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Babbage's two lives

MENACHEM FISCH*

Abstract. Babbage wrote two relatively detailed, yet significantly incongruous, autobiographical accounts of his pre-Cambridge and Cambridge days. He published one in 1864 and in it advertised the existence of the other, which he carefully retained in manuscript form. The aim of this paper is to chart in some detail for the first time the discrepancies between the two accounts, to compare and assess their relative credibility, and to explain their author's possible reasons for knowingly fabricating the less credible of the two.

Babbage wrote two very different autobiographical accounts of his pre-Cambridge and Cambridge years: the one he elected to publish as Chapters 3 and 4 of *Passages*,[1] and the one contained in the first third or so of his unpublished 289-page manuscript on the history of the calculus of functions usually referred to as Buxton MS 13.[2] The former was published in 1864; the latter cites numerous letters and papers supposedly dating from the period described, but was also narrated long after the event. The cautionary notice on the first page of the manuscript is dated 14 May 1869 in Babbage's hand,[3] and contains at least one more explicit reference to that year in the margins of page 8. Although the two accounts are incongruous, I am not aware of any study of Babbage that takes note of their discrepancies. The aims of the present paper are to briefly chart and assess the inconsistencies between the two accounts, to evaluate their relative credibility, and to speculate briefly on Babbage's possible reasons both for framing and for withholding the latter. To state my conclusions at the outset: while most historians – notably Enros, Wilkes, Grattan-Guinness and Grier[4] – have tended to take the MS 13

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1 Charles Babbage, *Passages from the Life of a Philosopher*, with new introduction by Martin Cambell-Kelly, New Brunswick: Rutgers University Press, 1994 (originally published by Longman in 1864).

2 Charles Babbage, 'The history of the origin and progress of the Calculus of Functions during the years 1809, 1810 … 1817', MSS Buxton 13, History of Science Museum, Oxford.

3 It 'earnestly requests' the finder of this book, should it be 'mislaid or lost … to return it to the author if living or in case of his death to present it to the public library of the University of Cambridge'. It is signed, 'No 1 Dorset St. Manchester Sq. 14 May 1869'. See also Allan Chapman, 'A year of gravity: the astronomical anniversaries of 1992', *Quarterly Journal of the Royal Astronomical Society* (1993) 34, pp. 33–51, 50 n. 47.

4 See Phillip C. Enros, 'The Analytical Society: mathematics at Cambridge University in the early nineteenth century', PhD dissertation, University of Toronto, 1979; Enros, 'Cambridge University and the adoption of analytics in early nineteenth-century England', in Herbert Mehrtens, Henk Bos and Ivo Schneider (eds.), *Social History of Nineteenth-Century Mathematics*, Boston, Basel and Stuttgart: Birkhauser, 1981, pp. 135–48; and Enros, 'The Analytical Society', *Historia Mathematica* (1983) 10, pp. 24–47; Maurice V. Wilkes, 'Herschel, Peacock, Babbage and the development of the Cambridge curriculum', *Notes and Records of the Royal Society of London* (1990) 44, pp. 205–219; Ivor Grattan-Guinness, 'Charles Babbage as an algorithmic thinker', *IEEE*

account at face value, I shall argue that doing so is a grave mistake; that the MS 13 account is tendentious to the point of deliberate distortion, and regarding Babbage's Cambridge days wholly unreliable. I shall insist, however, that because it goes to such lengths to fabricate the story it tells, the document provides a rare and valuable resource for understanding Babbage's mindset at least during the latter part of his life.

The one and only cross-reference between the two accounts occurs in Chapter 33 of *Passages*, in a brief section devoted to the 'Calculus of functions', in which Babbage mentions 'a small MS. volume' in which he had recorded 'many years ago . . . the facts, and also extracts of letters from Herschel, Bromhead and Maule, in which I believe I have done justice to my friends if not to myself'.[5]

The very notion of 'a small manuscript volume' written many years ago is both curious and revealing. Manuscripts usually fall under one of two categories: unfinished texts-in-the-making that one still hopes to complete and publish, and drafts set aside as unworthy of publication. Had the text mentioned in *Passages* belonged in the former category, Babbage would have referred to it differently – not as an 'MS volume' but as a history of the development of that 'department of analysis' he hoped someday to publish. Had it belonged in the category of discarded texts, he would not have mentioned it at all. The latter was obviously not the case. At the time *Passages* was published the 'MS. volume' had certainly not been discarded, as the references in it to the year 1869 clearly prove. On the other hand, the phrase 'MS. volume' used to describe it both in *Passages* and, five years later, in the cautionary note on its the first page, clearly imply that, although incomplete, it was a work that Babbage had evidently decided not to publish. And yet the same note firmly instructs that it be preserved for posterity at the Cambridge University Library. In other words, Babbage presents us with an intriguingly intermediary, almost contradictory, category of manuscript text whose existence is referred to and advertised in print, and measures are taken to preserve it for posthumous public viewing, yet whose author had nonetheless resolved not to publish it – at least in his lifetime.[6] I shall return to the possible significance of all of this below.

I now consider the accounts themselves. In the light of the explicit reference made in *Passages* to the other work, and the fact that the letters and papers on which that account builds had been clearly known to Babbage long before completing *Passages*, neither account can be seen as a correction of the other. MS 13 was evidently embarked on long before, yet 'signed off' five years after, *Passages* was published, yet, despite returning in considerable detail to some of the events related in *Passages*, it nowhere even acknowledges its existence, let alone presents itself as an attempt to put the record right.

Annals of the History of Computing (1992) 14, pp. 34–48; and David A. Grier, 'The inconsistent youth of Charles Babbage', *IEEE Annals of the History of Computing* (2010) 32, pp. 18–31, who all follow the uncritical meshing of the two accounts found in Harry W. Buxton, *Memoir of the Life and Labours of the Late Charles Babbage Esq. F.R.S.* (ed. Anthony Hyman), Cambridge, MA: The MIT Press, 1988.

5 Babbage, op. cit. (1), p. 327.

6 It ended up in the hands of Harry W. Buxton, who testifies to have received from Babbage 'some years before his death' the 'small MS volume . . . referred to at page 435 of the *Passages from the Life of a Philosopher*', along with other papers, in order 'that I should, after his decease, give to the world some account of his life and scientific labours'. Buxton, op. cit. (4), p. 3.

Babbage's *Nachlaß* simply and, as far as I can tell, quite intentionally presents us with two contrasting reflections on his pre-Cambridge and Cambridge days. He published one without any qualms, while relegating the other to the dubious status of a manuscript deliberately not published yet advertised and preserved for all to see. The former acknowledges the existence of the latter, as if to make sure it is noted, but neither of them acknowledges, or volunteers an explanation of, their extensive disagreements.

Let me briefly summarize how the two accounts relate the period in question.

Passages, 1864

Passages describes Babbage's 'boyhood' as lacking formal or structured mathematical training or focus, yet a period in which he became 'passionately fond of algebra' thanks to a motley assortment of 'mathematical works as accident brought to [his] knowledge', to which he 'employed all [his] leisure in studying' (p. 18), which included Ward's *The Young Mathematician's Guide* (1707), Ditton's *Institution of Fluxions* (1706) and Lagrange's *Théorie des fonctions* (1797). By the time he came up to Cambridge in autumn 1810, he had acquired a basic, if eclectic, ability, as he put it, to 'work out such questions as the very moderate amount of mathematics which I then possessed admitted, with equal facility, in the dots of Newton, the d's of Leibnitz, or the dashes of Lagrange'. *Passages* does not describe him as engaging in original research prior to his second year, but as arriving at the university after meeting with 'many difficulties' and looking forward with 'intense delight' to 'having them all removed' (p. 19).

According to *Passages*, his high hopes for mathematical enlightenment were dashed in the course of his first year. Because irrelevant to the Senate House exams, his difficulties were pooh-poohed as inconsequential by his teachers, who also struck him as incompetent (pp. 19–20). Urging him to set them aside and concentrate on his exams had the opposite effect. Babbage lost interest in Cambridge honours, and set about 'devour[ing] the papers of Euler and other mathematicians, scattered through innumerable volumes of the academies of Petersburgh, Berlin, and Paris, which the libraries I had resource to contained' (p. 20). The impression is that once again, lacking proper instruction, Babbage, left to his own devices as in his schooldays, 'let loose' haphazardly on whatever the university's libraries had to offer.[7] As before, he is not described as focusing on specific problems, but as 'devouring' anything on which he could lay his hands, but no longer eclectically, the text implies. Disappointed to the point of offence with what Cambridge was willing to offer, his new-found, if unsystematic, self-exposure to the best of Continental analysis served to tip the balance. 'Under these circumstances', he writes, 'it is not surprising that I should perceive and be penetrated with the superior power of the notation of Leibnitz' (p. 20). Though defiantly immersed in Continental mathematics, he was not a recluse. Other activities included a devotion to chess, whist and sailing, as well as a Sunday morning 'breakfast club' where 'ten or a dozen friends' would gather in his rooms to discuss 'all knowable and many unknowable things' (p. 25).

7 The term is Martin Campbell-Kelly's. See his 'Introduction' to Babbage, op. cit. (1), p. 11.

Babbage's unruly mathematical self-study acquired a more orderly and systematic format after he decided to stop at a London French bookseller on his way back to Cambridge at the beginning of his second year, and spend a small fortune[8] on 'the great work of Lacroix'.[9] Lacroix's masterly survey of the calculus enabled Babbage to better locate and articulate both his frustration with Cambridge mathematics and the field of analysis to which he was attracted – functional equations. Of the latter, *Passages* remains silent, refraining even from giving the titles of the papers comprising the 1813 *Memoirs of the Analytical Society*, which is mentioned at length (p. 21).

But on his frustration with Cambridge mathematics *Passages* is quite explicit. With Lacroix in hand his frustration and 'distaste for the routine of the studies of the place' were swiftly transformed into rebellion, and rebellion, with the help of his friends, into reasoned, organized and collective reformative action. The story of the formation of the Analytical Society is well known and I shall focus only on the details pertinent to comparing the two accounts. According to *Passages* it all began 'at the commencement of the second year' of his 'residence at Cambridge' – namely October 1811. Waiting for Michael Slegg to return from Chapel, he decided to parody a Bible Society poster and 'drew up the sketch for a society . . . for translating the small work of Lacroix' and holding 'periodic meetings for the propagation of D's; and consigning to perdition all who supported the heresy of dots' – maintaining that, like the Bible, Lacroix's textbook 'was so perfect that any comment was unnecessary'. Slegg – described as a friend, Trinity man and drinking partner, and likewise 'enthusiastically attached' to 'mathematical subjects' – loved the parody and asked to share the idea with 'a mathematical friend', Edward Ffrench Bromhead (p. 20), who also loved it and 'proposed seriously' that they form 'a society for the cultivation of mathematics' (pp. 20–21). With Babbage's permission Bromhead assembled several like-minded friends at his lodgings, who decided to 'constitute themselves "The Analytical Society"; hired a meeting-room, open daily; held meetings; read papers, and discussed them' (p. 21). According to *Passages*, in addition to 'the projectors' – Slegg, Babbage and Bromhead – the initial group consisted of John Herschel, George Peacock, Alexander D'Arblay, Edward Ryan, Thomas Robinson, Frederick Maule 'and several more'. And, again, not a word about content beyond the seemingly technical notational issue of d's versus dots.

Passages clearly implies that although the combination of Babbage's 'wicked pun' (p. 21) and Bromhead's organizational skills was what set the formation of a society in motion, what made it possible was the existence of a critical mass of students who shared their 'enthusiastic attachment' to 'mathematical subjects'. In fact, they would have required more than a mere 'enthusiastic attachment' to mathematics to appreciate Babbage's pun. Even the brightest of Cambridge men might have heard of the priority dispute between Newton and Leibniz, but would not have pledged wholehearted

8 Seven guineas, according to Babbage – an extraordinarily high price for someone whose annual allowance was £300.

9 Namely Silvestre F. Lacroix's *Traité du calcul différentiel et du calcul intégral*, 3 vols., Paris: Chez Courcier, 1797–1800. For a detailed appraisal of the work see João Caramalho Domingues, *Lacroix and the Calculus*, Basel: Birkhauser, 2008, p. 23.

allegiance to the promotion of d's in place of dots, had they not already been aware to some extent of the unrivalled achievements of Continental analysis.

The story of the collaborative translation of Lacroix by Herschel, Peacock and himself, and of the compilation, a little later, of the three companion collections of examples they had published – one devoted to the calculus (begun by Babbage and completed by Peacock), one to the calculus of functions (Babbage), and one to finite differences (Herschel) – is told below in some detail. But, again, there is no word about content except for their titles (pp. 27–29).

Its jerky narration and need of an editorial hand notwithstanding, *Passages* narrates Babbage's development from self-taught enthusiast to rebel–reformer in a way that is at once both coherent and true to character, and in a manner that rings true. It is clearly not always truthful, and some of its details have been seriously challenged by historians,[10] but it tells a credible story, which, barring evidence to the contrary, can in general be relied upon.

Buxton MS 13

Understandably perhaps for a work officially devoted to the history of a field rather than to that of a person, MS 13 passes over Babbage's schooldays, the mathematics he had acquired before coming to Cambridge, his mood and expectations upon entering the university, his life as a student and so forth in almost complete silence. But it is an autobiographical history of a field, cleverly crafted to display its 'development and progress' during the years it covers as owing decisively to its author. The narrative's main purpose, and much of the time its sole purpose, is to chart Babbage's ongoing preoccupation with the solution of functional equations. Other elements of the story are incorporated only insofar as they serve this purpose. And to this end, as we shall see, Babbage takes the liberty of manipulating them freely.

'Mr. Spence's … Logarithmic Transcendents'

The fact that MS 13 focuses exclusively on Babbage's work on functional equations stands to reason, as do its efforts to show that his thinking and writing on the subject pre-dated his arrival at Cambridge by the best part of a year. In this respect the two accounts would appear to dovetail nicely. From the books that *Passages* claims Babbage had studied before coming up to Cambridge, he could have easily acquired not only a good grasp of functions and of the functional turn in Continental analysis late of Euler, but of Lagrange's functional, power-series approach to the calculus.[11] But the fact that

10 Laura J. Snyder, *The Philosophical Breakfast Club: Four Remarkable Friends Who Transformed Science and Changed the World*, New York: Broadway Books, 2011, points to several occasions in which *Passages* takes credit for the achievements of others, such as hosting the Sunday 'breakfast club' and masterminding the establishment of Section F of the British Association. See also Silvan S. Schweber, *Aspects of the Life and Thought of Sir John Frederick Herschel*, New York: Arno Press, 1981.

11 Elaine Koppelman, 'The calculus of operations and the rise of abstract algebra', *Archives for History of Exact Science* (1971–1972) 8, pp. 155–242, 178. See also Craig G. Fraser, 'Joseph Louis Lagrange, *Théorie des*

the two accounts would seem to supplement each other so well only further accentuates their puzzling lack of overlap or cross-reference.

According to MS 13, Babbage's interest in functional equations was initially awakened at 'about the latter end of 1809' by 'a well-known proposition of Papus' (p. 5 [p. 24][12]). What had 'particularly excited' his attention was the problem of calculating the ratio of the area of a given hyperbola to the sum of the areas of the diminishing series of contiguous circles tangential to both the curve and the axes, and especially the converse problem of determining the curve given the law of the ratio of the areas. Babbage failed to solve it (pp. 4–5 [p. 24]), but it is evident that, remarkably for an English schoolboy in 1809, he had perceived the geometrical problem of determining the curve as an algebraic problem of determining a function.[13] But here the two accounts diverge conspicuously for no apparent reason. Bemoaning the fact that all record of these early attempts had been lost due to his use of a slate, and trying to recall 'the mode of trial' he had been using, Babbage names his source:

> I rather think I employed $fx = y$ to denote the equation of the curve to be found, because I well remember that soon after the time alluded to I made use of $f(a, b)$ to signify a function of a and b when engaged in some enquiries relative to the solution of equations. Mr. Spence's essay on Logarithmic Transcendents furnished me with the idea and symbol of functions, and to the same very excellent and learned work I was indebted for a great addition to my mathematical knowledge (p. 6 [pp. 25]).

This is a very curious statement, to say the least. If there is any truth to the list of works he claims in *Passages* to have 'employed all [his] leisure in studying' at the time, he would not have needed William Spence's *Essay on the various Orders of logarithmic Transcendents* to 'furnish' him 'with the idea and symbol of functions'. For one thing, Spence's work was only published in 1809.[14] It is most unlikely that such a work would have at all come to the young, untutored eighteen-year-old's attention so soon after being published, let alone been at his disposal by the end of 1809. Second, even if Spence's book had been at his disposal, for him to name Spence as the source of his introduction to the 'idea and symbol' of function, rather than Woodhouse or Lagrange, whom he claims to have studied intensively, and on whom Spence's book explicitly built, is little short of ridiculous. Finally, if, incredible as it might sound, Spence's work had played the formative role MS 13 claims it had, it should have figured prominently among his pre-Cambridge readings listed in *Passages*, which it does not.

I shall suggest below that "dropping" Spence's name at the outset of the MS 13 account was probably a calculated move that had less to do with Babbage's

Fonctions Analytiques', in Ivor Grattan-Guinness (ed.), *Landmark Writings in Western Mathematics 1640–1940*, Amsterdam: Elsevier, 2005, pp. 258–276.

12 Pagination in square brackets refers to Buxton, op. cit. (4), pp. 24–41, where the relevant passages of MS 13 are conveniently copied. I have retained Babbage's original spelling and punctuation, which Buxton frequently amends.

13 See also Charles Babbage, 'An essay towards the calculus of functions, Part I', *Philosophical Transactions* (1815) 105, pp. 389–423, esp. 391–392.

14 See *Philosophical Transactions of the Royal Society of London* (1809) 99, p. 473 (mistakenly marked 373).

mathematical development than with Herschel's role a few years later in publishing an edition of Spence's mathematical papers.

John Phillips Higman

The only acquaintance to whom MS 13 refers by name in connection with the period prior to the formation of the Analytical Society is John Phillip Higman. Two years Babbage's junior, Higman was admitted to Trinity in October 1812 and, according to Enros, served as secretary of the Analytical Society during his first year in College.[15] He graduated third wrangler in 1816, obtained a Trinity fellowship and served as tutor from 1822 to 1835. He then left Trinity under a cloud of scandal to take up a 'College living' at Fakenham Parish Church.[16] Although he did well as a student, ranked high on the tripos, left an impression on some students – including the young De Morgan[17] – was Babbage's friend, and was closely involved with the Analytical Society, Higman played no discernible role in the reform of the Cambridge mathematics, nor did he publish anything of mathematical note. He died in 1855, long before Babbage's two texts acquired their final shape. *Passages* mentions him briefly once, but only as a member of a 'set' that Babbage 'frequently joined . . . addicted to sixpenny whist' (p. 26).

But MS 13 portrays him as Babbage's sole mathematical lifeline well into his second year at Cambridge. Higman's first mention is of no consequence, but cleverly sets the stage for what is to come. Not all his pre-Cambridge musings were lost to the slate, Babbage writes. The expressions f^n and $fff(x)$, he states, adorn 'the back of a letter from Mr. Higman', written 'about August 1810', that had to have been written shortly after it was received 'because I preserved all Mr. Higman's letters carefully folded up in a parcel by themselves'. From which 'it will clearly follow that about August 1810 I was in possession of the notation of functions of any order and one variable and that I had attempted to determine the form of . . . a function from a functional equation' (pp. 6–7 [p. 25]).

Again, a very curious statement. First, merely jotting down f^n and $fff(x)$ does not constitute an attempt to solve an equation, functional or other. It does prove that whoever wrote it was in 'possession of the notation of functions', but then, why would someone who was supposedly reading Woodhouse and Lagrange, and claimed to have read Spence, not be? Unless what Babbage was really trying to establish was the Higman correspondence itself, for which he supposedly had the letters to prove. Indeed, the next mentions of Higman involve not their backs, but the letters themselves.

15 Enros, 'The Analytical Society', op. cit. (4), p. 136.

16 His obituary, published on 13 February 1857 in the *Monthly Notices of the Royal Astronomical Society* (1857) 17(4), pp. 98–99, talks laconically of 'his college life' exhibiting 'faults of a very grave kind, to which we cannot avoid allusion, as their consequences were so notorious'.

17 Though Frederick Pollock, *Personal Remembrances of Sir Frederich Pollock, Second Baronet, Sometime Queen's Remembrancer*, 2 vols., New York, Macmillan and Co., 1887, vol. 1, p. 40, deemed him 'a person of little account', Bromhead refers to him mockingly as 'one very high man'. Bromhead to Babbage, 9 February 1816, British Museum Add Mss 37182, No 46, cited by John M. Dubbey, *The Mathematical Work of Charles Babbage*, Cambridge: Cambridge University Press, 1978, p. 62, who fails, however, to identify the 'high man' as Higman.

These are cited to prove that by the end of his first year at Cambridge he was hard at work on functional equations and swiftly awakening to the problem of their inherent, and differing, levels of underdetermination. Thus, the equation $f(x, y) = f(x, -y)$, he allegedly writes to Higman in August 1811, 'is not sufficient to determine the nature of the function f', because '$ax + y^2$ will satisfy the equation and so will $ax^3 + bx^2 + cy^2 + xy^4$ and so will any function of x and y in which y is found only in the even powers' (p. 9 [p. 26]).

What is unnerving about this chapter of MS 13 is not the mathematical details of the exchange, which, if authentic, are truly impressive, but their autobiographical setting, which, again, fails both to cohere with the account given in *Passages* and to present a coherent alternative. For someone genuinely trying to reconstruct his early awakening to, and work on, functional equations before and after arriving in Cambridge, the picture painted by *Passages* would seem to provide the perfect setting for making the case – with the Higman correspondence possibly providing the final touch. But absolutely nothing we are told in *Passages* makes its way into MS 13, and vice versa.

And then there's Higman himself, who in *Passages* is denied any role in Babbage's mathematical life. Higman clearly emerged as less of a mathematical presence after joining Babbage in Cambridge, but if he was the pre-Cambridge pen pal and discussion partner that MS 13 makes him out to have been, he should have figured prominently in Chapter 3 of *Passages*, devoted to Babbage's schooldays. There, however, the only friend who merits a lengthy mention (pp. 14–16) is the mathematically insignificant Frederick Marryat. Here, again, at a point where the two accounts should have nicely meshed, they diverge completely.

But which is the more credible? MS 13 gives no account of the circumstances of Babbage's acquaintance with Higman. Nor does it mention his age. But we know it. Higman was Babbage's junior by two years. In August 1810 he was, therefore, sixteen. The following summer, Babbage claims to have engaged him in an extremely detailed correspondence concerning functional equations – a schoolboy of seventeen, still a whole year away from graduating from Kingsbridge grammar school. The unlikeliness of the Higman connection is all the more glaring when one also takes into account the social aspects of the *Passages* account. The portrayal of Slegg as a Trinity 'friend … enthusiastically attached to mathematical subjects', with whom the frustrated Babbage would take wine and discuss mathematics (p. 20), clearly suggests that Babbage was not alone in his mathematical interests and complaints. As noted, his satirical quip about the d's and the dots that inspired forming the Analytical Society would have been lost on anyone not yet convinced of the relative supremacy of Continental analysis. In such a context, Higman's role demands explanation. Why on earth would Babbage be communicating his most serious mathematical deliberations to a young schoolboy back home instead of to his newfound Cambridge comrades in arms?

With regard to functional equations per se, MS 13 portrays Babbage as someone mathematically self-made to whom his first year at Cambridge contributed nothing of note. The idea of a young mathematical confidant outside the university serves this picture well (as long as his age remains unmentioned), as does the following first blunt contradiction between the two accounts.

Purchasing Lacroix

According to *Passages*, Babbage's intensive and unruly self-fashioned study of Continental analysis during his first year acquired shape and structure with the purchase of Lacroix's three-tome work on the calculus, at the outset of his second year. In MS 13, however, Babbage claims to have purchased the work a year earlier, in October 1810, at the commencement of his first year, and to have 'devoted a considerable portion of [his] time to its perusal' (p. 7 [p. 25]).

This may not sound like a major inconsistency, but I believe that it is, and that it goes to the heart of the two accounts' conflicting agendas. The 'large Lacroix' provided a thorough and systematic survey and mapping of Continental analysis in unparallelled encyclopedic dimensions. It is not one of the works Babbage claims in *Passages* to have come upon before coming up to Cambridge, or to have stumbled upon in a Cambridge library. It is the kind of work one purchases either to serve as a reference book or to give oneself a systematic education. According to both accounts, the book clearly falls in the second category. In view of what had it cost him, it was a work he obviously knew he needed, and whose purchase he planned. The question is, at what point in his mathematical development could the *need* for such a work have been felt?

Many follow Hyman's correction of Buxton and amend the date stated in *Passages* to fit MS 13,[18] but they do so at the price of rendering the entire account of *Passages* unintelligible.[19] What incentive could the talented yet ill-schooled new student described in *Passages* have for purchasing such a work before receiving his first tutorial? Harboring a passion for algebra and an eclectic acquaintance with the d's, dashes and dots of the works he had spottily studied, Babbage of *Passages* describes himself as arriving at Trinity College in October 1810 in eager anticipation of having his difficulties swiftly removed by a university famous for its focus on mathematics. Why would someone like that, before enrolling at Trinity, go out of his way to chase down the large Lacroix, and at such an expense? A year later, having given up on the Cambridge system and realizing the vast supremacy of Continental analysis, a desire to substitute his tutors and college textbooks with Lacroix makes perfect sense.

The closer one studies the two accounts the clearer it becomes that they do not describe different aspects of the same story, but tell two different, and largely incompatible, stories. *Passages* tells a story of a gifted and enthusiastic Cambridge novice's transition first to student–rebel, then to university reformer. Of this not a trace can be found in MS 13, which tells the story of the awakening and self-maturation of the single-handed, self-made founder of the calculus of functions, in which no person or institution played a formative role, even negatively. The only other individual with whom Babbage

18 Buxton, op. cit. (4), juxtaposes the two accounts uncritically, giving 1811 as the purchase date when citing *Passages* (pp. 18, 21), and 1810 when citing MS 13 (p. 25). As editor, Hyman (Buxton, op. cit. (4), p. xv), overrules his author and sets the date consistently at 1810, including in his direct citations from *Passages* (p. 21).

19 Wilkes, op. cit. (4), p. 206; Grier, op. cit. (4), p. 20 n. 20; and Snyder, op. cit. (10), p. 30. Dubbey, op. cit. (17), p. 32, is more careful in this respect, but confuses the Lacroix purchased with the same author's *Traité élémentaire de Calcul différentiel et de Calcul intégral*, Paris: Duprat, 1802 (later editions published by Courcier), which he, Herschel and Peacock eventually translated.

will eventually share the stage is Herschel, whose contributions to functional equations are undeniable. But for Babbage to secure for himself the central founding role, Herschel's entry had to be postponed as far as possible, and his contributions rendered as far as possible derivative or limited to notation.

Viewed thus, the discrepancy regarding the purchase date of Lacroix goes to the heart of the matter. Given Lacroix's scarcity and cost, its purchase clearly proves that Babbage had acquired a sufficient grasp of the field not only to appreciate the work's value, but to realize that it was essential to his own mathematical development. Portraying himself reaching that point before coming to Cambridge is very different from picturing himself reaching it as a result of his frustrating first year. Rolling the purchase date back to October 1810 renders his first Cambridge year irrelevant to his mathematical development – at least with respect to the calculus and functional equations. Moreover, a Babbage who went out of his way to purchase Lacroix on his way up is a Babbage who arrived at Cambridge knowing that the university had little to offer him in that respect. The role MS 13 assigns to Lacroix is hence crucial to establishing Babbage as an original mathematician in the making, who long before meeting Herschel was laying the foundations of the field they would later work on together. The only drawback was that Lacroix's third volume addresses the topic of functional equations explicitly.[20] To allow the work to play its part without stealing any of the young Babbage's thunder, MS 13 tellingly insists that by August 1811 'I had not yet arrived at the third volume which contains the beautiful enquiries of Laplace concerning a particular class of functional equations of the first order', which in any case treats them in a manner 'totally different from the plan I have pursued' (p. 9 [p. 26]).

The Analytical Society

If in MS 13 the purchase of Lacroix had to be pushed as far back in time as possible, the formation of the Analytical Society, again, quite contrary to *Passages*, is delayed as far as possible – namely until very close to the end of Babbage's second year. But that is not the only difference between the two accounts. The setting, the motivation for its formation, its aim, and even the list of people involved, are all described very differently. Moreover, if in *Passages* the formation of the society is described as a natural outcome of his first-year experiences, in MS 13 it marks a dramatic turning point.

According to MS 13, it all began not at the commencement of his second year, as in *Passages*, but 'early in May 1812', namely very close to its end. Babbage obviously realized that the Bible Society parody was too well known a story not to be mentioned. He alludes to it briefly, but with no mention of d's, dots, or the sorry state of Cambridge mathematics. The entire episode is stripped of reformative incentive. Cambridge is

20 See, for example, John F.W. Herschel, 'On equations of differences and their application to the determination of functions from given conditions', in John F.W. Herschel and Charles Babbage, *Memoirs of the Analytical Society 1813*, Cambridge, 1813, pp. 65–114, 100 n.; Eduardo L. Ortiz, 'Babbage and French idéologie: functional equations, language, and the analytical method', in Jeremy J. Gray and Karen Hunger Parshall (eds.), *Episodes in the History of Modern Algebra (1800–1950)*, Providence: The American Mathematical Society, 2007, pp. 13–47, esp. 19–29; and Domingues, op. cit. (9), pp. 27, 200, 214 and 263.

criticized, but not for what it taught. Consequently, the Analytical Society is portrayed as a mathematical society par excellence.[21]

In *Passages*, long before mentioning the society, and bearing no relation to its formation, Babbage briefly recalls harbouring plans for 'the institution among my future friends of a chess club, and also of another club for the discussion of mathematical subjects' (p. 19). MS 13 makes much of this early 'scheme', describing it in detail as Babbage's one hope that was dashed by the university system. The complaint, however, was directed not against his teachers or the standard of the mathematics they taught, but against the way it was pursued merely as a means rather than an end:

> Previously to my first residence at Cambridge amongst the utopian schemes which I had projected was the establishment of a Mathematical Society, I shortly found however that such a plan was totally incompatible with the manners and customs of the place, and although I was myself much devoted to mathematical pursuits I scarcely remember to have met two individuals who viewed this science in any other light than as a path to University honors [*sic*] or whoever deviated into accidental originality. My hopes of forming a Society at such a place had entirely vanished I had not attempted it as I thought it impossible and I did not communicate it to any one lest it might appear ridiculous! (pp. 23–24 [p. 29])

Here, too, was the turning point owed to Slegg and Bromhead:

> Early in May 1812 on the occasion of Mr. Slegg's mentioning the disputes which at the time existed relative to the distributing the bible alone or the bible accompanied with comments. I suggested the plan of instituting a society for the distribution of the small Lacroix and drew up a series of resolutions which might be supposed to have been adopted at their first meeting [underlining in original].

Here, too, Slegg is reputed to have communicated the idea to Bromhead, who in turn is said to have 'entered into the scheme with [an] enthusiasm' (p. 24 [p. 29]), and to have invited a group of acquaintances 'most attached to mathematical subjects' to discuss the plan in his rooms on Thursday 7 May, where it was

> agreed that we should form ourselves into . . . the Analytical Society; and that its first meeting should be held on the following Monday the 11th of May; at this meeting several more members joined the society, rules and regulations were adopted, Mr. Herschel was chosen president, a room was engaged . . . and arrangements were made for the formation of a library to consist of everything related to the mathematical and physical sciences (p. 25 [p. 30]).

Bracketing the issues of dating, nothing so far seems inherently out of order. But the next paragraph signals a turn that defies credibility. For MS 13, the formation of the society marks the point at which Babbage the lone, self-trained, self-made, sole anticipator of the calculus of functions comes of age as the acknowledged founder of the field; the point at which his mathematical musings, formerly confined to slates, the fronts and backs of letters, and batches of scribbling paper, gain an appropriate public venue. The description of a Cambridge milieu so uninterested in pursuing mathematics for its own sake as to deem the very idea of such a society 'ridiculous' serves Babbage's self-portrayal to the left of the turning point perfectly. But to its right, a very different

21 Which explains why Enros, 'The Analytical Society', op. cit. (4), who relies heavily on MS 13, insists that the society was ultimately formed to promote research, not to reform Cambridge (e.g. Chapter 3).

Cambridge setting was required, to allow Babbage to begin to shine. In establishing the society, he writes,

> great emulation was excited amongst the members, a variety of original papers were presented . . . and numberless problems of considerable elegance were continually placed on its table, great facility was afforded for the acquirement of any information connected with the objects of pursuit, and the minds of the members were habituated to view the science in an enlarged and extensive point of view (p. 25 [p. 30]).

In a dramatic move credited (here too) to Bromhead, Babbage's Cambridge is transformed so radically as to defy belief. Is it possible for someone so deeply devoted to mathematics who genuinely harboured plans for a mathematics club to have spent almost two whole years in the company of such talented and knowledgeable mathematical enthusiasts and to have remained wholly unaware of their existence?

Taking all their discrepancies in the balance, the two accounts are clearly incompatible. We therefore face the triple task of (a) determining which of them (if any) to believe (i.e. which of them Babbage himself deemed true – or truer); (b) explaining his motives for framing the other; and (c) explaining his reasons for publishing the one and preserving the other in advertised, yet manuscript, form.

Enough has been said to confidently answer the first question. The cumulative effect of the central problematic elements of MS 13 that I have pointed out – the role attributed to Spence's book, the Higman correspondence, the purchase date of Lacroix, and the formation of the society and its Cambridge setting – leaves us little choice but to rule its account of these years far less credible than the alternative. For all its flaws and inaccuracies, the account given in *Passages* for these years proves incomparably more coherent and makes much better sense. What, then, was the considerably tweaked and manipulated story told by MS 13 meant to achieve? What did Babbage find so lacking in the *Passages* account to dismiss it so resolutely as the template for his early history of the calculus of functions? In what ways was MS 13 expected, then, to fare better? And, if it did, why refrain from publishing it after investing so much thought and effort?

The key to answering all of these questions lies, I propose, in what might seem the most puzzling discrepancy between the two accounts: their different dating of the formation of the society. Although they are related differently, the main elements of the rapid sequence of events leading to its official establishment are basically the same: the Bible Society poster, the meeting with Slegg, Slegg enlisting Bromhead, Bromhead assembling the core group, the core group deciding to form the society. Yet while *Passages* claims that the events took place in October or early November 1811, MS 13 insists they occurred six or seven months later, during the second week of May 1812. For the latter, the formation of the society marks the point of transition between Babbage's private self-schooling in the theory of functions and the beginning of his career as an active practitioner. Assuming that the earlier date is the correct one,[22] what is to be gained by changing it?

22 Which, as I have noted elsewhere, is supported by external evidence. See Schweber, op. cit. (10), pp. 59–60, citing Maule to Babbage, 16 January 1812, British Museum Add MS 37182, f3.

As noted, the one thing relevant to securing Babbage's unique role in the history of functional equations that is gained by postponing the formation of the society is that, given MS 13's account of the Cambridge scene, doing so necessarily postpones Herschel's entry into the story. Babbage is careful not to say so explicitly, but stressing 'the manners and customs of the place' and Bromhead's unique role in rounding up and first introducing the group strongly implies that he had not met Herschel prior to 7 May.[23] And Herschel, I shall argue, is the key to understanding Babbage's decision to produce the curiously distorted MS 13 account in the first place. But first to the text.

'The meetings of the Analytical Society', he writes, 'continued during the month of May and part of June when most of its members left Cambridge' (p. 27 [p. 30]). The society could not have achieved much during the four weeks or so before dispersing for the long vacation. Indeed, henceforth the society and its members are mentioned only incidentally, and Babbage's interactions with Herschel take centre stage.

Enter Herschel

The text's subtle oscillation between praising and playing down Herschel's role in the development of the subject is most revealing, all the more so as it is based on conversation reports of which no other record exists. Describing their supposedly first discussion of the topic, the tone is firmly set. 'During this time', i.e. from mid-May to mid-June 1812,

> I had frequent conversations with Mr. Herschel, one of these which took place in the Cloisters of Trinity College I particularly remember. I then explained my ideas respecting the Calculus of Functions and I mentioned many problems which could not be solved without its aid . . . (p. 27 [pp. 30–31]).

Herschel was Babbage's senior, the star of his year, president of the society, and mere months away from graduating senior wrangler and winning the Smith's prize, and yet the dashing Herschel was apparently no match for the Babbage of June 1812 portrayed here. With three years of intense self-schooling in Continental analysis supposedly under his belt, uncorrupted by the Cambridge system and 'devoted' to such works as Spence and the large Lacroix, and after the best of three years of grappling with functional equations, it comes as no surprise that their first meaningful meeting is described so one-sidedly. It is Herschel who calls on Babbage, and it is Babbage who does all the talking. There is no give and take between the two – that will come later – their first meeting is not an exchange, but an initiation, with Babbage introducing Herschel to the calculus of functions, its prospects and problems. Babbage can, therefore, claim credit for Herschel's entry into, and important contributions to, the field, which he does, as we shall see, at every opportunity.

Hence the dialectic: because Herschel's involvement in the field ultimately owes to Babbage, his contributions must be played up insofar as they can be shown to have

23 MS 13 describes Bromhead's list of invitees as 'those of *his* acquaintances who were most attached to mathematical subjects' (24, emphasis added).

grown from, owed to or even dovetailed with Babbage's preceding work. What makes this a tricky, and in some sense a tragic, undertaking is that as far as publication goes, Herschel beat Babbage to it at almost every juncture. On the other hand, it has to be said to Babbage's credit that at no point does he allow even the slightest resentment to creep into the narrative. If Herschel was dashing at Cambridge, by the time the two accounts were written up he was the most famous and admired scientist of his age. MS 13 makes subtle autobiographical use of the fact that Babbage's life-long friend, the great Herschel, collaborated with him so closely and fruitfully in laying the foundations of a field no one had envisaged as such before him. What his subtle, yet troubled, narrative sought to achieve was to give him the edge on Herschel, as the person to whom the field, which owed Herschel much, really owed him the most.

Space will not allow a comprehensive account of the entire text, but a few key moments in the narrative will suffice to make my point.

'Partial and simultaneous functions' (pp. 27–28 [p. 31])

'In the course of this or some other conversation' he had had with Herschel before leaving Cambridge that summer (for which, like the first, we only have Babbage's word), the idea of 'partial and simultaneous' functional equations was born. Again it is he who leads, explaining to Herschel 'the various orders of functions of one variable from which we were led to consider those of more than one variable'. Herschel, in response, is said to have offered his first contribution to the field by suggesting that 'if there is a function of two variables the second function may be taken relative either to the first variable or to the second in a manner somewhat similar to partial differentials', to which Babbage responded by distinguishing 'two different species of second and higher functions which would thus arise', partial or simultaneous, according to whether the second and higher functions are 'taken relative' to one or to both variables. Thus, notes Babbage, 'the origin of partial and simultaneous functions . . . arose from that intercourse between those who were engaged in the same pursuits, which was so much promoted by the establishment of the Analytical Society'.

The basic elements are all here: Babbage, founder of the field, explains a major aspect of it to Herschel, who makes a valuable suggestion, which inspires Babbage to make another. His reference to the society more than implies that the two men had not met, and could not have met, prior to its formation.

Inverse functions (pp. 28–30 [pp. 31–32])

Herschel's first independent contribution to the theory of functions noted by MS 13 is his 'particularly elegant' suggestion of the now familiar notational convention to denote the inverse of a given function:

> Having called on me one morning he observed that if we have an equation between x and y such as $y = \Psi x$ if we wish to express x in terms of y, it ought to be written thus $x = \Psi^{-1}x$ or in other words that the inverse of the function Ψ be expressed by Ψ^{-1}.

The narrative is again crafted with care. Herschel's notational proposal has survived independently, of course, but, as before, the details of their meeting and exchange rest on Babbage's testimony alone. Once again, it is Herschel who calls on Babbage, rather than vice versa, and it is Babbage who has the last word – first in giving Herschel's suggestion his warm approval, and then in carefully pointing out its predecessors:

> This method of denoting the inverse of any operation ... much resembles the well-known manner of denoting $\frac{1}{x}$ by x^{-1} which though now apparently trivial was at the time it was first observed by Dr. Wallis well worthy of his genius ... The advantages of this notation immediately struck me and I have uniformly employed it in all subsequent inquiries. I have however lately found that one part of it has been anticipated in a work entitled 'Essai d'Analyse Combinatiòre' by Burmâne 1803 in this work from the equation $fnfm = f^{n+m}$ the author deduces the consequence that if $y = fx$ then $x = x^{-1}y$ but it is somewhat singular that he neglects to deduce from it some of its most valuable conclusions and constantly uses such expressions as *arc (sine* $-\frac{x}{a}$*)* etc. instead of the more elegant ones which so immediately flow from the premises.

Establishing priority (pp. 32 ff.)

As noted, in terms of publication, Herschel beat Babbage to the mark almost consistently. By the time Babbage got round to submitting his masterly two-part 'An essay towards the calculus of functions' in 1815–1816, Herschel's 1813 'On the equations of differences', with its important third part 'On functional equations', had long been published, and with it several mathematical papers, all related to the solution of functional equations, which together earned him the youngest fellowship to date in the Royal Society.[24] Histories of the 'development and progress' of a field normally limit themselves to the public sphere – attributing priority according to publication date and gauging involvement and influence according to public citation, explicit and implied. Babbage's history is different. In order to secure his place as the person most centrally responsible for the early development of the field, two things were required: first, his communicated unpublished work – in letters, face-to-face conversation and papers read – had to be convincingly made public, and, second, the very first printed mention of the field as such, originally attributed jointly to himself and Herschel, had to be made his own.

In the course of the society's brief period of activity prior to the long vacation of 1812, Babbage and Herschel, according to MS 13, each communicated two memoirs to the society.[25] The text on page 31 is discontinued after the words 'the titles of those of Mr. Herschel were', and resumes on the following page with lengthy extracts from the first of Babbage's own two papers, which are introduced thus:[26]

> The title of the first paper of mine is 'solutions of two problems requiring the application of mixed differences.' The solution of the first problem in this paper formed the basis of the first

24 See especially John F.W. Herschel, 'On trigonometrical series, particularly those whose terms are multiplied by the tangents, cotangents, secants, &c. of quantities in arithmetic progression, together with some singular transformations; with notes relating to a variety of subjects connected with the preceding memoir', in John F.W. Herschel and Charles Babbage, *Memoirs of the Analytical Society 1813*, Cambridge, 1813, pp. 33–64; 'On a remarkable application of Cotes's theorem', *Philosophical Transactions* (1813) 103, pp. 8–26; and Herschel, op. cit. (20).

25 This alone renders the MSS 13's dating of the formation of the society quite unrealistic.

26 The entire episode, pp. 32–37, is conveniently skipped by Buxton, op. cit. (4).

> general solution of a functional equation of the second order which was discovered by Mr. Herschel nearly a year and a half after (p. 32).

This he does at length, deferring 'the remarks I have to offer on this problem until the order of time shall bring me to speak of Mr. Herschel's first solution of the equation $\Psi^2 x = x$' (p. 34), which occurs thirty or so pages later with reference to Herschel's first paper communicated to the Royal Society. Babbage's text is telling and I shall cite it at length. Despite being the story of perhaps Herschel's most lasting and significant contribution to the field, the setting, as before, leaves no doubt as to who commanded authority at the time, and who sought whose approval.

> One morning I believe early in February [1814, MF], Mr. H called on me and stated that he just overcome one of our difficulties and had arrived at the solution to the equation $\Psi^2 x = x$. I immediately requested him to show me the method which he had employed to obtain the object we had both bestowed so much time upon when he gave me the following solution, which I shall extract from a paper of his in the Transactions of the Royal Society where it appeared soon after (p. 63 [p. 37]).

The lengthy extract amply proves the quality and importance of Herschel's solution; the only question left is that of its origin, which Herschel himself is reputed to have settled decisively:

> After expressing my surprise and admiration at this unexpected result I enquired what had led Mr. Herschel to his solution, who directly referred me to a paper of mine communicated to the Analytical Society about twelve months before entitled 'Solutions to two problems requiring the application of mixed differences' which has already been noticed on page 32 of this history and if the reader will compare my solution of the problem there proposed with that of Mr. Herschel he will perceive the almost perfect identity in the manner of accomplishing them . . . (p. 65 [partially cited p. 37]).

Babbage's little dance between the result being at once 'unexpected' and 'almost perfectly identical' to one of his own notwithstanding, once the question of priority had been settled, praise can be richly bestowed:

> We are then indebted to Mr. Herschel for the first general (perhaps I may say complete) solution of functional equations of the second order: it was indeed a considerable step and I well remember the satisfaction with [which] I beheld it at the time it was first produced (pp. 65–66 [pp. 37–38]).

The publication of Herschel's paper is a foundational turning point in the history of the field. Babbage's praise is wholly justified, but his attempt to partially turn it into self-praise is less compelling.[27] Again, despite the confident tone, and the fact that Herschel is reported as the first to acknowledge Babbage's priority, as before, we only have Babbage's word to go on. Indeed, although the reported conversation supposedly took

27 By contrast, the credit granted to Herschel in Babbage, op. cit. (13), pp. 393–395, is unqualified: Herschel's 'excellent paper on functional equations' (p. 394) is praised as correcting an oversight of Babbage's, and extending Laplace's 'peculiarly elegant' reduction of functional equations of the first order to those of finite difference, in a manner 'equally elegant and quite general'.

place long before Herschel's paper was submitted, no such acknowledgement was volunteered in the printed version.[28]

The *Memoirs* (1813)

The problem of taking the edge off Herschel's pioneering *Philosophical Transactions* papers applied even more acutely to his contributions to the sole volume of *Memoirs of the Analytical Society* of 1813. MS 13 devotes over twenty pages (pp. 39–60 partly cited [pp. 34–37]) to the *Memoirs*, which are mentioned in *Passages* only in passing (p. 21). The volume comprised an extended preface and three memoirs: 'On continued products', written by Babbage, and 'On trigonometrical series; etc.' and 'On equations of differences, etc.' written by Herschel. As noted, Part III of the last presents a systematic and general account of functional equations as equations of finite differences of the first order late of Laplace,[29] extended to functions of any number of variables, which it declares 'complete'.[30]

In MS 13, Babbage begins his account of the *Memoirs* by noting who wrote each of the four items and the precise chronology of their composition:

> The volume . . . consisted of a preface and three memoirs. The first which treated of continued Products was written first in the order of time . . . As soon as this paper was written out I employed myself in collecting materials and arranging them in the form of a preface which should comprise a brief outline of the history of pure analysis. Mr. Herschel who had left Cambridge for a short time occupied himself in preparing the second memoir (pp. 39–40 [p. 34]).

MS 13's adamant attribution of the preface to Babbage alone (reiterated again on page 47) is highly doubtful in view of the manuscript copy held at St John's College, which, as Schweber points out, is in Herschel's hand with corrections in someone else's.[31] Indeed, in the 'List of Mr. Babbage's printed papers' appended to *Passages*, Babbage lists the preface as written 'jointly with Sir John Herschel' (p. 372) – another interesting discrepancy between the two accounts, not an accidental or frivolous one, however.

Having testified that the preface was written before Herschel's second memoir, attributing its authorship to Babbage becomes crucial to securing his position as founder and prime visionary of the field whose history MS 13 purports to track:

> The preface . . . contains some account of the progress which had been made in the solution of functional equations of the first order and it also promised the necessity of a new method of

28 MS 13 goes on the describe an additional method of Herschel's for solving $\Psi^2 x = x$ again as an equation of finite differences. And, again, credits Herschel not for discovering, but for realizing, the viability of an approach they had both previously dismissed. Similarly, the 'very ingenious mode' of solving the equation $\Psi^n x = x$, proposed by Herschel 'about this time', is described as having been anticipated more generally by Maule in a letter of 15 May 1814 (pp. 67–76 [pp. 38–39]). See also Augustus De Morgan, *A Treatise on the Calculus of Functions (Extracted from the Encyclopaedia Metropolitana)*, London: Baldwin and Cradock, Paternoster Row, 1836, p. 14; and Grattan-Guinness, op. cit. (4), p. 38.

29 Herschel and Babbage, op. cit. (20), pp. 97 ff.

30 Herschel and Babbage, op. cit. (20), p. 111.

31 The annotated copy of the *Memoirs* Babbage presented to his son also attributes the preface to himself alone, as does the information he apparently conveyed to Ivory in campaigning for the East India College chair of mathematics in 1816. Schweber, op. cit. (10), pp. 60–62.

> investigation whose object would be the determination not of quantity, but of the form assumed by functions submitted to given conditions: as this passage contains *a very general view of the subject*, and is, as far as I am acquainted, *the first on that subject which points out the immense extent of the enquiries which it opens to us*, I shall extract the whole of it (pp. 48–49 [p. 35], emphasis added).

After copying out pages xvi–xviii of the preface, Babbage summarizes its significance – tellingly in the first person:

> In the passage *I* have stated *my* views relative to the Calculus of functions in the most general terms which could be employed. *I* have stated it to be the determining functions from given conditions of any nature whatever – those which *I* had more immediately in view at that time were equations of higher orders than the first, those which involved definite integrals, and those which contained the unknown function of a constant quantity ... (pp. 52–53 [p. 36], emphasis added).

The problem is that just at the point in which the preface thus defines and describes the object of the calculus of functions, it explicitly locates its 'first mention' in 'one of the papers which compose the present volume' (p. xvi) – thus giving the distinct impression that the preface was written, as prefaces normally are, only after the three memoirs were submitted. In copying out the passage, Babbage places the self-defeating sentence in large parentheses (p. 50), declaring that it was not part of the original text, but 'added at Mr. Herschel's desire' after the event with reference 'to the investigations at the latter end of his last memoir' (p. 53).

Back to Spence

The closest Herschel came to giving his version of stages in the development of the calculus of functions is in the 'Preface to the essays' and especially the 'Note on essay II' he appended to the posthumous edition of Spence's mathematical essays that he edited in 1819.[32] Herschel's Preface describes Spence's above-mentioned paper of 1809,[33] reprinted as Essay I of the collection, as 'given' by its author 'as a specimen of a much more extensive design ... which should embrace in one view the whole system of analytical operations, and of which the groundwork is there in part traced out'. Although many of the essays and fragments left by Spence 'bear relation to such scheme, and seem marked to take their place in its arrangement', none of them provide, according to Herschel, a 'clear and general exposition of the ulterior principles' on which such a 'design was to be conducted' (p. xxvii). Nonetheless, he notes, 'whenever the inverse process, the discovery of the function from the equation, is under contemplation', Spence appears 'to have begun to feel the want of, and would undoubtedly soon have endeavoured to supply' (p. xxix), the 'very interesting and hitherto unexplored branch of the functional calculus' (p. 152) – which Herschel's 'Note on Essay II' purports to

32 William Spence, *Mathematical Essays by the late William Spence*, edited by J.F.W. Herschel, Edinburgh: Oliver and Boyd, 1819, pp. xxvii–xxxii and 151–170 respectively.

33 *An Essay on the Various Orders of logarithmic Transcendents; with an Inquiry into their Applications to the Integral Calculus, and the Summation of Series*, London and Edinburgh: John Murray and Archibald Constable, 1809.

outline.[34] Spence considered functions that satisfy the condition $\Phi x = \Phi \frac{1}{x}$ but, according to Herschel, not only deemed the 'general resolution of this functional equation' to be 'a thing utterly hopeless and impracticable' (p. 152), but failed to realize its relation to the general class of second-order functional equations for the form $\Phi^2 = x$, whose 'complete solution' Herschel proceeds to sketch along the lines set forth in his own *Philosophical Transactions* paper of 1814 (p. 153).[35]

Herschel then goes on to consider the class of 'periodic functions' that satisfy functional equations of any order *n*: $\Phi^n x = x$. He refers the reader to 'two excellent papers By Mr. Babbage in the Transactions of the Royal Society' that offer 'a copious enumeration' of the relevant functions, 'as well as general methods of resolving the equations' (pp. 153–154).[36] He then turns to the more complicated question of the class of functional equations satisfied by symmetrical functions (pp. 157 ff.) – the reasoning and notation of which he attributes again to Babbage (p. 158)[37] – 'whose general solution' he obtained 'by a most singular and ingenious consideration' (p. 159). Herschel, however, purporting to provide 'not only a *general* but *the complete* solution' (p. 163),[38] offers a different route that rests 'on other grounds' (p. 160, original italics), arriving at a theorem that he deems to be 'the first step which has been made in' the 'much wanted' 'theory of the number and nature of the arbitrary functions which enter in to the solutions of functional equations in general' (p. 163).

Herschel's brief account of the field resembles Babbage's, but reverses their roles: it is Babbage who contributes an apt terminology and effective notation, and contributes highly original, if limited, solutions to specific classes of functional equations. But, in Herschel's account, it is he who provided the novel major step toward the complete solution of all such equations; it is he, taking a different tack than Babbage, who provided the foundation for the new 'branch of analysis' that 'Mr. Spence seems to have begun', starting in his 'to feel the want of' (1809). It is not surprising, therefore, that MS 13 would want not only to reverse Herschel's narrative, but to insist, fantastically, that Spence's paper had been formative of its author's own thinking from the day it was originally published.

34 On 30 January 1817 Herschel had written to Babbage asking that they meet because 'Spence's papers have set me mad. After wading through immense heaps of trash . . . I . . . struck upon an unfinished Essay full of the most beautiful properties of strange transcendents of the form

$$\int \frac{dx}{x} \int \frac{dx}{x} \int \frac{dx}{x} \ldots \int dx \Phi(x)$$

. . . I devoured the Essay with avidity – the field it opens is immense'. Royal Society of London, Correspondence of Sir John F.W. Herschel: Herschel to Babbage: (f) HS.20.38, 30 January 1817.

35 'Consideration of various points of analysis', *Philosophical Transactions* (1814) 104, pp. 440–468.

36 Babbage, op. cit. (13); and Part II, *Philosophical Transactions* (1816) 106, pp. 179–256.

37 This time to Babbage, 'Observations on the analogy which subsists between the calculus of functions and other branches of analysis', *Philosophical Transactions* 107 (1817), pp. 197–216.

38 With explicit reference 'to the distinction between these terms in Mr. Babbage's second paper, prob. xxviii'.

Writing for posterity

The composition of MS 13 required considerable time and effort. It spans almost three hundred pages and required copying numerous real and imagined extracts from a considerable personal archive of letters, notes and drafts, and reconstructing conversations that took place many years earlier. Even from the little we have seen, it is quite evident that it was crafted with great subtlety and care for detail. It is also clear what it aimed to achieve, and to what extent Babbage was willing to bend the truth to achieve it.

But why? Babbage's place in the history of functional equations was secure from the start. Once his two-part, 111-page 'Essay on the calculus of functions' was published, and especially after it was joined by his 1820 set of examples, it became clear that unlike Herschel, who treated the topic as an application of finite differences, it was Babbage who established it as an independent 'calculus' in its own right. De Morgan's definitive *Treatise on the Calculus of Functions* of 1836 points explicitly to 'Mr. Babbage's papers in the Philosophical Transactions, and the elementary treatise of examples' without which the field could not have been said to 'exist in a defined form'.[39] What was to be achieved by going to all the trouble of compiling MS 13? As noted above, I believe that the key to Babbage's motives concerns Herschel, but in a broader and more general sense than we have so far considered.

Sir John Herschel was regarded as the scientific sage of his era, the ultimate and iconic "gentleman of science", to use Morrell and Thackray's fitting term. As noted, his early mathematical papers earned him an unprecedented early fellowship in the Royal Society – more or less corresponding with his graduation in 1813. Over the years, his scientific and mathematical work earned him three Royal Medals (in 1833, 1836 and 1840) and two Copley Medals (1821, 1847). When he died in May 1871, his unique scientific reputation and standing were richly confirmed by the splendour of the national funeral he was given and his burial next to Isaac Newton at Westminster Abbey. His obituary in the *Proceedings of the Royal Society* stated that in the death of Herschel 'British science has sustained a loss greater than any which it has suffered since the death of Newton, and one not likely to be replaced'.[40]

And yet there was something paradoxical about the status he achieved. Herschel was a brilliant scientist and prolific writer. His numerous contributions to and accounts of mathematics, chemistry, optics, astronomy and photography were impressive, and his *Preliminary Discourse on the Study of Natural Philosophy* was influential and widely read. And yet none of his lasting contributions to the several fields he mastered profoundly challenged or changed them. He represented science perfectly; as Walter Canon famously put it, to be a scientist at the time was to be 'as much like John Herschel as possible',[41] but nothing he did came close to the groundbreaking achievements of

39 De Morgan, op. cit. (28), p. 39.

40 Written by Sir Thomas Romney Robinson, *Proceedings of the Royal Society of London* (1872) 20, pp. xvii–xxiii

41 Walter F. Cannon, 'John Herschel', *Encyclopedia of Philosophy* (ed. P. Edwards), New York: Collier and Macmillan, vol. 3, 1967.

Faraday, Lyell, Darwin, Hamilton, Maxwell or even Whewell, whose work both on the tides and in the history and philosophy of science set their respective fields on an entirely new footing.[42] Herschel had an inspiring scientific presence and charisma, but somehow lacked the creative vision and leadership, and perhaps also the critical reflection, that makes for great scientific innovation. And yet, and perhaps because he avoided ever seriously challenging the accepted norms of his many areas of expertise, he enjoyed the kind of scientific reputation in his lifetime that his inevitably controversial more innovative friends would at most enjoy in retrospect.

But because he commanded such unqualified respect, Herschel, like the scientific sages of *New Atlantis*, lent a respectability to science of which his philosophical hero, Francis Bacon, could have only dreamt.[43] Maintaining Herschel's reputation and standing was, therefore, in the vital interest first and foremost of his less conservative colleagues whose life's work was dependent on the kind of governmental and public support Herschel's image helped to ensure. On the other hand, in an age of such deep revolutionary rethinking, the disparity between Herschel's standing and actual accomplishments was bound to be felt by the very colleagues who most needed him. Once Whewell's mature philosophy of science began seriously to take form during the mid-1830s, the admiration he had so profoundly felt for Herschel since their 'undergraduateship' twenty years earlier can be shown to have considerably waned.

Babbage, needless to say, belonged squarely on the less conservative side of the scientific divide, alongside the Darwins, Maxwells and Whewells of his generation, but with one major exception. Unlike the rest of this illustrious peer group, by the late 1860s, and regardless of the exceedingly more favourable verdict that would eventually be handed down by future historians, Babbage had come painfully to realize that the one project to which he had devoted himself obsessively was a failure.[44] Laura Snyder captures the tragic dimensions of Babbage's last years beautifully, as the truth regarding his own scientific standing gradually sank in against the growingly disturbing backdrop of Herschel's disproportional 'high priest'-like fame.[45] Unable to attend Herschel's funeral, Babbage drafted a letter to Lady Margaret lamenting 'the loss of one of the earliest and most valued of the friends of my youth'.[46] It was the last letter he would

42 On Whewell's 'tidology' see Michael S. Reidy, *Tides of History: Ocean Science and Her Majesty's Navy*, Chicago: The University of Chicago Press, 2008, Chapter 4; and Snyder, op. cit. (10), Chapter 7.

43 Steven Ruskin, *John Herschel's Cape Voyage: Private Science, Public Imagination and the Ambitions of Empire*, Burlington: Ashgate, 2004, follows Walter F. Cannon, 'John Herschel and the idea of science', *Journal of the History of Ideas* (1961) 22, pp. 215–237, in claiming that Herschel's 'near deification' and rise to 'living legend' came as a result of his voyage to the Cape. James A. Secord, *Victorian Sensation: The Extraordinary Publication, Reception, and Secret Authorship of* Vestiges of the Natural History of Creation, Chicago: The University of Chicago Press, 2000, attributes his standing to the 'ideal of the Christian Philosopher' presented by the *Preliminary Discourse*.

44 See Snyder's touching description of his failure to interest anyone to attend his lecture on the Analytical Engine at the 1869 meeting of the British Association, but for 'two American gentleman'. Snyder, op. cit. (10), p. 356.

45 The term is Juliet Margaret Cameron's, cited by Snyder, op. cit. (10), p. 349.

46 Babbage to Margaret Herschel, draft, May 1871, British Library 37,199 f.537, cited in Snyder, op. cit. (10), p. 410 n. 67.

write. Babbage, however, 'could not resist one last bitter comment, recalling', as Snyder puts it, 'the opportunities his friend had enjoyed that were never open to him':

> The effect of the possession of an illustrious name, will open for your children paths inaccessible to others less fortunately born and will doubtless lead them to arrive at eminence in whatever line their tastes may induce them to pursue.

Snyder rightly characterizes Babbage's emotional state as 'sad, envious, and slightly curmudgeonly',[47] but I believe the source of the ill-spirited sadness and envy he had come to feel towards Herschel ran deeper than the unfairness of his friend's privileged birthright. Babbage lived in an age in which it was possible for people of exceedingly more humble origins than his own to pursue their scientific 'tastes' and achieve 'eminence'. By the time Babbage was writing, the dramatic Cinderella stories of his youth, such as those of Faraday and Whewell, had become less and less uncommon. His father's name may well have paved Herschel's way into the Royal Society at an uncommonly early age, but Herschel had achieved everything else in his own right.

Even in his darkest moods, I doubt Babbage would have attributed the frustratingly low levels of the scientific reputation, standing and fame he enjoyed in comparison to Herschel to a lack of opportunity on his part, as his letter to Lady Margaret might imply. It was an age in which scientific opportunities were not given, but created and taken. The problem with Herschel is that he always chose to play safe, and, to an extent, to play to the gallery. Babbage did neither. Herschel played safe by consistently building on, rather than challenging, the work of others, and he played to the gallery because, in doing so, his work could shine in accord with well-established and widely accepted standards. He made lasting discoveries – especially in astronomy and optics – but none that challenged fundamentals. He was as deep, brilliant and knowledgeable as the very best of his generation, but in preferring never to take a risk, never to really stick his neck out, Herschel, people like Babbage were bound to think, systematically wasted the rich opportunities he had had to contribute centrally to the great scientific and mathematical upheavals of his time. And for Babbage, who, unlike his friends, failed to see through to completion his radically innovative lifetime project, Herschel's exaggerated fame became a growing eyesore. The most telling aspect of his last letter to Margaret Herschel is what is conspicuously missing from it. It laments the loss of an early and most valued friend, but says not a word about Herschel's achievements as a scientist or contributions to knowledge. He laments the passing of a friend, not of a lifelong valued fellow scientist or comrade-in-arms.

The way Herschel's astronomical work skilfully built on, continued and expanded that of his father was paradigmatic of his science, as was his similar, completely safe and completely conventional, hence philosophically insignificant, reworking of Bacon's philosophy of science.[48] In the historiographical terminology of his friend William Whewell, Herschel positioned himself consistently not in the epoch-making moments of

47 Snyder, op. cit. (10), pp. 353–354.

48 For a more detailed appraisal of Herschel's scientific and metascientific achievements, see my forthcoming *Creatively Undecided: Toward a History and Philosophy of Scientific Agency*, Chapter 7.

scientific advancement, but in their sequels. Babbage, by contrast, challenged conventions in comparable consistency. To keep with Whewell's idiom, the one field in which Babbage could rightly feel he had succeeded, and could rightly boast playing an epoch-making role, was the calculus of functions, which was also the field that lent his equally epoch-making, yet failed, calculating-machines project its solid theoretical grounding.[49] The aim of MS 13, I submit, was to secure for posterity his role as sole and successful founder of the calculus of functions, by consistently relegating Herschel's contributions to the status of sequels to his own epoch-making efforts.

As we have seen, it was a subtle and complex ploy. For while systematically undermining Herschel's contributions to functional equations by portraying him as always building on the work of others (for example, Laplace), or following Babbage's lead, MS 13 uses Herschel's lofty standing to boost Babbage's. But it was also a tragic ploy, especially in retrospect. Given the nature of the two men's different schooling, and different early 'tastes' for mathematics, their early relationship at Cambridge was most probably far too symmetrical and bilateral for the story Babbage wished now to tell to hold water. In order to retell the 'oral' and 'private' chapters of the early history of functional equations in a way that suited his purpose, the historical record had to be tweaked, reinterpreted and, at major junctures, untruthfully manipulated. To be convincing, certain details had to be downplayed, some added, and some changed: his reading of Spence, the Higman connection, the purchase date of Lacroix, his Cambridge life, the society's dates, his first acquaintance with Herschel and the nature of their meetings, the authorship of the Preface to the *Memoirs*, and so forth, which, when taken together, wholly distort the story of his Cambridge years. It was a story that, I believe, he believed was capable of restoring him to the place he deserved in the annals of nineteenth-century British science, while (or by) bringing Herschel convincingly down a peg or two. But it was one he could not afford to make public during the lifetime of any of its *dramatis personae*, especially Herschel.

This is why the work is described already in *Passages* as a 'small MS volume' – not as a book in the making, or a project long set aside, but as a work he sees fit to advertise, yet carefully leave in manuscript form with written instructions for preserving it at the Cambridge University Library. At some point, close to the end of his life, Babbage took an additional step towards making MS 13 posthumously public by entrusting it to Buxton, whom he asked to write his biography after his death.[50] But by a curious twist of fate, Buxton's biography, completed as we know it in 1880, was never published, and also remained in manuscript form until it was edited and published by Hyman in 1988.

Bracketing certain doubtful details, Chapters 3 and 4 of *Passages* told the truth about his Cambridge years, at least as Babbage recalled it. It was an account he could safely afford to publish in 1864 confident that many of those with whom he shared those formative years would read it with great interest and at least general agreement. MS 13, by contrast, knowingly diverged far more significantly from the truth in order to enhance

49 See Babbage, op. cit. (1), p. 327.

50 See note 6 above; and Anthony Hyman, *Charles Babbage: Pioneer of the Computer*, Princeton: Princeton University Press, 1982, p. 246.

Babbage's role in founding the calculus of functions by belittling Herschel's. This was an account he could not afford to publish in his lifetime. Babbage could not have read the obituary of Herschel that deemed him the 'Homer of Science' published after Babbage's death by Nathaniel S. Dodge for the Smithsonian annual report of 1871. But Babbage had undoubtedly read many others that sang Herschel's praise in comparably exaggerated idiom. Neither could he have read the obituaries that would be written for him just a few months later, and for the same reason. Yet for several years he was sufficiently self-aware and clear-sighted to anticipate the way they would praise his great originality while unable 'to restrain themselves', as Snyder puts it, 'from mentioning the gaping disparity between his great abilities and his great failures'.[51] MS 13, I submit, perhaps with the additional help of Buxton, was designed to put this right, at least to an extent; at least posthumously.

51 Snyder, op. cit. (10), pp. 358–359.