula a-b on no account requires contemplating the performative extension of subtraction *within the realm of number*! What it does require is a willingness to regard the operation in radically pure syntactical abstraction. But that is a different matter entirely.

Peacock's reasoning is weak, but the uneasiness that motivated it was strong. The only way algebra as he knew it could be preserved and justified intact was by setting it loose of arithmetic, or, at the very least, setting loose those parts of it that allegedly defy arithmetic interpretation. But algebra as Peacock knew it constitutes a logically connected whole, and cannot be broken down into independent numerical and non-numerical components. His solution, therefore, was to retain symbolical algebra intact as a system while breaking it down into *numerically interpretable* and non-interpretable components. To do so, however, amounted to rendering arithmetical algebra wholly subordinate to symbolical algebra, and, contrary to his strong Cambridge/Pauline bias, stripping the numerical intuitions that ground arithmetic of their traditional constitutive role for algebra. Despite his own introductory statements to the contrary, the latter bias appears to have emerged triumphant. In order to preserve a constitutive role for arithmetical algebra Peacock simply reversed his own very formulation, requiring interpretation, as it were, to *precede* rather than follow "the operations of symbolical algebra". In order to formally ensure in advance that, when interpreted, symbolical algebra would yield arithmetical algebra in its entirety, Peacock formulated the one foundational principle of his twofold algebraical system. He dubbed it: the "Principle of the Permanence of Equivalent Forms". "Equivalent forms" are algebraical identities. The principle of their "permanence" serves to guarantee their preservation each time they are "transported" across the arithmetical/symbolical divide. This requires separate principles for crossing back and forth. Peacock formulated them in the form of two "propositions":

> *Direct proposition:*
> Whatever form is algebraically equivalent to another when expressed in general symbols, must continue to be equivalent, whatever those symbols denote.
>
> *Converse proposition:*
> Whatever equivalent form is discoverable in arithmetical algebra considered as a science of suggestion, when the symbols are general in form, though specific in their value, will continue to be an equivalent form when the symbols are general in their nature as in their form.[93]

It is the 'converse proposition' that serves formally to clinch the foundational role of arithmetical algebra for symbolical algebra in Peacock's system. Duly generalized, their symbols and operations carefully dispossessed of numerical connotation, each and every equation of the former is automatically forced upon the latter. This, of course, amounts to much more than "suggestion". "Determination" seems to be the proper term.[94] Sym-

93 Peacock (1833, 198–9). See also Peacock (1830, xv–xvii).
94 Which is indeed the term adopted by Peacock fifteen years later in the second edition. For references, and assessment of the differences between the two editions see below §4.

bolical algebra may, in principle, include operations and identities that resist arithmetical interpretation, but Peacock's system *prima facie* disallows the reverse. Given the 'converse proposition' there can be no such thing as an exclusively arithmetical truth. All arithmetical truths automatically generate valid symbolical formulae. The move up from quantitative equality to formal identity, states the "converse proposition", is purely a matter abstraction, and, in principle, can harbor no exceptions. In Babbage's system, by contrast, pure symbolical identity is significantly more restrictive for pure analysis than quantitative equality is for arithmetic. Consequently, as we have seen, Babbage banished all talk of infinite series from the former although he regarded them perfectly legitimate in the latter. This type of move is not merely absent, but is quite impossible in Peacock's algebra, in which, for lack of a separate standard, all valid arithmetical equations necessarily end up, suitably abstracted, as symbolical identities.

The "direct proposition" – which supposedly imposes upon arithmetical algebra every symbolical identity deemed arithmetically interpretable – might appear to redress the balance by working in the opposite direction. Peacock, after all, promised that symbolical algebra be "constructed upon its own rules" independently of, and prior to its application (back down) to arithmetical algebra, or any other of its applications. But the sense of independence, logical primacy and autonomy created for symbolical algebra by the "direct proposition" and his preemptive rhetoric turns out to be an illusion. Throughout the *Treatise* the "direct proposition" is applied exclusively to low-level results – never at the level of fundamental principle or rule of operation. All the fundamental principles of symbolical algebra, its definitions and rules of operation are, without exception, funneled up from arithmetical algebra courtesy of the "converse proposition".

His treatment of exponents[95] is typical. The operation is defined for arithmetical algebra in the usual manner as shorthand for repeated multiplication:

$$\text{def} : a^n = \underbrace{a \cdot a \cdot a \cdot a \ldots a}_{n \text{ times}} ,$$

from which are derived the three well-known arithmetical 'equivalent forms' for m and n whole and positive:

$$a^m \cdot a^n = a^{m+n},$$

$$\frac{a^m}{a^n} = a^{m-n},$$

$$\left(a^m\right)^n = a^{m \cdot n}.$$

The three formulae are then generalized by means of the 'converse proposition' and rendered bona fide 'equivalent forms' of symbolical algebra. No longer constrained, m and n can then be manipulated freely to yield such symbolical identities as:

$$a^{m/n} = \left(\sqrt[n]{a}\right)^m ,$$

[95] Peacock (1833, 201ff.).

which, in turn, are interpreted back 'down' into arithmetical algebra, by means of the 'direct proposition', to yield such equations as:

$$a^{1/2} = \sqrt[2]{a}.$$

In doing so the original definition is transcended. Such equations defy the arithmetical definition of a^n, and are, therefore, unobtainable, of themselves, within arithmetical algebra. None the less, they do not represent mere meaningless notational conventions. Once obtained, they are allowed to enter the three arithmetical formulae from which fractional exponents were originally barred. In doing so the original stipulation regarding the wholeness of m and n are, in effect, canceled, and from now on the three formulae are permitted to apply *within arithmetical algebra* to all rational m and n, despite the fact that the chain of logical entailment from the original definition has been severed!

To a great extent this brief example summarizes the entire enterprise of the *Treatise*. In Peacock's theory there are simply no independent rules of symbolical algebra. What he misleadingly dubs 'suggestion' turns out to be a systematic and exhaustive two-tier process of generalization of arithmetic by which, in its second stage, the entire system of symbolical algebra is *generated*. Without exception, all deductive points of commencement within symbolical algebra have their origin in arithmetical algebra (which has *its* origin in arithmetic), systematically funneled up care of the Converse Proposition. Only lower-level, secondary symbolical results (e.g. $a^{m/n} = \left(\sqrt[n]{a}\right)^m$) are interpreted back down and imposed upon arithmetical algebra by virtue of the Direct Proposition. In the last analysis Peacock's symbolical algebra is generalized arithmetic with the additional license to generalize both operations *and* symbols. Traditional generalized arithmetic – e.g. Maseres' and Frend's – only generalized operations, and was therefore constrained by the numerical character of the symbols upon which they operated. Peacock's symbolical algebra goes the additional step of ridding the symbols themselves of all designation (with the obvious exception of most indexicals). Aware of Peacock's predicament, we are able to appreciate the nature of his compromise. We are also able to appreciate just how problematic it was.

4. Facing the consequences

The *Treatise*, I have argued, was the work of a frustrated 'theorist', forced reluctantly to abandon his 'Pauline' principles, yet unable to turn his back on them completely. In his desire to carve a foundational role for the science of number, Peacock, in effect, fashioned symbolical algebra as a subsidiary of arithmetical algebra rather than the other way round (much rhetoric to the contrary notwithstanding). Though unconstrained, wider in scope, and laying no claim to either meaning or truth, symbolical algebra crucially remained a derivative of arithmetic, and, hence, a far cry from the type of truly independent formal system anticipated by Babbage. The system Babbage's "Philosophy of Analysis" envisaged was the basis for all mathematics, the fountainhead that nourishes, and lends structure to all fields of mathematical analysis. In the case of exponents, to stay with our current example, Babbage's point of departure would not be, as in Peacock, the numerical definition of repeated multiplication (on the side of arithmetical algebra), but the three purely formal rules of operation:

$$a^p \cdot a^q = a^{p+q}, \qquad \frac{a^p}{a^q} = a^{p-q}, \qquad (a^p)^q = a^{p \cdot q},$$

jointly construed (on the side of symbolical algebra) as the primary formal, and official *definition* of the operation. Remaining in the realm of pure analysis, all algebraic results are formally obtained from the tripartite definition *prior to* their application to number. In Babbage's system symbolical algebra is the foundation and arithmetical algebra the by-product – the former, deductively complete, clean and well-founded, the latter, partly parasitic, necessarily incomplete, messy. Peacock not only wanted it the other way around, but would have liked both of his algebras to boast completeness. However, instead of enjoying the best of both worlds, the system he proposed in 1830 ended up plagued by the sum of their worse.

Wholly parasitic on arithmetical algebra, Peacock's symbolical algebra adds up to little more than an elaborate disconnected set of exercises in exploring the consequences of ignoring the former's conceptual basis. Results are first obtained in arithmetical algebra, by virtue of the foundational definitions and principles of that system, and are only then emptied of content and further manipulated. It is not an exercise in seriously determining algebra's syntactical infra-structure as was Babbage's.[96] Peacock's *Treatise* did nothing of the sort. What, then, was the point of Peacock's elaborate system? The obvious answer is that by setting arithmetical algebra loose of its numerical moorings one was able to prove a large number of otherwise unobtainable results (e.g. $a^{p/q} = \left(\sqrt[q]{a}\right)^p$) which when applied back down, enriched arithmetical algebra in ways the likes of Masers and Frend had so dismally failed (e.g. $a^{1/2} = \sqrt[2]{a}$). The problem is that, while it is perfectly legitimate (if pointless from a strictly 'Pauline' perspective) to generalize an arithmetical result 'up' to symbolical algebra care of the Converse Proposition, each time a symbolical result, unobtainable of itself in arithmetical algebra, is applied by means of the Direct Proposition back 'down' to arithmetical algebra, Berkeley's fallacy of "shifting of the hypothesis" is inevitably committed! Obtaining $a^{p/q} = \left(\sqrt[q]{a}\right)^p$ on the assumption that p and q are vacuous symbols and applying it as a general result to a system in which a^b is defined exclusively for b's whole and positive, is the same as manipulating an expression under the assumption that ΔX is something, and, after dividing by it, deciding in mid proof that it has vanished! Peacock does it all the time, enthusiastically exploring the consequences for arithmetical algebra of generalizing the indexicals of indefinite series, the coefficients of cyclical functions, the orders of differentiation, and even factorials.[97] To use the Direct Proposition in order to supplement arithmetical algebra with results that, of themselves, fly in the face of its basic principles, strictures and definitions, is at best arbitrary, and at worse fallacious. There is nothing inherently wrong in developing the two algebras proposed by the *Treatise*. Nor is there anything inherently wrong in creating a symbolical algebra merely by further generalizing generalized arithmetic. But if the point of doing so is to somehow improve arithmetical algebra by enriching it with otherwise unobtainable results, then Peacock's system is quite inadmissible. A result

[96] Babbage had aimed even higher than that, he believed his was an attempt to discern the very grammar of human thought.

[97] Peacock (1833, 204–23).

cannot be considered *true* of the realm of numbers if, in order to obtain it, one was forced first to assume that it was not.

Peacock never openly acknowledged there being such a problem. Nor, to the best of my knowledge, did any of his friends or critics, except perhaps for Whewell, who, even during the brief period in which he eagerly endorsed the *Treatise*, shows clear signs of discomfort with the Direct Proposition. Whewell never openly criticized Peacock's algebra, in public or in private. Still, as I have noted elsewhere,[98] in the crucial passage on Peacock's Principle of the Permanence of Equivalent Forms in his "The Philosophy of the Pure Sciences" – the draft of an ambitious work on the philosophy of the mathematical (as opposed to the 'inductive') sciences, that Whewell completed in 1833 but chose not to publish – he seems clearly to have hesitated and to have had second thoughts: copying down the Direct and Converse Propositions *verbatim* but immediately crossing out the former.[99] Whewell nowhere explains what he found wrong with the Peacock's Direct Proposition, and I have found no written evidence of Whewell and Peacock exchanging views on this, or on related matters, although, as close friends and fellows of the same college they must have talked long and hard about all matters algebraical. Whatever was the nature of these conversations, or the extent to which Whewell's criticism might have borne on the further development of Peacock's views (Peacock certainly didn't change anything between the *Treatise* of 1830 and BAAS Report of 1833), one thing is clear: Peacock also eventually saw fit to do away with the Direct Proposition – which is carefully omitted in the second edition of the *Treatise* published during the early 1840's.

A full consideration of this later development of Peacock's position properly belongs in a separate study devoted to the reactions to his initial work and their effect on his subsequent thinking. None the less, it is fitting, by way of concluding the present paper, to briefly describe the main differences between the first and second editions of the *Treatise*, for the additional light they shed on the nature of the indecision Peacock exhibited in 1830. The elimination of the Direct Proposition in the second edition of the *Treatise* is much more than a technicality. It has a profound effect on the entire structure of the work, which, I believe, signals a significant change of heart on Peacock's behalf.

The second edition was produced in two volumes (published three years apart) in which arithmetical and symbolical algebra are treated separately. In them the Principle of the Permanence of Equivalent Forms is discussed twice: once in the preface to Volume I,[100] and again in a short chapter devoted to its "formal statement" in volume II.[101] In both cases it comprises one proposition only, the one Peacock dubbed 'Converse' in 1830 and 1833. It is described in the preface to Vol. I, as the general principle that expresses "the general law of transition from the results of arithmetical to those of symbolical

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98 Fisch (1994b, 273–4).

99 Whewell (1833a, 86). I have suggested that his decision not to publish the work independently, and only to print a significantly watered-down version of it as the first part of his 1840 *Philosophy of the Inductive Sciences*, owed to him realizing the impossibility in principle of squaring Peacock's algebra with his own developing views on the natural sciences. His reluctance to adopt the Direct Proposition in a work that fully endorsed Peacock's approach, by contrast, attests to him perceiving a problem *within* the system, rather than with it.

100 Peacock (1842, vi–vii).

101 Peacock (1845, 59–60).

algebra" with no mention made of subsequently transversing the divide in the opposite direction. "Upon this view of the principles of symbolical algebra", he continues, no longer mincing his words, "it will follow that its operations are determined by the definitions of arithmetical algebra, as far as they proceed in common".[102] Nowhere throughout the two volumes is the question of applying the results of symbolical algebra back down to arithmetical algebra even raised. Volume II speaks much of applying new results of symbolical algebra – not back to arithmetical algebra, however, but to geometry and trigonometry (or as Peacock prefers: "goniometry"). The overall effect is quite impressive, and a major step away from the *Treatise* of fifteen years earlier. Arithmetical algebra is still made to 'suggest' symbolical algebra as before by an exhaustive all-embracing full generalization of the system. Still, the differences are significant. The book no longer reasons back and forth across the great divide between the two algebras in the bootstrapping way of the first edition. Arithmetical algebra is first fully developed as an autonomous self contained system. Only then, in a separate volume, and in a manner no longer related to number, is it further generalized, restated formally, and declared the basis for a fully-fledged symbolical system that, from that point onwards, is developed in its own right, from its own principles, and whose quality, strength and value are measured by its formal completeness, on the one hand, and the richness of its application, on the other. Symbolical algebra is no longer presented as a question-begging operation for the sake of enriching arithmetical algebra, but as a formal, logically connected system, lacking intrinsic meaning or a truth relation, for which the science of number (arithmetical algebra) is considered but one of its several (partial) models – and that of geometry the most important. With symbolical algebra fully developed and in place, and its areas capable of arithmetical interpretation (i.e. those initially 'suggested' by arithmetical algebra) well charted, the idea is then to cast around for other models capable of rendering other portions meaningful. Unlike the *Treatise* of 1830, the second edition no longer regards the science of number algebra's main beneficiary in this respect. Rather than those of number, "it is from the relations of space", announces Peacock at the outset of Vol. II, "that Symbolical Algebra[103] derives its largest range of interpretations, as well as the chief sources of its power of dealing with those branches of science and natural philosophy which are essentially connected with them". Indeed, Peacock's most significant contribution to mathematics is the important anticipation of vector analysis found in the geometrical interpretation of imaginaries dealt with at length in Vol. II.[104] Peacock seems well-aware of the significance of his move, for he continues:

> . . . it is for this reason that I have endeavoured to associate Algebra with Geometry throughout the whole course of its development, beginning with the geometrical interpretation of the signs $+$ and $-$ when considered with reference to each other, and advancing to that of the various other signs which are symbolized by the roots of 1: we are thus enabled, in the present volume, to bring the Geometry of Position, embracing the whole theory of

[102] Peacock (1842, vii).

[103] The inconsistent capitalizing is Peacock's. Arithmetical and symbolical algebra, geometry, etc. spelt with lower case throughout Vol. I, are all capitalized Carlyle-style in Vol. II.

[104] Crowe (1994, 14, 15, 34, 108).

> lines considered both in relative position and magnitude and the properties of rectilinear figures, under the Dominion of Algebra.[105]

(Needless to say, a prudent 'Pauline' theoretician would find little point in developing symbolical algebra to this end. For him the value and worth of a field of mathematics is measured by the conceptual clarity of its foundations and the logical connectiveness of what follows from them. Hamilton's Quaternions project is precisely an attempt to construct a fully fledged algebra without having first to construct a purely formal, conceptually vacuous system to serve as its 'dominion'. But to this end Hamilton was willing to do what Peacock, I believe, never considered: namely, to modify and enrich the very notion of number.) In 1842 Peacock fully admits that the original *Treatise* of 1830 had been seriously flawed, and that, despite their shared title, the new work is not a second edition in the simple sense of the term.[106]

> It is true that the same views of the relations of the principles of Arithmetical and Symbolical Algebra formed the basis of my first publication on Algebra in 1830: but not only was the nature of the dependence of Symbolical upon Arithmetical Algebra very imperfectly developed in that work, but no sufficient attempt was made to reduce its principles and their applications to a complete and regular system, all whose parts were connected with each other: they have consequently been sometimes controverted upon grounds more or less erroneous . . . [107]

By 1840 or so, Peacock had clearly made his peace with the idea of an autonomous, and genuinely symbolical algebra, which, though initially suggested by a system of generalized arithmetic à la Frend and Maseres, provided syntactical grounding for a variety of mathematical applications much in the spirit of Babbage's initial suggestions. The dependence of Peacock's algebra on arithmetic remained problematic in ways that Babbage's system did not, but his talk of its autonomy was no longer mere lip-service, and the formative role he gave it with respect to geometry was real.

The problem was that by 1845 Peacock's system, improved as it may have been, was no longer news. With the development of abstract algebra well underway in the good hands of DeMorgan, Gregory, and later Boole, and with Hamilton's quaternions project approaching completion (and resembling less and less the meaningful, theoretical enterprise it had set out to become), Peacock's new and improved textbook no longer represented the cutting edge of metamathematical deliberation. It broke no new ground. Peacock, one could say, reacted to his own system too slow for his second thoughts to be much relevant to the different ways in which his colleagues and contemporaries had reacted to it. And thus, the work for which he is justly remembered is the gravely flawed version of 1830, rather than the better thought out, mathematically more solid refashioning of it of fifteen years later. This is not to be regretted, however. Flawed as it was, Peacock's original *Treatise on Algebra*, as I have argued, was a bold, pained, creative attempt to negotiate a dilemma he was incapable of resolving. Unable to decide the issue this way or that, the solution he proposed in effect preserved the impasse rather

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105 Peacock (1845, iv).
106 Peacock (1842, iii).
107 *Loc. cit.* p. iii.

than break it. For Peacock (and those who shared his Pauline approach to mathematics, intellectual integrity, and deep knowledge and appreciation of continental analysis) traditional portrayals of algebra as generalized arithmetic were as unacceptable as the type of radical formal operationalism that Babbage had proposed. The novelty and significance of the *Treatise* becomes apparent when one realizes, as this paper has argued, the extent to which it consciously aspired to forge a creative compromise between these two alternatives, of which its author, we now know, was all too well aware. The compromise itself was exceedingly problematic. It never stood a chance. The importance of the *Treatise* to the history of mathematics lies, then, not in the particular dualism it offered, as much as in the way that the dualism it proposed preserved, drew attention to, and further emphasized the very dilemma it was designed to resolve! Peacock had no disciples, and the tale of two algebras he proposed was taken up by no one. To those unaware of Babbage's unpublished work, Peacock's symbolical algebra might have been their first exposure to algebraic formalism (DeMorgan may have been thus influenced). But for the larger part, the *Treatise*'s real impact lay in the way it so forcefully raised the problem, dithering undecidedly, as it did, between the two seemingly unacceptable and irreconcilable alternatives. There is more to be said for the role of creative dithering in moments of grave conceptual crisis, and for the tendency to try first to resolve such dilemmas by constructing dualisms. And needless to say, there is much more to be said of the various reactions to Peacock's initial suggestions, including, as noted, his own. All of which will have to wait for future publications.

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